SCIENCE PROGRESS

# SCIENCE PROGRESS

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## THE GREAT STAR MAP

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The simpler name "star map" is here applied to the chart generally known as the "Astrographic Chart," because this latter conveys a suggestion of technicality which is absent from the project. What astronomers in different parts of the world are really about is the making of a large and much more detailed map of the stars than has hitherto been produced. The map is being made by photography; but though the word "astrographic" has been coined for use when photography is applied to the stars, the work does not involve much technicality that is not familiar to the users of an ordinary Kodak. In three details only does the work of the astronomer differ from that of the amateur photographer: he uses a much longer camera; he drives the camera by clockwork so that it may follow the stars; and he takes pictures at night instead of in the daytime. It may perhaps be added that he uses the light emitted by the stars, instead of photographing objects by the reflected light of the sun. But of these details more presently.

Let us first consider what is the nature of a map of the stars, as this differs somewhat in character from the maps of the earth's surface with which we are familiar. There is no question of finding our way, no question of delimiting property, no question of showing hills and valleys. A map of the stars is of a more monotonous character, being practically limited to showing the exact positions and the brightnesses of individual points of light. Maps of the stars may differ from one another in scale, in accuracy, and in completeness: in scale because we may show two given stars separated on the map either by a foot or by an inch, according to requirements; accuracy will have a tendency to be greater on the larger scale; and they may indicate either a few bright stars or many faint ones. We are familiar with the fact that there are only a few very bright stars, more of a degree less bright, more still of fainter stars; and the increase continues as the luminosity diminishes, long after they have ceased to be visible to our eyes, no limit being reached even by the longest exposures given with our largest telescopes. Completeness then can only be a relative term. It is at present impossible to think of giving all the stars in the sky; we can only settle to give all those brighter than a certain fixed standard.

The earliest maps of the stars were probably made for astrological purposes; later they were required for the use of sailors. But through all the centuries so little had been done towards making accurate maps that in 1674, when there arose a question of finding the longitude at sea by observations of the moon and stars, it was pointed out by Flamsteed that no sufficiently accurate maps or catalogues of the stars were available. King Charles II., to whom this information was brought, was thoroughly alarmed at the state of affairs and immediately said that he must have the omission rectified. Thus was Greenwich Observatory established. When asked who was to take charge of the Observatory, the King immediately replied that Flamsteed, who had pointed out the need of such an institution, was the man to put in charge. Modern observation of the positions of the stars may be said to have begun at this period. Greenwich took a great step forward half a century later, when Bradley was made the third Astronomer Royal and increased the accuracy of observation very considerably, so that his results have formed the basis of our knowledge of the positions of the stars to the present time. But Bradley and his successors for the most part confined their attention to the brighter stars, not concerning themselves with those much fainter than can be seen with the naked eye. There are two good reasons for this. In the first place, the number of stars required for the use of sailors is not large; indeed, sailors themselves use remarkably few, for only the brightest are suitable for observation by the small telescopes of their sextants. Indirectly, however, sailors depend upon the keeping of accurate time—Greenwich time is in use all the world over for determining longitude: and for keeping accurate time a much larger number of stars, called "clock stars," is required. These have had the first claim upon the attention of astronomers at our great observatories during a couple of centuries. A second reason for confining attention to these brighter stars arises from the limitations of instruments. The observations were generally made by watching the star cross the field of view, in which were certain spider lines for reference. Now these lines cannot be seen unless the field of view is illuminated, and a faint star is then lost in the illumination. In these days of electric light it is comparatively easy to adopt a new instrumental method, whereby the wires themselves (and not the background) are illuminated; they then appear as bright lines but are not sufficiently dazzling to obscure even a faint star, which can thus be observed as well as a bright one. But in former times this method had not been sufficiently developed and in any case the brighter stars were easier to observe. For these reasons therefore the fainter stars have not attracted attention until comparatively recently. One motive for studying them came with the discovery of the minor planets, which dates from the first day of the nineteenth century. It had been realised that there was a gap in the sequence of planets (as arranged in order of distance from the sun) between Mars and Jupiter. It was clear that there could not be any large planet in this position, for it would have been noticed; but there might be a small one, so search was made for it. The method of search was somewhat laborious. It was necessary to identify all the stars within a certain region in order that any strange body might be detected. It is now easy to accomplish this by taking a photograph of the region; but at the end of the eighteenth century no such compendious process was available; then the positions of individual stars were either patiently and laboriously measured one by one, or learned by the astronomer so that he could carry a picture of the region in his memory. In default of an actual material photograph he practically photographed the image on his own retina. It is astonishing to think how much was accomplished by this toilsome process. Not one only but hundreds of minor planets were discovered in this way, though not without difficulty and delay. Four were found at first in rapid succession and then came a long blank during nearly half a century, so that it seemed as though the number were complete: but though this view proved quite erroneous, it was only after a search of fifteen years that Hencke, an ex-postmaster of Driessen, was at last rewarded by another discovery. From that time the number has been extended almost continuously, so that we now know nearly seven hundred of these tiny bodies. From the circumstances attending the discovery and the subsequent observation of them has arisen one need for charting the places of the fainter stars. The easiest way to record the movements of these small bodies is to measure their distances from adjacent faint stars, and this is only satisfactory when we know the places of the stars themselves. This led astronomers to undertake the great work of charting the zone of the heavens called the Zodiac, in or near which all the planets move. Such an enterprise was started at Berlin early in the nineteenth century; another, initiated by Chacornac many years later was continued by the brothers Henry of Paris, who ultimately took the great step of employing photography in the work; and this led to the inception of the scheme we are now considering.

The introduction of the photographic method was at first fitful and tentative. Apparently the earliest attempts were made in America by the Bonds and by Rutherfurd. It is curious now to read of the difficulties in obtaining impressions of any but the brighter stars in the old days of wet plates. The wet plate of course was not nearly so sensitive as the dry plate; also it could only be exposed for a limited time before it dried up, and during such limited exposures only the brightest stars left an image upon it. Even the wildest hopes of these early pioneers in forecasting the future fell far short of what is now easily attainable: witness the following extract[1] from a letter of George Bond to the Hon. William Mitchell, Nantucket, dated from Cambridge (Mass.) July 6, 1857:

"As far as I am informed, the attempt to photograph the fixed stars by their own light has been made nowhere else up to the present date. The rumor of a daguerreotype of a nebula made in Italy some years since, was unfounded. . . .

"About seven years since (July 17, 1850) Mr. Whipple obtained daguerreotype impressions from the image of *a Lyræ*

[1] *Memorials of William Cranch Bond, Director of the Harvard College Observatory* 1840–59, *and of his son George Phillips Bond, Director of the Harvard College Observatory* 1859–65, by Edward S. Holden (Lemcke & Buechner, New York, 1897), p. 155.

formed in the focus of the great equatorial and subsequently from *Castor*, thus establishing a simple but not uninteresting fact—the possibility of such an achievement. On these occasions a long exposure of one or two minutes was required before the plate was acted upon by the light. . . .

"Messrs. WHIPPLE and BLACK recommenced their trials on other images (taken by the collodion process) in March of the present year and they are still in progress. . . . Could another step in advance be taken equal to that gained since 1850, the consequences could not fail of being of incalculable importance in astronomy. The same object *α Lyræ*, which in 1850 required $100^s$ to impart its image to the plate, and even then imperfectly, is now photographed *instantaneously* with a symmetrical disc fit for exact micrometer measurement. We then were confined to a dozen or two of the brightest stars whereas now we take all that are visible to the naked eye. Even from week to week we can distinguish decided progress. . . . At present the chief object of attention must be to improve the sensitiveness of the plates, to which I am assured by high authorities in chemistry there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh magnitude stars. It is reasonable to suppose that on some lofty mountain and in a purer atmosphere we might, with the same telescope, include the eighth magnitude. To increase the size of the telescope threefold in aperture is a practicable thing if money can be found. This would increase the brightness of the stellar images, say eightfold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude inclusive. There is nothing then so extravagant in predicting a future application of photography to stellar astronomy on a most magnificent scale.

. . . . . .

"P.S.—I find I have forgotten to allude to two important features in stellar photography—one is that the intensity and size of the images taken in connection with the length of time during which the plate has been exposed measures the relative magnitudes of the stars. The other point is that the measurements of distances and angles of position of the double stars from the plates, we have ascertained by many trials on our earliest impressions, to be as exact as the best micrometric work."

The letter is a remarkable one for the date. The three forecasts of improvement—increased sensitiveness in plates, larger instruments, and better climate—have all been realised within fifty years. There are two mountain observatories in California; there is a 40-inch lens, nearly three times the size of the 15-inch Harvard equatorial, at the Yerkes Observatory and two 5-foot mirrors represent an even greater advance; there has been also an enormous increase in sensitiveness of plates. It was in this last particular that Bond failed to allow sufficient play to his imagination, as instead of an increase represented by one stellar magnitude we have more than ten times that estimate. But Bond's discernment was otherwise so great that this slight failure may be pardoned. His post-script shows that he realised even thus early the accuracy of the photographic method, and in this his judgment agreed with that of L. M. Rutherfurd, who set to work to measure his photographs systematically and soon found that they recorded the positions of the stars more accurately than his own apparatus would measure them. He used a micrometer screw and found, though he had provided himself with the best one available, that its errors were sufficiently large to prevent his doing justice to the photographs, and he turned aside from his original project to the construction of a better screw. Ultimately he made a screw so accurate that his attention was again distracted towards the completest possible test of its accuracy. This he found in the ruling of very fine lines close together on metal—several thousands within an inch—the result being what is called a grating, which can be used like a prism to spread out light into a spectrum. This work was so engrossing that Rutherfurd never seriously returned to his original purpose of measuring his photographs, but many of them have been measured since and have shown clearly how correct was his judgment of the accuracy of the photographic method. In spite of this accuracy, however, the inconvenience of the wet plate long delayed serious use of the method for the determination of star places. Photographs of the sun were taken showing the spots (requiring only a momentary exposure); measures of spot positions were made on these and found satisfactory. But a sun spot is an irregular object having no very definite position and does not afford a very severe test of accuracy; consequently this work failed to draw the attention of astronomers to the full resources at their command.

The complete change in attitude came in a rather sensational manner on the appearance of the great comet of 1882. This comet, which was quite a respectable object in the Northern Hemisphere, was much more magnificent in the

Southern. The dry plate had by this time made photography easy and many members of the public who had recently become possessors of cameras essayed to photograph the comet; they found to their disappointment that the rotation of the earth carrying them and their cameras with it was sufficient to spoil their pictures. Thereupon Sir David Gill, then H.M. Astronomer at the Cape, invited one of them to come to the Observatory and to strap his camera to the equatorial telescope (which was fitted with clockwork to counteract the earth's motion); immediately some beautiful pictures of the comet were obtained, and not only of the comet but of the surrounding stars. The number of stars shown on the photographs was indeed striking, and attracted widespread attention. The late Dr. Common of Ealing, who had been constructing telescopes for himself, without however any definite intention of using them photographically, immediately turned them to this new purpose and obtained some beautiful pictures of nebulæ. The brothers Henry in Paris saw the possibility of substituting the new process for the immensely laborious method by which they had been making their ecliptic charts; but in their case the change could not be made so easily, as their telescope had been made for visual use and could not immediately be used photographically. The difficulty arises from the existence of numerous colours in white light, the colours with which we are familiar in the rainbow. When looking through a telescope with the eye we use chiefly rays nearly yellow in colour, whilst the photographic plate is sensitive to blue and violet. Now a lens cannot be constructed to focus all these rays at the same time and consequently for photography a new lens must be made which will focus the blue and violet light instead of the yellow. There are ways of avoiding this difficulty which may be briefly mentioned. In the first place if we use a mirror which brings the rays to focus by reflection, instead of a lens which combines them by refraction, no colour difficulty arises. (It was for this reason that Dr. Common was able to use at once for photography the reflecting telescope which he had originally built for eye observation.) Secondly, modern improvements in the construction of photographic plates have made them sensitive to yellow light under certain conditions, so that visual telescopes can be used to take photographs if a yellow screen cuts out the unfocussed blue rays, leaving only those for which the telescope has been properly focussed. When a suitable plate is then put behind the screen, pictures of the moon and stars can be and have been obtained quite as good as those obtained with a telescope specially made for photography. But in 1882 this had not been realised and the Brothers Henry saw no way of using the new and promising photographic method but to make a new lens specially adapted for it. This they set about with great skill and determination. After a few trials on small lenses they at last succeeded in producing a photographic lens of 13 inches aperture, a veritable triumph of optical workmanship at that time. They were of course amateurs at the work. Admiral Mouchez, the Director of the Paris Observatory, gave them every encouragement and put at their disposal such resources as he had available; but their workshop was after all a mere shed. I have often heard Dr. Common speak with amusement of his visit to the workshop which had turned out to the admiration of the world the first successful photographic refractor—the modest building and the humble appliances were so surprising. We are reminded of the simple apparatus with which great experimenters like Faraday have often achieved the most remarkable results.

It was the work of the lens thus produced by the Henrys that led directly to the inception of the project we are considering. The specimen maps of small regions of the sky which they soon obtained suggested the possibility of producing such maps for the whole sky. The work contemplated was no child's play. At least 10,000 maps would be required to cover the whole sky; and a labour of this magnitude was beyond the resources of a single observatory. Correspondence between Sir David Gill—under whose direction the comet photographs had been taken—and Admiral Mouchez, who had encouraged the work of the Henrys, led ultimately to the assembling of a great international Conference at Paris in 1887. It was a remarkable meeting, the first of its kind in the history of astronomy; and it has shown the way for subsequent gatherings which have already made their mark upon that history. Conferences of a similar kind have since been held in 1889, 1891, 1896, 1900; and after a long interval in 1909. On all these occasions the French have acted as hosts and have discharged these duties with a cordiality and hospitality that has never failed to impress their colleagues from the most distant parts of the world. It would be difficult indeed to imagine a more pleasing centre for our meetings than Paris or a nation more admirably adapted to play the part of hosts than the French; and they have been rewarded by an increasing success in the gatherings. At the last meeting it became clear that the assembly had developed from a mere collection of those interested in a particular project into an organisation of the world's resources for the promotion of the astronomy of position. The physical side of astronomy has recently been organised on somewhat similar lines (profiting no doubt by the example provided), and the existence of these two great organisations will have a notable effect in economising our labours in the future. In 1887 such an important outcome was scarcely anticipated: attention was then concentrated on the immediate task before the assembly, which was a difficult one in every way. Astronomers from distant quarters of the globe speaking different languages, none of them with much experience of photography or of its possibilities but most of them with opinions more or less formed, met together to try and secure unanimity, not only in generalities but equally in small details. We need not be surprised at some of the results. The discussions were, to say the least of it, animated. There are no universal rules for conducting such business and astronomers of one country were not familiar with rules in use elsewhere. It interested Englishmen, for instance, who are accustomed to have resolutions moved by any one rather than the chairman, to learn that this was by no means a universal rule. On the contrary, the chairman of the first conference considered it part of his duties to move all the resolutions. After listening to a discussion, he took it to be his function to summarise the sense of the meeting in a resolution which he put from the chair and in favour of which he held up his own hand. Unfortunately for his success his was sometimes the only hand held up and the discussion was necessarily resumed. Another feature of such discussions on the Continent is a little strange to our insular prejudices but might perhaps be adopted by us with advantage. Occasions sometimes arise when the collision of contrary opinions produces considerable heat and there is an obvious desire on the part of two gentlemen (or even more) to speak at the same time. On such occasions the chairman rings a bell and declares the sitting intermitted for a few minutes. What has been public discussion can now be developed as private conversation. Gentlemen of opposite views who have been addressing one another excitedly across the width of the room may now rush together and arrive at a better understanding at close quarters. The effect of such an opportunity soon becomes evident when after a few minutes' interval the chairman again rings his bell—a calm has succeeded to the storm and not infrequently it is possible to crystallise out a resolution.

Let us glance at one or two of the matters which had to be decided in 1887. The first and most important was the choice of an instrument or instruments—for it was a preliminary question whether the same pattern should be used by all those co-operating in the work. This preliminary question, however, was soon settled in the affirmative. All were to use similar instruments; and now what were they to be? Should they be reflecting telescopes as used by Dr. Common, refracting telescopes as made by the brothers Henry, or refracting telescopes of a different pattern and more closely similar to camera lenses as advocated by Prof. Pickering of Harvard?

The advantages of the reflector were that it was cheap and that it existed. It is cheap because there is only one surface to be polished. Reflectors used to be made of speculum metal polished to a concave form; such were, for example, the great telescopes of Sir William Herschel and of Lord Rosse: nowadays instead of metal we use glass silvered on the face (not on the back as in a domestic looking-glass): but in either case there is only one surface to be prepared optically. Now with lenses there are two, four, or even more surfaces, all of which must be optically true. Moreover the glass must be entirely free from blemishes; if there is a fault in the substance of the glass which forms a mirror it is behind the reflecting surface and may not spoil the image but a fault in the interior of a lens cannot fail to produce its effect. Hence a lens is always much more costly than a mirror of the same size and the greatest telescopes in the world have always been reflecting telescopes. Lord Rosse's 6-foot mirror has not yet been surpassed in size, although

Dr. Common and Dr. Richey have both succeeded in making mirrors of 5 feet and a mirror of no less than $8\frac{1}{2}$ feet diameter is proposed; but the largest *lens* in the world is the Yerkes of 40 inches. Hence it could not fail to impress the conference of 1887 that the more economical instrument would be a reflector; moreover several such reflectors were already in existence and could, so it was hoped, be utilised without further expense. Thus at Oxford there was a reflecting telescope, which Dr. De la Rue had presented to the University Observatory, with which Prof. Pritchard hoped to take a share in the great project: if it were decided to use a different pattern of instrument his hopes would be disappointed unless he could obtain the money necessary to purchase one of the adopted pattern.

As regards the two forms of refracting telescope, the refractor and the doublet, that advocated by Prof. Pickering was the more expensive and the less known. In the light of our modern knowledge of its advantages (especially for the purpose of covering a larger area of the sky at once) it is very strange to find so little in support of it in the accounts of the discussion. It seems to have been put aside almost at once, in spite of the letter urging its adoption from Prof. Pickering. The chief reason for this was undoubtedly lack of information as to the accuracy with which plates taken by such an instrument would give the places of the stars. Specimen photographs taken by the brothers Henry with the other form of refractor had been measured and shown to be very satisfactory, but there was no corresponding information about the "doublet" as this third form of instrument is now usually called. Hence the doublet was put aside from the start and the choice was made between the reflector and the simple refractor.

The decision fell upon the latter. The choice has proved to be a wise one and it is satisfactory to remember that it was made without any acrimonious discussion. This was largely due to Dr. Common himself, who might perhaps have been expected to lay stress on the particular advantages of his own special instrument. His experience however had impressed him rather with its defects, especially with its uncertainty. This uncertainty is not due to the instrument itself so much as to our fitful climate: the reflector is so seriously influenced at times by air currents and changes of temperature as to be an instrument of moods and Dr. Common has accordingly compared it, somewhat ungallantly, to the female sex. He himself took the initiative in recognising that the Conference should adopt for a work of such magnitude the more trustworthy refractor as made by the brothers Henry; this straightforward course had its due effect on the formulation of a decision. There are now therefore a score of such instruments scattered about the world, varying a little in non-essentials but all closely resembling one another in the size of the lens (which is 13½ inches in diameter) and in the focal length of the telescope (which is about 11½ feet). The focal length is actually defined to be that which represents one minute of arc by a millimetre on the photographic plate; and this relation is so useful that in cases where a larger telescope has been built, the relationship has been recognised by making the scale exactly twice the size. Dr. Common adopted the same focal length (of about 11 feet 6 inches) for his excellent mirrors of 30 inches aperture; with these recently the beautiful photographs of comets have been taken and their power of discovering faint satellites has also been shown.

Another very important decision taken by the Conference of 1887 had a rather curious history. It arose from the ignorance, at that time, of the behaviour of a photographic film and the fear lest it should shrink in drying or otherwise become distorted. Experience of photography generally—as for instance the taking of portraits or landscapes—was sufficient to show that such distortion was at any rate not large; but in astronomy we are concerned with very minute quantities and it was not known whether minute disturbances might not affect the relative positions of the images on the plate. Accordingly it was proposed to imprint upon each plate a series of accurately ruled cross-lines called a *reseau*. They were to be photographed on the plate before development by exposing it to an artificial light behind a silver matrix (a flat plate coated with silver ruled with such lines); on development the lines appear together with the star images and if the film has shrunk during any of the processes of development, fixing, washing, etc., these lines will have shrunk sympathetically and will be no longer straight or at exactly equal distances as they were in the matrix. We have now learned that such shrinkage is so very small as to be negligible, at any rate for the purposes of our star map; indeed, even in the most minute investigations it is easier to neglect the shrinkage as accidental in character than to investigate it. Accidental errors can be obviated by taking another plate (or a number of other plates) and so far as our present experience goes the whole series of plates is very unlikely to be affected by any common or systematic error. Hence the function assigned to the *reseau* was due to a misapprehension and it has never been used for the purpose originally proposed. Fortunately it has been of immense value in another way. The lines have served as reference marks in determining the places of the stars with facility. To measure the distance between one image and another we might have used a long screw to carry a microscope from one to the other, but it is better to compare the distance with a standard scale, using a screw to connect the stars with the ends of the scale; the latter method is to be preferred because it avoids the use of a great length of screw. Screws can now be made very accurately if necessary; Rutherfurd's work laid the foundations of such accuracy. But they are costly; their use over a large range takes time in turning the screw through many revolutions; and continual use is apt to wear away the screw and render it no longer accurate. Hence it is preferable to use the method of comparing with a scale; the *reseau* has practically supplied an accurate scale in both directions for the rapid measurement of star positions on the plate.

We may pause here to remark that the term "map" when applied to the present project must be used in a rather comprehensive sense. The scheme includes not only the pictorial representations on the plates or on any prints made from them but also the measurement of these plates and the publication of the measures of the individual stars. We can if preferred use a descriptive name for these measures. The printed books containing them are often called the Astrographic Catalogue as opposed to the prints which are the Astrographic Chart proper; but the whole project is really one and the same, although the usual process adopted in making a terrestrial map is here inverted. Surveyors of the face of the earth make careful measurements first and then plot them on a map and that was the method of astronomers before the days of photography. Now, however, we first take photographs and then measure them; but the project would be incomplete without full measures and charts. An illustration may be given of the risk involved in using one of these methods alone from the practice of Egyptian surveyors. They have been accustomed by centuries of tradition to enter their measurements of land in books without proceeding to make a map. It is only within the last few years that the Egyptian survey under Captain Lyons made maps for the first time of the landed property in Egypt; and when these beautiful maps were exhibited in Cairo thousands of landowners saw their property thus represented for the first time. When the maps came to be made the disadvantages of the old plan soon became apparent; some pieces of land had been recorded twice over while others had been omitted altogether. We can readily understand how this can happen in mere numerical records, though it is not so easy to understand how some individuals became reconciled to pay taxes as an annual consequence twice over; that some should have failed to resent their escape from taxes altogether is more intelligible.

*(To be continued)*

# THE BIOLOGICAL WRITINGS OF SAMUEL BUTLER AND THEIR RELATION TO CONTEMPORARY AND SUBSEQUENT BIOLOGICAL THOUGHT[1]

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In the reissue of Samuel Butler's works there has long been a gap; both stock and plates of *Unconscious Memory* had been destroyed in an accidental fire. As it was necessary to reprint the book, Mr. Streatfeild, Butler's literary executor, thought that it would afford a good opportunity for an introductory essay by a professed biologist, dealing with Butler's biological writings and his relation to biological thought during the last thirty years; and he requested me to undertake this work. I could not refuse so honourable a task; but no one can be more humbly aware how trying it is to find one's prose in the same covers as Butler's, and that too in front of it. Still, the mace-bearer who walks before the Chancellor, to do him honour, is yet not therefore regarded as immodest.

Samuel Butler's *Unconscious Memory* itself gives an invaluable lead; for it tells us (chaps. ii., iii.) how the author came to write the "Books of the Machines" chapters in *Erewhon* (1872), with its foreshadowing of the later theory, *Life and Habit* (1878), *Evolution, Old and New* (1879), as well as *Unconscious Memory* (1880) itself. His fourth book on biological theory was *Luck, or Cunning?* (1887).[2]

Besides these books, his contributions to biology comprise

[1] Written as an introduction to the reissue of *Unconscious Memory* and printed with slight alterations by kind permission of Mr. R. A. Streatfeild and Mr. A. C. Fifield.

[2] The dates are those given by Mr. H. Festing Jones in the Chronology of Butler's life prefixed to the "Extracts from the Notebooks" in the *New Quarterly Review*.

several essays, notably "The Deadlock in Darwinism" (*Universal Review*, April–June 1890), republished in the posthumous volume of *Essays on Life, Art, and Science* (1904), and, finally, some of the "Extracts from the Notebooks of the late Samuel Butler" edited by Mr. H. Festing Jones, now in course of publication in the *New Quarterly Review*.

Of all these, LIFE AND HABIT (1878) is the most important, the main building to which the other writings are buttresses or, at most, annexes. Its teaching has been summarised in *Unconscious Memory* in four main principles: "(1) the oneness of personality between parent and offspring; (2) memory on the part of the offspring of certain actions which it did when in the persons of its forefathers; (3) the latency of that memory until it is rekindled by a recurrence of the associated ideas; (4) the unconsciousness with which habitual actions come to be performed." To these we must add a fifth: the purposiveness of the actions of living beings, as of the machines which they make or select.

Butler tells (*Life and Habit*, p. 33) that he sometimes hoped "that this book would be regarded as a valuable adjunct to Darwinism." He was bitterly disappointed in the event, for the book, as a whole, was received by professional biologists as a gigantic joke—a joke, moreover, not in the best possible taste. True, its central ideas, largely those of Lamarck, had been presented by Hering in 1870 (as Butler found shortly after his publication); they had been favourably received, developed by Haeckel, expounded and praised by Ray Lankester. Coming from Butler, they met with contumely—even from such men as Romanes, who, as Butler had no difficulty in proving, were unconsciously inspired by the same ideas, "*Nur mit ein bischen ander'n Wörter*."

It is easy, looking back, to see why *Life and Habit* so missed its mark. Charles Darwin's presentation of the evolution theory had, for the first time, rendered it possible for a "sound naturalist" to accept the doctrine of common descent with divergence; and so given a real meaning to the term "natural relationship," which had forced itself upon the older naturalists, despite their belief in special and independent creations. The immediate aim of the naturalists of the day was now to fill up the gaps in their knowledge, so as to strengthen the fabric of a unified biology. For this purpose they found their actual scientific equipment so inadequate that they were fully occupied in inventing fresh technique and working therewith at facts—save a few critics, such as St. George Mivart, who was regarded as negligible, since he evidently held a brief for a party standing outside the scientific world.

Butler introduced himself as what we now call "The Man in the Street," far too bare of scientific clothing to satisfy the Mrs. Grundy of the domain; lacking all recognised tools of science and all sense of the difficulties in his way, he proceeded to tackle the problems of science with little save the deft pen of the literary expert in his hand. His very failure to appreciate the difficulties gave greater power to his work—much as Tartarin of Tarascon ascended the Jungfrau and faced successfully all dangers of Alpine travel, so long as he believed them to be the mere *blagues de réclame* of the wily Swiss host. His brilliant qualities of style and irony themselves told heavily against him. Was he not already known for having written the most trenchant satire that had appeared since *Gulliver's Travels*? Had he not sneered therein at the very foundations of society, and followed up its success by a pseudo-biography that had taken in the *Record* and the *Rock*? In *Life and Habit*, at the very start, he goes out of his way to heap scorn on the respected names of Marcus Aurelius, Lord Bacon, Goethe, Arnold of Rugby, and Dr. W. B. Carpenter. He expressed the lowest opinion of the Fellows of the Royal Society. To him the professional man of science, with self-conscious knowledge of his ideal and aim, was a medicine-man, priest, augur—useful, perhaps, in his way, but to be carefully watched by all who value freedom of thought and person, lest with opportunity he develop into a persecutor of the worst type. Not content with blackguarding the audience to whom his work should most appeal, he went on to depreciate that work itself and its author in his finest vein of irony. Having argued that our best and highest knowledge is that of whose possession we are most ignorant, he proceeds: "Above all, let no unwary reader do me the injustice of believing in me. In that I write at all I am among the damned."

His writing of EVOLUTION, OLD AND NEW (1879), was due to his conviction that scant justice had been done by Charles

Darwin and Alfred Wallace and their admirers to the pioneering work of Buffon, Erasmus Darwin and Lamarck. To repair this he gives a brilliant exposition of what seemed to him the most valuable portion of their teachings on evolution. His analysis of Buffon's true meaning, veiled by the reticences due to the conditions under which he wrote, is as masterly as the English in which he develops it. His sense of wounded justice explains the vigorous polemic which here, as in all his later writings, he carries to the extreme.

As a matter of fact, he never realised Charles Darwin's utter lack of sympathetic understanding of the work of his French precursors, let alone his own grandfather, Erasmus. Yet this practical ignorance, which to Butler was so strange as to transcend belief, was altogether genuine, and easy to realise when we recall the position of Natural Science in the early thirties, in Darwin's student days at Cambridge and for a decade or two later. Catastrophism was the tenet of the day: to the last it commended itself to his Professors of Botany and Geology, to whom Darwin held the fervent allegiance of the Indian *chela* to his *guru*. As Geikie has recently pointed out, it was only later, when Lyell had shown that the breaks in the succession of the rocks were only partial and local, without involving the universal catastrophes that destroyed all life and rendered fresh creations thereof necessary, that any general acceptance of a descent theory could be expected. We may be very sure that Darwin must have received many solemn warnings against the dangerous speculations of the "French Revolutionary School." He himself was far too busy at the time with the reception and assimilation of new facts to be awake to the deeper interest of far-reaching theories.

It is the more unfortunate that Butler's lack of appreciation of these points should have led to the enormous proportion of bitter personal controversy that we find in the remainder of his biological writings. Possibly, as suggested by George Bernard Shaw, his acquaintance and admirer, he was also swayed by philosophical resentment at that banishment of mind from the organic universe which was generally thought to have been achieved by Charles Darwin's theory. Still, we must remember that this mindless view is not implicit in Charles Darwin's presentment of his own theory, nor was it accepted by him as it has been by so many of his professed disciples.

We have already alluded to an anticipation of Butler's Unconscious Memory (1880) main theses. In 1870 Dr. Ewald Hering, one of the most eminent physiologists of the day, Professor at Vienna, gave an Inaugural Address to the Imperial Royal Academy of Sciences: "Das Gedächtniss als allgemeine Funktion der organisirter Substanz" ("Memory as a Universal Function of Organised Matter"). When *Life and Habit* was well advanced, Francis Darwin, at the time his frequent visitor, called Butler's attention to this essay, which he himself only knew from an article in *Nature*. Herein Professor E. Ray Lankester had referred to it with admiring sympathy in connection with its further development by Haeckel in a pamphlet entitled *Die Perigenese der Plastidule*. We may note, however, that in his collected essays, *The Advancement of Science* (1890), Sir Ray Lankester, while including this essay, inserts on the blank page[1]—we had almost written "the white sheet"—at the back of it an apology for having ever advocated the possibility of the transmission of acquired characters.

*Unconscious Memory* was largely written to show the relation of Butler's views to Hering's and contains an exquisitely written translation of the essay. Hering does, indeed, anticipate Butler, and that in language far more suitable to the persuasion of the scientific public. It contains a subsidiary hypothesis that memory has for its mechanism special vibrations of the protoplasm, and the acquired capacity to respond to such vibrations once felt upon their repetition. I do not think that the theory gains anything by the introduction of this even as a mere formal hypothesis; and there is no evidence for its being anything more. Butler, however, gives it a warm, nay enthusiastic, reception in his introduction and notes to the translation of the address, which bulks so large in this book; but points out that he was "not committed to this hypothesis, though inclined to accept it on a *prima facie* view." Later on, as we shall see, he attached more importance to it.

The Hering address is followed in *Unconscious Memory* by translations of selected passages from Von Hartmann's *Philosophy of the Unconscious* and annotations to explain the difference from this personification of "The Unconscious" as a mighty all-ruling, all-creating personality, and his own scientific recognition

[1] *i.e.* after p. 285: it bears no number of its own!

of the great part played by *unconscious processes* in the region of mind and memory.

These are the essentials of the book as a contribution to biological philosophy. The closing chapters contain a lucid statement of objections to his theory as they might be put by a rigid necessitarian, and a refutation of that interpretation as applied to human action.

But in the second chapter Butler states his recession from the strong logical position he had hitherto developed in his writings from *Erewhon* onwards; so far he had not only distinguished the living from the non-living, but distinguished among the latter *machines* or *tools* from *things at large*.[1] Machines or tools are the external organs of living beings, as organs are their internal machines: they are fashioned, assembled or selected by the beings for a purpose, so they have a *future purpose* as well as a *past history*. "Things at large" have a past history, but no purpose (so long as some being does not convert them into tools and give them a purpose). Machines have a Why? as well as a How?: "things at large" have a How? only.

In *Unconscious Memory* the allurements of unitary or monistic views have gained the upper hand, and Butler writes (p. 23):

"The only thing of which I am sure is, that the distinction between the organic and inorganic is arbitrary, that it is more coherent with our other ideas, and therefore more acceptable, to start with every molecule as a living thing, and then deduce death as the breaking up of an association or corporation, than to start with inanimate molecules and smuggle life into them; and that, therefore, what we call the inorganic world must be regarded as up to a certain point living, and instinct, without certain limits, with consciousness, volition, and power of concerted action. *It is only of late, however, that I have come to this opinion.*"

I have italicised the last sentence, to show that Butler was more or less conscious of its irreconcilability with much of his most characteristic doctrine. Again, in the closing chapter, Butler writes (p. 275):

[1] The distinction was merely implicit in his published writings but has been printed since his death from his "Notebooks," *New Quarterly Review*, April 1908. I had developed this thesis, without knowing of Butler's explicit anticipation, in an article then in the press: "Mechanism and Life," *Contemporary Review*, May 1908.

"We should endeavour to see the so-called inorganic as living in respect of the qualities it has in common with the organic, rather than the organic as non-living in respect of the qualities it has in common with the inorganic."

We conclude our survey of this book by mentioning the literary controversial part chiefly to be found in chapter iv. but cropping up elsewhere. It refers to interpolations made in the authorised translation of Krause's *Life of Erasmus Darwin*. Only one side is presented; and we are not called upon, here or elsewhere, to discuss the merits of the question.

"LUCK, OR CUNNING, as the Main Means of Organic Modification? An attempt to throw Additional Light upon the late Mr. Charles Darwin's Theory of Natural Selection" (1887), completes the series of biological books. This is mainly a book of strenuous polemic. It brings out still more forcibly the Hering-Butler doctrine of continued personality from generation to generation and of the working of unconscious memory throughout: it points out that, while this is implicit in much of the teaching of Herbert Spencer, Romanes and others, it was nowhere—even after the appearance of *Life and Habit*—explicitly recognised by them but, on the contrary, masked by inconsistent statements and teaching. Not Luck, but Cunning, not the uninspired weeding out by Natural Selection, but the intelligent striving of the organism, is at the bottom of the useful variety of organic life. And the parallel is drawn that not the happy accident of time and place, but the Macchiavellian cunning of Charles Darwin, succeeded in imposing, as entirely his own, on the civilised world a maimed, uninspired and inadequate theory of evolution wherein luck played the leading part; while the more inspired and inspiring views of the older evolutionists had failed by the inferiority of their luck and their failure in cunning. On this controversy I am bound to say that I do not in the very least share Butler's opinions; and I must ascribe them to his lack of personal familiarity with the biologists of the day and their modes of thought and of work. Butler everywhere undervalues the important work of elimination played by Natural Selection in its widest sense.

The "conclusion" of *Luck, or Cunning?* shows a strong advance in monistic views and a yet more marked development of the vibration hypothesis given by Hering and only adopted with the greatest reserve in *Unconscious Memory.*

"Our conceptions, then, concerning the nature of any matter depends solely upon its kind and degree of unrest—that is to say, on the characteristics of the vibrations that are going on within it. The exterior object vibrating in a certain way imparts some of its vibrations to our brain, but if the state of the thing itself depends upon its vibrations, it [the thing] must be considered as to all intents and purposes the vibrations themselves, *plus*, of course, the underlying substance that is vibrating. . . . The same vibrations, therefore, form the substance remembered, introduce an infinitesimal dose of it within the brain, modify the substance remembering and, in the course of time, create and further modify the mechanism of both the sensory and the motor nerves. Thought and thing are one.

"I commend these last two speculations to the reader's charitable consideration, as feeling that I am here travelling beyond the ground on which I can safely venture. . . . I believe they are both substantially true."

In 1885 he had written an abstract of these ideas in his Notebooks (see *New Quarterly Review*, 1910, p. 116) and as in *Luck, or Cunning?* associated them vaguely with the unitary conceptions introduced into chemistry by Newlands and Mendelejeff.

Judging himself as an outsider, the author of *Life and Habit* would certainly have considered this mild expression of faith — "I believe they are both substantially true" — equivalent to one of extreme doubt. Thus—

"The fact of the Archbishop recognising this as among the number of his beliefs is conclusive evidence with those who have devoted attention to the laws of thought that his mind is not yet clear"

on the matter in question (*L. and H.* pp. 25–6). To sum up: Butler's fundamental attitude to the vibration hypothesis was all through that taken in *Unconscious Memory.* He played with it as a pretty pet; he fancied it more and more as time went on; but instead of backing it for all he was worth, like the main theses of *Life and Habit*, he put a big stake on it—and then hedged.

The last of Butler's biological writings is the essay, The Deadlock in Darwinism, containing much valuable criticism of Wallace and Weismann. It is in allusion to the misnomer of Wallace's book, *Darwinism*, that he introduces the term "Wallaceism"[1] for a theory of descent that excludes the transmission of acquired characters. This, indeed, was the chief factor that led Charles Darwin to invent his hypothesis of pangenesis, which, unacceptable as it has proved, had far more to recommend it as a formal hypothesis than the equally formal germ-plasm hypothesis of Weismann.

The chief difficulty in accepting the main thesis of Butler and Hering is one familiar to every biologist and not at all difficult to understand by the layman. Every one knows that the complicated beings that we term "Animals" and "Plants" consist of a number of more or less individualised units, the cells, each analogous to a simpler being, a Protist—save in so far as the characters of the cell unit of the Higher being is modified in accordance with the part it plays in that complex being as a whole. Most people, too, are familiar with the fact that the complex being starts as a single cell, separated from its parent, or, where bisexual reproduction occurs, from a cell due to the fusion of two cells, each detached from its parent. Such cells are called "Germ-cells." The germ-cell, whether of single or of dual origin, starts by dividing repeatedly, so as to form the *primary embryonic cells*, a complex mass of cells, at first essentially similar, which, however, as they go on multiplying, undergo differentiations and migrations, losing their simplicity as they do so. Those cells that are modified to take part in the proper work of the whole are called tissue-cells. In virtue of their activities, their growth and reproductive power are limited—much more in Animals than in Plants, in Higher than in Lower beings. It is these tissues, or some of them, that receive the impressions from the outside which leave the imprint of memory. Other cells, which may be closely associated into a continuous organ or more or less surrounded by tissue-cells whose part it is to nourish them, are called "secondary embryonic cells" or "germ-cells." The germ-cells may be differentiated in the

[1] The term has recently been revived by Prof. Hubrecht and by myself (*Contemporary Review*, November 1908).

young organism at a very early stage, but in Plants they are separated at a much later date from the less isolated embryonic regions that provide for the Plant's branching. In all cases we find embryonic and germ-cells screened from the life processes of the complex organism, or taking no very obvious part in it, save to form new tissues or new organs, notably in Plants.

Again, in ourselves and to a greater or less extent in all Animals, we find a system of special tissues set apart for the reception and storage of impressions from the outer world and for guiding the other organs in their appropriate responses—the "Nervous System"; and when this system is ill-developed or out of gear the remaining organs work badly from lack of proper skilled guidance and co-ordination. How can we, then, speak of "memory" in a germ-cell which has been screened from the experiences of the organism, which is too simple in structure to realise them if it were exposed to them? My own answer is that we cannot form any theory on the subject. The only question is whether we have any right to *infer* this "memory" from the *behaviour* of living beings; and Butler, like Hering, Haeckel and some more modern authors, has shown that the inference is a very strong presumption. Again, it is easy to over-value such complex instruments as we possess. The possessor of an up-to-date camera, well instructed in the function and manipulation of every part but ignorant of all optics save a hand-to-mouth knowledge of the properties of his own lens, might say that *a priori* no picture could be taken with a cigar-box perforated by a pin-hole; and our ignorance of the mechanism of the psychology of any organism is greater by many times than that of my supposed photographer. We know that Plants are able to do many things that can only be accounted for by ascribing to them a "psyche," and these co-ordinated enough to satisfy their needs; and yet they possess no such central organ comparable to the brain, no highly specialised system for intercommunication like our nerve trunks and fibres. As Oscar Hertwig says, we are as ignorant of the mechanism of the development of the individual as we are of that of hereditary transmission of acquired characters, and the absence of such mechanism in either case is no reason for rejecting the proven fact.

However, the relations of germ and body just described led Jäger, Nussbaum, Galton, Lankester and, above all, Weismann, to the view that the germ-cells or "stirp" (Galton) were *in* the body but not *of* it. Indeed, in the body and out of it, whether as reproductive cells set free or in the developing embryo, they are regarded as forming one continuous homogeneity, in contrast to the differentiation of the body; and it is to these cells, regarded as a continuum, that the terms stirp and germ-plasm are especially applied. Yet on this view, so eagerly advocated by its supporters, we have to substitute for the hypothesis of memory, which they declare to have no real meaning here, the far more fantastic hypotheses of Weismann: by these they explain the process of differentiation in the young embryo into new germ and body; and in the young body the differentiation of its cells, each in due time and place, into the varied tissue-cells and organs. Such views might perhaps be acceptable if it could be shown that over each cell-division there presided a wise all-guiding genie of transcending intellect, to which Clerk-Maxwell's sorting demons were mere fools. Yet these views have so enchanted many distinguished biologists that in dealing with the subject they have actually ignored the existence of equally able workers who hesitate to share the extremest of their views. The phenomenon is one well known in hypnotic practice. So long as the non-Weismannian biologists deal with matters outside this discussion, their existence and their work is rated at its just value; but any work of theirs on this point so affects the orthodox Weismannite (whether he accept this label or reject it does not matter), that for the time being their existence and the good work they have done are alike non-existent.[1]

Butler founded no school and wished to found none. He desired that what was true in his work should prevail, and he looked forward calmly to the time when the recognition of that truth and of his share in advancing it should give him in the lives of others that immortality for which alone he craved.

Lamarckian views have never lacked defenders here and in America. Of the English, Herbert Spencer, who, however,

[1] See *Fortnightly Review*, February 1908, and *Contemporary Review*, September and November 1909. Since these publications the hypnosis seems to have somewhat weakened.

was averse to the vitalistic attitude, Vines and Henslow among botanists, Cunningham among zoologists, have always resisted Weismannism; but, I think, none was distinctly influenced by Hering and Butler. In America the majority of the great school of palæontologists have been strong Lamarckians, notably Cope, who has pointed out, moreover, that the transformations of energy in living beings are peculiar to them.

We have already adverted to Haeckel's acceptance and development of Hering's ideas in his *Perigenese der Plastidule.* Oscar Hertwig has been a consistent Lamarckian, like Yves Delage of the Sorbonne; these occupy pre-eminent positions not only as observers but as discriminating theorists and historians of the recent progress of biology. We may also cite as a Lamarckian—of a sort—Felix Le Dantec, the leader of the chemico-physical school in France to-day.

But we must seek elsewhere for special attention to the points which Butler regarded as the essentials of *Life and Habit.* In 1893 Henry P. Orr, Professor of Biology in the University of Louisiana, published a little book entitled *A Theory of Heredity.* Herein he insists on the nervous control of the whole body and on the transmission to the reproductive cells of such stimuli, received by the body, as will guide them on their path until they shall have acquired adequate experience of their own in the new body they have formed. I have found the name of neither Butler nor Hering, but the treatment is essentially on their lines and is both clear and interesting.

In 1896 I wrote an essay on *The Fundamental Principles of Heredity,* primarily directed to the man in the street. This, after being held over for more than a year by one leading review, was "declined with regret," and again after some weeks met the same fate from another editor. It appeared in the pages of *Natural Science* for October, 1897, and in the *Biologisches Centralblatt* for the same year. I reproduce its closing paragraph:

> "This theory [Hering-Butler's] has, indeed, a tentative character, and lacks symmetrical completeness, but is the more welcome as not aiming at the impossible. A whole series of phenomena in organic beings are correlated under the term of *memory, conscious and unconscious, patent and latent.* . . .

Of the order of unconscious memory, latent till the arrival of the appropriate stimulus, is all the co-operative growth and work of the organism, including its development from the reproductive cells. Concerning the *modus operandi* we know nothing; the phenomena may be due, as Hering suggests, to molecular vibrations, which must be at least as distinct from ordinary physical disturbances as Röntgen's rays are from ordinary light; or it may be correlated, as we ourselves are inclined to think, with complex chemical changes in an intricate but orderly succession. For the present, at least, the problem of heredity can only be elucidated by the light of mental, and not material, processes."

It will be seen that I express doubts as to the validity of Hering's invocation of molecular vibrations as the mechanism of memory, and suggest as an alternative cyclic chemical changes. This view has recently been put forth in detail by J. T. Cunningham in his essay on the "Hormone[1] Theory of Heredity," in the *Archiv fur Entwicklungsmechanik* (1909); but I have failed to note any direct effect of my essay on the trend of biological thought.

Among post-Darwinian controversies the one that has latterly assumed the greatest prominence is that of the relative importance of small variations in the way of more or less—"fluctuations"—and of "discontinuous variations," or "mutations," as De Vries has called them. Darwin attached more importance to the latter in the first four editions of the *Origin of Species* than in subsequent editions: he was swayed in his attitude, as is well known, by an article of the physicist, Fleeming Jenkin, which appeared in the *North British Review.* The mathematics of this article were unimpeachable but they were founded on the assumption that exceptional variations would only occur in single individuals, which is, indeed, often the case among those domesticated races on which Darwin especially studied the phenomena of variation. Darwin was no mathematician or physicist, and we are told in his biography that he regarded every tool-shop rule or optician's thermometer as an instrument of precision: so he appears to have regarded Fleeming Jenkin's demonstration as a mathematical deduction which he was bound to accept without criticism.

Mr. William Bateson, formerly Professor of Biology in the

[1] A "hormone" is a chemical substance which, formed in one part of the body, alters the reactions of another part, normally for the good of the organism.

University of Cambridge, as early as 1894 laid great stress on the importance of discontinuous variations, collecting and collating the known facts in his *Materials for the Study of Variations*; but this important work, now become scarce and valuable, at the time excited so little interest as to be "remaindered" within a very few years of publication.

In 1901 Hugo De Vries, Professor of Botany in the University of Amsterdam, published *Die Mutationstheorie*, wherein he showed that mutations or discontinuous variations in various directions may appear simultaneously in many individuals and in various directions. In the gardener's phrase, the species may take to sporting in various directions at the same time, and each sport may be represented by numerous specimens.

De Vries shows the probability that species go on for long periods showing only fluctuations, and then suddenly take to sporting in the way described, short periods of mutation alternating with long intervals of relative constancy. It is to mutations that De Vries and his school, as well as Luther Burbank, the great former of new fruit- and flower-plants, look for those variations which afford the material of Natural as of Human Selection. In "God the Known and God the Unknown," which appeared in the *Examiner* (May, June and July, 1879)—but though then revised was only published in 1909 post-humously—Butler anticipates this distinction:

"Under these circumstances the organism must act in one or other of these two ways: it must either change slowly and continuously with the surroundings, paying cash for everything, meeting the smallest change with a corresponding modification, so far as is found convenient, or it must put off change as long as possible, and then make larger and more sweeping modifications.

"Both these courses are the same in principle, the difference being one of scale, and the one being a miniature of the other, as a ripple is an Atlantic wave in little; both have their advantages and disadvantages, so that most organisms will take the one course for one set of things and the other for another. They will deal promptly with things which they can get at easily, and which lie more upon the surface; *those, however, which are more troublesome to reach, and lie deeper, will be handled upon more cataclysmic principles, being allowed longer periods of repose followed by short periods of greater activity.* . . . It may be questioned whether what is called a sport is not the organic expression of discontent which has been long felt, but which has not been attended to, nor been met step by step by as much small remedial modification as was found practicable: so that when a change does come it comes by way of revolution. Or, again (only that it comes to much the same thing), it may be compared to one of those happy thoughts which sometimes come to us unbidden after we have been thinking for a long time what to do, or how to arrange our ideas, and have yet been unable to arrive at any conclusion" (pp. 14, 15).[1]

We come to another order of mind in Hans Driesch. At the time he began his work biologists were largely busy in a region indicated by Darwin, and roughly mapped out by Haeckel—that of phylogeny. From the facts of development of the individual, from the comparison of fossils in successive strata, they set to work at the construction of pedigrees, and strove to bring into line the principles of classification with the more or less hypothetical "stem-trees." Driesch considered this futile, since we never could reconstruct from such evidence anything certain in the history of the past. He therefore asserted that a more complete knowledge of the physics and chemistry of the organic world might give a scientific explanation of the phenomena, and maintained that the proper work of the biologist was to deepen our knowledge in these respects. He embodied his views, seeking an explanation on these lines, filling up gaps and tracing projected roads along lines of probable truth in his *Analytische Theorie der organische Entwicklung*. But his own work convinced him of the hopelessness of the task he had undertaken and he has become as strenuous a vitalist as Butler. The most complete statement of his present views is to be found in *The Philosophy of Life* (1908–9), being the Gifford Lectures for 1907-8. Herein he postulates a quality (" psychoid ") in all living beings, directing energy and matter for the purpose of the organism, and to this he applies the Aristotelian designation " Entelechy. The question of the transmission of acquired characters is regarded as doubtful, and he does not emphasise—if he accepts—the doctrine of continuous personality. His early youthful impatience with descent theories and hypotheses has, however, disappeared.

In the next work the influence of Hering and Butler is definitely present and recognised. In 1906 Signor Eugenio

[1] Mr. H. Festing Jones first directed my attention to these passages and their bearing on the Mutation Theory.

Rignano, an engineer keenly interested in all branches of science, and a little later the founder of the international review, "Rivistà di Scienza" (now called *Scientia*), published in French a volume entitled *De la transmissibilité des Caractères acquis—Hypothèse d'une Centro-épigenèse.* Into the details of the author's work we will not enter fully. Suffice it to know that he accepts the Hering-Butler theory and makes a distinct advance on Hering's rather crude hypothesis of persistent vibrations by suggesting that the remembering centres store slightly different forms of energy, to give out energy of the same kind as they have received, like electrical accumulators. The last chapter, "Phenomènes mnémoniques Phénomènes vitales," is frankly based on Hering.

In *The Lesson of Evolution* (1907, posthumous and only published for private circulation) Frederick Wollaston Hutton, F.R.S., late Professor of Biology and Geology, first at Dunedin and after at Christchurch, New Zealand, puts forward a strongly vitalistic view and adopts Hering's teaching. After stating this he adds, "The same idea of heredity being due to unconscious memory was advocated by Mr. Samuel Butler in his *Life and Habit.*"

Dr. James Mark Baldwin, Stuart Professor of Psychology in Princeton University, U.S.A., called attention early in the nineties to a reaction characteristic of all living beings, which he terms the "Circular reaction." We take his most recent account of this from his *Development and Evolution* (1902):

> "The general fact is that the organism reacts by concentration upon the locality stimulated for the *continuance* of the conditions, movements, stimulations, *which are vitally beneficial*, and for the *cessation* of the conditions, movements, stimulations *which are vitally depressing.*" [1]

This amounts to saying in the terminology of Jennings (see below) that the living organism alters its "physiological states" either for its direct benefit, or for its indirect benefit in the reduction of harmful conditions.

Again:

> "This form of concentration of energy on stimulated locali-

[1] He says in a note, "This general type of reaction was described and illustrated in a different connection by Pfluger in *Pfluger's Archiv f.d. ges. Physiologie*, Bd. XV." The essay bears the significant title "Die teleologische Mechanik der lebendige Natur," and is a very remarkable one, as coming from an official physiologist in 1877, when the chemico-physical school was nearly at its zenith.

ties, with the resulting renewal through movement of conditions that are pleasure-giving and beneficial, and the consequent repetition of the movements is called 'circular reaction.'"

Of course the inhibition of such movements as would be painful on repetition is merely the negative case of the circular reaction. We must not put too much of our own ideas into the author's mind: he nowhere says explicitly that the animal or plant shows its sense and does this because it likes the one thing and wants it repeated, or dislikes the other and stops its repetition, as Butler would have said. Baldwin is very strong in insisting that no full explanation can be given of living processes, any more than of history, on purely chemico-physical grounds.

The same view is put differently and independently by H. S. Jennings,[1] who started his investigations of living Protista, the simplest of living beings, with the idea that only accurate and ample observation was needed to enable us to explain all their activities on a mechanical basis; and devised ingenious models of protoplastic movements. He was led, like Driesch, to renounce such efforts as illusory, and has come to the conviction that in the behaviour of these lowly beings there is a purposive and a tentative character—a method of "trial and error"—that can only be interpreted by the invocation of psychology. He points out that after stimulation the "state" of the organism may be altered, so that the response to the same stimulus on repetition is other. Or, as he puts it, the first stimulus has caused the organism to pass into a new "physiological state." As the change of state from what we may call the "primary indifferent state" is advantageous to the organism, we may regard this as equivalent to the doctrine of the "circular reaction" and also as containing the essence of Semon's doctrine of "engrams" or imprints which we are about to consider. We cite one passage which for audacity of thought (underlying, it is true, most guarded expression) may well compare with many of the boldest flights in *Life and Habit*:

"It may be noted that regulation in the manner we have set forth is what, in the behaviour of higher organisms, at

[1] "Contributions to the Study of the Lower Animals" (1904); "Modifiability in Behaviour"; and "Method of Regulability in Behaviour and in Other Fields," in *Journ. Exp. Zool.* ii. 1905.

least, is called intelligence. [The examples have been taken from Protozoa, Corals, and the lowest Worms.] If the same method of regulation is found in other fields, there is no reason for refusing to compare the action to intelligence. Comparison of the regulatory processes that are shown in internal physiological changes and in regeneration to intelligence seems to be looked upon sometimes as heretical and unscientific. Yet intelligence is a name applied to processes that actually exist in the regulation of movements, and there is, *à priori*, no reason why similar processes should not occur in regulation in other fields. When we analyse regulation objectively there seems indeed reason to think that the processes are of the same character in behaviour as elsewhere. If the term intelligence be reserved for the subjective accompaniments of such regulation, then of course we have no direct knowledge of its existence in any of the fields of regulation outside of the self, and in the self perhaps only in behaviour. But in a purely objective consideration there seems no reason to suppose that regulation in behaviour (intelligence) is of a fundamentally different character from regulation elsewhere" (*Method of Regulation*, p. 492).

Jennings makes no mention of questions of the theory of heredity: he has made some experiments on the transmission of an acquired character in Protozoa, but it was a mutilation character, which is, as has been often shown,[1] not to the point.

One of the most obvious criticisms of Hering's exposition is based upon the extended use he makes of the word "Memory"; this he had seen and deprecated.

"We have a perfect right," he says, "to extend our conception of memory so as to make it embrace involuntary [and also unconscious] reproductions of sensations, ideas, perceptions, and efforts; but we find, on having done so, that we have so far enlarged her boundaries that she proves to be an ultimate and original power, the source and, at the same time, the unifying bond of our whole conscious life" (*Unconscious Memory*, p. 106).

This sentence, coupled with Hering's omission to give to the concept of memory so enlarged a new name, clear alike of the limitations and of the stains of habitual use, may well have been the inspiration of the next work on our list.

[1] See "The Hereditary Transmission of Acquired Characters" in *Contemporary Review*, September and November 1908, in which references are given to earlier statements.

Richard Semon is a professional zoologist and anthropologist of such high status for his original observations and researches in the mere technical sense, that in these countries he would assuredly have been acclaimed as one of the Fellows of the Royal Society who were Samuel Butler's special aversion. The full title of his book is *Die Mneme als erhaltende Prinzip im Wechsel des organischen Geschehens* (Munich: 1st ed. 1904; 2nd ed. 1908). We may translate it *Mneme: a Principle of Conservation in the Transformations of Organic Existence.*

From this I quote in free translation the opening passage of chapter ii.:

"We have shown that in very many cases, whether in Protist, Plant, or Animal, when an organism has passed into an indifferent state after the reaction to a stimulus has ceased, its irritable substance has suffered a lasting change: I call this after-action of the stimulus its 'imprint' or 'engraphic' action, since it penetrates and imprints itself in the organic substance; and I term the change so effected an 'imprint' or 'engram' of the stimulus; and the sum of all the imprints possessed by the organism may be called its 'store of imprints,' wherein we must distinguish between those which it has inherited from its forbears and those which it has acquired itself. Any phenomenon displayed by an organism as the result either of a single imprint or of a sum of them, I term a 'mnemic phenomenon'; and the mnemic possibilities of an organism may be termed, collectively, its 'Mneme.'

"I have selected my own terms for the concepts that I have just defined. On many grounds I refrain from making any use of the good German terms 'Gedächtniss, Erinnerungsbild.' The first and chiefest ground is that for my purpose I should have to employ the German words in a much wider sense than what they usually convey, and thus leave the door open to countless misunderstandings and idle controversies. It would, indeed, even amount to an error of fact to give to the wider concept the name already current in the narrower sense—nay, actually limited, like 'Erinnerungsbild,' to phenomena of consciousness. . . . In Animals, during the course of history, one set of organs, so to speak, specialised itself for the reception and transmission of stimuli—the Nervous System. But from this specialisation we are not justified in ascribing to the nervous system any monopoly of the function, even when it is as highly developed as in Man. . . . Just as the direct excitability of the nervous system has progressed in the history of the race, so has its capacity for receiving imprints; but neither susceptibility nor retentiveness is its monopoly; and, indeed, retentiveness seems inseparable from susceptibility in living matter."

Semon here takes as an instance of stimuli and imprint actions affecting the nervous system of a dog

"who has up till now never experienced ought but kindness from the Lord of Creation; and then one day that he is out alone is pelted with stones by a boy. . . . Here he is affected at once by two sets of stimuli: (1) the optic stimulus of seeing the boy stoop for stones and throw them, and (2) the skin stimulus of the pain felt when they hit him. Here both stimuli leave their imprints; and the organism is permanently changed in relation to the recurrence of the stimuli. Hitherto the sight of a human figure quickly stooping had produced no constant special reaction. Now the reaction is constant and may remain so till death. . . . The dog tucks its tail between its legs and takes flight, often with a howl [as of] pain.

"Here we gain on one side a deeper insight into the imprint action of stimuli. It reposes on the lasting change in the conditions of the living matter, so that the repetition of the immediate or synchronous reaction to its first stimulus (in this case the stooping of the boy, the flying stones, and the pain on the ribs), no longer demands, as in the original state of indifference, the full stimulus *a*, but may be called forth by a partial or different stimulus, *b* (in this case the mere stooping to the ground). I term the influences by which such changed reaction are rendered possible 'outcome-reactions,' and when such influences assume the form of stimuli, 'outcome-stimuli.'"

They are termed "outcome" ("ecphoria") stimuli, because the author regards them, and would have us regard them, as the outcome, manifestation or efference of an imprint of a previous stimulus. We have noted that the imprint is equivalent to the changed "physiological state" of Jennings. Again, the capacity for gaining imprints and revealing them by outcomes favourable to the individual is the "circular reaction" of Baldwin, but Semon gives no reference to either author.[1]

In the preface to his first edition (reprinted in the second) Semon writes, after discussing the work of Hering and Haeckel:

"The problem received a more detailed treatment in Samuel Butler's book, *Life and Habit*, published in 1878. Though he only made acquaintance with Hering's essay after this publica-

[1] Semon's technical terms are exclusively taken from the Greek, but as experience tells me that plain men in England have a special dread of suchlike, I have substituted "imprint" for "engram," "outcome" for "ecphoria"; for the latter term I had thought of "efference," "manifestation," etc., but decided on what looked more homely, and at the same time was quite distinctive enough to avoid that confusion which Semon has dodged with his Græcisms.

tion, Butler gave what was in many respects a more detailed view of the coincidences of these different phenomena of organic reproduction than did Hering. With much that is untenable, Butler's writings present many a brilliant idea; yet, on the whole, they are rather a retrogression than an advance upon Hering. Evidently they failed to exercise any marked influence upon the literature of the day."

This judgment needs a little examination. Butler claimed that his *Life and Habit* was an advance on Hering in its dealing with questions of hybridity and of longevity, puberty and sterility.

Since Semon's extended treatment of the phenomena of crosses might also be regarded as the rewriting of the corresponding section of *Life and Habit* in the "Mneme" terminology, we may infer that this view of the question was one of Butler's "brilliant ideas." That Butler did not commit himself to such a formal explanation of memory as Hering with his wave hypothesis should certainly be counted as a distinct "advance upon Hering," for Semon also avoids any attempt at an explanation of "Mneme." I think, however, we may gather the real meaning of Semon's strictures from the following passages:

"I refrain here from a discussion of the development of this theory of Lamarck's by those neo-Lamarckians who would ascribe to the individual elementary organism an equipment of complex psychical powers—so to say, anthropomorphic perception and volitions. Their treatment is no longer directed by the scientific principle of referring complex phenomena to simpler laws, of deducing even human intellect and will from simpler elements. On the contrary, they follow that most abhorrent method of taking the most complex and unresolved as a datum and employing it as an explanation. The adoption of such a method, as formerly by Samuel Butler, and recently by Pauly, I regard as a big and dangerous step backward" (2nd ed. pp. 285–6, note).

Thus Butler's alleged retrogressions belong to the same order of thinking that we have seen shared by Driesch, Baldwin and Jennings, and most explicitly avowed, as we shall see, by Francis Darwin. Semon makes one rather candid admission: "The impossibility of interpreting the phenomena of physiological stimulation by those of direct reaction, and the undeception of those who had put faith in this being possible, have led many on the *backward path of vitalism*." Semon assuredly will never be able to complete his theory of "Mneme" until, guided by the experience of Jennings and Driesch, he forsakes the blind alley of mechanisticism and retraces his steps to reasonable vitalism.

But the most notable publications bearing on our matter are incidental to the Darwin Celebrations of 1908–9. Dr. Francis Darwin, son, collaborator, and biographer of Charles Darwin, was selected to preside over the meeting of the British Association held in Dublin in 1908, the jubilee of the first publications on Natural Selection by his father and Alfred Russel Wallace. In this address we find the theory of Hering, Butler, Rignano and Semon taking its place as a *vera causa* of that variation which natural selection must find before it can act, and recognised as the basis of a rational theory of the development of the individual and of the race. The organism is essentially purposive: the impossibility of devising any adequate accounts of organic form and function without taking account of the psychical side is most strenuously asserted. And with our regret that past misunderstandings should be so prominent in Butler's works, it was very pleasant to hear Francis Darwin's quotation from Butler's translation of Hering[1] followed by a personal tribute to Butler himself.

In commemoration of the centenary of the birth of Charles Darwin and of the fiftieth anniversary of the publication of the *Origin of Species*, at the suggestion of the Cambridge Philosophical Society the University Press published during the past year a volume entitled *Darwin and Modern Science*, edited by Mr. A. C. Seward, Professor of Botany in the University. Of the twenty-nine essays by men of science of the highest distinction, of peculiar interest to the readers of Samuel Butler, is that on *Heredity and Variation in Modern Lights*, by Prof. W. Bateson, F.R.S., to whose work on *Discontinuous Variations* we have already referred. Here once more Butler receives from an official biologist of the first rank full recognition for his wonderful insight and keen critical power; this is the more noteworthy because Bateson has apparently no faith in the transmission of acquired characters. Such a

[1] "Between the 'me' of to-day and the 'me' of yesterday lie night and sleep, abysses of unconsciousness; nor is there any bridge but memory with which to span them" (*Unconscious Memory*, p. 110).

passage as the following would have commended itself to Butler's admiration:

"All this indicates a definiteness and specific order in heredity, and therefore in variation. This order cannot by the nature of the case be dependent on Natural Selection for its existence, but must be a consequence of the fundamental chemical and physical nature of living things. The study of Variation had from the first shown that an orderliness of this kind was present. The bodies and properties of living things are cosmic, not chaotic. No matter how low in the scale we go, never do we find the slightest hint of a diminution in that all-pervading orderliness, nor can we conceive an organism existing for one moment in any other state."

We have now before us the materials to determine the problem of Butler's relation to biology and to biologists. He was, we have seen, anticipated by Hering, but his attitude was his own, fresh and original. He did not hamper his exposition, like Hering, by a subsidiary hypothesis of vibrations which may or may not be true, which burdens the theory without giving it greater carrying power or persuasiveness, which is based on no objective facts, and, as Semon has practically demonstrated, is also needless for the detailed working out of the theory. Butler failed to impress the biologists of his day, even those on whom, like Romanes, he might have reasonably counted for understanding and for support. But he kept alive Hering's work when it bade fair to sink into the limbo of obsolete hypotheses. To use Oliver Wendell Holmes's phrase, he "depolarised" evolutionary thought. We quote the words of a young biologist, who, when an ardent and dogmatic Weismannist of the most pronounced type, was induced to read *Life and Habit*: "The book was to me a transformation and an inspiration." Such learned writings as Semon's or Hering's could never produce such an effect: they do not penetrate to the heart of man: they cannot carry conviction to the intellect already filled full with rival theories, and with the unreasoned faith that to-morrow or next day a new discovery will obliterate all distinction between Man and his makings. The mind must be open for the reception of truth, for the rejection of prejudice; and the violence of a Samuel Butler may in the future, as in the past, be needed to shatter the coat of mail forged by too exclusively professional a training.

# TRANS-HIMALAYA AND TIBET

By FELIX OSWALD, D.Sc., B.A., F.G.S.

Exception has been taken by some geographers to Dr. Sven Hedin's use of the name Trans-Himalaya to designate the lofty system of ranges which he recently explored on the north side of the head-waters of the Brahmaputra and the Indus. The chief objection seems to lie in the transference of the term from its somewhat vague reference by Sir Alexander Cunningham[1] or by Colonel Godwin-Austen[2] to the mountains north of the Upper Indus and Brahmaputra to a more restricted usage for a system or rather zone of mountains lying between the Brahmaputra (or Tsangpo) and the actual plateau of Tibet. Lord Curzon has already remarked, with reference to the controversy, that a similar objection might be raised to the use of the word Trans-Alai, which, however, is well established for a range bearing an analogous orientation relatively to the Alai range of the Tian-Shan as the Trans-Himalayan system does to the Himalaya. Dr. Hedin,[3] moreover, is careful to lay stress on the fact that the Trans-Himalaya—in the sense in which he uses the term—is not a single range, but consists of a number of parallel chains, extending for 590 miles, between the Khalamba-La Pass in the east and the Jukti-La Pass in the west; in other words, between the eighty-first and ninetieth meridians. He also states explicitly[4] that "between these limits lie all the passes, by crossing which I was able to trace the course of the Trans-Himalaya and prove that its known eastern and western sections are connected and belong to the same mountain system, and that this system is one of the loftiest and mightiest in the world, only to be compared with the Himalayas, the Karakoram, Arka-tag and Kuen-lun. On the north and south its boundaries are sharp and clearly defined;

[1] *Ladak and Surrounding Countries, Physical, Statistical and Historical.* London, 1854.

[2] *Proc. Roy. Geog. Soc.* 1883, p. 610, and 1884, p. 83.

[3] *Trans-Himalaya*, ii. p. 403. London, 1909.

[4] *Op. cit.* p. 410.

the northern is formed by the central lakes discovered by Nain Singh and myself, and the southern by the Indus–Tsangpo valley."

This zone of numerous lakes to which Dr. Hedin refers varies in level only from 15,100 to 15,700 ft. above the sea and evidently marks the site of a continuous depression, extending from west to east along the southern border of the Tibetan plateau. Starting from the Rartse plain (15,695 ft.) in the west, we find a close and continuous linear succession of the following lakes: Ngang-tsing-tso (15,573 ft.), Shovo-tso (15,696 ft.), Tabie-tsaka, Tarok-tso (15,180 ft.), Terinam-tso (15,637 ft.), Dangra-yum-tso, Ngang-tse-tso (15,417 ft.), Marchar-tso, Kyaring-tso (15,541 ft.), Makiou-tso, Bul-tso, Ring-tso, Shudun-tso and Nam-tso or Tengri-nor (15,190 ft.). This depression, extending mainly along the thirty-first parallel, is indeed so pronounced a feature in the Tibetan plateau as to cause Nain Singh[1] (the original discoverer of this lake-system) to state that "along this line a cart might easily travel eastwards to the Nam-tso lake (Tengri-nor) without meeting a single obstacle *en route* and that this plain is as a rule confined between mountains which run parallel to the direction of the road."

To the north of this zone all the ridges traversing the plateau show a general direction from east to west without imposing any serious impediment to the traveller; and their intermediate troughs, with their rivers and lakes, exhibit the same latitudinal orientation. In fact, Littledale,[2] in marching southwards from the Kuen-lun to the Tengri-nor—a distance of six degrees—states that "we never saw a single mountain range till we came to the Nien-chen-tang-la," on the south coast of the Tengri-nor.

To the south, however, of the lake-zone, especially between the eighty-second and eighty-fifth meridians, these latitudinal ranges have given place abruptly to the Trans-Himalayan system of parallel ranges, running at an acute angle to the Tibetan ranges, viz. from N.N.W. to S.S.E., of which the chief are named successively (from west to east) Surnge-La, Ding-La, Lavar-gangri, Pedang, Sur-la, Kapta and Lunkar. Farther to the eastward, the direction of these ranges curves round so that

[1] Captain Trotter's Report on the Trans-Himalayan explorations of 1873-4-5.

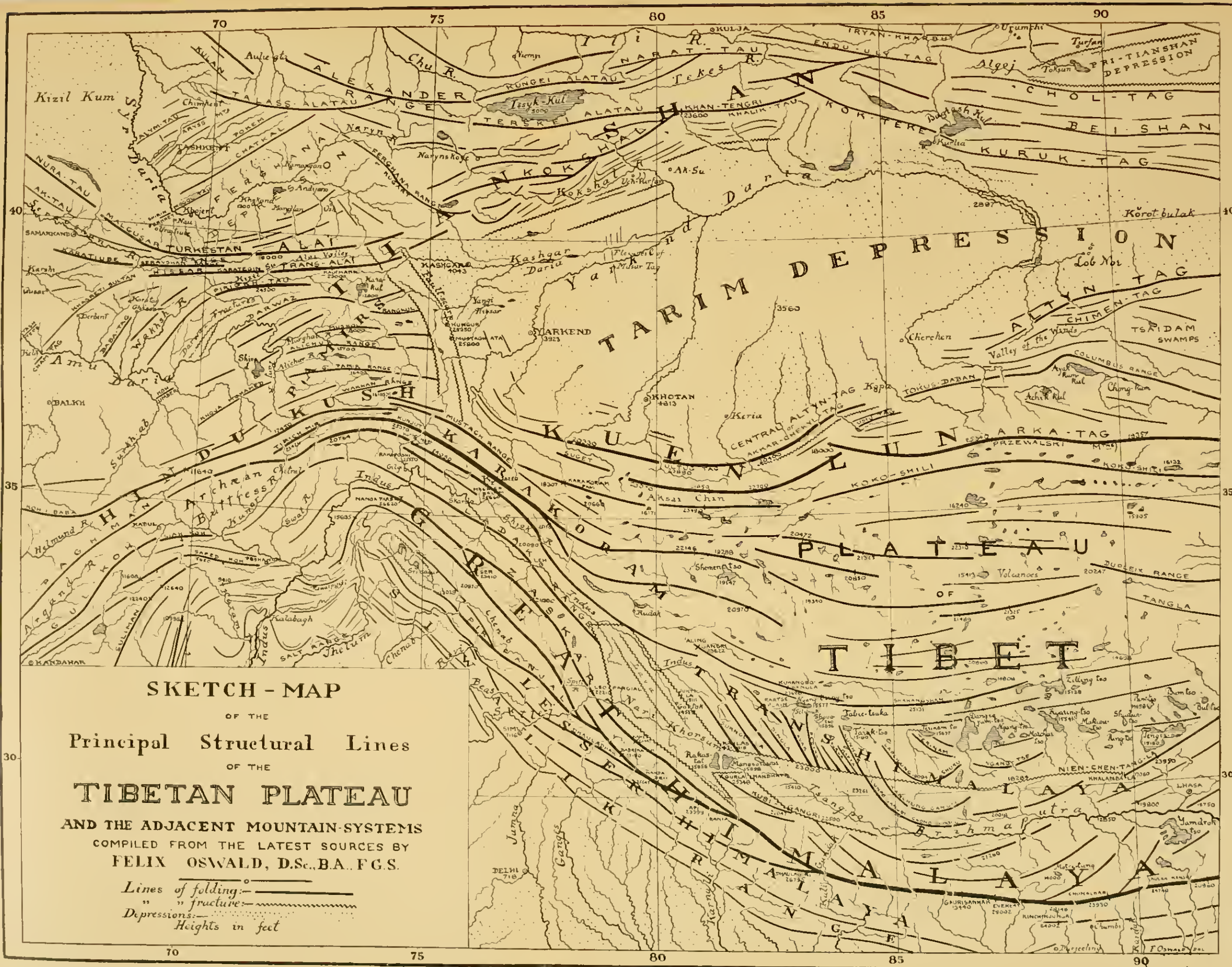
[2] "A Journey across Tibet," etc. *Geogr. Journ.* 1896, vol. vii. p. 463. London.

we find four arcs, concave to the north and bending round to the north-east, forming the nucleus, so to speak, of the Trans-Himalayan series—viz., in succession from north to south, the Terinam arc, the Lapchung-Shuru arc, the Kanchung-gangri arc and the Lunpo-gangri—Chomo-uchong arc.

It is to the western part of Dr. Sven Hedin's Trans-Himalayan system that I wish to draw particular attention, for in 1898[1] I was able to discover a very similar mountain-entity in the Taurus or southern border-range of the Armenian plateau, which is merely a western member of the long chain of plateaux stretching from Central Asia to the Mediterranean Sea. The comparison in this paper between the Taurus and the Trans-Himalaya will, it seems to me, throw some new light upon the significance of the nature and position of the latter with regard to the Tibetan plateau on the one hand and to the Himalaya on the other hand.

A glance at the sketch-map is in itself sufficient to show how abruptly the parallel N.N.W.—S.S.E. ranges of the Trans-Himalayan block abut against the east-west lines (with slightly undulating crest-lines) of the Tibetan plateau; it is obvious that these ranges must have been truncated on the north by an east-west fracture coinciding with the great depression marked by the chain of lakes already mentioned, and that on the south they have been similarly truncated by a nearly parallel fracture (W.N.W.—E.S.E.). The altitude of this system of mountain ranges, consisting of green schists and grey granite, rises steadily from 15,577 ft., the level of Lake Ngang-tsing at their northern foot, to over 23,000 ft. at their southern termination, where they are broken off no less abruptly, so that their ends form a continuous but serrated wall facing the broad valley of the Upper Brahmaputra and rising to form a watershed 8,000 ft. above the river. All the rivers which rise from the lofty snow-covered edge of this inclined block flow to the north-west, in "consequent" courses down to the plain of depression, dotted with lakes, at the northern foot of the Trans-Himalaya. It is evidently the truncated edge of these serried ranges which has given rise to the conception of an east-west range running parallel to the Brahmaputra: it has indeed been laid down by

[1] *A Treatise on the Geology of Armenia*, p. 110: Beeston, Notts, 1906; and my *Geological Map of Armenia*, with explanatory pamphlet, pp. 5, 6: Nottingham, 1906.


SKETCH-MAP
OF THE
Principal Structural Lines
OF THE
TIBETAN PLATEAU
AND THE ADJACENT MOUNTAIN-SYSTEMS
COMPILED FROM THE LATEST SOURCES BY
FELIX OSWALD, D.Sc., B.A., F.G.S.
Lines of folding:—
" " fracture:—
Depressions:—
Heights in feet
TARIM DEPRESSION
PLATEAU OF TIBET
KUEN LUN
HINDU KUSH
KARAKORAM
HIMALAYA
TRANS-HIMALAYA
TIAN SHAN
ALEXANDER RANGE
TURKESTAN ALAI
TRANS-ALAI
PRI-TIANSHAN DEPRESSION
CHOL-TAG
BEI-SHAN
KURUK-TAG
ALTYN-TAG
CHIMEN-TAG
ARKA-TAG
TSAIDAM SWAMPS
Kizil Kum
Syr Daria
Amu Daria
Yarkand Daria
Issyk-Kul
Lob Nor
KASHGAR
YARKEND
KHOTAN
LHASA
Indus
Brahmaputra
Ganges
Jumna
DELHI
KANDAHAR
SAMARKAND
TASHKENT
BALKH
F. Oswald del.

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Colonel Burrard[1] in his map as the Kailas range, and he calls it in this district "the northern rim of the Brahmaputra's trough."

Now it appears to be a not infrequent occurrence and a very intelligible error for explorers to mistake a mountain wall for a mountain range. For instance, to the south of Lake Van and the Armenian plateau I found that the Taurus, instead of being a continuous range running from north-west to south-east, as it had hitherto been depicted on maps, was in reality a mountain wall, in which the ends of closely serried S.W.—N.E. ranges and mountain folds have been abruptly cut off by a great N.W.—S.E. fault. The south coast of Lake Van rises up precipitously into lofty snow-capped peaks and ridges, composed of mica-schists, alternating with foliated grey and white marbles, chlorite-schists, limestone-schists, clay-slates, etc., which are certainly older than the Devonian. The continuous southern boundary of the plain of Mush to the westward is identical in structure and composition, forming a jagged and lofty wall, running altogether for over a hundred miles in a N.W.—S.E. direction, traversed by step-faults heading towards the plain and the lake at its northern foot. An important characteristic of this uptilted Tauric block of metamorphic schists lies in the fact that the watershed lies quite close to the edge of its great fault-scarp, and the main streams flow normal to its direction—*e.g.* the Kulp, Ab-ul-Jevis, Batman, Bitlis, Ghindig, Mukus and Mirjem rivers all flow from north-east to south-west—*i.e.* in "consequent" courses along what must have been the original slope of the uptilted block.

I have elsewhere[2] shown at length that the eastern border-ranges of Armenia similarly consist of uptilted blocks of ancient resistant rocks, which had lost the plasticity necessary for being thrown into folds. Here too the watershed of the Gokcha block lies close to a lake (Lake Gokcha or Sevanga) and parallel to its shore, and the rivers also take their courses in a direction normal to the watershed and the strike-fault—viz. to the north-east. Exactly similar characteristics are exhibited in the Somketian block situated to the north of the Gokcha block,

[1] *A Sketch of the Geography and Geology of the Himalaya Mountains and Tibet*, p. 94. Calcutta, 1907-8.

[2] "Zur tektonischen Entwicklungsgeschichte des armenischen Hochlandes," Petermann's *Geographische Mitteilungen*, 1910, Januar-Heft, with structural map.

and in the Karabagh and Karadagh blocks to the south; in all these cases, as in the Tauric block, the fault-scarp looks down upon depressions partly filled with recent lake-deposits and with lavas, which have welled up either from this fracture or from fractures parallel to it. The fact that the rivers in each case adopted "consequent" courses goes to show that the surface of the blocks must have been planed down either to a plateau of marine erosion or to a pene-plain prior to the uptilting.

Now in Central Asia we find that the Kashgar ridge is always mapped as a range running from N.N.W. to S.S.E.; yet here again it is merely a mountain wall, in which the ends of the Pamir ranges (with a W.S.W.—E.N.E. direction) are suddenly broken off by a great fault to face the deep Tarim depression (3,500–4,000 ft.) in a lofty snow-capped escarpment rising to 25,800 ft. in the peak of Mustagh-ata (see map). Although it is many years since Fedchenko[1] took this view of the so-called Kashgar range, this brilliant generalisation, so far ahead of the time, has hitherto escaped recognition, with the result that the structure of this part of Central Asia has been rendered unnecessarily obscure and complicated. It is noteworthy that (as is typically the case with lines of fracture) hot springs occur along this line, *e.g.* at Khajan-aksai (lat. 37°) in the valley of the Raskan Daria. The Pamirs may in fact be regarded, broadly speaking, as an uptilted block of previously folded strata, breaking off in an abrupt fault-scarp on the north-east towards the deep Tarim basin, whilst its surface, furrowed by "consequent" rivers, slopes gently from this uptilted edge of the Kashgar mountain wall down to the transverse valley of the Panj or upper Amu Daria in the south-west, where there is no such border-ridge. In this part of the Tian Shan system, as well as in the district of the Ferghana depression, the N.E.—S.W. chains belong to an older period of folding, whilst the N.W.—S.E. chains belong to the more recent (Tertiary) period, to which the fractures in the same direction, *e.g.* the Kashgar and Indus-Sutlej (Nari Khorsum) faults also belong. The Darwas fracture or dislocation, curving round from S.W. to S.S.W., to which Suess[2] and Krafft[3] have called attention, probably indicates (together with

[1] *Journ. Roy. Geogr. Soc.*, London, 1870, xl.

[2] *Antlitz der Erde*, iii. (1), p. 377 and Taf. xiii. Vienna, 1901.

[3] "Geologische Ergebnisse einer Reise durch das Chanat Bokhara." *Denkschr. Akad. Wien*, 1900, lxx. pp. 49-72 and map.

its south-westerly extension along the Surkh Ab valley) the line of the western fracture of the uptilted Pamir block.

Just as the uptilted blocks of the Armenian border-ranges show evidence, by means of the river-courses, of having been planed down prior to the differential movements which have raised them to their present position, so in like manner Prof. W. M. Davis[1] and Mr. Ellsworth Huntington have independently come to the conclusion that not only the Tian Shan mountains but also the Pamir plateau had been worn down to a fairly uniform surface after their principal folding had occurred; and that they owe their present irregular surface more to subsequent differential uplift than to denudation. "Even in the lofty Pamir there are certain ranges where the snowy peaks are mostly truncated as though by the old pene-plain, in spite of the fact that they are from 15,000 to 20,000 ft. high."

To return to the Trans-Himalaya—all its characteristics point to the inference that here we have to deal with a block of ancient rocks, which indeed had been thrown into folds at a long distant epoch, but at the time of the folding of the Tibetan plateau had lost all their original plasticity and could only yield to the mountain-making forces by becoming first of all fractured and then uptilted. These earth-movements are probably still in progress; at any rate this surmise receives some support from the observation by Dr. Sven Hedin[2] that whilst at Selipuk, in the Rartee plain (which, according to my interpretation, lies on the actual northern line of fracture of the Trans-Himalayan block) he was shaken by an earthquake, the only one he ever experienced during his journeyings in Tibet.

Although the Tibetan plateau is traversed, in the first place, by latitudinal mountain-folds, which are to be regarded essentially as the expanding branches or fan-like virgation of the Karakoram ranges, yet in all probability Tibet consists, in its present condition, of a succession of uptilted and depressed blocks of resistant strata, no longer capable of being folded, just as I have shown (*op. cit.*) to be the case in the plateau of Armenia. The innumerable lakes still scattered over its surface, although many more have completely dried up, lend support to

[1] *Explorations in Turkestan, with an Account of the Basin of Eastern Persia and Sistan.* Expedition of the Carnegie Institution of Washington in 1903 under the direction of Raphael Pumpelly, pp. 73, 80, 168. Washington, 1905.

[2] *Op. cit.* ii. p. 399.

this view, for their origin is now explained[1] by local uplift of the beds of the rivers which originally traversed the region. Hence, if the rate of elevation exceed that of the erosion of the river, the stream would be unable to keep its channel open and consequently a lake would be formed. In other words, "local elevation has enhanced the erosive power of the river below and diminished it above the line of uplift." However, in some cases at any rate, a river has been able to keep pace with the local uplift in the plateau-region. In particular, this occurrence was noticed by Dr. Hedin[2] in the case of the Bog-tsangpo; and he remarks, "one is often astonished at the whim of the stream in turning sharply to cut through a rocky crest, whereas it would seem much easier to flow on along the open longitudinal valley." This is a phenomenon which I observed[3] on more than one occasion on the Armenian plateau, *e.g.* at Gop and at Sheep in the valley of the Murad or Eastern Euphrates. The discovery of volcanoes, only recently extinct, in the interior of Tibet, near the Dupleix range, on the thirty-fourth parallel, also lends colour to this view of the land having been fractured into blocks, which have been subjected to differential movement.

As a corollary to the explanation which I offer of the Trans-Himalayan system, it follows that the natural continuation of the parallel ranges of the block lies now sunk beneath the Brahmaputra valley, at the base of the great fault-scarp, to which the river flows in parallel alignment. Accordingly this valley must be of the nature of a rift-valley or sunken trench, especially since the opposite (southern) wall of the valley lies parallel to the northern wall and in like manner possesses an average height of 23,000 feet. This deduction again receives some substantiation even from the scanty geological data which (until Dr. Sven Hedin's scientific results are published, *i.e.* in two or three years' time) we at present possess concerning the Upper Brahmaputra valley and its continuation westwards in the Nari-Khorsum (15,000 ft.) or upper Sutlej valley (Hundes). The sacred lakes Mansaro-war and Rakas-tal lie centrally in a glacial trough in this W.N.W.—E.S.E. rift-valley, which occupies the site of a relative depression between uptilted mountain blocks. It is filled not only by quite recent

[1] R. D. Oldham, *Records, Geol. Survey of India*, 1888, xxi. p. 156.
[2] *Op. cit.* i. p. 207.
[3] *A Treatise on the Geology of Armenia*, pp. 181, 192.

horizontal alluvial deposits (with bones of rhinoceros, etc.) but also by volcanic rocks, which must have risen up in the form of molten lava from vents along the fractures bordering the rift valley, exactly as in similar cases of such valleys in Syria, Armenia, East Africa and other parts of the world. Immediately on either side of this longitudinal depression or groove, however, we find only much older rocks—viz. Jurassic schists, granites, porphyries, etc.

The Gartok-Indus valley appears to be of a similar nature, branching off in a more north-westerly direction; here also volcanic rocks occur in the valley, lying between and at the foot of high granitic walls as far as Kangmar, near the seventy-eighth meridian. Although this region has not been extensively explored, yet hot springs have been noticed in several places along the northern side of this rift-valley—*e.g.* about twenty miles south-west of Gartok and also on the north side of the Gye-gong-la pass on the Kanchung-gangri, etc.—and it is well known (as I have already had occasion to remark) that hot springs are characteristic of tectonic lines of fracture.

The map will readily show that the Trans-Himalayan arcs, at least those which are complete arcs and have escaped being truncated by faults, exhibit a sharper curvature than the present arc of the Himalaya taken as a whole. Now the folding of the Trans-Himalayan rocks probably took place in the Upper Cretaceous or the Lower Tertiary period, for Nain Singh[1] discovered *Omphalia Trotteri*, Feistmantel, of Upper Cretaceous (Turonian) age on the shores of the Nam-tso or Tengri-nor, on the northern border of the Trans-Himalayan zone. It has been established that at this period the Punjab Himalaya was not in existence (its formation was subsequent to that of the Nepal Himalaya) and that even down to the Upper Eocene the sea still existed in that region. It must have been a steadily sinking area to allow of the great accumulation of nummulitic limestone, which has now been raised to the height of 18,500 ft. in the Zanskar range, on the peaks above the Singhgi-la.[2] This geosyncline of the Punjab Himalayan area would at the time of the folding of the Trans-Himalayan arcs have stood in a similar relation to them as the Mesopotamian and Persian Gulf geosyncline stands at the present day to the outer Iranian

[1] *Records Geol. Survey of India*, x. p. 21.

[2] T. D. La Touche, *Records Geol. Survey of India*, 1888, xxi. p. 160.

| Anterior Tableland. | Anterior Wave. | | Median Wave. | | Posterior Wave. | | Posterior Tableland. |
|---|---|---|---|---|---|---|---|
| | *Anterior Geosyncline.* | *Posterior Geanticline.* | *Anterior Geosyncline.* | *Posterior Geanticline.* | *Anterior Geosyncline.* | *Posterior Geanticline.* | |
| Arabia. | Mesopotamian plains filled with alluvial deposits. | Sub-Taurus and Taurus. | Armenian plateau. | Northern border-ranges of Armenia (Thrialetic, Gokcha, Karabagh, etc.). | Rion and Kur plains, separated at greatest point of compression by Meschic Horst. | Caucasus. | Russia. |
| Arabia and bed of Indian Ocean. | Persian Gulf to Indus valley with its alluvium. | Zagros to Mekran and Suliman Mts. (Outer Iranian arc.) | Iranian plateau. | Inner Iranian arc (Alburs and Khorassan Mts.). | South Caspian depression. | Mid-Caspian ridge to Great Balkhan and Parapomisus. | Siberia. |
| Hindustan. | Punjab plains filled with alluvial deposits. | Salt Range. | ? Crushed up in the Indus bend. | Hindu Kush and Karakoram. | Amu Daria and Tarim depressions, separated at greatest point of compression by Pamir Horst. | Tian-Shan. | Siberia. |
| Hindustan. | Ganges plains filled with alluvial deposits. | Siwaliks, Himalaya, and Trans-Himalaya. | Tibetan plateau. | Kuen-Lun. | Tarim depression. | Tian-Shan. | Siberia. |

arcs; and the Trans-Himalayan arcs would have had a correspondingly restricted area in which to develop. It will also be noticed that the orientation of the Trans-Himalayan arcs has more in common with that of the Great Himalaya on the south side of the Brahmaputran trough than with the latitudinal alignment of the ranges of Tibet, that the convexity of the arcs decreases from north to south, and that structurally Trans-Himalaya must be regarded as part of the Himalayan system. Its mountain-folds must also have been formed at a date anterior to those of the Tibetan plateau.

In conclusion it would seem to be something more than a mere chance coincidence that both in the Armenian and in the Tibetan plateau we should find so close a similarity of structure. In both cases the plateau is delimited from its southern border-ranges by a fracture cutting off abruptly the structural lines of the more ancient rocks of these border-ranges, so that they form an acute angle with the nearly latitudinal orographic features of the plateau. In both cases a depression, marked by a zone of lakes or lake-deposits, occurs along this fracture. In both cases the border-ranges have been fractured into blocks, which either have been uptilted or else have sunk down to form deep rift-valleys, and in the act of sinking have squeezed up molten lava to issue from volcanic vents along the lines of fracture. In both Armenia and Tibet the outer zone of their border-ranges is occupied by Tertiary strata, which have been thrown into folds and are separated in each case from the ancient tablelands of Arabia and India by the broad and deep alluvial depressions of Mesopotamia and the Ganges respectively.

All these points of similarity in structure and relative position appear to be sufficiently striking in two members of the continuous plateau-belt stretching from the confines of China to the Ægean Sea to permit of being homologised in a comparative table. The close parallelism of all the corresponding orographical units is in this manner succinctly emphasised and the table printed on the opposite page may serve to bring out the essential unity of construction underlying the formation of each of these lofty plateaux.

# AGRICULTURAL PROGRESS IN THE TROPICS

## PART I

By J. C. WILLIS, Sc.D., F.L.S.,
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With the great opening up of the countries of the tropics, more especially of Africa and south-eastern Asia, now taking place, the question of what is to be the line of progress of agriculture in them becomes of very great importance. Is it to be merely, as at present is more or less the case in some countries, the establishment of a great and progressive "planting" industry beside the unprogressive and time-honoured peasant cultivations of the natives, or are the latter to be made to progress, the planting industry perhaps being even shut out, or are both to advance together?

Looking back over the agricultural history of most of the tropical colonies, what may almost be called a conflict of ideals is apparent—those of the commercial planting community, and those of the officials entrusted with the government of the country. A careful analysis of the position shows that both of these ideals are equally wrong and that the truth lies between them. The native of most tropical countries is, indeed, at a very early stage in agricultural progress, and the ideal, more or less acknowledged, of Government officials has been to hold him back at that stage. The planting community, feeling that if this were the case it was hopeless ever to get the countries of the tropics to produce commodities for export, have pushed for their ideal, which errs as far in the other direction.

It will repay us consequently to inquire into this question in more detail and to trace out the ideals and the way in which their attainment has been attempted.

In the present condition of the world the temperate zones cannot get on without the products of the tropics. The latter provide many things, such as rubber, tea, coffee, cinchona, jute, cane sugar, spices, etc., which are among the necessaries of modern civilised life. The need for these has led to the settlement of Europeans at trading stations in the tropics, at Calcutta, Malacca, Calabar and many other places. Once settled there, the insecurity of the traders and the inefficiency of the natives have led to the conquest of adjacent territories, until now most of the valuable areas of the tropics are in European or in American hands. In these hands they are likely to remain, for the reason that the people of the tropics seem unable to form any kind of stable and progressive government, and must consequently, for their own good and that of the world at large, be subject to the strong northern powers.

These work on the principle of governing the country for the benefit of the governed; but they must also so arrange matters that the tropical countries shall take their share in the progress of the world at large and produce and export certain commodities for the benefit of that world which cannot get along properly without them. If the countries of the tropics can be made to progress so far that they shall themselves, with their own population, produce these things, so much the better; but the things must be produced.

This, then, is the position: it is open to argument that the natives of the tropics have come to the end of their possible progress and that the only way to get the export products is for white people to settle and grow them in the tropics. On the other hand, certain countries in the tropics do export produce grown by their own people; and in others the wealthier natives have followed the example of the white planters settled in their midst and are themselves growing produce for export. These facts may be taken as evidence that under certain conditions the natives of the tropics can progress along modern lines and therefore that such progress must be stimulated and encouraged. It is evident, however, that this progress is very slow and therefore, for the present at any rate, the white and Chinese planters must be encouraged or the supply of the necessary useful products from the tropics will sink almost to nothing.

To understand fully the ideals aimed at, the history of agriculture must be briefly followed. As is the case with so many other departments of knowledge, the tropics show the early stages far more fully and clearly than do the countries of the north. The very earliest stages of agriculture, if indeed such a name may be given, may be seen to this day among the remnant of the primitive Veddas of Ceylon and other disappearing relics of an older type of mankind. Hunters in the forest with the bow and arrow, these men live, so far as vegetable products are concerned, simply upon the roots and fruits of the wild forest plants, and neither till the ground nor grow any actual crops. There can be little doubt that all mankind began at this early stage but it is now only of historical interest. Be it noted, in connection with the ideas we are now following out, that the Vedda need not own the land, that he requires no capital to live upon until his crop is ripe, that he needs no means of transport, and that he can get on without the assistance of any other labour than his own.

The next stage in most countries of the tropics involves the clearing of the forest to a greater or less degree; only in a few countries is there any open grass land, and as a matter of fact such land is less suited to primitive agriculture than the forest land. At first this clearing would undoubtedly take the form of felling and burning the smaller trees, the fire destroying also the larger trees and the undergrowth. This method survives to the present in the widespread practice of what is called in Ceylon *chena*, in Malaya *ladang*, in India *jhuming*. A very large population to this day makes chena part at least of its regular agriculture; consequently improvement must begin very far back.

The land having been chenaed, *i.e.* the forest felled and burned, the crop, whatever it may be, is sown among the ashes, and a good return is commonly obtained in the first year. In the second year the return is less; in the third year the land is usually abandoned to jungle once more. After a period of from ten to fifty years, varying with the soil and the rainfall, a scrubby jungle has grown over the land which is said by natives to be once more worth chena. It has commonly been supposed that this means that the soil has again recovered its fertility; but this is only part, and probably the least important part, of the truth. In actual fact, the growth of shrubs and small trees has choked out most of the weeds of open ground and given it an undergrowth of the weeds of shady ground. These weeds, when the land is cleared, will not grow any more, while the weeds of open ground, so long as they survive on the land, make it impossible to grow any crops without tillage for their suppression.

Chena is a very primitive stage of agriculture, but it is very widely practised and makes certain demands upon those who carry it on, which were not made upon those only collecting wild jungle-stuffs. There is no absolute necessity for private ownership of land, nor for any facilities for transport, but the chena cultivator must have a certain amount of capital to tide over the period of waiting while the crop ripens. This capital need not, of course, be money; it may be simply the stored food from an unusually successful raid upon the produce of the jungle, or it may be an advance of food-stuffs from some other person. The important point is that capital is needed.

Progress beyond chena takes two directions: on the one side the development of the "mixed garden," on the other that of the "field." The mixed garden is a very characteristic feature of the agriculture of many if not most tropical countries at the present day. It is simply a casual mixture of useful trees, shrubs and herbs. A mango may stand next to a jak (*Artocarpus integrifolia*), and both to a coco-nut, while the space between is filled in with bananas, oranges, and other smaller plants; on the ground grow a few herbs, but the soil is generally covered with grass, affording grazing to a few miserable specimens of cattle. Such a garden would naturally arise from a chena as perennial crops came to be appreciated and planted.

The mixed garden is a kind of cul-de-sac in agriculture, for only by giving it up in favour of something better can there be progress. It makes greater demands than did the chena, for not only is capital required but also some settled ownership of land—a man will not plant a mixed garden upon common land.

In the other direction the chena might develop into the field, or portion of ground kept permanently tilled and free from weeds, upon which annual crops were grown, at least one every year. Such agriculture would also require settled land tenure and more capital than any of the preceding forms. Labour would have to be more systematic and more regularly applied, and if the field were anything beyond the very smallest area, hired labour would also be needed.

Once agriculture takes the field type, the way is open for unlimited improvement but there are many preliminary conditions to be fulfilled. Proper tools would soon be found a necessity, if the land was not to become a mere nursery of weeds; and though at first the man might make his own tools, the gain in efficiency would soon lead to a division of labour, the making of tools falling to the carpenter and the blacksmith, who would have to be paid by a percentage on the crops or in some similar way, they themselves not practising agriculture. In this way, probably, the first germ of division of labour would appear and the first non-agricultural population.

As the country became settled and populous, means of transport of goods would arise—at first perhaps by the aid of water, or by carriage on the heads of coolies—and with it a further differentiation of the population, men engaged in these new occupations also appearing. At the same time, the agricultural population itself would gradually realise that a differentiation might with advantage come into its own ranks, for when the produce could be carried elsewhere, it would be more efficient for A and B to specialise a little more in their cultivations, grow somewhat different things and exchange them.

As transport facilities increased, local markets, where produce might be exchanged or sold, would grow up. But it must be clearly understood that so long as the market is simply local, so long will there be liability to great fluctuations in demand, supply and price, and consequently that to embark upon large cultivations of one particular product will be taking very great risks. The cultivators must continue to grow the bulk of what they require, but may devote a small part of their available land to the cultivation of products for sale or exchange.

This is the state of affairs to which perhaps the most civilised countries of the tropics, such as India or Ceylon, had attained at the time of the first appearances of the trading nations in their midst. At this stage they have in general remained to this day, in spite of centuries of intercourse with Europeans, though they have probably progressed a little in the specialisation that we have indicated, and some have taken to more modern ways. The great bulk of the population, and this the more conspicuously the nearer one comes to the equator, live to-day, as their forefathers lived, by growing most of their own food and other necessaries, and growing also a very small amount of produce for sale, with the proceeds of which they buy the few external things that they find need for in their simple mode of life.

To such a population entered the enterprising trading nations —first the Arab, later the European, the American and in some cases the Chinaman. The first comers did not bring much capital with them, but settled in places where transport from the interior was easy and collected and exported the products of the country, thus increasing the markets for these and rendering possible a somewhat greater specialisation in their production.

The later comers, the white races, while at first they settled at suitable places and collected and exported in the same way, presently conquered the countries and set about obtaining larger supplies of their products for export. The early method of doing this involved the establishment of a Government monopoly of these products, such as the famous cinnamon monopoly, which lasted in Ceylon through the rule of the Portuguese and Dutch, and until 1840 under the English. Under these monopolies the natives were granted certain privileges in return for bringing in definite supplies of the spice, and any left over, after the traders to Europe had been supplied, was destroyed, in order to keep up the prices and to prevent the natives relaxing their toil in collection.

In these monopolies the labour that was needed for anything in the way of agriculture larger than the villager's garden and little field was obtained practically by compulsion. The next stage in agriculture under white management was the sugar industry of the West Indies. The country proved good for the cultivation but labour could not be got on the spot. With the aid of the capital early invested in the work, slaves were obtained from Africa and a prosperous industry soon sprang up. With the abolition of slavery the industry fell upon parlous times and has never really recovered its lost ground.

The next phase was seen in Ceylon, where in 1824 the Governor, Sir Edward Barnes, opened up an estate at Peradeniya, close to the great military road that had just been opened through the centre of the island, and thus in a place provided with means of transport. Slave labour here was impossible; but Ceylon then and always has had the great advantage of being near to an almost inexhaustible supply of labour from the densely peopled districts of the Madras Presidency. With this supply of cheap labour the estate at Peradeniya was soon made a financial success; the great planting industry of Ceylon and of other eastern countries sprang up, and at this day is probably in the most prosperous position it has ever known.

A little later than Barnes, Governor Van den Bosch of the Dutch East Indies, made his famous attempt to push forward in agriculture the natives of a tropical country by establishing the Java Culture system. The idea at the root of this system was an excellent one and had it been carried out with scrupulous honesty, the results might have been even more far-reaching. In brief, the natives, who were taxed a certain proportion of their crop—usually rice—were to give instead a certain proportion of their land and of their labour, to growing, not rice, a comparatively unremunerative crop, but coffee or other things in which there was a good trade with Europe and a large profit to be made. The Government promised to bear any loss there might be, if it was not directly the fault of the cultivators. Coffee, indigo, sugar and other things were grown under this system and enormous profits were made. The Government made good roads, and the natives of Java undoubtedly learnt much about agriculture that they would otherwise never have known. But the system has gradually fallen into desuetude; Java, like the rest of the countries with labour available, has become the home of a great industry carried on by white planters.

The great success of the white planting industry is to be ascribed not only to the superior energy and intelligence of the white men but also to the fact that they have possessed enough capital to lay out land, employ labour and wait some years for a return. As their enterprises have shown success, they have been imitated by numbers of native capitalists, and these latter have done sufficiently well to show that capital has a very great deal indeed to do with success in this line, and that racial differences are not enough to account for the great contrast between the agriculture of the immigrants and that of the natives of the country.

It is important that these points be clearly understood, for the present state of tropical agriculture is simply that we have been outlining. There exist in general two agricultures side by side, with but few intermediate stages—the great and progressive capitalist agriculture, mainly but not exclusively pursued by white men, and the still greater peasant agriculture, pursued by the poorer folk, almost, though not entirely, upon the principle of grow-what-you-want-and-consume-what-you-grow.

Are these two agricultures to continue in this state or are they to become more assimilated, the peasant agriculture becoming more progressive and additional intermediate types appearing? This is the great problem of the day, and we come back now in fact to the question of the ideals, with which we began.

The ideal of some administrators, and of many people in the north who have no acquaintance with the actual conditions of tropical countries, has been to hold back progress, at any rate so far as the natives are concerned, at the early stage which we have called grow-what-you-want-and-consume-what-you-grow. Now it is at least open to argument that such a position cannot in any case be maintained. It is liable to be upset by any immigration of alien races and cannot be adhered to if the course of opening up roads, railways and other means of transport, which is being followed in most tropical countries, be not abandoned. Further, as we have already pointed out, the white powers have taken possession of the great bulk of the tropics, and must have the agricultural products of these countries. To adhere strictly to this position would of course mean that the Government would have scarcely any money whatever to work upon, for the only taxable value in most tropical countries in the long run is the exportable products. Tropical governments have in consequence had to give way to modern progress.

So long as the natives continue at this stage, or at one near to it, so long can there be no export from the country, and it might just as well be non-existent. These facts, with the success of European planting enterprises, have led to the ideal which is at the other extreme, the opening up of most of the country as a planting country for Europeans, leaving the native to continue to grow what he wants and consume what he grows or to become a labourer on the estate of the white man.

As we have already pointed out, this ideal is that of the commercial and planting community, though not acknowledged in so many words, nor by the more far-sighted. It is an ideal which is forced upon the acceptance of the governing class by the necessity of earning some revenue, and for long their great aim has been to find some means of harmonising it with the other ideal which we have just dealt with.

The true ideal to be aimed at is, we think, between these two, and we have set forth the position in a recent book dealing with the theory and practice of tropical agriculture.[1] To sum it up in a few words, it is to encourage the diversification of agriculture and to get established in any country a fairly dense population, engaged in all forms of agriculture, from the largest capitalist enterprises employing large numbers of hired labourers and exporting most of the produce, down through smaller and smaller concerns working with hired labour to the smallest village agriculture, carried on by the labour of the owner and his family upon small patches of land on which is grown or made whatever that family may require. The larger capitalist concerns will at first almost of necessity be foreign; but the object of the government of the country should be to train up an agricultural class in the country which shall in the end be able to manage these large concerns and practically oust the foreign planter altogether. This must obviously be the work of many years, perhaps centuries; the foreign planter should at present receive every encouragement.

We have now to consider the best means of bringing about such a state of affairs. Tropical agriculture has in the past suffered very much from the efforts of enthusiasts who have not properly thought out the subject and who have each been convinced that his own panacea was the right one and the only one that could effect progress. At present progress is fairly rapid among the capitalist planters and almost absent among the poorer villagers; if we are to arrive at the ideal mentioned, we must cause it to occur among the latter as much as among the former.

It is now coming to be recognised that agricultural progress, like growth in the vegetable kingdom, depends upon many factors and cannot go on unless they are all put into operation.

[1] Willis, *Agriculture in the Tropics*, Cambridge University Press, 1909. 7*s*. 6*d*. [Reviewed in SCIENCE PROGRESS, January 1910, iv. 517–9.]

It is perfectly useless to supply a plant with large quantities of food and expect it to grow, unless at the same time it be given plenty of water and kept at a sufficiently high temperature; it is equally useless to expect agricultural progress to go on, if the country be supplied, say, with admirable means of transport but no capital be forthcoming.

The factors in agricultural progress are many, but they come into operation at different periods in that progress, as we have indicated in the foregoing historical sketch. But once a factor has come into operation, it must continue to work, or agriculture will again fall back.

Now, in the countries of the north most of the earlier factors—land, capital, transport, labour, education—have come into full work so long ago, and have been acting for so long a time, that most people take their action for granted and look only at the effect of the later factors; whereas in the tropics the early factors have not yet produced their full effect, if indeed they are always all in operation. Many of the enthusiasts who have had to do with tropical agriculture have tried to bring the later factors, which are operative in Europe, into action in the tropics before the primary factors, or have taken one of these alone and tried to make it active without the aid of the others.

In the sketch of the history of agriculture with which we began, we have tried to make clear what are these factors in progress, and the order in which they come into operation. They are land and its availability, capital, transport, labour and to some extent education. It is very important to understand that these must all come into operation before there can be any result from what we may call the scientific factors of progress, the scientific improvement of crops, cattle, tools, methods and the rest. For the latter factors there is an unlimited field open but the way must first be cleared by the operation of the preliminary factors, as we may call them. In *Agriculture in the Tropics* we have called these preliminary factors A, the scientific factors B, and shown that all A must operate before B can begin. It is not intended to imply that the preliminary factors are not scientific or that no science comes in among them, for indeed it does. Under land and its availability there comes in what until recently was the only application of science in tropical agriculture—the introduction and acclimatisation of numerous crops new to the countries concerned. But it is important to recognise that the scientific factors, properly so called, cannot operate fully till the preliminary factors have all come into full operation; and it is because in many countries attempts are now being made to perform this impossible feat, that this question is of such paramount importance at the present time. Until the preliminary factors have been started, the only result of such proceedings can be the following out of the second ideal mentioned, and the further accentuation of the already large gap between the capitalist agriculture of the immigrants and a few of the natives, and the non-capitalist agriculture of the bulk of the peasantry. Capital is the weak point in most tropical agriculture at the present time; the vast bulk of the population has no capital, and is in debt to the money-lender. Only a very few have capital available; of these many do not embark it directly in agriculture but prefer to lend it to needy agriculturists at rates of interests higher than they could earn if they themselves engaged in agricultural practice.

Adopting the classification of the factors elsewhere laid down, we may divide them into two groups—the preliminary and the later. Under the former we may consider (1) land and its availability, including systems of land tenure, crops suitable to the climate, drainage, irrigation and kindred topics; (2) capital (using the word in the very broadest sense, as will be explained); (3) transport facilities; (4) labour and (5) education. Under the latter we must deal with improvements of crops, cattle, tools and methods, *i.e.* with the applications of science, strictly so called, to agricultural practice. We shall proceed with these subjects in the second part of the paper and consider the advances that are being made under all these heads; in this first paper we have endeavoured to make clear the complexity of the subject and to show that until the preliminary factors are satisfied, it is useless to go on to the secondary, except for the capitalist agriculturist. Without capital more available than it is at present, the poorer peasantry cannot afford to try any improvement, but must stick to the well-tried crops, methods, and cattle. If cattle, for example, are to be improved, it is useless to attempt this without at the same time improving their food supply, and this means changes in many other departments; further, the improvement of the cattle will be useless unless we at the same time improve the tools; this again means many changes. All parts of the problem hang together. All the factors which we have called preliminary must first be put into operation and each kept up to the standard of the others. It is useless to lay out too many roads till the question of capital is taken up; it is useless to take this up in much detail without settling education, and so on.

*(To be continued)*

# THE SIGNIFICANCE OF THE PULSE-RATE IN VERTEBRATE ANIMALS[1]

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We should expect the frequency with which a heart beats to be determined by its own properties—by its size, the minute structure of its muscle fibres, the inorganic salts in and outside the fibres, temperature, its relation to the nervous system, etc.; it probably is *immediately* determined by such things as these. At present, however, we do not know the properties in which the hearts of allied animals, beating with very different frequencies, differ from one another and we are not therefore in a position to point to the immediate determining factors. All we know is that the properties, whatever they are, which determine frequency have come to be such as to enable the heart to serve the purposes of the animal to which it belongs. It is proposed in this paper to attempt to ascertain whether we can find out some of the different ways in which the heart serves these purposes and whether, or to what extent, alteration in frequency of beat is one. To do this we must first know something about the different purposes for which the heart is required in different animals.

In the first place the amount of driving work the heart has to do varies a good deal in the different craniate vertebrates and both with the structure and the habits of the animal. In fish, *e.g.*, it has only to pump the blood as far as the gills and it has not much to do even in effecting this, as the passive dilatation of the gill capillaries with each inspiratory movement of the buccal cavity helps the blood to get there (1).[2] In accordance with this small amount of work we find the heart to be of relatively small size in fish. Its weight in the common round fish is on the average only 0·09 per cent. of the body weight; in the notably inert flat-fish it is even less, only

[1] From a lecture delivered to the Oxford University Junior Scientific Club in November 1909.

[2] These numbers refer to a list of authorities given at the end of the paper.

about 0·04 per cent. of the body-weight (2). In birds, on the other hand, the heart has a very large amount of work to do, especially in the birds of passage and those that sing. Accordingly they have relatively very large hearts—1 to 2 per cent. of the body-weight as a rule (3) and sometimes, as in the thrush and golden oriole, as much as about 2·6 per cent. The size of the heart has thus no fixed relation to the size of the animal to which it belongs. The heart of a pigeon, *e.g.*, weighs twenty-five times as much as that of a plaice of the same weight, and is about equal to that of a salmon fifteen times as heavy as the pigeon. A thrush, and a guinea-pig of six or seven times its weight, have hearts of about equal size.

Frequency of beat, if it be in any way determined by the absolute size of the heart, is certainly no direct function of it. It is true that we have reason to believe, as we shall presently see, that the pulse-rate in the thrush is not very different from what it is known to be in the guinea-pig, but it would also not be very different from what it is in the rabbit, which has a heart of over twice the size. We know very little about what the frequency is in different fish and for those in which it has been accurately determined (*Telestes* and *Barbus*) the size of the heart has not been ascertained, though, assuming the relative size to be the same as in the other round fish, we should expect it to be no larger than that of a canary. In both these fish the frequency varies, in different individuals, between 40 and 70 per minute at room temperature, and no elevation of temperature raises it to beyond 125 per minute (1), whereas the heart of a canary may (7) beat with a frequency of 1,000 per minute.

If the animal made some demand on the heart for a definite volume of blood in unit time, frequency of beat might be expected to bear some relation to the *relative* size of the heart. Only it would be difficult to discover such a relation unless in a group of animals having the same circulatory arrangements some required a quicker and others a slower circulation for some assignable reason.

For the lower groups of craniate vertebrates (Fish, Dipnoi, Amphibians and Reptiles) we know very little as to the special demands made upon the heart. It has certainly more work to do in amphibians and reptiles than in fish, having to drive the blood all round the body without the help of the respiratory movements which seem to play so large a part in maintaining the circulation in fish (1). The relative heart-size is accordingly greater in amphibians and reptiles. In the frog (*R. temporaria*) and in a crocodile the heart was found to be about 0·4 per cent. of the body-weight and to be nearly 0·8 per cent. in the common snake (7). But we do not as yet know what the tissues take most from the blood in these lower vertebrates; we only know for a certain number of species of fish and amphibians and for the crocodile (6) that they take very little oxygen and that the rate at which this at least is supplied is not likely to cause difficulty. Neither has anything yet been ascertained about differences in pulse-rate in different genera of amphibians nor in those of any of the different classes of reptiles; so that we have not the material for deciding whether the frequency with which the heart beats has become one of the factors in natural selection. In the few species of amphibians and reptiles (6) for which—sporadically—the frequency is known, it seems to be not very different from what it is in fish—*i.e.* varying (and varying in individuals of the same species) between about 20 and 80 per minute at ordinary room temperature.

In birds and mammals the case is different. We not only know that the tissues take a great deal of oxygen from the blood, but that those of small animals take much more than those of large ones; and we can assign a reason. Birds and mammals are able to maintain a nearly constant temperature whatever that of their surroundings may be. They are *homœothermic* or (the temperature they maintain being usually higher than that of the environment) "warm-blooded" animals; they have in consequence to generate more heat than those animals which maintain no constant temperature—the *poikolothermic* or "cold-blooded" animals—and to try to prevent loss of heat. To generate heat the muscles—the chief heat-producing organs of the body—require oxygen, and they take it from the blood according to their need, the need being greatest in those species or individuals in which the loss of heat is greatest. The maximum loss is of course in those animals in which the surface exposed to the environment is largest in proportion to the mass of the animal—*i.e.* the smaller the animal the more heat must it give off, other things being equal, to a colder environment, and to maintain a constant body temperature the more heat must it generate and the more oxygen must its muscles consume.

The heart therefore, being asked to replenish the supply, must, if it respond, give out the larger relative volume of oxygen-containing blood in unit time the smaller the animal, and it might do so either by expelling a larger amount with each beat or by increasing the frequency of the beat.

But by regulating the volume of blood supplied to the muscles in unit time, the heart can only regulate the rate of supply of oxygen if the oxygen is present in a constant percentage. This is the case in birds and mammals, in which the blood in the systemic circulation leaves the left ventricle of the heart with its hæmoglobin saturated with oxygen. It is not the case in the lower vertebrates, not even in crocodiles; for although they, like birds and mammals, have the oxygenated blood completely separated from the rest in the heart, it becomes mixed with other blood in the dorsal aorta and may become so even in the conus. In other reptiles facilities for the admixture of the blood coming from the lungs with blood coming from other parts of the body are greater, since it may happen even in the ventricle. In the dipnoi and amphibians, other organs besides the lungs have respiratory functions and the blood from the rest of the organs in the body may mix with oxygenated blood elsewhere than in the ventricle and the arterial system. Where there is only one auricle, as in the dipnoi, and blood of all qualities enters the ventricle simultaneously, the percentage of oxygen in *all* the blood leaving the ventricle must be variable. Where there are two auricles, the one of which receives only oxygenated blood, as in amphibians and reptiles, this need not be the case, since even where there is only one ventricle the blood from the lungs, by entering and leaving the heart after the rest, may remain very nearly saturated. But such blood is by special arrangement supplied to the head only, and the blood to the limbs and other muscles is unsaturated, or very soon becomes so. In all these classes of lower vertebrates, therefore, the heart itself could not regulate the rate of oxygen-supply to meet different demands by altering either the volume given out per beat or the frequency of the beat. In fish, on the other hand, there is the possibility of regulating it either by altering the frequency of the respiratory movements or by altering the volume of blood expelled in each heart-beat, since all the blood which supplies the body has to pass first through the respiratory organs and so would contain a constant percentage of oxygen even if its hæmoglobin did not become fully saturated.

In fish, amphibians and snakes the attempt seems occasionally to be made to maintain a temperature above that of the environment (16A), but in how far it approaches to being constant we only know for two specimens of the Indian python (13A). It would be interesting to find out whether in a species of *Thynnus*, the bonito, which may have an internal temperature as much above that of the environment as the python, it is more nearly constant and how far the demand for oxygen in the one and in the other varies both with the external temperature and with the size of the individual; moreover, if it so varies, in what way the supply is regulated to meet the different demands.

The difference obtaining between the warm-blooded vertebrates on the one hand and all the cold-blooded except fish on the other, with regard to the relation of the oxygen to the volume of the blood in the systemic circulation, is illustrated in

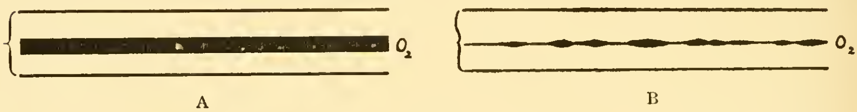


FIG. 1.—Diagrams to show the sort of relation of the oxygen to the blood-volume in the systemic circulation.

A, warm-blooded vertebrate. B, reptile and amphibian.

fig. 1. Regulating the volume rate would regulate the oxygen supply only with arrangement A. With any other arrangement, such as that in B, the absolute amount of oxygen supplied in unit time could be increased by increasing the frequency of the beat, but it could not in that way be regulated at all accurately to suit special demands.[1]

[1] Since this paper went to press, Krogh has published a series of articles in the *Skand. Arch. f. Physiol.* (1910) in which amongst other things it is shown that a method of regulating the oxygen-supply to the body does exist in reptiles and amphibians. This consists in adjusting the relative volumes of blood in the pulmonary and systemic arches by alteration of resistance in the pulmonary arteries effected by variations in the tonus of their vaso-constrictor nerves. By this means the blood per beat driven into the systemic circulation becomes less in volume the more oxygen the tissues consume, but its oxygen-tension not only relatively but absolutely greater, in consequence of the increase in the volume going per beat through the lungs, involving as it does a greater absolute absorption of oxygen. Although a convenient way of meeting differences of oxygen-requirement in the individual, it is not one that would lend itself to

What is true of the cold-blooded vertebrates is true of the embryo of the warm-blooded animal in respect of want of constant percentage of oxygen in the blood-supply to the body as in so many other respects. Although the blood leaving the left ventricle is blood brought straight from the respiratory organ of the embryo, the allantois, this serves only to supply the head, the great aorta through which it flows being joined, after having given off the vessels to the head, by the ductus Botalli bringing blood (through what is afterwards the pulmonary artery) from the right ventricle, which has received it from all organs and kept all but that from the respiratory organs. Thus the percentage of oxygen, which may have been constant in the blood leaving the left ventricle, is no longer so by the time it reaches the body of the animal and this must continue to be the case so long as the ductus Botalli remains open, which it does until the time of hatching or birth. When it closes, the blood from the right ventricle which would otherwise have gone along it, can only go to the lungs, and the channels from the lungs to the left auricle, the pulmonary veins, become functional with the lungs themselves, so that now blood saturated with oxygen enters the left auricle from the respiratory organ of the adult, and (the septum between the two auricles being now complete) passes unaltered into the left ventricle, whence it is driven to supply not only the head but now also the body. It would be interesting to know whether in the young guinea-pig and chick, which are able to regulate their temperature as soon as they come into the world, the ductus Botalli closes earlier than it does, *e.g.*, in young mice, rats and pigeons which can only regulate their temperature very imperfectly when born or hatched and take a week or more to develop this power. It would help us to find out whether, or to what extent, the want of power to regulate temperature depends upon the fact that any attempt of the heart to adapt itself to meet special demands made upon it for oxygen, potentially or actually, could meet with only imperfect success.

Let us now see in how far the hearts of birds and mammals, having the power to regulate the oxygen-supply by regulating the volume of blood expelled in unit time, succeed in doing so

meeting permanent differences of oxygen-requirement, did these exist, in the different species of reptiles and amphibians, in the way that alteration of volume-rate lends itself in birds and mammals.

when it is asked of them. As a measure of the rate at which oxygen is consumed in the different animals we may take either the oxygen-intake or the carbon-dioxide-output of a unit of weight in unit time, as the two things run roughly parallel. In the two following tables the carbon-dioxide-output is given because it happens to be known for a larger number of species than the oxygen-intake. The numbers are for the most part taken from the table in Pembrey's article on "Chemistry of Respiration" in Schäfer's *Text-book of Physiology* and represent the average in round numbers when several results are there given by different observers. Those for birds which are not to be found there are determinations kindly made for me by Dr. C. G. Douglas, Fellow of St. John's College, Oxford.[1] The pulse-rates of all the birds and of the smaller mammals have been determined by myself in a manner to be described immediately; those of the larger mammals have been taken on text-book authority when none other was available. As a measure of the volume of blood expelled per beat the weight of the heart in percentage of the body-weight has been taken. This has been determined for a very large number of birds by Parrot (3) but unfortunately not for many of which the pulse-rates are known. For most of these as well as for the mouse I have determined it myself. For most of the other mammals mentioned it has been determined by Bergmann (8) but the results of his observations are referred to, together with some more determinations of his own and of a few other people for other mammals, by Joseph (9). Unfortunately the number of individuals from which the "average," either of pulse-rate or of relative heart-weight, is taken was usually small and sometimes (in all the cases marked with an asterisk) the data were only ascertained from a single individual of a species; as we know that in other species there is a good deal of individual variation, the numbers given in these columns may not hereafter be found to be the correct averages. They probably are so however in the case of man and the rabbit, in which they have already been ascertained from large numbers

[1] For each bird he determined also the oxygen-intake; since this datum for the canary and for the tame duck has not yet been put on record, this occasion may be used for stating that it was found to be 10·99 and 1·66 grms. per kilo. per hour respectively for the two birds. The canary was remarkably quiet all the time it was under observation.

of individuals. It is of course highly desirable that the correct average should be known for every case, but it is difficult to get people to make large collections of facts and it is debateable in how far their doing so is a thing to be encouraged, so long as the interest attaching to them is not in evidence. The following tables, if they serve no other purpose, at least indicate the sort of value which would attach to a large collection of these particular facts.

TABLE I. BIRDS

| Bird. | Average weight (in grammes). | Average carbon-dioxide per kilo. per hour (in grammes). | Average heart-weight in percentage of body-weight. | Average frequency of beat per minute, when at rest. |
|---|---|---|---|---|
| Goldfinch | *16 | 12·6 | ? | *920 (6) |
| Canary | 20 | *11·7 | *1·04 (3) | *1,000 (7) |
| Sparrow | 24 | 12·2 | 1·36 (5) | 800 (5) |
| Greenfinch | 26 | 11·7 | ? | 740 (6) |
| Kingfisher (young) | *42 | ? | ? | *440 (7) |
| Thrush | *75 | ? | *2·56 (3) | ? |
| Pigeon | 300 | 3·4 | 1·5 (5) | 185 (5) |
| Parrot (*Psittacus erithacus*) | *430 | ? | ? | *320 (7) |
| Hen | 1,500 | 1·5 | 0·42 (5) | 330 (5) |
| Duck (wild) | *1,134 | ? | *1·06 (3) | ? |
| Duck (tame) | *2,060 | *1·62 | 0·63 (2) | *240 (7) |
| Goose | 4,400 | 1·07 | *0·8 (3) | ? |

The table for birds shows us at a glance that, roughly, the smaller the bird and therefore the greater the surface relatively to the mass, the larger is the amount of oxygen consumed or rather of carbon-dioxide given off, by a unit of weight in unit time. If the rate of supply of oxygen to the tissues is greatest, as it ought to be, in those in which the oxidation processes take place most rapidly, we should expect the pulse-rates to vary directly with what we take as a measure of these processes, so long as the relative volume of blood expelled with each beat is the same. Since the relative heart-size varies we should expect to find a reciprocal relation between pulse-rate and relative heart-size dependent upon the rate at which oxidation processes occur. Where we have these data, or a measure of them, the table shows us that this is the case. Thus comparing the pigeon and the sparrow and knowing the pulse-rate of the pigeon, we should expect that of the sparrow to be $\frac{185 \times 12{\cdot}2}{3{\cdot}4} = 693$ in consideration of the different metabolisms, but to be $\frac{693 \times 1{\cdot}5}{1{\cdot}36} = 770$ per minute in consideration also of the different relative weights of the hearts; this is what it is in some sparrows, though it is lower than what was found to be the average for four sparrows. Again the hen, which compared with the sparrow would be expected to have a pulse-rate of $\frac{800 \times 1·5}{12·2} = 98·4$ per minute in virtue of its size and its metabolism alone, would be expected to have one of $\frac{98·4 \times 1·36}{0·42} = 319$ considering also the small size of its heart. If we were to take the carbon-dioxide-output of the thrush as being, as from the size of the bird it is likely to be, about 10 grammes per kilo. per hour, we should have expected its pulse-rate by comparison with that of the sparrow to have been about 666 per minute were it not for the large size of its heart, which makes us expect instead one of only 225 per minute. From what is known of the metabolism of the goose, we should expect its pulse-rate to be about 144 per minute when it is in good condition; we should expect that of the wild duck to be not much more than half that of the tame, allowing for its carbon-dioxide-output per kilo. per hour being, on account of its smaller size, somewhat less than what Dr. Douglas found it to be in the tame duck whose pulse-rate was recorded. Small hearts and correspondingly quick pulses seem, therefore, to be more characteristic of tame birds than of wild, a subject to which we shall have to return.

TABLE II. MAMMALS

| Weight in grammes. | Mammal. | Average carbon-dioxide per kilo. per hour (in grammes). | Average heart-weight in percentage of body-weight. | Frequency of beat per minute. | |
|---|---|---|---|---|---|
| | | | | Observed average when at rest. | Average to be expected by comparison with man. |
| 25 | Mouse | 8·4 | 0·79 (4) & (2) | 700 (4) | 32 |
| 300—500 | Guinea-pig | 1·8 | 0·40 (9) | 300 (7) | 309 |
| 2,000—6,000 | Small Dog | 1·5 | 0·9 (9) | ? | 115 |
| | Cat | 1·3 | 0·45 (9) | 160 (7) & (12) | 198 |
| | Rabbit | 1·2 | 0·27 (9) | 205 (11) | 306 |
| | Hare | ? | 0·75 (8) | 64 (12) | ? |
| 6,000—10,000 | Medium Dog | 1·37 | 0·75 (9) | 120 (7) | 128 |
| 10,000—50,000 | Large Dog | 1·0 | ? | 85 (7) | ? |
| 20,000—100,000 | Deer | ? | 1·15 (8) | ? | *45*? |
| | Sheep | 0·7 | 0·60 (8) | 75 | 80 |
| | Pig | ? | 0·45 (8) | 75 | *100*? |
| | Man | 0·6 | 0·59 (8) | 70 = | 70 |
| 400,000—600,000 | Ox | 0·45 | 0·39 (8) | 48 | 78 |
| | Horse | 0·3 | 0·63 (8) | 37 | 34 |
| | Racehorse | ? | 1·12 (13) | ? | ? |

In the table for mammals a column has been added giving the pulse-rate which, taking both carbon-dioxide-output and relative heart-weight into consideration, we should expect the animal to have compared with man. Man has been chosen as the standard because so many more observations have been made on him that the averages are more likely to be correct than those for the others, with the exception perhaps of the rabbit. Of course somewhat different frequencies would be expected had we chosen for comparison some other animal. If, *e.g.*, we take the relation of the mouse to the cat or rabbit we should expect its pulse-rate to be only about 590 or 490 respectively per minute which is lower than the average found for six mice.

| | Carbon-dioxide-output. | | Pulse-rate. | Relative heart-weight. | | Pulse-rate. |
|---|---|---|---|---|---|---|
| $\frac{\text{mouse}}{\text{man}}$ | $\frac{8{\cdot}4}{0{\cdot}6}$ | = | $\frac{980}{70}$, | $\frac{0{\cdot}59}{0{\cdot}79}$ | = | $\frac{732}{980}$ |
| $\frac{\text{mouse}}{\text{rabbit}}$ | $\frac{8{\cdot}4}{1{\cdot}2}$ | = | $\frac{1435}{205}$, | $\frac{0{\cdot}27}{0{\cdot}79}$ | = | $\frac{490}{1435}$ |

Considering that the rate of formation of carbon-dioxide, the relative heart-size and the frequency of beat, have in the case of nearly all the species been determined by independent observers, it is really rather remarkable how closely the observed and expected frequencies agree. Only in the rabbit and ox[1] is the observed frequency considerably (over 30 per cent.) lower than was to be expected from that of man. It is probably also about 25 per cent. lower in the pig, though we have not yet the data for knowing what to expect for the pig. A higher hæmoglobin percentage in the blood would compensate for what seems to be otherwise too slow a blood-supply to enable the oxygen loss to be made good, but although we know this percentage to be higher in the ox than in man, it is in the rabbit a good deal lower than in man. Since in the rabbit at any rate the averages are likely to be correct, we have probably still to seek for some other factor which enables the supply of oxygen to meet the demand. But it must be remembered that the relative heart-weights may not run strictly parallel with the volumes of blood expelled at each systole in the different species. Unfortunately we do not know and it would be difficult during

[1] If the ox had the same relative heart-weight as the bull (0·53 per cent.), the pulse-rate to be expected by comparison with man would be almost precisely what it actually is in the ox.

life to ascertain what that volume is for any heart; we have had therefore to take the only available data which were at all likely to be a measure of it.

None of the mammals referred to have pulse-rates appreciably higher than those expected by comparison with man. All birds, however, so far as we know, have higher frequencies than would be expected when compared with mammals, *e.g.* that anticipated for the sparrow would be only 618, instead of 800, per minute.

Carbon-dioxide-output. Pulse-rate. Relative heart-weight. Pulse-rate.

$$\frac{\text{sparrow}}{\text{man}} \qquad \frac{12{\cdot}2}{0{\cdot}6} = \frac{1423}{70}, \qquad \frac{0{\cdot}59}{1{\cdot}36} = \frac{618}{1423}$$

The hæmoglobin-percentage does not appear to have been determined in the blood of birds, but in view of the greater size of the red blood corpuscles of birds as compared with those of mammals we might expect it to be lower. On the other hand, the fact that the consumption of oxygen and both relative heart-weight and pulse-frequency are higher in a bird than in a mammal of the same size (*e.g.* in the sparrow than in the mouse, in the pigeon than in the guinea-pig) may have some bearing on the fact that birds maintain a higher constant temperature than mammals.

In this connection it is interesting to note that in the lowest mammals, the Monotremes, and also in the Marsupials, in which a lower body temperature is maintained, the heat produced, as measured by the carbon-dioxide-output per kilo. per hour, is much less than in so-called placental mammals of the same size (10). We know nothing at present about relative heart-size or pulse-rate in these animals. But since the Monotremes, like the small placental mammals, regulate their temperature by the production of more heat when required (*i.e.* in cold surroundings) instead of by always producing a large amount and getting rid of the excess when necessary, as the larger of the higher mammals do, we should expect the pulse-rate in them to vary a good deal, and inversely, with the external temperature. The Marsupials, utilising also variations in loss of heat although to a less extent than the larger of the placental mammals, seem to regulate their body-temperature extremely well. Of the two Monotremes still living, *Ornithorhyncus* succeeds in doing so quite as well as some of the placental mammals;

*Echidna*, although it fails, makes the attempt for the greater part of the year, and the oxygen consumption in the individual, at any given external temperature, seems to some extent to vary inversely with the size, judging from the determinations made by Dr. Martin of the carbon-dioxide-output per unit weight and time in three individuals (10). Those placental mammals which do not regulate their temperature the whole year round do not succeed much better than *Echidna* when they make the attempt, especially on first awaking from hibernation. In some of them the temperature seems to remain lower than in other placental mammals. The rectal temperature of a bat, for instance, may be only 30° C. when it is wide awake and active (16). The low temperature in such cases seems again to be due to the production of heat being small in comparison with other mammals of the same size. Thus in an active bat weighing about 20 grms. the carbon-dioxide-output per kilo. per hour was found to be only about 4·5 grms. and therefore considerably less than in a mouse. If we may take this as a measure of the demand for oxygen in an active bat, the heart need not beat with a frequency of more than 250 per minute to supply the demand, seeing that the heart of the bat, as we happen to know from two independent sources (3) and (16), weighs as much as 1·2 per cent. of the body-weight, and is therefore relatively larger than that of the mouse. A small dormouse on the other hand, in which the carbon-dioxide-output may be as much as 20·4 grms. per kilo. per hour when awake (16), we should expect to have a pulse-rate of over 1000 per minute, even if it has as large a heart (relatively) as a bat. It is said to have a pulse-rate of only 16 or 14 per minute when hibernating (16A).

Before going further a few words should be said about the method of ascertaining the frequency of the beat in small warm-blooded animals. It would be difficult to count a frequency of over 300 a minute or to record any mechanical movements of the heart when they are so rapid in the living intact animal. We can however make use of the fact, the meaning of which is not yet sufficiently understood (5) and (6), that the electrical changes, accompanying muscular activity produce, with each ventricular systole in the case of the hearts of mammals, birds and certain if not all reptiles, two electric fields, the one of which pervades the anterior, the other the posterior part of the body. In order to record the rate at which the fields appear and disappear, we select some spot in each, *e.g.* the mouth and one of the hind legs, and with some good conductor of electricity (such as wool or thread soaked in salt water) connect each with a basin of salt water, these in their turn being connected with the terminals of an instrument sensitive enough to record such small differences of potential as come into existence between the two fields. Such an instrument is the capillary electrometer represented diagrammatically in fig. 2, which shows a bird ready to have its pulse taken. The instrument consists

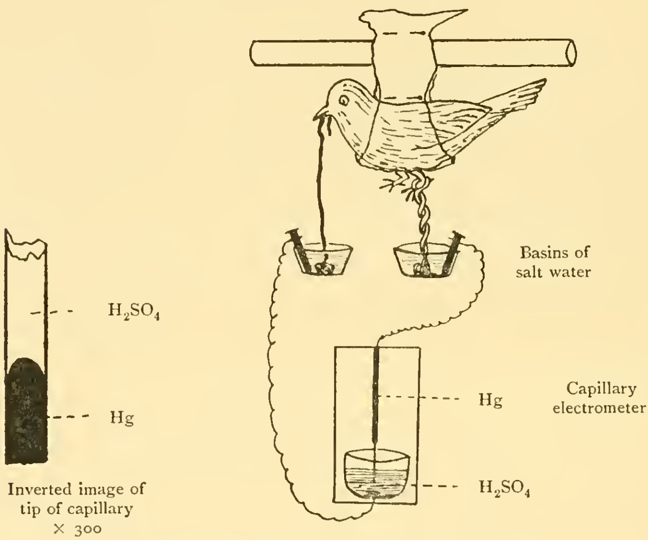


FIG. 2.—Diagram of a bird having its pulse taken.

essentially of a fine glass tube, drawn out so as to be only a few thousandths of a millimetre in diameter near the tip, filled with mercury. The tip dips into dilute sulphuric acid, which enters so far as the mercury permits, the tube being very slightly conical so as just to prevent the mercury running out however near it be to the tip. The properties of the instrument are such that if the mercury becomes (galvanometrically) positive to the acid it moves towards it, if negative it moves in the opposite direction. Since the one field always comes into existence before the other, even though it may be by no more than a thousandth of a second, there is always a quick movement of the mercury in one direction while the single field exists.

There may be other movements, but these first quick ones, each the precursor of a ventricular systole, are easiest to count when recorded. To record the movements the image of the tip of the tube is magnified some 300 times and the boundary between mercury and acid is photographed on a moving plate on which is simultaneously projected the shadow of one end of a tuning fork vibrating at a known rate, so that the speed of the plate may be gauged. Fig. 3 is two-seconds' worth of a record taken with a goldfinch arranged in the way shown diagrammatically in fig. 2. The tracing of a tuning fork vibrating 100 times a second is seen above, and a thick and a thin horizontal line which do not here concern us; the white below is the acid and the black the mercury. The record reads from

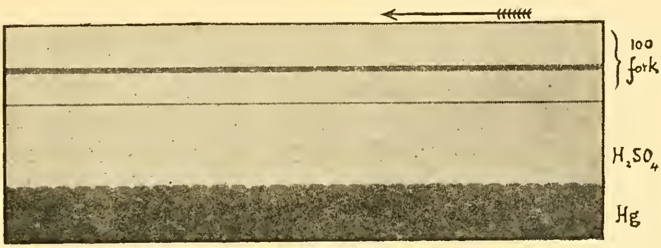


FIG. 3.—Electro-cardiogram of a goldfinch.

right to left and it will be seen that the acid moved towards the mercury at regular intervals. These can be counted; in this particular photograph $30\frac{1}{2}$ of them occurred in the two seconds, indicating that the heart was beating at the rate of 915 times per minute.

Until this method was introduced the frequencies of beat in small warm-blooded animals were not actually known. Their order had, however, already been inferred by Dr. Haldane from the known quick rate of consumption of oxygen. The method he introduced some fourteen years ago of detecting the presence of carbon-monoxide in mines, which has been the means of averting many disasters, depends essentially upon the fact that the more rapid the circulation is through the lungs, the more quickly is an animal affected by poisonous gases absorbed from the atmosphere and the more quickly does it recover in air free from such gases. Since carbon-monoxide, which is far more dangerous to life than any of the other gases which are formed when explosions or fires occur in mines, neither affects the sense organs nor produces pain, miners may remain unaware of its existence and so do nothing to avoid it, until they suddenly succumb. Had they only with them a mouse or a sparrow in a cage, forming as much a part of their equipment as a safety lamp, they would have sufficient time to escape from a place which is dangerous, by leaving as soon as the animal showed symptoms, long before they themselves had absorbed a sufficient quantity to be incapacitated. If they are quick the animal will live to aid them in finding a safe place of retreat. As it takes fourteen to fifteen times as long when at rest and seven to eight times as long when at work, for a man to be disabled as for a mouse, the miner, even if working, would have one or two hours for escape with such percentages of carbon-monoxide in the air as usually occur in mines (14).

The frequency of beat, as we have seen, has not become adapted by itself to regulate the supply of oxygen to the demands of the different warm-blooded animals, but other factors also play their part. We have shown that of these the principal one is the volume of blood expelled per beat. We have now to inquire what significance is to be attached to the fact that now the one and now the other of the two main regulating factors plays the more important part.

Parrot's observations on the relative heart-weights of over fifty different species of birds and those others of birds and mammals referred to in our tables show that the relatively large heart is found in the more active animals. This is so not only in warm-blooded animals but also, as we have already noticed, in fish, flat fish having a relative heart-weight less than half that of more active fish. It is probably also the case in amphibians and reptiles, although we have not yet the determining data; for, however small the demand for oxygen, all animals when active must consume more than when at rest and those that are habitually active must have some means of obtaining more. Moreover, since a large heart works more economically than a small one in that it spends less of its time in overcoming inertia, it would for that reason also be favoured when much work has to be done. The range of variation in relative heart-size is fairly large in all species in which it has been determined in several specimens, but more so in some species than in others. Thus in four specimens of the golden oriole it varied between 1·8 and 2·6 per cent., whilst in seven of the curlew-sandpiper it varied only between 1·6 and 2·0 per cent. In man, according to the determinations made by Bergmann from thirty-six people in whom death was accidental, the variation may be from 0·43 to 0·75 per cent. Müller (2A) dealing not with the weight of the whole heart but only with that of the musculature, in percentage of body-weight, in a large number of individuals who had died of different diseases, shows by his tables that in about 800 people dying between the ages of thirty and sixty, this varied from 0·26 to 0·89 per cent. and further that the percentage weights of this musculature did not vary symmetrically about a mean, but asymmetrically about a mode (*i.e.* the percentage weight of the greatest number) and in such fashion that the mode (0·49 to 0·50 per cent.) was nearer to the relatively small hearts than to the large ones, suggesting that the heart in man is becoming relatively smaller. The suggestion that man's ancestors were larger-hearted is perhaps supported by the fact that in infants the modal ratio is about 0·6 per cent. and even in children from four weeks to three years of age it is further in the direction of the large heart than in the adult, being about 0·53 per cent. But we have to be careful in drawing such inferences from data which cannot be determined in the living, since we do not know in how far the heart-ratio affects the death-rate, a point which Müller, who interprets his tables in a way very different from that which is here suggested, seems to neglect. However this may be, we have ample evidence that in man, as in other mammals, in birds, and so far as we know also in the lower vertebrates, the material is there to be selected from should it for any reason become advantageous for a species to alter its heart-ratio in the future as it has probably done in the past. With regard to the past it seems probable that such variations were used as material for selection before they became correlated with frequency of beat and that it was with the size of the heart more or less already determined that this frequency, which is also known to be variable in individuals, began to be used when it began to be advantageous to be independent of external temperature, owing perhaps to a change from an equable to a variable climate. In the present state of our knowledge it is difficult to point to any advantage which might accrue to any species of poikilothermic vertebrate from having a particular pulse-rate, nor apparently is the variation in different species greater than that in individuals in this respect.

When frequency came to be correlated with relative heart-size for the regulation of the rate of oxygen-supply to the heat-forming tissues, the slow pulse would tell as an advantage as well as the large heart in animals having to make great sustained effort; for a slow pulse as compared with a quick one means longer diastoles more than longer systoles, the systole requiring to be very little longer to expel a much larger quantity of blood, since, in contracting, the walls (the surface) of the ventricles decrease with the square, the contents with the cube. The longer the diastole the more time has the heart to recuperate between the beats when the animal is at rest and the greater power has it in time of need of increasing the oxygen-supply to the tissues by increasing the frequency of the beat. The pulse-rate of the rabbit only goes up to an average of 324 per minute after a few minutes' chasing about or after section of the vagi (11), thus increasing the oxygen-supply by one and a half times at the most; that of the hare goes up under similar circumstances to 264 per minute (12), so that if the same amount of blood were expelled in each systole as when the animal was at rest the oxygen-supply might be increased as much as four and a half times. MacWilliam, drawing attention to the connection between slow pulse and staying power, remarks (12) with regard to these particular closely allied animals, that "the rabbit is able to run short distances with great rapidity, but not to traverse long distances without intermission—this being no doubt in relation to the fact of their having burrows to flee to; the hare, on the other hand, destitute of such means of protection, has to depend, in the open country, upon its endurance in swift locomotion." The relative size of the hare's heart, according to Bergmann's estimations, appears to be nearly three times that of the rabbit's; and, of the pulse-rates of the two animals at rest, that of the rabbit is about three times that of the hare. That staying power rather than wildness itself has led to the larger heart being favoured is shown by the fact that there is very little difference in the relative heart-weights of the tame and the wild rabbit (9).

The relatively small heart of animals kept for food, such as the hen, the tame duck, the pig, the ox and the cow (in which it is the same as in the ox) is on the other hand a consequence of the artificial fattening up of these animals, thus increasing their body-weight while their hearts, having little to do, do not keep pace, finding it possible to supply the oxygen demanded by increasing the frequency of beat. The animals with the smallest hearts would be selected for such purpose by man just because of their being the least active. By similar artificial (though also unconscious) selection in the other direction, *i.e.* of large hearts, that of the race-horse would be accounted for, whilst in the case of the deer and the bat, which are the only other mammals, of those in which relative heart-weight has already been determined, with so large a heart as the race-horse, the same end has been achieved by natural selection.

That frequency of beat in a resting condition, as well as relative heart-size, furnishes material (whether it is used or not) for natural (or artificial) selection to work on is a fact of common experience so far as man is concerned. I have found it to vary between 45 and 90 per minute in quite healthy people. The extent of the range seems to be very different in different species, thus in the mouse it varies between 520 and 810 per minute, in the rabbit between 123 and 306 per minute, whilst a veterinary surgeon informs me that in the ordinary horse its range of variation is between thirty-four and forty only in health. Hering's observations on the pulse-rates of forty-three rabbits show that the modal resting frequency is lower than the average frequency, thus suggesting in the case of the rabbit what Müller's observations did in the case of man, that it has come from a slower-pulsed and larger-hearted race.

Can we go further than showing that variations in frequency exist to be selected from if need be, and indicate also the method by which the heart in birds and mammals has succeeded in adapting itself to the needs of the organism ? We know that regulation of heat in every individual warm-blooded animal is brought about by the agency of the central nervous system. We know also that a warm-blooded animal never *is* cold, although it *feels* cold when brought into cold surroundings, while a so-called "cold-blooded" one which really does become cold under similar circumstances does not feel cold, if we may judge from its behaviour. We find that instead of making the attempt to produce more heat to counterbalance the loss, by eating or moving about, it refuses to do either of these things in the cold. It will not even choose the warmest place and so prevent as much loss of heat as possible. I kept a young crocodile for some months in a long trough so arranged that one end but not the other could be heated from outside. It was so heated every night when the weather was cold, but the crocodile was found indifferently in any part of the trough in the morning, until at last one night in a somewhat longer spell of cold weather it died at the very furthest extremity of the trough from the warmed part. It could have been in a surrounding temperature of 8° C. had it liked; it chose one that was hardly above freezing-point and died there. A warm-blooded animal, feeling the cold, would have made every effort both to prevent loss of heat and to produce more heat; even without effort it would, with the aid of the central nervous system, that is to say reflexly, have done one or other or both things, in some species more the one, in some more the other.

Is it also by means of the central nervous system that the muscles, put into play either voluntarily or involuntarily to produce the extra amount of heat and taking up more oxygen from the blood, ask the heart to make good the loss? It is well known that muscular action is accompanied by acceleration of the heart, and that acceleration of the heart may be brought about by the intervention of nerves. But to answer the question we have to know a good deal more than this, and, in the first place, whether either reflexly by the excitation of the afferent nerves from the muscle or by the action of motor cells of the cortex such acceleration can be produced, also whether poikolothermic vertebrates differ from homœothermic ones in this respect. That it can be produced in one or other of these ways in one species of homœothermic vertebrate, namely man, is shown, I think conclusively, by the results obtained from experiments which, by the kindness of several Oxford undergraduates in serving as subjects for them, I have been able to make. Having recorded the frequency of the beat with the subject sitting quietly with one hand and one foot in basins of salt water connected with the terminals of the capillary electrometer, it was then again recorded when, instead of being at rest, he clenched the fist that was free, or made some other definite muscular action, on hearing a signal given automatically just as the plate began to pass behind the capillary electrometer, and throwing its shadow on to the plate so that the exact moment at which the sound was made was recorded. The reaction-time of the subject to the particular sound had been first ascertained with the same instrument, in a way which need not be here described, to enable us to tell the moment at which the muscular action began to be made and to see in how long or how short a time after it the acceleration of the heart took place. We have of course to take our chance as to when in a cardiac cycle the signal is given, but by taking a sufficient number of records we are likely to meet with it in all the phases of the cycle. The amount of the acceleration with such a slight action as clenching a fist is very different in different people, but if it is marked at all we have no difficulty in ascertaining that it occurs so promptly that if the muscle begins to contract only at the end of a systole, the immediately ensuing diastole of the same cardiac cycle is considerably shortened and that of the following cycles still more so. Thus in a man whose heart when at rest was beating very regularly 73 times a minute, the period of the cycle being therefore 0·82 sec., the period became 0·67 sec. when the fist was clenched at the end of the systole, and the next ones were 0·57 or 0·56 sec., raising the frequency to over 100 per minute. That any stimulus involving mechanical movements of the blood could produce an effect so promptly is hardly conceivable. The shortening of the cycle in cases of such slight action is due to shortening of the diastole only, and MacWilliam's researches (12) on cats have shown us that it is the vagus nerve which principally, if not solely, affects the duration of diastole, and that stimulation of the peripheral end of this nerve produces an immediate effect, whereas that of the accelerator nerve to the heart (the sympathetic) takes some few seconds to produce one. We can therefore not only say from the promptitude with which the heart accelerates when a voluntary action is made that it is due to nerve action, but also that it is the vagus nerve which conveys the impulse to the heart and therefore that the afferent nerve, whether the sensory nerve of a muscle or an axon from a cortex cell, acts on the vagus centre in such a way as to suspend its tonic action. Bowen, in a paper (15) discovered after these experiments had been made, has shown that even so small an action as gently tapping a key, the subject being at rest with his arm supported on a table, produces a prompt acceleration of the heart. His method of recording does not show so well as that described above *how* prompt it is, but he saw that it was enough to indicate that it could only be brought about by the mediation of the vagus.

Of course many other factors—chemical, mechanical and thermal, as well as nervous—must play some part in producing the strong acceleration of the heart consequent on severe exercise, when the frequency may become in man 170 or 180 per minute, and when the duration of the systole as well as that of the diastole is shortened. To answer our question we require to know whether it is to them or to nervous factors only that the acceleration is due which occurs with involuntary, reflexly produced, muscular movements for the regulation of temperature, such as shivering, evidence of which I have obtained from one or two medical undergraduates who kindly took their pulse-rates several times under conditions which induced shivering for comparison with what it was before the shivering commenced. Since the shivering cannot be made to begin at a precise moment, we cannot ascertain in the same way as for voluntary movements whether the heart-acceleration, as well as the movement itself, is brought about by the agency of the central nervous system; but there is a certain amount of likelihood that the two things should be effected in the first instance by the same agency. The fact that the arousing to activity of the central nervous system of a hibernating animal makes it not only begin to shiver (17) or become very active so as to produce heat, but at the same time (or even previously) quickens the heart-beat very considerably (see 6A), also suggests it. It might perhaps be determined whether it were so or not by seeing whether in the first place the animal managed to hibernate if the action of the vagus on the heart were prevented, *e.g.* by the administration of atropine; whether in such case the frequency of beat was reduced to the same extent, and if so, secondly, whether under a continuation of the treatment heart-acceleration occurred, and occurred as promptly, on awakening from hibernation. If in spite of such procedure the animal when awake still succeeded in regulating its temperature, we should know that other agencies than the central nervous system were more intimately concerned in adapting the heart to meet the demands made upon it. We should then be in a better position than we are now to discuss whether the power which we have shown to be exercised by the heart in the different species of warm-blooded animals of complying with the demands made upon it, not on occasion only but for life, has been evolved under nervous control.

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# NEW THEORIES OF THE EVOLUTION OF STELLAR SYSTEMS

By F. W. HENKEL, B.A., F.R.A.S.

During the last few years the researches of Chamberlin, F. R. Moulton and See on the evolution of our system have greatly shaken the faith of astronomers in Laplace's well-known Nebular Hypothesis. More than a century ago Laplace, who more completely than any other had worked out the consequences of Newton's theory of gravitation to the satisfactory explanation of almost every known feature of the motions of the planets, developed a hypothesis previously proposed by Swedenborg, Wright and the great philosopher Kant. The Solar System consists of a number of bodies arranged in an orderly manner, all moving in nearly circular paths round the central body, these paths being all nearly in the same plane and their motion in the same direction, whilst there is a fairly regular progression of distances from the sun (Bode's Law), and the bodies are either spherical or spheroidal. These features are by no means a necessary consequence of gravitation and seemed to imply an original connection or common origin. Laplace supposed that at one time the matter now forming the sun, earth and other planets was in the form of an intensely hot gas, perhaps hotter than the sun is now. This mass was of approximately spherical form and rotated slowly on its own axis, the rotation becoming swifter as the mass grew colder and contracted. In time rings of matter would be left behind the main mass (not thrown off as is sometimes stated); each of these rings would gradually collect into a single globe, and thus the planets would be formed. A planet thus formed continuing to revolve might itself abandon rings in contracting; these rings would form into the satellites. The rings of Saturn were at one time thought to be examples of this process, but we now know that they are composed of swarms of meteorites rather than of continuous substance. Plateau devised an experiment illustrating this formation of rings. He prepared a mixture of alcohol and water of a specific gravity as nearly as possible equal to that of oil. Some oil was then poured into the mixture. As the bottom of the mixture was slightly more dense than the oil and the top slightly less dense, the oil sank half-way and floated in the middle as a round ball. By means of a disc attached to a wire the ball of oil was set rotating. The effect of the rotation caused the oil globe to expand into the form of a spheroid flattened at the poles, and this flattening increased with the speed, until at last a ring was formed which revolved round the globe. After a time the ring broke up and gathered into a smaller globe which rotated, besides revolving round the large globe.

Laplace supposed that the rings would rotate as though solid, their outer edges thus moving more swiftly than the inner, and thus the planets formed therefrom would rotate in the same direction. The "exceptional cases" of our system—the fact that the satellites of Uranus and Neptune move in the opposite direction to that in which most of the other members do, and the swift revolution of the inner satellite of Mars—cannot be explained by this form of the hypothesis.

M. Faye, however, by modifying the original idea of Laplace and supposing that the planets were formed by local condensations (not by the detachment of rings) within the revolving nebula, and that the outer planets, Uranus and Neptune, have been more recently formed than the rest, has shown that these bodies would have retrograde rotation on their axes, which he supposed to be the case from the motion of their satellites. Since, however, Saturn's rotation is in the same direction as that of our own earth, and eight of its satellites move in one direction, but the last discovered (Phœbe) moves in the opposite direction, we have still a difficulty, unless we suppose this body to be a *recent capture and not an original member of the Saturnian family*. The same thing is the case also with the eighth satellite of Jupiter, whose motion is *retrograde*, whilst the other seven have *direct* motion. Prof. Sir George Darwin by his theory of Tidal Evolution has attempted to explain the swift motion of the inner satellite of Mars, the fact that the moon always turns the same face towards the earth, and that the rotation period of Mercury (and probably that of some of the satellites of Jupiter and Saturn) is the same as that of its revolution. He has given reasons for thinking that in former ages the period of rotation of Mars was much shorter than at present, but that by tidal action of the sun this period has been gradually lengthened to its present value; at the same time the satellite's period is supposed to be shortening and its distance from the planet slowly diminishing. In the case of the moon, he considers that millions of years ago our earth was rotating much more quickly than at present. In contracting, a portion separated from the rest, and gradually receded, becoming the moon. The earth's tidal action upon the latter has resulted in the periods of rotation and revolution becoming equal to one another. The ancient Arcadians are said to have boasted that their race *came into existence before the moon*, but they were probably unaware of the period they claimed for their ancestry (fifty-seven millions of years!). The observations of Schiaparelli having led him to the conclusion that the planet Mercury (which is the nearest *known* planet to the sun) rotates on its axis in a period equal to that of its revolution round the sun (88 days), Sir George Darwin considers this is due in a similar manner to the tidal action of the central body having lengthened the planet's period of rotation until the latter always presents the same hemisphere towards the sun, just as the moon does towards the earth. The same thing has been asserted of the planet Venus also, but it still seems probable that the shorter period of $23\frac{1}{2}$ hours, determined by the early Italian observers, is the true length of the "day" on the earth's "twin-sister."

Further modifications, in consequence of increased knowledge of actual existing nebulæ and the applications of the principles of energy and thermodynamics, have been proposed from time to time; and most supporters of the nebular hypothesis no longer believe that in its original condition the "nebula" was even at so high a temperature as that of the sun at present. It is considered probable that the original nebula was largely composed of meteorites, which by collisions during their gradual drawing together would grow hotter and hotter. After a time the central mass would become an intensely hot "white" star. Later on, the loss of heat from radiation exceeding the gain from contraction and condensation, the star would cool down and perhaps finally become a dark body like the companion to Algol. The planets, being smaller than the star or sun round which they revolved, would cool down at a much quicker rate, losing more heat from their surfaces and becoming non-luminous bodies, whilst their interiors would be still very hot. Our earth and the inner planets seem to have reached this stage, whilst Jupiter and Saturn appear to be still, to a small extent, self-luminous. Estimates of the past and future duration of our system have been formed by Lord Kelvin, Helmholtz and others; but the very various lengths of time given, ranging from twenty to four hundred millions of years, alone show that these periods are little more than rough guesses, needing further knowledge to be of value. The discovery of the properties of radium has enormously extended the probable future duration of the sun's heat. "We have every reason to think," says Arrhenius, "that the sun's chemical energy will suffice to maintain its heat during thousands of millions, perhaps billions of years."

In 1861 Babinet proposed the application of a criterion, based on the mechanical principle of the "conservation of areas." He showed that if $\omega$ be the sun's angular velocity of rotation, with radius $r$, and $\omega'$ $r'$ represent these quantities when the globe is expanded so as to have the radius $r'$, then

$\omega r^2 = \omega^1 r^{1\,2}$ [Moment of momentum, a constant quantity for a system rotating freely and subject to no *external* forces]

or

$$C = \Sigma m r^2 \omega = \omega \Sigma m r^2 = \omega^1 \Sigma m r^{1\,2}.$$

Suppose now the "solar nebula" extending to the earth's orbit, let us find its time of rotation. We get for this

$$25{\cdot}3 \text{ days} \left(\frac{23{,}445}{109{\cdot}5}\right)^2 = 3{,}192 \textit{ years}.$$

For the case of Neptune, whose mean distance is thirty times that of the earth from the sun, the solar nebula when reaching to that distance will rotate in

$$25{\cdot}3 \text{ days} \left(\frac{30 \times 23{,}445}{109{\cdot}5}\right)^2 = 2{,}888{,}533 \text{ years}.$$

(These figures are taken from a paper by Dr. See.)

Applying this criterion to the case of the various planets and satellites of our system, we find periods in every case much greater than the known periods of revolution of these bodies. The earth revolves in one year about the sun, Neptune in about a hundred and sixty-five years. Thus it follows that the "hypothetical solar nebula could not have rotated with sufficient speed to detach the masses," when it extended to the orbits of the several planets, as Laplace supposed. The evolution of the planets by separation of rings of matter from the central condensation, through *rotational* instability, must therefore be abandoned. It is, however, possible that secondary condensation nuclei might be formed by *gravitational* instability within the gaseous nebula; and this has been pointed out by Mr. Jeans in papers which he has contributed to the Philosophical Transactions of the Royal Society.

We turn now to the alternative hypothesis developed by the work of Prof. T. J. J. See. He has recently pointed out that some remarkable anticipations of his views as to the action of a resisting medium were made by Euler in 1749. The essential features of this hypothesis are that the Solar System has been formed from a spiral nebula, and that the planets have not been detached from the central mass through its rotation but have been captured or added on from the outer parts of the nebula. The roundness of the orbits of the planets and satellites in general is due to the action of a resisting medium which has reduced the size of their paths, and well nigh obliterated the deviations from circularity. Just as the planets have been captured by the sun's action, so in like manner the satellites have been captured by their several primaries, *not* detached by rotation of these latter. The moon too was originally a planet, which neared the earth and was finally captured and made a satellite. The asteroids or minor planets between the orbits of Mars and Jupiter are the surviving remains of millions of small planets, most of which have been swallowed up by colliding with larger ones, though many are still moving in independent paths round the sun. Our own earth frequently encounters some of these objects, and we have then a more or less brilliant "meteor shower." The satellites having been captured in this way, it is not surprising that a few of them should revolve in the opposite direction to the rest. It is also remarkable that the paths of Phœbe (Saturn's 9th satellite) and of the 8th satellite of Jupiter are much more oval than those of any other known satellites, from which it would appear that the density of the resisting medium must have been very slight at the great distances from the planets at which they revolve. The planetary rotations have also been produced by the capture and absorption of small bodies; and thus the larger planets Jupiter and Saturn should rotate most rapidly, as is known to be the case.

It has long been known that the effect of a resisting medium on the paths of bodies moving in it, in a manner analogous to the planets moving round the sun is (1) to reduce their distances from the central body; (2) to diminish the eccentricity of their orbits, *i.e.* to make these more nearly circular.

The proof of this is given in works on analytical dynamics (Cheyne's, *Planetary Theory*, and other books) and was of course well known to Laplace, who says, "At the same time the planet approaches the sun, by the effect of the resisting medium the orbit also becomes rounder." The well-known comet of Encké is thought to be gradually drawing nearer to the sun by such an action.

Thus the present shape of the planetary paths is accounted for, the action of the resisting medium having changed their orbits. Around each planet circulates a vortex of cosmical dust, and the descent of this material upon the surfaces of sun and planets is considered to give rise to the accelerations of their equatorial regions—*i.e.* the fact that the parts of the sun, Jupiter and Saturn near their respective equators have a shorter period of rotation than those farther north or south. However, there is no perceptible difference of rotation in different regions of our own earth or of Mars, so far as known and the amount of matter required to produce such an effect (at present) seems greater than can reasonably be supposed to fall upon the surfaces of the planetary bodies.

A similar difficulty occurs in the meteoric theory of the sun's heat, attributing the latter to the impact produced by the fall of countless meteorites upon its surface. No doubt such bodies do fall upon the surface of the sun in considerable quantities, but the amount required to maintain the sun's output of heat is so enormous that there should be an enormously greater quantity in regions near the sun, so that our own earth ought to receive nearly half as much heat as she gets from the sun by impact with meteors. This is certainly not the case.

The descent of matter upon the sun increasing its mass may also account for the small secular acceleration of the earth indicated by the observations of eclipses, and the outstanding motion of the perihelion of Mercury, which Leverrier attributed to a planet or ring of small planets lying between Mercury and the sun, may be also explained in this manner.

The moon having suffered numerous collisions with smaller satellites has had its surface marked with the round sunken craters which are so distinctive a feature.

So different a theory from the ordinary volcanic one, however, will not be easily accepted by selenologists. Prof. See considers that the almost perfect circularity of Neptune's orbit shows that it cannot be the outermost planet of our system, the roundness indicating that the nebulous medium was quite dense at that distance, and consequently the limits of the system are much farther out. Other planets lying beyond Neptune have been suspected and may yet be discovered by the telescope. It is remarkable that Prof. Forbes considers that one of these bodies, whose distance he supposes is about a hundred times that of the earth from the sun, and consequently would have a period of a thousand years (by Kepler's third law, squares of periodic times as cubes of distances from sun, $1000^2 = 1,000,000 = 100^3$) moves in a very eccentric orbit, whose plane makes a large angle to that of the ecliptic, the resisting medium at that distance apparently having had little effect on its motion.

The solar system, in the opinion of Prof. See, was formed from a spiral nebula, the latter arising from the meeting of two or more streams of cosmical dust. The system began to whirl about a central point and thus gave rise to a vortex. Great numbers of spiral nebulæ are now known to exist scattered all over the heavens, millions of these objects being visible in the most powerful telescopes. On the other hand, it has been pointed out that there are very few nebulæ of the oblate spheroidal form, such as the hypothesis of Laplace assumed, to be met with in the sky. "Such nebulæ as we see have, it seems, a greater analogy with the solar corona than with the fiery condensing mists conceived of by Laplace" (Proctor, *Old and New Astronomy*, § 1445).

The rotation period of Mars being about 24 h. 37 m. and that of our own earth 23 h. 56 m., Prof. See considers that the period 23 h. 21 m. for Venus, obtained by the early Italian observers, is probably about its true value, and thus the planet is habitable and probably inhabited by intelligent beings.

It is well known that periodic comets probably owe their present position as permanent members of our system to the action of the planets. When a comet coming from outer space in a parabolic orbit approaches a planet its motion is either accelerated or retarded. In the latter case the parabola becomes an ellipse, and the comet henceforth moves in a closed path around the sun, always coming at each revolution to (or near to) the point where this retardation commences. Thus arise the planets' families of comets. A very large number of members of Jupiter's family of comets are known; Halley's famous comet is a member of Neptune's family. In a similar manner it is supposed that the asteroids and satellites have attained their present positions. The whirling of the gaseous matter of a spiral nebula is considered to be due to the unsymmetrical meeting of two streams or to the settling down of a nebula of unsymmetrical figure. From this ultimately results a star surrounded by a system of planets and satellites. The two opposite branches of spiral nebulæ often seen on photographs represent the "original streams of cosmical dust which are coiling up and forming spiral systems." If the streams so converge that the nebulous mass becomes very concentrated, the nebula may divide at its centre and give rise to a double star.

This theory of the capture of the planets and the rounding of their orbits by the action of the resisting medium gives results in some cases the exact opposite to those which are given by the theory of tidal evolution, as investigated by Sir George Darwin.

Whilst tidal friction usually increases the major axis and eccentricity of an orbit, the resisting medium as regularly decreases both elements. "In the actual physical universe both causes are at work together, sometimes one influence preponderating and then the other." With a large central sun and small planets, as in our system, the action of the resisting medium is most effective; for systems made up of two large masses, tidal friction is the predominating agency.

There can be little doubt that these researches form a most important advance in our knowledge of the genesis of our system; and though answers more or less satisfactory may be found to parts of the criticism of Laplace's famous hypothesis, yet we may fairly say that, if not completely disproved, it has been very seriously undermined.

It is not to be supposed, however, that the alternative hypothesis is free from difficulties, some of which have been slightly outlined, but we may still say that it gives a reasonable explanation of many remarkable peculiarities. Further evidence in its favour is no doubt wanted, as well as spectroscopic proofs of motions derived from the study of actual existing spiral nebulæ. Some recent work by Dr. Nolke on the effect of a resisting medium in the evolution of the solar system from a primitive nebulous condition has been published by him at Berlin. Sir George Darwin in his article on "The Genesis of Double Stars" gives an interesting historical account of work on the theory of the equilibrium of revolving liquid bodies, by Poincaré, Jeans and others, together with an application of their results to stars of the Algol type. Probably there is no subject more fascinating than the question as to the past and future of our system; and though from our limited experience, both in time and space, there is the greatest necessity for caution in drawing conclusions, yet the mind of man seems so constituted that it cannot help doing so. It remains for the future to show whether "the vast masses of observational data accumulated by the persevering industry of self-denying men of science" can be put together in the manner indicated above to yield the laws of stellar evolution.

# THE PHYLOGENY AND INTER-RELATIONSHIPS OF THE GREEN ALGÆ

*(Continued from p. 648)*

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The first instalment of this article (Science Progress, No. 16, April, 1910, pp. 623–48) was devoted to a discussion of the lower unicellular and colonial representatives of the Green Algæ. We may now pass on to consider the higher filamentous types.

Among these we are in the first place concerned with the two series of the Siphonales and Ulotrichales, both of which, as we have seen, are probably derivable from the Protococcaceous stock. The strictly siphoneous (*i.e.* non-septate) forms of the Siphonales no doubt constitute a very natural group, about the present-day classification of which there is practically no difference of opinion (Wille 68, Blackman and Tansley 6). A form like *Protosiphon* (fig. 3, E, p. 644[1]) is mostly looked upon as the starting-point; from it evolution no doubt went on in the various directions exemplified by *Bryopsis* and *Caulerpa* on the one hand and the Codiaceæ and Dasycladaceæ on the other. Space does not admit of our dealing with these different types but a brief consideration of *Protosiphon* will be useful (Klebs 40). This is a small cœnocytic organism inhabiting damp mud and consisting of a green vesicular subaerial portion and a long thin subterranean rhizoid, which is colourless. The subaerial portion contains a reticulate chloroplast (cf. *Hydrodictyon*), readily derivable, however, from that of the Chlamydomonad type by way of a form like *Chlamydomonas reticulata* (Goroschankin 30), in which the chloroplast shows a tendency to reticulation; the reticulate chloroplast of *Protosiphon* sometimes breaks up into segments, and this probably indicates the way in which the numerous small discoid chloroplasts of the majority of the Siphoneæ were evolved. Internal to the chloroplast of *Protosiphon* numerous small nuclei lie embedded in the

[1] The page refers to the position of the figure in the first part of this article.

parietal protoplasm, which surrounds the large central vacuole. Reproduction takes place by a process of vegetative budding (fig. 3, E) and by the formation of numerous isogametes by subdivision of the protoplasmic contents of the individual. These gametes give rise to resting zygospores but are also capable of germinating parthenogenetically. If the substratum dries up, the protoplasmic contents of the individual subdivide to form a number of multinucleate cysts, which become coloured red by hæmatochrome and enveloped by a thick membrane. We may regard this latter phenomenon as a primitive characteristic retained from a Flagellate ancestry. It will be seen, therefore, that *Protosiphon* furnishes a useful connecting link between the more elaborate Siphoneæ and the Protococcales (to which *Protosiphon* is referred by some authorities; Oltmanns 53, cf. also Wille 70a), as in both groups we have the same antagonism to vegetative division, while a form like *Phyllobium* of course already shows a marked siphoneous tendency.

In recent systems of classification (Blackman and Tansley 6, Oltmanns 53 and 54) it has become customary to distinguish between two subdivisions of the Siphonales—the typically nonseptate Siphoneæ and the septate (although still cœnocytic) Siphonocladeæ. In certain forms of the latter (*e.g.* Valoniaceæ, fig. 3, F) the thallus is strictly siphoneous at first, and septa only appear as the individual gets older, whilst in others (*e.g.* Cladophoraceæ, fig. 3, G) septation is apparent from early stages onwards. The general tendency is to look upon a form like *Valonia* (fig. 3, F) or *Siphonocladus* (both members of the Valoniaceæ) as the starting-point for the Siphonocladeous series; such forms are of course readily referable to the *Protosiphon*-type. Oltmanns (53 and 54) has, however, propounded a rather different theory as to the phylogeny of the Siphonocladeæ, which also influences the view of the origin of the whole group of the Siphonales. This theory attempts to bring the Siphonocladeæ into line with the Ulotrichales (as illustrated, for instance, by the familiar genus *Ulothrix*)—a view which finds some support in the rather close correspondence between the methods of reproduction in Cladophoraceæ and Ulotrichaceæ. The Cladophoraceæ are assumed to have arisen from a branched or unbranched *Ulothrix*-like ancestor by an increase in the size of the cells, accompanied by a corresponding increase in the number of the nuclei and a gradual elaboration of the chloroplast (cf. *Hydrodictyon*). The acquisition of the multinucleate condition led to a gradual loss of the power of forming separating walls; and so, according to Oltmanns' theory, we would get by successive stages the Valoniaceæ, the Dasycladaceæ and the typical Siphoneæ. It is impossible with the limited space at my disposal to deal with all the evidence in favour of this view but it may be mentioned that the thick annular ingrowths (resembling arrested transverse septa of a *Cladophora*) occurring at perfectly regular intervals in the threads of diverse members of the Codiaceæ (Ernst 24, Küster 43) are interpreted as remnants of original septa, whilst the formation of separating walls in connection with the gametangia of many of the Siphoneæ is looked upon as a reminiscence of an ancestral feature. Oltmann's theory, therefore, involves our giving up the view of the Protococcaceous origin of the Siphonales, and, as a matter of fact, completely inverts this series, the Cladophoraceæ becoming the simplest forms and *Caulerpa*, *Bryopsis*, etc., the most highly developed. In view of the very obvious siphoneous tendency of the Protococcaceæ this seems a rather forced position and one's sympathies will naturally incline to the older theory, which places the Siphoneæ first and derives the Siphonocladeæ from them. There may, however, be some doubt whether the typical Cladophoraceæ (*Cladophora*, *Rhizoclonium*, *Chætomorpha*, etc.) really belong to the Siphonocladeæ at all, and whether just these representatives are not phylogenetically connected with the Ulotrichales. To me there seems much to favour this view, although I should derive all the remaining Siphoneæ from a Protococcaceous ancestry by way of some simple form like *Protosiphon*.

Before leaving the Siphonales it is necessary to consider briefly the position of the genus *Vaucheria*. In recent years there has been a tendency to remove this genus from among the Siphoneæ, chiefly on account of its peculiar zoospore (fig. 3, K), its method of sexual reproduction, and the formation of oil as an assimilatory product (Bohlin 9, Blackman and Tansley 6). For the latter reason and owing to the fact that the two cilia of the spermatozoids point one forwards and one backwards, a position among the Confervales (Heterokontæ) has been advocated (see below, p. 104); the frequent yellow-green colour of the chloroplasts has also been brought forward in support of this view. Nevertheless, the position of *Vaucheria* among the Confervales is a very uncertain one and opinion is again veering round to the view of the Siphoneous affinities of this genus (Oltmanns 53 and 54, Lotsy 46; cf. also Wille 70a). There is no doubt a good deal to be said in favour of such an affinity, and we may briefly review such evidence as is available. The peculiar multiciliate zoospore (so-called synzoospore) of *Vaucheria* no doubt represents the entire contents of a zoosporangium which have failed to become separated from one another (Schmitz 60). This is shown by the fact that the cilia arise in pairs opposite the numerous nuclei and that the vacuole of the zoosporangium remains included within the body of the massive zoospore, although, as a rule (*e.g.* in *Codium*, *Ulothrix*, etc.), the vacuole of the zoosporangium is not employed in the formation of the zoospores. This compound zoospore is therefore quite homologous with the gonidium of a Fungus like *Pythium* (except that the latter remains enveloped by the wall of the sporangium) or other of the Peronosporaceæ, and may be regarded as a peculiar expression of tendency in *Vaucheria*, to be sought for in the allies of the latter. The work of various American botanists (Davis 19, Stevens 65, and others) on the cytology of the sexual organs of the lower Fungi (Phycomycetes) has shown that these organs, even when outwardly oogamous[1] in character, are to be regarded as derived from gametangia, in which the gametes do not become distinct from one another (constituting a so-called cœnogamete). In many of these forms the cœnogamete has become uninucleate by diverse methods of suppression of the remaining nuclei, all of which are functional, for instance, in such a form as *Cystopus Bliti* (Stevens 65). On the basis of this and other pieces of evidence the view that the Phycomycetes have arisen from a siphoneous algal ancestor has become fairly well established; in this connection we may call to mind the fact that there is a marked saprophytic and parasitic tendency among the Protococcaceous allies of the Siphoneæ (cf. the first part of this article, p. 645). In the oogonium of *Vaucheria* we have a structure which is a multinucleate cœnogamete to begin with, although it subsequently (as in many of the Phycomycetes) becomes uninucleate in a way about which there is still some difference of opinion (Oltmanns 52 and 53, Davis 20, Heidinger 32). Nevertheless, both in its oogonium and its zoospore *Vau-*

[1] The pseudo-oogamy may perhaps be an arrangement for the better nourishment of the female cell.

*cheria* obviously exhibits very marked and fundamental analogies with the Phycomycetes, and it seems probable that *Vaucheria* shows us a remnant of those forms which were transitional between the Siphoneæ and the Phycomycetes. The reproductive organs of *Vaucheria* in themselves exemplify a number of possible stages in the transition between the two groups, as in the antheridium of this genus we still have liberation of the individual motile elements as spermatozoids, in the zoosporangium we have coalescence of the motile elements to form the synzoospore (or we might call it cœnozoospore), whilst in the oogonium we have a motionless cœnogamete, becoming uninucleate at maturity (cf. also Lotsy 46). The fact that some species of *Vaucheria* favour a terrestrial habitat may perhaps have some connection with the evolution of its oogonium and peculiar type of zoospore.

The process of zoospore-formation in the curious genus *Derbesia* (a member of the Siphoneæ), which has recently been investigated by Davis (Davis 20a), shows many points of interest in this connection. The young zoosporangium, as in *Vaucheria*, contains numerous nuclei, a large number of which, however, subsequently degenerate. Cleavage of the protoplasm takes place around the remaining nuclei, and in this way a number of uninucleate zoospores are formed. Each zoospore is provided with a circle of cilia at its front end. In the zoosporangium of *Derbesia* one therefore finds the same tendency towards abortion of nuclei as in the oogonium of *Vaucheria* or of the Phycomycetes, but this process of abortion does not go to the extreme of leaving only a single functional nucleus within the sporangium. In its vegetative characters *Derbesia* shows considerable resemblance to *Vaucheria*; the results of Davis's investigation of the former afford additional evidence of the existence of a group intermediate between Siphonales and Phycomycetes.

If we decide to regard *Vaucheria* as a form leading over to the Phycomycetes, we have still to consider its relation to the siphoneous series. Although the spermatozoids show an indication of Heterokontan characters (cf. below, p. 104), the units of the zoospore are typically Isokontan (*i.e.* with two equal cilia) and agree with the type of motile element prevalent among the Siphoneæ. The chloroplasts of *Vaucheria* are, on the whole, more like those of the Siphoneæ than those of the Heterokontæ; I am not aware that the yellowish tinge of its chloroplasts has been shown definitely to be due to an excess of xanthophyll (which is a prominent characteristic of the latter group). We are thus left with the occurrence of oil as the assimilatory product as the most prominent peculiarity of *Vaucheria*, distinguishing it from other Siphoneæ. In the genus *Phyllosiphon* (Buscalioni 11), which is parasitic in the leaves of various plants, we have a form of doubtful position, although certainly of siphoneous affinities. In this Alga, however, both starch and oil occur side by side, and this may help to bridge over the gap between the starch-producing Siphoneæ and the oil-producing *Vaucheria*.

In recent years a new siphoneous genus has been described by Ernst (23) under the name of *Dichotomosiphon*; this seems to constitute a further connecting link between *Vaucheria* and the Siphoneæ (especially the Codiaceæ among the latter, cf. also Lotsy 46). Although *Dichotomosiphon* forms starch, it has usually been placed side by side with *Vaucheria* (Blackman and Tansley 6, Wille 70a) owing to the close resemblance between the two in other respects. *Dichotomosiphon* (fig. 3, M) consists of branched unseptate filaments, which are peculiar in their dichotomous branching and in the possession of annular membrane-thickenings (cf. fig. 3, M) at regular intervals, and in both respects resemble very closely the filaments of many of the Codiaceæ. Zoospores have not been observed, but their place is taken by large club-shaped spores (gemmæ) formed at the ends of special rhizoid-like branches (fig. 3, M) and on germination giving rise to a new filament. We have only to imagine that the terminal zoosporangium of a *Vaucheria* ceases to form the synzoospore, and after a certain time germinates as a whole, to obtain a structure very similar to the gemma of *Dichotomosiphon*. This is almost precisely what occurs in certain species of *Vaucheria* (e.g. *V. piloboloides*, cf. Ernst 25). Here the contracted contents of the zoosporangium do not develop any cilia, but become enveloped by a membrane before being protruded from the sporangium (fig. 3, H), or they may even germinate from within the latter; in other cases the whole sporangium grows out vegetatively to form a new filament (fig. 3, L) and this is obviously almost identical with what we have in *Dichotomosiphon*.[1] We have thus an almost complete transition between

[1] It is also useful to compare these structures with the characteristic resting-spores (akinetes) of *Pithophora* (Cladophoraceæ).

the zoospore of *Vaucheria* and the gemmæ of *Dichotomosiphon*. In the structure and character of the sexual organs there is almost complete agreement between the two genera, but their position in *Dichotomosiphon* is peculiar, since they are here seated at the ends of the final forking branchlets of the filament and not laterally as is the case in the majority of species of *Vaucheria*; but even in this respect we find in certain species of the latter genus (e.g. *V. piloboloides*) some analogy. The previous considerations seem sufficient to establish a fairly close relationship between *Vaucheria* and the Siphoneæ; at the same time we have learnt to regard the former as a transitional type to the Phycomycetes.

The Ulotrichales, to which we may now turn our attention, do not require a lengthy treatment. We have already seen how this series can be derived from the Protococcaceous stock by way of a form like *Chlorosphæra*, which combines their two chief characteristics—vegetative division and zoospore-formation. Another view derives them from the "filamentous" Tetrasporaceæ (Blackman 5) but, as already pointed out above, the forms included under this name are probably largely stages of Ulotrichales, and, although not altogether indisputable, such an ancestry is not easy to support in the light of our present knowledge. The actual line of evolution followed by the Ulotrichales is shrouded in darkness. They show the most elaborate development of the septate filamentous habit among the green Algæ, especially if (as above advocated) we include the Cladophoraceæ in this series. They are the most likely group from which to derive the higher plants (Archegoniatæ), but how this derivation was effected it is at present impossible to say, although a form like *Draparnaldia* (Berthold 4), with its upright and almost purely supporting main axis bearing richly branched lateral assimilating threads, shows the morphological possibilities of the simple filamentous habit. There are various other evolutionary tendencies among the Ulotrichales that may be briefly indicated. One of the most important is the tendency to depart from the upright habit and to replace it by an epiphytic thallus (Huber 35) of richly branched filaments emanating from a central point (well seen in the common genus *Aphanochæte* and its allies); in the highest forms (*Coleochæte*, etc.) this culminates in the formation of a compact disc-like plate, which in some cases may even become an endophyte. It is probable that this tendency led in another direction to the evolution of *Pleurococcus* (Artari 2, Chodat 12 and 13), which is best regarded as a type reduced from a filamentous Ulotrichaceous ancestor and only rarely forming zoospores (cf. however Wille 70a, p. 36); this view is supported by the fact that under certain circumstances *Pleurococcus* may form short filaments (fig. 4, L). Another tendency shown by the Ulotrichales is towards a terrestrial

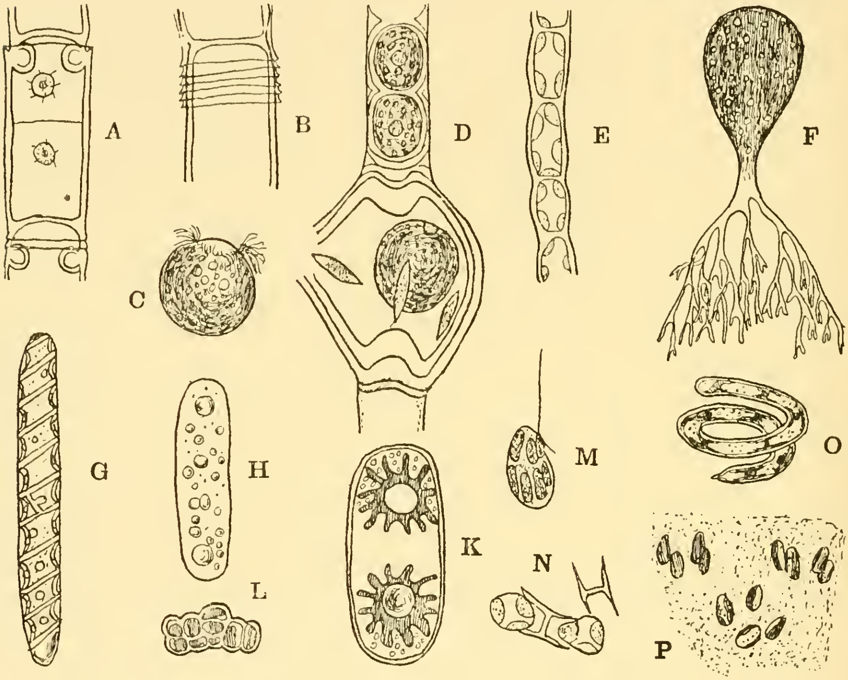


FIG. 4.

A[2] *Œdogonium Borisianum*, Wittr. (cell-division). B, *Œd. gemelliparum*, Pringsh. (cap-cell). C, *Œd. concatenatum*, Hass. (zoospore). D, *Cylindrocapsa involuta*, Reinsch. E, *Conferva bombycina* (Ag.), Lagerh. F, *Botrydium granulatum*, Rost. et Wor. G, *Spirotænia condensata*, Breb. H, *Mesotænium De Greyi*, Turn. K, *Cylindrocystis diplospora*, Lund. L, *Pleurococcus Nägeli*, Chod. M, *Conferva bombycina* (Ag.), Lagerh. (zoospore). N, *C. bombycina* (Ag.), Lagerh. (liberating aplanospores). O, *Ophiocytium majus*, Nägeli. P, *Chlorosaccus fluidus*, Luther. (A and C after Hirn; B after Pringsheim; D after Cienkowski; E after Gay; F after Rostafinski and Woronin; G–K after West and West; L after Chodat; M and P after Luther; N after Oltmanns; O after West.)

habit (Chroolepideæ, see Oltmanns 53, p. 247), an evolutionary line showing some interesting parallels with the lower Fungi, which lack of space, however, prevents our following up. Another prominent peculiarity of the Ulotrichales, the formation of hairs, is not of any phylogenetic importance.

All the different algal forms hitherto considered are at the present day generally grouped together as Isokontæ, one of the groups that has emerged from the old class of the Chlorophyceæ. The establishment of this group dates from the definite recognition of the Flagellate ancestry of the Algæ (Bohlin 9, Blackman 5). All the different genera considered in


Table showing probable relationships of the different groups of the Green Algæ.

the preceding pages are therefore supposed to arise directly or indirectly from a Flagellate group, having two (or four) cilia of equal length, green chloroplasts with pyrenoids and starch as the first visible product of assimilation. In the preceding pages we have endeavoured to trace back all the diverse series of the Isokontæ to a Chlamydomonad ancestry, and the Chlamydomonads themselves have been shown to present many similarities to certain of the living Flagellates (Cryptomonadineæ), so that the origin of the Isokontæ from Flagellata is fairly well established. As groups equivalent to the Isokontæ, certain authorities (Bohlin 8 and 9, Blackman and Tansley 6) have established the Heterokontæ, the Stephanokontæ (Œdogoniaceæ), and the Akontæ (Conjugatæ). There is little doubt about the origin of the first of these groups from a Flagellate ancestry (cf. however Wille 70a, p. 2) distinct from that of the Isokontæ, but we cannot say the same for the other two. The Stephanokontæ are a small group, including but a single order, the Œdogoniaceæ, the best-known representatives of which are the genera *Œdogonium* and *Bulbochæte* (Pringsheim 56, Hirn 34). Among the numerous peculiarities of the Œdogoniaceæ is the production of large zoospores (cf. also Pascher 55), formed singly from the cells of the filament and provided with a crown of cilia inserted a little way from the front end and around the base of a prominent colourless beak (fig. 4, C). The establishment of the group of the Stephanokontæ is based on these peculiarities of the zoospore (the androspores and spermatozoids show similar features), but it must be borne in mind that the large size of the Œdogoniaceous zoospore as compared with that of the Isokontæ may well have brought with it an increase in the number of cilia, and that in the zoospores of the Isokontæ the cilia also frequently arise from the base of a small colourless papilla at the anterior end. The zoospores alone, therefore, would not give too much weight to the view of the independent origin of the Œdogoniaceæ, particularly as in *Polyblepharis* (fig. 1, L, p. 629) we have among undoubted Isokontan forms a motile organism with a considerable number of cilia. Moreover, we have at present no knowledge of a Flagellate showing any marked resemblance to the Œdogoniaceous zoospore. The origin of the Stephanokontæ from a distinct Flagellate group is therefore highly doubtful. Nevertheless, the group presents certain marked peculiarities that distinguish it rather sharply from the Isokontæ and which might favour a distinct origin more than the characters of the zoospore do. Thus the method of growth and cell-division, which takes place by means of a curious thickening-ring at the upper end of the cells (fig. 4, A) and leads to the formation of the characteristic caps (fig. 4, B), is without direct parallel (Hirn 34); and the same may be said of the dwarf-males (found in many species of *Œdogonium* and in all of those of *Bulbochæte*), which are formed from modified zoospores (androspores), settling down in the immediate neighbourhood of the oogonia and producing a small unicellular or few-celled plant, which gives rise to a limited number of spermatozoids. These dwarf-males may possibly have arisen from zoospores, forming antheridia precociously (cf. Fritsch 27).

Recent advocates (Oltmanns 54, p. 13) of an Ulotrichaceous affinity of the Œdogoniaceæ have attempted to connect up the two groups by way of the rare freshwater genus *Cylindrocapsa* (Cienkowski 14). This forms unbranched filaments (fig. 4, D), the cells of which are very similar to those of an *Ulothrix*, which *Cylindrocapsa* resembles also in its marked faculty of forming palmelloid stages. Zoospores have not yet been observed, but in its sexual reproduction *Cylindrocapsa* diverges widely from *Ulothrix*, inasmuch as it is oogamous (fig. 4, D) like that of *Œdogonium*. The spermatozoids, however, are biciliate and not multiciliate like those of the latter genus. *Cylindrocapsa* certainly helps to a slight extent to bridge the gap between Ulotrichaceæ and Œdogoniaceæ, but there are still many difficulties in the way of such a connection, and for the present we must regard the Stephanokontæ, even if of Isokontan affinity, as standing markedly apart from the remainder of the Isokontæ.

Long before the division of the Akontæ became established under this name, the Conjugatæ, with which they are synonymous, were separated off as a distinct group from the remainder of the Chlorophyceæ (Wille 68). The main basis for this separation was the special character of the sexual process (familiar to all in the case of *Spirogyra*) and the absence of motile reproductive units of any kind, and there can be no doubt that in view of these marked peculiarities such a separation is warranted, just as in the case of the Stephanokontæ. No one, however, has ever seriously suggested a distinct Flagellate origin of the Akontæ, and most, if not all, authorities consider them to be connected in some way with the Isokontæ, either as a collateral group arising from the same Flagellate ancestry or even branching off from the Isokontan stock itself. There are not very many points of analogy between present-day Conjugatæ and Isokontæ, but the most important are the similarity between the sexual process of the former and that of *Chlamydomonas Braunii* and some other species of the same genus, and the bivalved character of the wall of the Phacotaceæ (Chlamydomonadineæ). The peculiarity of the sexual process of *Chlamydomonas Braunii* (see first part of this article, fig. 1, G, p. 629) lies in the fact that the clothed gametes become united by fusion of their walls at the apex, after which the contents of the microgamete pass through the canal thus established into those of the macrogamete, a process very similar to the conjugation of a *Spirogyra*. It is quite possible that in forms exhibiting this type of conjugation the power of free movement (by cilia) of the gametes might be given up altogether, simultaneously with its disappearance in the ordinary vegetative individual, and especially if a filamentous tendency set in. In some such way the Conjugatæ may have arisen. Although the character of the sexual process and the lack of motile power in the gametes is thus not a point of insuperable difficulty, the bivalved nature of the wall in the Desmids (one of the two large subdivisions of the Akontæ) is not easily paralleled (Lütkemüller 48). The Phacotaceæ (cf. p. 631 in the first part of this article) show a similar feature, but the valves there do not fit over one another as they do in the Desmids; nor is the method of cell-division at all similar in the two cases, for in the latter each daughter-cell receives one valve of the mother-cell and only forms the other half afresh. The method of cell-division and the structure of the cell-wall in certain Heterokontæ (like *Conferva*) and in the Œdogoniaceæ shows some analogies with that in Desmids, but they are at the best rather remote. The Akontæ, therefore, occupy much the same sort of position at the present moment as do the Stephanokontæ, but the evidence for their direct origin from the Isokontan stock is rather more weighty than in the case of the latter group.

The Conjugatæ are usually divided into two series, the Zygnemoideæ and the Desmidioideæ (*e.g.* Blackman and Tansley 6). Of these the former consist of unbranched filamentous forms (e.g. *Spirogyra*, *Zygnema*, *Mougeotia*), while the latter are prevalently unicellular, although there are a number of filamentous representatives. The Desmids are also peculiar in the bivalved character of the cell-wall referred to above, and in the perforation of the wall by numerous pores through which mucilage is excreted (Schröder 61, Lütkemüller 48). Both series agree in the possession of large and usually complex chloroplasts often provided with numerous pyrenoids. There is much difference of opinion as to the phylogenetic relation of the Zygnemoideæ and Desmidioideæ, some looking upon the one and some upon the other group as the more primitive (West and West 67, Blackman and Tansley 6, etc.). This is a point however which, with our present knowledge, it is impossible to determine, for every argument can easily be inverted and employed to demonstrate the reverse view. It is, therefore, unnecessary to discuss these theories here, but a few words may be added on a very ingenious interpretation of the Conjugatæ recently put forward by Oltmanns (53, p. 89). The latter removes three unicellular forms (the genera *Mesotænium*, *Cylindrocystis*, and *Spirotænia*) from the Desmids and classes them separately as the Mesotæniaceæ (fig. 4, G–K). These are distinguished by lacking the bivalved wall and the mucilage-pores of the true Desmids, and by the development of four new individuals from each zygospore on germination. The three genera further show the three most characteristic types of chloroplasts of the Conjugate group—*Mesotænium* (fig. 4, H) having the flat plate of a *Mougeotia*, *Cylindrocystis* (fig. 4, K) the two stellate chloroplasts of a *Zygnema*, and *Spirotænia* (fig. 4, G) a spiral chloroplast like that of a *Spirogyra*. In the majority of the Mesotæniaceæ the conjugation-process is of a very simple type, the contents of the two conjugating cells meeting and fusing in the much-enlarged copulation-canal. Oltmanns regards the Mesotæniaceæ as simple forms resembling the ancestors of the Conjugate group, and derives from them on the one hand the Zygnemaceæ and on the other hand the Desmidiaceæ. The former are characterised by the formation of a single individual from the zygospore on germination, while in the latter two individuals are always formed. The zygospore of the Desmidiaceæ, however, shows two nuclear divisions on germination, but of the four nuclei thus produced two regularly degenerate (Klebahn 36). This Oltmanns regards as evidence of derivation from an ancestor forming four individuals from each zygospore, *i.e.* from forms like the Mesotæniaceæ. There can be no doubt that this opens up a very suggestive point of view; on the evidence furnished by the zygospores and other characters Oltmanns' Mesotæniaceæ certainly seem to be simpler than the true Desmids, while they are well suited to stand as a group resembling the ancestors of the Zygnemaceæ. Oltmanns' theory, moreover, dispenses with the problematic relationship of Desmids and Zygnemaceæ, and makes these two groups diverge collaterally from a common ancestry.

It finally remains to consider the case of the Heterokontæ, a group in which a distinct Flagellate origin is now well established, and which is fairly sharply marked off from the other green Algæ (Bohlin 8 and 9, Blackman 5), although Wille (70a, p. 2) in his recent revision of the Chlorophyceæ does not accept the group. The most distinctive characters of the Heterokontæ are furnished by the zoospores (cf. also Pascher 55), which have a certain power of amœboid change of shape, possess two cilia of *unequal* length (or, in some cases, but a single long one[1]), and have yellowish green chloroplasts without pyrenoids (fig. 4, M), the yellow colour being due to the presence of an excess of xanthophyll. Other characteristics are the absence of starch, oil or soluble carbohydrates being the first assimilatory products, and the presence of a considerable quantity of pectic substances in the cell-wall. The best-known algal representative of the Heterokontan series is the genus *Conferva* (Lagerheim 44), and we may briefly consider it as a type. It consists of unbranched filaments (fig. 4, E), which are composed of cylindrical cells, the genus occurring both in fresh and salt water. Each cell contains a considerable number of somewhat irregular discoid chloroplasts, having the yellow-green colour characteristic of the group, while the markedly stratified wall of the cell consists of two halves, the edges of which fit over one another. Since the half-pieces of adjacent cells are joined together by the transverse wall, the filaments tend to break up into a number of H-shaped pieces, when subjected to gentle maceration or when zoospores (or aplanospores) are liberated (fig. 4, N). The latter are formed to the number of one or two in each cell, and show the characters mentioned above. On coming to rest the zoospores exhibit amœboid movements and then become attached by their back end, while the front end grows out to form a new filament. Sexual reproduction has recently been described, and appears in some cases to show indications of anisogamy (Scherffel 58).

It is in the group of the Chloromonadineæ (one of the algal Flagellate groups) that we have Flagellate forms, which show

[1] It is not improbable that a second short cilium may still be discovered in these cases, as the biciliate zoospores of *Conferva* were originally described as uniciliate.

much affinity with *Conferva* and its allies (Senn 63). In the genus *Chloramœba* (Bohlin 7), which is the simplest representative of these Chloromonadineæ, we have a naked unicellular motile form, possessed of two cilia of unequal length and capable of amœboid movement by the protrusion of broad and blunt pseudopodia (see the first part of this article, fig. 1, P, p. 629). Embedded in the protoplasm are from two to six lens-shaped chloroplasts of a yellow-green colour and abundant oil, which is the product of assimilation, while a contractile vacuole is situated beneath the point of attachment of the cilia. The method of reproduction by division has not yet been observed, but resting-cells provided with a thick membrane are formed in response to unfavourable conditions. In *Chloramœba* we therefore have a Flagellate form agreeing in all essential respects with the zoospores of a *Conferva*, and a form very like *Chloramœba* is therefore regarded as being the ancestor of the whole group of the Heterokontæ. Another member of the Chloromonadineæ, the genus *Chlorosaccus* of Luther (47), serves as a connecting link between *Chloramœba* and the algal representatives of the Heterokontæ. *Chlorosaccus* (fig. 4, P) may be described as the *Tetraspora* of the Heterokontan series, although it is still a Flagellate in its characteristics. It is a colonial form appearing in the shape of bright green spheres attached to other water-plants. These spheres consist of soft mucilage, which is composed of pectic substances, and in which green pear-shaped cells with the pointed end directed outwards are embedded at considerable distances. When the cells multiply they divide longitudinally, twice in rapid succession, so that they tend to lie in groups of fours, a feature which heightens the resemblance to a *Tetraspora*. Each cell contains two yellowish green chloroplasts and a nucleus and contractile vacuole (fig. 4, P), as in *Chloramœba*. Reproduction is effected by means of zoospores provided with two unequal cilia, which slip out of the enveloping mucilage, and, after a swarming-stage, divide to form new colonies. Resting-cells are formed in the same way as in *Chloramœba*.

The Heterokontan series thus shows different types, which are parallel to those found in the Isokontæ; we have noticed the motile unicellular type, the palmelloid type, and the filamentous type, but others (like the motionless unicellular type reproducing by zoospores, as seen in *Ophiocytium* (fig. 4, O), etc.)

are also represented. The series is much more definitely circumscribed than is that of the Isokontæ, although this may possibly be due to lack of knowledge on our part. It is interesting to note that the transition from Flagellate to algal characteristics took place at rather a different point in the Heterokontæ than it did in the Isokontæ. For the algal groups of the former do not include any motile unicellular forms comparable to *Chlamydomonas* and its allies, and even the *Tetraspora*-like *Chlorosaccus* is still a Flagellate in all essential characters.

Since the establishment of the group of the Heterokontæ it has become customary (Blackman and Tansley 6, Oltmanns 53) to include in it the genus *Botrydium*,[1] a small cœnocytic form inhabiting the damp mud round the edges of pieces of water. It shows some points of resemblance to the genus *Protosiphon* (cf. above), with which it was formerly confused (Klebs 40). Like this genus it consists of an overground vesicular portion and a colourless subterranean rhizoid, which is, however, richly branched (fig. 4, F). It agrees with the Heterokontæ in forming oil as the product of assimilation and in possessing zoospores showing considerable resemblances to the Heterokontan type. These zoospores are produced in immense numbers by subdivision of the contents of the vesicular portion, and are liberated through a small aperture formed at the apex of the latter. They are provided with a single cilium (perhaps accompanied by another very short one?; cf. footnote on p. 104) and have two chloroplasts, as is frequently the case in the zoospores of *Conferva* and its allies. There is also resemblance to the latter in the fact that *Ophiocytium* (Confervales, fig. 4, O) shows a marked cœnocytic tendency. These indications of affinity are, however, all somewhat remote, and a difficulty is also to be found in the fact that the chloroplasts of *Botrydium* are stated to have pyrenoids (not forming starch) in young stages (Klebs 40, p. 224), although they disappear later on This latter feature may possibly point to derivation from an Isokontan ancestor, and the cœnocytic character of *Botrydium* of course suggests an affinity with the Siphoneæ. Wille (70a, p. 51) classes *Botrydium* together with *Protosiphon* in a separate family, the Hydrogastraceæ, which he regards as a connecting link between his Protococcaceæ and Valoniaceæ. Apart from their general organisation the two genera do not, however, appear to have

[1] Regarding *Vaucheria* and *Dichotomosiphon*, see pp. 93 and 96.

much in common, and on the whole, *Botrydium* seems most happily placed for the present among the Heterokontæ, although occupying rather an isolated position in this group.

In conclusion it may be mentioned that Scherffel (58) assumes a relationship between the Cryptomonadineæ and the Chloromonadineæ by way of a peculiar representative of the latter, the genus *Vacuolaria* (Senn 63). This is a motile unicellular form having two cilia of equal length, one of which, however, points forwards and the other backwards (possibly a preliminary to unequal development); these cilia arise from a depression at the front end in the same way as do those of the Cryptomonadineæ (see the first part of this article, fig. 1, s, p. 629). The symmetry of the body in *Vacuolaria*, moreover, appears to be dorsiventral, like that in the Cryptomonadineæ. The numerous chloroplasts of *Vacuolaria*, on the other hand, agree more with the characters of the Chloromonadineæ. This cannot, however, be regarded as establishing a very close relationship, and we shall require to know a good deal more about these two Flagellate groups before such an assumption can be ratified. There is every reason to assume, however, that both Cryptomonadineæ and Chloromonadineæ originated from a common group of colourless Flagellates like those represented by Senn's Protomastigineæ (Senn 63) at the present day, but such an origin probably lies rather far back in the Flagellate series. We have thus traced the phylogeny of the green Algæ, as far as our present knowledge admits, along the numerous lines of evolution originating from the Flagellate ancestry, which formed the basis for the whole series of forms.

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[1] In view of the very extensive ground covered by this article, only a very limited number of literature-references could be included. In most cases the inclusion of the most recent or most fundamental contribution on a certain topic has been aimed at.

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# MAGNETIC ALLOYS

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With respect to their magnetic properties all materials can be divided into three classes :

(1) Diamagnetic substances, such as bismuth, which, in a magnetic field, set themselves with the long axis of the specimen across the lines of force and tend to move from the stronger to the weaker part of the field.

(2) Weakly para-magnetic substances, such as many metallic compounds and their aqueous solutions, which are drawn into the strongest part of the field but do not exhibit the phenomena of hysteresis or magnetic saturation.

(3) Strongly para-magnetic or ferro-magnetic substances, such as iron, including those usually spoken of as magnetic, which are drawn with a considerable force towards the strongest part of the magnetic field and are further distinguished from the weakly para-magnetic substances by the phenomena of magnetic saturation.

Besides iron, the latter class includes magnetite ($Fe_3O_4$), pyrrhotine ($Fe_7S_8$), nickel, cobalt and a number of alloys, some of which contain one or more of the ferro-magnetic metals, whilst others are composed wholly of metals which are non-magnetic when pure, *i.e.* either diamagnetic like copper or weakly para-magnetic like manganese.

The most important example of this latter class is the so-called Heusler alloy, discovered in 1903, which consists of copper, manganese and aluminium. The composition by weight of a typical example may be taken as copper 65 per cent., manganese 23 per cent., aluminium 12 per cent., although equally good quality may be found in samples which differ considerably in composition. In any case, the manganese and aluminium should be present in approximately atomic proportions, as if more than about 25 per cent. of manganese be used the alloy becomes so hard as to be unworkable. The magnetic properties appear to be inherent in certain crystalline masses, not themselves the magnetic units but which contain, as one of their structural elements, molecular groups that are magnetic at proper temperatures.

The magnetic quality of any particular specimen depends much more upon its thermal history than upon its composition,

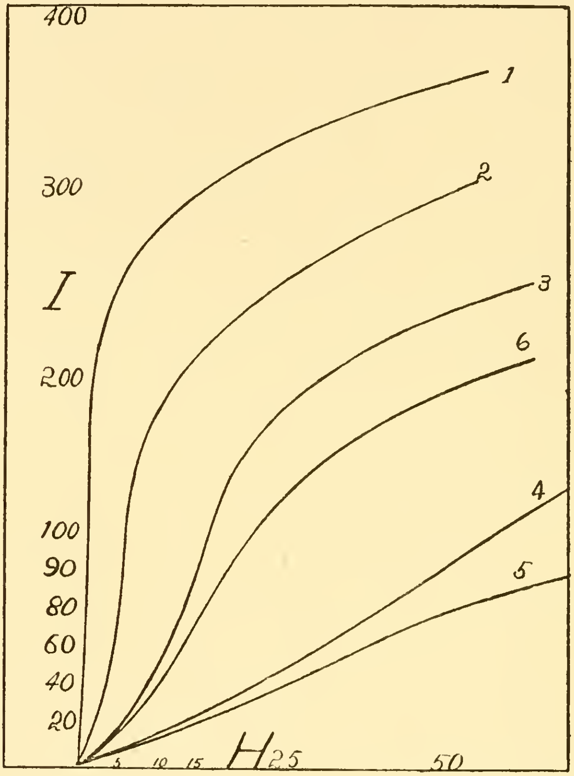


FIG. 1.—Explanation of curves. All curves refer to the same sample.

29·6 per cent. Mn, 8·2 per cent. Al, 6·2 per cent. Cu, 0·2 per cent. Si. Curve 1, best conditions after heating to 125° during thirteen hours; 2, after eight hours near 200° and five hours between 275° and 300° (another heating of five hours at 100° and two hours at 200° produced no further change); 3, five hours at 200°–225°; 4, annealed at 225° after heating above transformation point; 5, after nine and a half hours at 250°; 6, after fourteen hours at 290°–350°.

although the maximum attainable intensity of magnetisation—*i.e.* the saturation value after most favourable heat treatment—depends upon the composition. As is well known, iron loses its magnetic properties when heated to 785° C. and regains them upon being cooled to a slightly lower temperature. The Heusler alloys undergo a similar transformation at temperatures which range downward from 300° C. to 0° C. or even lower, according to their composition. When cooled below their transformation temperature in some cases the alloys are found to be nearly or quite non-magnetic, whilst in other cases their magnetic quality is greatly improved after passing through a thermal cycle. These contradictory results have been shown to depend upon the manner of cooling. In general, the transformation from the magnetic to the non-magnetic state takes place over a range of about 50° C. The effect of annealing at or just above the upper limit and subsequently cooling at a fairly rapid rate is generally favourable, whilst annealing at or just above the lower limit is exceedingly injurious. The effect of passing the alloy through several such heat cycles is shown by the curves in fig. 1 ; in each case the specimen was kept at the temperature indicated during several hours and then allowed to cool in air. If quenched from a temperature 50° to 100° above the upper limit, the specimens were non-magnetic.

As noted, the transformation temperature of the alloys appears to depend on the percentage of copper present. One specimen, containing about 70 per cent. of copper, can be picked up from a cake of ice by a small magnet but cannot be picked up from a table in a room at ordinary temperatures, as the upper limit of its transformation range is about 20° C. and its full magnetic quality is not attained above about 15° C. below zero. This specimen, when tested at 0°, shows a rather low magnetic permeability, becoming saturated at low values of the field ; it exhibits almost no hysteresis loss, all of which phenomena are characteristic of all specimens near their upper transformation limit.

Microscopic examination of a considerable number of specimens has shown that extensive changes in the crystalline structure always accompanies the changes in magnetic quality. Fig. 2 is a micro-photograph of a specimen of good quality when in its best condition. Fig. 3 shows the appearance of the same specimen in the non-magnetic condition.

No sample in which the bright crystals of fig. 2 were lacking was at all magnetic under any conditions, whilst all specimens showing these crystals were magnetic at temperatures below the transformation temperature. Apparently the magnetic quality depends upon some constituent of the bright crystals which may take on or lose its magnetic quality without the crystalline mass being destroyed; the transformation involves very slight energy changes, as careful observation has failed to afford any definite evidence of recalescence within the transformation range.

The alloys in which aluminium is displaced by some other trivalent metal have been less studied but appear to behave in a way quite like the above. Besides these, an alloy of a small amount of iron with larger amounts of nickel and chromium is strongly magnetic, whilst curiously enough an alloy containing 25 per cent. nickel and 75 per cent. iron is non-magnetic unless cooled to a temperature somewhat below 0°, when it becomes magnetic, and remains so until strongly heated; consequently, a bar of nickel steel may be cut in two parts and, after proper treatment, under apparently the same conditions, the one piece may be in the magnetic, and the other in the non-magnetic condition.

Within the last few years, alloys of iron with about 4 per cent. of silicon have been found to be of great value for commercial use in building transformers, as the hysteresis loss may be considerably less when the transformer core is made of such alloyed steel than when soft iron is used.

From what has been said, it is evident that the study of the magnetic properties of alloys is of great importance, both because it seems likely to help us toward a better understanding of the nature of magnetism and because of the technical improvements which may result. It should, perhaps, be stated that none of the alloys, except those containing large amounts of iron, are valuable in commercial work.

In conclusion, it is interesting to notice that Mr. O. C. Clifford has found alloys of copper and tin more strongly diamagnetic than copper itself, thus duplicating the phenomena of the Heusler alloys among diamagnetic materials.

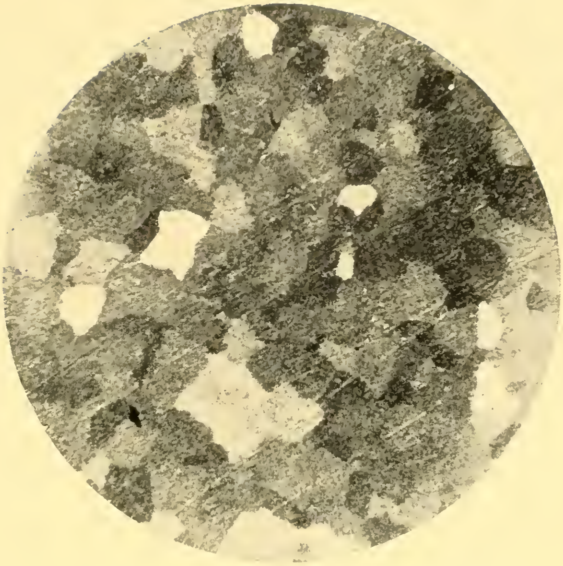

FIG. 2.—Structure corresponding to curve 1, fig. 1, magnified about 100 diameters.

FIG. 3.—Structure of same specimen when non-magnetic.

# THE INDIAN INDUSTRIAL PROBLEM

(*Continued from Vol. IV., p* 569)

By ALFRED CHATTERTON
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In India provision must be made for training the men diverted from literary pursuits to take an active part in the re-establishment of the hereditary artisans of their native lands. It would be premature to discuss the details of the training, as that must depend on inquiries and researches not yet made. Certain general principles are of application from the outset. There must be trade schools in which foremen can be trained for the specific industries and these should be furnished with a model equipment the value of which should be clearly demonstrated under strictly practical conditions. In order that hand labour may be developed to its highest possible efficiency, it is essential that the appliances, tools and machinery should be maintained in the best possible order; mechanical workshops will be required to train fitters, mechanics and carpenters and to afford instruction in the elements of mechanical engineering which underlie and are necessary to all manufacturing processes. Lastly, technical colleges and schools of science will be required, in which the best intellects the country can place at the disposal of its industries will be prepared to take up the leadership and carry on the work initiated by those having qualifications acquired abroad who will act as pioneers to the movement.

India sustains great loss and will continue to suffer so long as the best of her sons devote their energies and abilities almost solely to the legal profession and Government service; such service, however valuable it may be, does not directly contribute to the material welfare of the community. In any country litigation is a necessary evil but it is ten times worse when it is allowed to absorb such an enormous proportion of the available trained intelligence as is the case in India.

There the legal profession is unduly prominent and its ranks are consequently overcrowded. Litigation is fostered and the growth of technicalities stimulated, so that the machinery of justice is clogged. Indians are naturally prone to resort to the law courts on every possible occasion, the luxury of a civil suit having a strange fascination which few who can afford it succeed in withstanding permanently. The introduction of new interests into the life of the people would tend to check this tendency; anything that will create a wider outlook and broader views should be encouraged. The backwardness of India is not a little due to this parasitic growth and it is time that it were checked. The diversion to industrial pursuits of part of the stream of graduates flowing from the universities is a promising antidote and will perhaps gradually educate the public to consider the man who devotes his life to the promotion of the well-being and prosperity of his fellows deserving of greater honour than he who keeps them at variance and battens upon their failings and misfortunes.

### The Possible Industrial Future of India

We are come now to the last stage in our discussion of India's future industrial position and that is to illustrate by concrete examples the possibility of working upon the lines briefly indicated. It has been assumed that her industries can be developed without leading to the hideous concentration of human life and human activity in smoke-begrimed cities, with unparalleled luxury for the few and squalour for the many. This is based upon the idea that our ever-increasing command of natural forces will enable us to operate with equal advantage on a small as on a large scale; that there is a reaction against the deadening influence of production by machinery, in favour of the greater variety offered by products into the fabrication of which individual skill and fancy have been allowed to enter; that as there is therefore a field for Indian labour which can be developed by a judicious combination of the man with the machine, the former should be trained to afford the fullest possible play to his God-given faculties and that mechanical ingenuity should be directed to providing him with the means to exercise those faculties to the greatest possible advantage.

The problem to be solved is the difficult one of finding the happy mean between the individual working for himself and the great capitalistic organisation employing thousands of operatives in lives of monotony and drudgery. The single man or family is too small an economic unit to succeed, the modern mill or factory entails too much social degradation to be encouraged. The free play of private enterprise in the West has produced an unstable civilisation, in which the various elements are in antagonism with one another. Is it necessary that India should follow on the same lines? Is it not rather worth our while to attempt to direct her course so that advantage may be taken of the experience that has been gained to avoid, as far as may be, the unhealthy and undesirable features which are becoming so prominent in Europe and America?

The Government are clearly justified in intervening to prevent the artisan, if they can, from being driven out of his hereditary calling and to start him upon a new line of progress that will not land him in the evil plight that has befallen his fellows under the modern industrial system. The object to be obtained is the amelioration of the condition of vast numbers of people and not the creation of opportunities for concentrating great wealth in a few centres and in the hands of a small minority of the population. If this premiss be accepted, the problem should be studied with a view to working along the lines indicated and such assistance obtained from outside as is likely to prove useful. Much work has already been done by such scientific services as the Geological Survey of India in determining the available mineral resources, by the Forest Departments of the various provinces in ascertaining and conserving the value of the vegetation, by the Public Works Department in its various branches in all that pertains to improving means of communication and utilising sources of irrigation. The scientist, the mechanical engineer and the manufacturer have all done something to demonstrate the value of these resources, which should now be examined in greater detail with the specific object of increasing the opportunities of the indigenous industrial population. Industrial experiment and investigation are required and for such specially qualified men must be employed. Something in this direction has already been done and may be brought to notice, not because of its intrinsic importance but because it is pioneer work that will serve to show clearly the method adopted of solving this question.

*Lifting Water.*—The chief requisites of the Indian agriculturist are water and manure, both of which, in the absence of public sources of water supply, he has obtained hitherto through the agency of cattle. Water is lifted from between three and four million wells; as the quantity required is large, the expense is a very heavy charge upon the ryots. Careful investigation of the indigenous methods of lifting water demonstrated the high degree of efficiency attained in applying the power and no improvement seemed to be practicable until the oil engine became a source of motive power, so economical in fuel, so simple in action and involving so small a capital outlay that it was easily brought within the range of the wealthier ryot who had a sufficient water supply to justify using it to drive a centrifugal pump. In the South of India through Government agency large numbers have been installed and there is no doubt that their use will extend rapidly as their advantages become better appreciated. The requirements of India in this direction have now attracted the attention of engineers in England and, especially since the invention by Mr. Humphrey of the gas pump, it cannot be doubted that there will be a rapid development of mechanical methods of lifting water on a small scale that will greatly conduce to the prosperity of the ryot and at the same time familiarise him with the advantages of employing better tools or appliances in his daily work. Where the individual ryots are farming on too small a scale, the advantages of a number co-operating are apparent and have already been utilised.

*Searching for Water.*—The application of the oil engine and pump to lifting water for irrigation has extended the range through which water can be lifted profitably and rendered it possible to go to greater depths in search of water. To facilitate this work boring tools have been introduced and through their agency valuable supplies have been discovered; these have greatly increased the value of the land in the neighbourhood. The cost of a set of boring tools being beyond their means of individual ryots and special experience being necessary to make use of them, the work of boring for water has been taken up in some cases by public bodies and in others by private individuals who are making it a special business. An immense amount of work in this direction may profitably be undertaken in India but there are difficulties, especially in connection with deep boring, that render it desirable that Government should continue the work and assume the risks. So far the pioneer work has been done in an entirely haphazard way, though with great success. It now requires to be put on a more scientific basis under the direction of geological experts.

*Leather.*—The manufacture of leather is an old village industry which has been much affected by the growth of the export trade in raw hides and skins and in partially tanned leather. This is by no means to be regretted, as the "chuckler" made very inferior leather and spoilt a vast quantity of valuable raw material. The modern chrome process supplies a material much better suited to Indian requirements; through the efforts the Government experimental tannery in Madras has made, this is now becoming widely known and appreciated for such purposes as water bags, sandals, harness and boots and shoes. Small Indian tanneries are being started and afford excellent examples of what can be accomplished by private enterprise, either by co-operation or by individuals. The advantage to the country at large of the general employment of chrome leather will be very considerable, as it will reduce the Indian consumption of hides by approximately one-half and thus throw on the market for export a large quantity of raw material for which there is always a good demand.

*Weaving.*—This is the most important of the indigenous industries, and, despite the competition of imported piece goods and the products of the Indian power-loom factories, still gives employment to about two million looms. Much attention has of late been directed to the question as to how best to assist the hand-loom weavers and several new forms of hand-loom have been invented but none has as yet proved superior to the English hand-loom. The fly-shuttle is slowly making headway and will eventually be used by all plain weavers. By its use the rate of picking can be doubled but this does not mean that the out-turn of the weaver will be increased by the same amount, as extended observations show that the hand-loom weaver does not spend more than half his time throwing the shuttle, the balance being spent in mending broken ends, adjusting the warp and performing other minor operations. Experimental weaving-sheds have thrown a good deal of light on the problems connected with this industry and there is now a fair prospect that eventually it will be put on a much more satisfactory basis. Indian methods of preparing the warp and of sizing and dressing it are in even greater need of improvement, and experiments are now in progress to determine how this can be achieved. The arrangement of the warp presents no difficulty, but the dressing, to obtain the same results as by hand-brushing, is still in the experimental stage.

It is much to be desired that the Lancashire weaving mechanicians should have their attention directed to the Indian hand-loom problem and efforts are being made to supply them with adequate data as a preliminary. What is wanted is an improved hand-loom and not a light power-loom driven by hand or by pedals. The material of which it is constructed should be of wood preferably and a high rate of picking is less essential than a gentle handling of the warp when opening the shed and when beating up. Some modification of what are known as "linen-dressing machines" will probably be found suitable but they have not yet been tried under the conditions which prevail in India.

Already a revolution is in progress in the hand-weaving industry, brought about by attempts to make practical application of the clearer knowledge we now possess of the conditions under which it has hitherto been carried on. Both brains and capital are flowing into it, to the advantage of the hand weaver and the general improvement of the relations between the artisan and the other castes. It is true that no great success has attended the efforts of those who have organised the hand weavers into small factories but they have managed to hold their own, in spite of the mistakes and ignorance of the pioneers in this movement; the former will be remedied and the latter dissipated as experience is acquired. The weavers themselves are so backward that the attempts to get them to co-operate have not been successful; nevertheless the small factories will probably do well when the technical questions connected with their equipment have been solved. What we may look forward to in the future are groups of from fifty to two hundred weavers centred round a warping and dressing plant. This will supply warps to the weavers, who may either be collected in a shed or will work in their own homes. The trade will be in the hands of those who run the warping plant and on them will mainly fall the work of introducing improved looms and methods among the hand weavers. Though trades unionism is undeveloped in India, the passive resistance of the weavers to any change is a serious factor which those experienced in the ways of the artisan will not lightly ignore. The part which Government should play in this movement is to supply the skilled technical knowledge required to devise the equipment and when that step has been taken to start demonstration factories and trade schools for the instruction of those who want to become foremen and master weavers.

*Metal-Working.*—The metal-workers of India are skilled craftsmen working with very crude and imperfect tools and possessing little or no technical knowledge. Some years ago aluminium was introduced into the metal-working class at the School of Arts at Madras and a large business created in hollow-ware made of that metal. This was eventually disposed of by sale to a private company, which still continues to deal exclusively in such goods. The processes of drawing and spinning were employed for the first time in Southern India and a large number of workmen trained; unfortunately the factory is now a purely private concern and has little influence on the practices of the artisans outside. The teaching of metal-working processes can only be done in a factory and anything similar to the aluminium venture is not likely to be attempted again in view of the opposition which is aroused when any State or State-aided institution adopts commercial methods for the disposal of the finished products which must be made to furnish sufficient opportunity for the acquisition of skill and experience. Glass, earthenware and enamelled iron-ware have made serious inroads in the trade of the brass and copper workers and there is but little hope that the loss can be made good. The increasing wealth of the country to some extent counteracts the tendency to introduce cheap substitutes for the ancient metal wares; this tendency might be greatly assisted if the metal-workers were taught to turn out lighter and better finished work. That this could be done there is no doubt and a trade school in one of the big metal-work centres, with a staff of competent teachers in each branch of the trade, is the only way in which the desired end can be attained. The workshops should be furnished with good tools and the metal-workers encouraged to come and use them for their own work. Gradually they would discover the value of such appliances and it would not be long before they found a way of getting them for themselves. Very small factories are already common in the trade and the lines along which development will naturally take place are clearly indicated.

*Artistic Handicrafts.*—The art industries of India have declined chiefly because the wealthy Indian patrons have disappeared and all that is wanted to revive them is an appreciative market. There are signs that the frequent exhibitions now held in various parts of India have done something to create a new interest in these old arts and it is probable that the Swadeshi movement has strengthened it. In Madras, the Victoria Memorial has taken the form of a hall in which a permanent exhibition of the art handicrafts of the Presidency are exhibited. A large fund is available for the purchase of good specimens of the various crafts; when these are sold new commissions are given and a much-needed stimulus to the production of only the best work provided. It is too early to say what the ultimate result of this novel method of dealing with the decadence will be, as it has not yet developed to its full extent; there is justification for the hope that it will be a success. The collections are steadily increasing in size and in artistic merit and attract purchasers, who will buy a thing they can see and admire but who formerly would not give orders because there was no certainty either as to the date on which they would be completed or as to the quality of the work put into them.

*Tools and Machinery.*—The manufacturing engineers and mechanicians have devoted themselves mainly to the design and production of machinery as automatic as possible in its action and with as large an out-turn as possible. This tendency has encouraged industrial concentration. In India all work is done by manual labour or with the assistance of cattle; water power is only available and to but a limited extent in the hills; wind power has never been used, as over the greater part of the country the energy of the winds is too slight and of too variable a character to be of any value. The oil engine, when of small size, is much more economical than a steam engine of the same size; it costs less and is much simpler to look after. For these reasons it has to some extent come into use in India and will probably be very largely used in the future. The ideal engine would be a small gas engine working with gas made from wood. Already engines of about twenty horse-power with suction gas producers using charcoal are employed; something much smaller than this is wanted and if wood can be substituted for charcoal it will greatly reduce the working expenses. Already there are hundreds of oil engines in use and there will, in course of time, be many thousands. There is therefore a fair inducement to engineers to study Indian requirements, as every improvement will extend the range of their employment. It is the very rapid progress that has been made with internal combustion engines that has raised hopes that India may gradually acquire an industrial system based on small units of production and that is all the more likely to come about if the attention of the engineering world is drawn to this fact. Each industry and every branch of it should be the subject of investigation to ascertain the lines along which motive power may with advantage be introduced. The water-lifting question has already been discussed and need not be further alluded to. This is the largest field for the immediate application of power but there are several others of great importance which have been opened out, in which a great deal more could be done if the machinery on the market were better adapted to the work to be carried out.

(1) *Sugar Mills.*—For the crushing of sugar cane, rolls 9 in. × 18 in. are now in use in several places and are driven by oil engines. The results are very satisfactory where there is a sufficient area of cane in the immediate neighbourhood of the mill to keep it at work throughout the season. About 100 acres of cane could be dealt with by a single mill of this size; as no such area is grown by a single ryot, co-operative working is the only way out of the difficulty. Growing sugar is a very profitable operation but it requires capital and is subject to risks. Heavy manuring is a necessity and with cattle-driven mills the crushing of the canes is a long and tedious operation. Consequently, ryots usually only grow a small patch of cane. The extended use of artificial manures and of power-driven mills would probably result in a very considerable increase in the production of sugar.

(2) *Oil Mills.*—Oil is usually extracted in wooden rotary mills, of a very primitive type worked by cattle, or in large screw presses worked by men. Both systems are naturally expensive; attempts have been and are still being made to apply oil engines to do the work. The mill or press has yet to be designed which will displace those now in use. The home consumption of oil in India is very considerable and it only requires the application of some of the ingenuity which has been devoted to large extraction plants to the production of a small plant which can be driven by a small engine to effect a considerable saving in the cost of producing a prime necessity of life. Oil seeds are very widely grown and as the primitive methods of extraction easily hold their own against the big mills, the improvement of the small mills and the substitution of oil engines for animal power in driving them is obviously the direction in which to work. If the problem be solved, the demand for such mills will be very large, as the labour costs are now very heavy and for years past have been steadily rising.

(3) *Rice-Hulling Machines.*—Almost all the rice consumed in India is still cleaned by hand, only that portion of the crop which is exported being treated in mills driven by power. There are a number of rice-hullers on the market, but those that are satisfactory are too large to suit the restricted scale on which village rice merchants deal, and a really good huller that would not require more than four or five horse-power to drive it would be in good demand. Many ryots who have an engine and pump would like to employ the engine to drive either an oil mill or a rice-huller when there is no necessity to lift water for irrigation.

(4) *Saw Mills.*—There are but few steam saw mills in the country, nearly all the timber being reduced to scantlings by hand-cutting. Not only is the cost of labour for such work high but there is also a considerable waste of wood, owing to the irregularity of hand-sawing. Circular saws or large band saws require too much power but a simple type of frame saw, with a single blade, can be constructed to do a great variety of work and take not more than three or four horse-power. There is sufficient work for a plant on this scale in almost every town in the country and it only requires that the advantages to be obtained from their employment should be demonstrated for a demand for them to spring up.

(5) *Fibre-Cleaning Machinery.*—The cost of extracting fibres, even with the cheap labour available for such work, is very high and improvements in the machines already in existence are urgently called for, especially for aloe and plantain fibres. These machines should be of small capacity, as the quantity of raw material from which the fibre is extracted is not usually very large in any one place and the cost of carting it from a distance is prohibitive.

It is not necessary to give further examples of the opportunities for the display of mechanical ingenuity in meeting the requirements of the people of India. The object of this paper will be to a large extent gained if attention be directed by it to the field which is open to original workers; further inquiry will probably reveal a large number of instances in which a comparatively small amount of capital expended on tools and plant would greatly increase the efficiency of Indian labour. At the outset, progress will be slow, chiefly because of the difficulty of bringing the men with sufficient inventive skill into touch with the rural communities whose wants have to be studied. India now requires the services of many industrial experts and it should be recognised that adequate rewards must be offered to those who will take up Indian industrial problems. In technical colleges, in trade schools and in demonstration workshops, the science and engineering skill of the West must be applied to the peculiar industrial problems which call for solution. Scientific research having no other object than that of enlarging the bounds of human knowledge is a luxury which India cannot at present afford to indulge in, nor does it greatly attract the Indian mind. Scientific methods have first to be taught in the country and applied to the practical problem of raising the industrial status of the people. This work affords as much opportunity for the exercise of intellectual attainments as will be found in any laboratory and it is that to which men in the service of India must devote themselves if they are to render her real assistance.

# "PROFESSOR RIDGEWAY AND RACIAL ORIGINS": A REPLY

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In my address to the Anthropology Section of the British Association in 1908 I attempted to show that many of the chief errors which impede the scientific study of Man, which lead to the maladministration of alien races and beget blunders of the gravest issue in our own social legislation, are due in no small degree to man's pride in shutting his eyes to the fact that he is controlled by the same laws as the rest of the animal kingdom. My arguments excited considerable comment at the time and since, in this country, on the Continent and in America, evoking favourable comments from not a few leading scientific men and some tirades from gentlemen of Socialistic views. Only one systematic attempt to refute my doctrines has been made, an article by Mr. Bernard Houghton, I.C.S., which appeared in Science Progress for October 1909.

Foremost in importance amongst the problems relating to Man now being discussed by physical anthropologists is the stratification of populations in Europe. It had, before I wrote, generally been held as an article of faith that Europe was first peopled by a non-Aryan race. Of course it is difficult for us to say what were the physical characteristics and language of Palæolithic man, for, apart from a certain number of skulls, our evidence for him is entirely confined to his implements of flint found in the river-gravels, caves, and on plateaux. But when we come to Neolithic Man the problem becomes less hopeless. It has been generally held that the first Neolithic men in Europe, whether they were descended or not from their Palæolithic predecessors, had long skulls, but were not Aryans; that later on came a migration of short-skulled people from Asia, who spread along Central Europe to France, becoming what is commonly termed the Alpine, by some the Ligurian and by others the Celtic race; later these two primitive non-Aryan races are supposed to have been overrun by the Aryans, who when these theories were first started were universally considered to have come from the Hindu Kush, but are now generally believed (as held by Latham) to have originated in Upper Central Europe. The Aryans were generally assumed to have a blond complexion.

In my *Early Age of Greece* (1901) I had refused to regard the short-skulled Alpine race as differing materially on the one hand from the dark, long-headed race found in Italy, Greece and Spain, and on the other from the blond race of Northern Europe. At the York meeting of the British Association I maintained that the "Alpine" race was in no sense Mongoloid and that its short skull was due to modification along the Alps; in other words, that the brachycephalic race found in Europe is of European and not of Asiatic origin. This view was later on supported by Prof. William Wright in his Hunterian Lectures, whilst it has recently been strongly expressed by Prof. Gustave Retzius of Stockholm in his Huxley Memorial Lecture, delivered last December before the Royal Anthropological Institute. Moreover, in the most recent publication of the Danish Anthropological Committee, Dr. Soren Hansen has drawn the conclusion, from the very complete data obtained from the Anthropological Survey of Denmark, that the doctrine with which the Danish investigators started—that the population of Denmark consisted of two distinct elements: (1) a tall blond race with long skulls, and (2) a short dark race with short skulls—must be rejected. The evidence points rather to a shading off from the dark short type into the tall blond type.

In my address I maintained, as also in a monograph ("Who were the Romans?"—British Academy 1907), that the blond tall race of Upper Europe is identical in origin with the small dark long-headed race of the three southern peninsulas of Europe, generally included up till then under the name of "Mediterranean Race" with the Hamites of North Africa and the Semites of South-western Asia.

My argument was, and is, that as the ice-sheet receded man passed upwards from the south-east into Europe and settled in the three southern peninsulas, gradually spreading northwards over the Alps and eventually extending up to the Baltic. As they gradually spread upwards, under the influence of their environment (and in environment I of course include food), they grew less dark, those of them who settled permanently along the axis of the Alps tending to have shorter skulls, whilst those who had passed north earliest became the tallest and most blond people in the world. On the other hand, I revolted from Dr. Sergi's theory of a "Mediterranean Race" comprising Hamites and Semites as well as those speaking Aryan languages. I pointed out the very weighty evidence that the dark races of Greece, Italy and Spain (the Basques excepted) have always spoken an Aryan tongue, and that Sergi has simply assumed that similarity of physical type means identity of race. I urged that the similarity between the Aryan-speaking populations of Greece, Italy, Spain, parts of France and of the British Isles to the Hamites and Semites, is merely due to convergence of physical types under similar conditions, instancing various analogies from the lower animals.

As my theme had of necessity to concern itself with questions of race, I examined the criteria by which the anthropologists distinguish one race from another. If you ask an anthropologist how he discriminates an Aryan from a non-Aryan race, he will tell you that he relies on three main tests—the colour of the skin, hair and eyes; the shape of the skull and other osteological characteristics; and the system of descent through males. Formerly language was included in the tests of race, but when it was pointed out that the negroes of Jamaica speak English, those of Louisiana French, it was thenceforward assumed that one race can embrace the language of another with the greatest ease. Yet it may turn out, after all, that language was too hastily expelled from the criteria of race. On the other hand we may find that too implicit faith has been placed in the three criteria of cranial characteristics, pigmentation, and law of succession. It will be thus seen that I have not substituted language, as a criterion of race, for cranial characteristics, pigmentation, or law of succession. I then examined the value of these three criteria in general use, and I was forced to conclude that osteological differences could not be implicitly relied on and might in some cases be foundations of sand, because it is certain that such variations take place within very short periods, not only in the case of the lower animals, as in the horse family, but in man himself. Pigmentation also is not an infallible criterion, for with the lower animals there is a steady tendency in the same species to change in colour from latitude to latitude, whilst in the case of man the steady shading off in colour from dark to blond may be traced from the Equator to the Baltic. Unless, then, we postulate that man is entirely free from the natural laws which condition the osteology and pigmentation of other animals, we must admit that neither bone nor colour differences can be regarded as crucial criteria. The test of descent through males breaks down completely, as descent through females can be proved for those who never spoke any but an Aryan language. Finally, I was led to the conclusion that language, when once we realise the laws which govern its borrowing by one race from another, may be taken as a test of race, and really as the surest, when dealt with broadly and over wide areas, and not merely in the way of guesswork etymologies.

I have constantly been asked, "How do you explain the fact that in Ireland and in Great Britain, countries lying so far north, we have the dark 'Mediterranean Race'?" To this I invariably reply, "Yes, a dark race, but very different from the dark race as seen in the southern peninsulas." The hair is certainly very dark, yet it is not the blue-black hair of the South, whilst the skin is beautifully fair, and the eyes are often blue, especially in the west of Ireland, where there has been the least admixture of population. In this type I maintain that we have the transition stage between the full melanochrous met in Spain, Italy and Greece, with its olive skin, blue-black hair and black eyes, and the tall blond Scandinavian, where the change in pigmentation is now fully accomplished, the hair as well as the eyes being of a light hue. The mild climate is enough to account for this retardation of change in Ireland, owing to which the Flora of the west of Ireland approximates to that of the Spanish Peninsula, whilst there are also approximations between the Fauna of both regions. As the dark type in Ireland so frequently shows blue eyes, I was led to conclude that the pigmentation of the eye is less stable than that of the hair. Let us turn again to the most recent Danish evidence. The data show all kinds of pigmentation both in the hair and eyes, but with some very important limitations: (1) a very large proportion have blond hair and blue eyes; (2) a very large number have dark hair and dark eyes; (3) a considerable number have dark hair, but blue eyes (just as in Ireland); whilst (4) a few, but very few, have blond hair and dark eyes—a phenomenon also known in Ireland, but likewise very rare.

My doctrine of the instability of eye colour has recently received remarkable confirmation. Dr. William Wright, in his Hunterian Lectures (III. 7) writes: "The effect of sunlight in darkening the skin is well known. As to eye colour, my friend Mr. J. V. Hodgson, biologist to the Scott Antartic Expedition, informed me that as a result of living under such unusual conditions, the eyes of the members of the expedition became so blue as to occasion remark on their return to New Zealand, and also on their arrival home in this country. Colour therefore, like the cephalic index and stature, is also prone to change, and in itself is not deserving of implicit trust." But though the pigmentation of the eye can be quickly modified in the individual under new conditions, the race would probably have to live under the like conditions for a very long period before such blueness would become a fixed racial trait.

It will be seen (1) that my views respecting the short-skulled "Alpine race" have been endorsed by eminent craniologists and by the conclusions drawn by the Danish anthropologists from their own anthropometric survey ; (2) that my theory of the pigmentation has likewise been confirmed by the same survey and by the evidence derived from the Antarctic Expedition. Thus, within a short period since it was first propounded, my theory of the origin of the blond Aryans has been corroborated by various kinds of evidence, as well as endorsed by leading anatomists.

Let us now consider the reasons by which Mr. Houghton attempts "to show that the arguments used rest on foundations of quicksand, and that the inferences do not really arise from the facts adduced." "The fundamental error in his (Prof. Ridgeway's) position consists in an assertion of the essential fluidity of head-form and such-like physical characteristics and in their derivation from climatic and other surroundings, in contrast with an alleged permanence over a given area of the language originally spoken there. He predicates also a similar local permanence of idiosyncrasy, polity, and social and religious ideas. The central and dominant feature of the first portion of his address consists in an ascription to local influences of those physical traits of mankind which have hitherto by all competent investigators been referred to racial causes, that is, to Heredity."

Let me at once point out that, while I do ascribe great importance to the influence of environment, I hold also very strongly the doctrine of heredity—in fact, too strongly for Mr. Houghton's fancy when he has to deal with my doctrines of the value of Heredity as a most important, if not the most important factor, in our own chief social problems. But the grand problem of the true relation between Heredity and Environment has yet to be solved.

Mr. Houghton's case depends wholly on the assumption that man is absolutely free from the natural laws which condition the osteology and pigmentation of other animals. This he thinks was settled once for all by Sir E. Ray Lankester. "As Sir Ray Lankester," he writes (p. 273) "demonstrated so brilliantly three years ago, man is an insurgent against nature. Once proto-man utilised skins as a protection against the inclemency of the weather, once he kindled fire to serve as a shield against cold and wild beasts, and fabricated for himself cunning weapons of offence, he withdrew himself definitely and for ever from the operation of the old zoological environment." These are bold words, but they are certainly an accurate statement of the views put forward by Sir E. Ray Lankester in his Romanes Lecture at Oxford (1905), since republished in his book entitled *The Kingdom of Man.* On p. 25 of the latter work he writes: "The mental qualities which have developed in Man, though traceable in a vague and rudimentary condition in some of his animal associates, are of such an unprecedented power, and so far dominate everything else in his activities as a living organism, that they have to a very large extent, if not entirely, cut him off from the general operation of that process of Natural Selection and survival of the fittest, which up to their appearance had been the law of the living world. . . . If for the purpose of analysis, as it were, we extract Man from the rest of Nature, of which he is truly a product and part, then we may say that Man is Nature's rebel. Where Nature says die, Man says I will live. According to the law previously in universal operation, Man should have been limited in geographical area, killed by extremes of cold or of heat, subject to starvation if one kind of diet were unattainable, should have been unable to increase and multiply, just as are his animal relatives, without losing his specific structure and acquiring new physical characteristics according to the requirements of the new conditions into which he strayed, and should have perished except on the condition of becoming a new morphological 'species.' But Man's wits and his will have enabled him to cross rivers and oceans by rafts and boats, to clothe himself against cold, to shelter himself from heat and rain, to prepare an endless variety of food by fire, and to increase and multiply as no other animal without change of form, without submitting to the terrible axe of selection wielded by ruthless Nature over all other living things on the globe."

Again (p. 27) we read: "In spite of the frequent assertions to the contrary, it seems that neither the more ancient wars of mankind for conquest and migration, nor the present and future wars for commercial privilege, have any real equivalents to the simple removal by death of the unfit and the survival and reproduction of the fit, which we know as Natural Selection."

Yet after all these bold statements of the freedom of Man from Natural Selection, Sir E. Ray Lankester (in a footnote, p. 28) says: "It would be an error to maintain that the process of Natural Selection is entirely in abeyance in regard to Man. In an interesting book, *The Present Evolution of Man*, Dr. Archdall Reid has shown that in regard to zymotic diseases, and also in regard to the use of dangerous drugs, such as alcohol and opium, there is first of all the acquirement of immunity by powerful races of men, through the survival among them of those strains tolerant of the disease or of the drug, and secondly the introduction of those diseases and drugs by the powerful immune race, in its migrations, to races not previously exposed either to the disease or to the drug, and a consequent destruction of the invaded race. The survival of the fittest is in these cases a survival of the tolerant and eventually of the immune."

This is not the place to point out the series of assumptions made by Sir E. Ray Lankester in his brilliant description (chiefly imaginative) which he drew for his Oxford audience of the emergence of Man from the stage when, like all other animals, he was under the law of Natural Selection. His own admission, contained in the footnote just cited, that Natural Selection is still at work, and that too in most potent forms, is sufficient to demonstrate the untenable nature of the position which he took up in his lecture, and which is adopted by Mr. Houghton as the basis of all his criticisms of my doctrines.

The assumption that Man can go into climes very different from those in which his race has long dwelt without any morphological change is in direct conflict with many known facts. The American of New England, with his hatchet face and his thin scraggy beard, differs essentially in his type from his English ancestor, whilst the Boer of South Africa shows no less variation from the type of his Dutch progenitors. The instance just cited of the influence of the Antarctic environment on the colour of the eyes is in itself sufficient to demonstrate the falseness of his assumptions.

Mr. Torday and Mr. Joyce have furnished me with a still more striking example (from their book on the Congo shortly to be published). It relates to the pigmies who live in the forests of that region, and are known as Ba-Twa. The Bu-Shongo, who found the Ba-Twa in possession, hold them in superstitious awe, regarding them as spirits born from trees. In some cases bands of these pigmies have been induced to leave the forest, settle in villages, and practise agriculture. In such cases they are regarded by the Bu-Shongo as becoming more human; but no intermarriage ever seems to take place between Bu-Shongo and Ba-Twa. Mr. Torday visited two of these villages of settled Ba-Twa. The Bu-Shongo told him that it is only three generations since these Ba-Twa left the forest. He noted that the stature of the inhabitants was considerably above that of the nomad Ba-Twa, though it did not equal that of their Bu-Shongo neighbours. "As the possibility of intermarriage seems quite out of the question, it seems necessary to conclude that the short stature of the pigmy is in some way due to the dwarfing effect of forest life or to the Natural Selection exercised by environment."

Mr. Claude White (in his recent book *Sikhim and Bhutan*) writes: "The people of the West (of Bhutan) are for the most part of Tibetan origin, and came into the country centuries ago. They are of the same original stock as the Bhutias in Sikhim, but have developed in Bhutan into a magnificent race of men physically. Why there should be this marked contrast, I cannot say. It may be due to the difference in climate; but there is no comparison between the two, although the Sikhim Bhutia is a strong, sturdy fellow in his own way." My friend Mr. J. D. Anderson, I.C.S., Reader in Bengali to the University of Cambridge, has pointed out to me that the greater stature of the Khasias of Assam, who dwell in the midst of Tibeto-Burman tribes, compared with that of their Burmese cousins the Monds, is to be attributed to their environment in Assam, where they continue to speak their own language, though assimilated in physical type to the Assamese tribes around.

Sir H. H. Risley, K.C.I.E., has called my attention to some very interesting facts lately published respecting the difference of the arterial pressure and blood constituents of Bengalis as compared with our European standards in the same respects, from which we must infer that one race differs morphologically from another.

As Mr. Houghton's strictures on my principles are based entirely on the dogmatic assertion of Sir E. Ray Lankester that Man had once for all cut himself free from the action of natural laws, it is a pity that he did not make himself acquainted with the footnote which I have cited, in which his master admits the activity of Natural Selection at the present moment in the human family. But Mr. Houghton himself makes admissions which cut away the ground from under his feet. He writes: "When he (Ridgeway) goes on to say that the skins of mankind tend to get lighter in gradations from the Equator to the Pole he stands on firmer ground. Undoubtedly the skin of races long inhabiting the tropics evinces a deeper pigmentation than in those residing in more temperate regions. The reason for this is obvious: although histologists are not agreed as to the cytological facts of pigmentation, it undoubtedly tends, just as do freckles, to protect the outer layers from the actinic rays of the sun." Yet when he comes to deal with my theory that the white skin of the blond race of Northern Europe is due also to climatic causes, analogous to those which have produced the white hares and white bears, and make the ptarmigan turn white in winter, he declares that such a view "implies a singular inability to grasp the relevant facts of the case or to frame inductions upon them. The whiteness of animals inhabiting northern regions, whether perennial or seasonal, is a very simple case of adaptive colouring first demonstrated by Dr. A. Russell Wallace, and now obvious to the merest tyro in biology. Who will assert that blondness of hair in any way favours a race in a northern habitat? Does Prof. Ridgeway mean to assert that in winter our ancestors pursued game or eluded their foes in a state of nudity?"

"Words," says Hobbes, "are the counters of wise men, but the money of fools." Mr. Houghton, like many others, catches up terms, such as protective colouring, mimicry, or mutation, and believes that by merely repeating them he is enunciating unshakable scientific truths. But what is "adaptive colouring"? Adaptive is a relative term. To what is the colour adapted? To the environment in which the animal lives. But it by no means follows that white is only to protect the animal from its animal foes or to render it easier for it to stalk its prey. I have made no such assumption respecting the blondness of the northern race. I only argue from the analogy of the dark colour of the Negro in the tropics, which Mr. Houghton himself admits to be protective against "the actinic rays of the sun"; in other words, it is a case of "adaptive colouring," as he might have seen, had he understood the use of that term.

He is evidently not aware that the leading biologists now explain the white colour of Arctic animals, not as a protection against living foes, but against the cold, white being the best colour for keeping in the heat of the body. The blondness of the northern race may have therefore a real protective value, as has the blackness of the Negro, by Mr. Houghton's own admission. But this is not mere theory. When the Nares and Markham Arctic expedition was being organised, it was stated in the Press that in selecting men for the crews, preference was given to the blonds, because the experience of whalers had shown that fair-complexioned men stood the rigours of the Arctic winter better than those of melanochrous hue. Conversely there is a large body of evidence to show that in West Africa and other tropical regions men of blond complexion suffer far more from the climate than those of a dark complexion. The change in hue of the eyes under Antarctic conditions, cited above, clearly proves a connection between light colour and Antarctic or Arctic conditions, which is not for the purpose of protection against foes. This "adaptive colouring" is certainly not to protect men from the penguins and other birds, nor yet to enable men to capture these birds more easily, but it has probably a much deeper protective significance.

Now, as Mr. Houghton admits that the action of environment affects the pigmentation of the skin in tropical and subtropical countries, but on the other hand denies it for northern regions, he is bound to show at what point, let us say, between the

Soudan and Northern Europe this natural law ceases to be operative. Does it suddenly cease to act amongst the Nilotic tribes, or is it in Egypt that he draws his line, or is it the Mediterranean which says, "So far and no farther shall atmosphere act upon skin"? No scientific man who admits that the skin of certain races is affected by their environment would dream of excluding the rest of mankind from similar action; even though Sir E. Ray Lankester may state dogmatically that man can advance from the Equator to the Arctic circle without undergoing any morphological change, no man of science when once the facts are presented would believe this for a moment.

It is admitted by Mr. Houghton, as well as by every one else, that the pigmentation of the Negro acts as a protection against tropical light. At what point on the globe do the inventions of Man, by which according to Sir E. Ray Lankester he has freed himself from the laws which condition the rest of nature, cease to act? At what point as we go north will Sir E. Ray Lankester assert, "Here Man's clothes and houses and fire shut him off from Nature's laws"? So too, when we come to Europe. Even in these climates where we northerns dwell, arrayed in warm vesture against the assaults of Boreas, our faces and hands are exposed to the direct action of the atmosphere, and the air must circulate round us unless we be clad in plaster. Yet our remote ancestors in their slow struggle against Nature had but scanty raiment. The action of the atmosphere suffered but little check from a skin thrown over the shoulders to keep off the pelting rain.

But even if clothes could check climatic action on the skin, there are other ways in which environment is constantly acting on man as it does on the rest of the mammals. Man has to breathe, and therefore, unless he were able to rid himself of his respiratory organs as he advanced north, the chemical and physical processes of his body must be influenced by the nature of the air inhaled by his lungs. No sane person will doubt that the atmosphere of one region differs from that of another. If it does not, why do we send those who are suffering from pulmonary consumption to high altitudes, or to dry climates, such as Australia or the Cape?

Again, Man, especially primitive Man, depends for subsistence on the food produced by the locality in which he lives or in that from which he draws his supplies. But foods differ according to the nature of the soil and climate. Accordingly men in each locality must be modified by the character of the food produced in that area, when it is assimilated by the chemical processes of the body, unless they are provided with tin or copper linings throughout the entire length of the alimentary canal.

I have pointed out that altitude operates like latitude. This Mr. Houghton disputes, on the ground that the pigmentation survey of Scotland shows blondness to be predominant in the valleys and dark hair in the mountains, and because in the Himalayas and elsewhere melanochrous people are to be found at the present time. But the Scottish example is at once explained by the settlements of fair-haired folk from Northern Europe well within historical time, who drove into the hills the weaker aboriginal dark race. He is careful not to deny that the dark tribes found occasionally in mountain areas in India and elsewhere have only taken refuge there in recent times.

I have shown by numerous historical examples how difficult it is for a conquering race to impress its language on the conquered, unless they come in large numbers and bring women of their own race. Otherwise they marry the daughters of the land and their children speak their mothers' tongue. Familiar illustrations of this principle are afforded by the story of the Normans in France, England and Ireland, the Angles and Saxons in Britain, and the persistence of Welsh and Gaelic.

I hold that the aboriginal dark populations of Greece, Italy and Spain (Basques excepted, supposing that they were originally dark) are Aryans, that they spoke always an Aryan language, and that, accordingly, Greek and Latin are the languages of dark aboriginal Aryan races and not tongues taken over from small bodies of blond northern Aryan invaders. I have pointed out that in Egypt there is an apparent exception, since the Egyptians had taken over Arabic after the Muhammadan conquest. This I explain as due to Arabic being the religious language of Islam; for whilst the Egyptians who embraced Islam learned Arabic, those who remained Christians retained their ancient language (Coptic). It is significant that under both Greeks and Romans the Egyptians continued to use their own tongue. This doubtless was due to the fact that neither of these races were proselytisers, but always tolerated and frequently adopted the gods of their subjects as their own.

Mr. Houghton thinks that he has disposed of my induction by pointing out certain cases in India where non-Aryan peoples are known to have adopted an Aryan language. But he overlooks two vital facts. In the first place, I am assured, on excellent authority, that non-Aryan tribes learn Hindustani or Bengali when they adopt Hinduism as their religion; they are thus an exact parallel to the Egyptians. Secondly, he ignores the fact that, were it not for the retention of their own speech by the Khasis, the origin of that tribe would have been lost for ever. Yet Mr. Houghton admits that in Gaelic, Welsh, Basque and such languages, survivals in mountainous regions, we have good evidence that these languages were once spoken by peoples who now use other tongues. But this is simply to admit my contention that language, when properly understood and used, must be included as a valuable criterion of race along with osteology, pigmentation and system of descent.

My critic thinks that the non-Aryans in Greece may have learned their Aryan speech from some conquerors who swept over them and departed leaving not a wrack behind. But if there was sufficient time for them to impress their tongue upon the conquered, they must have left some material relics. But where are these to be found in Greece? The Achæan domination was not long, yet we have now many material proofs of its occurrence. Now the old view, supported by Mr. Houghton, assumes that not only did the non-Aryan aboriginals of Greece take over the Aryan tense-system in all its perfection from a band of invaders, who rushed down upon the land, then disappeared for ever, but, as the Greek tense-system reached a greater perfection than is found in any other Aryan language, they are compelled to hold that a non-Aryan people actually made an Aryan tongue more perfectly Aryan than that found amongst admittedly Aryan peoples.

In my address I maintained that, as physical characteristics are in the main the result of environment, social institutions and religious ideas are also largely the product of environment. "Several of our most distinguished Indian and Colonial administrators," I wrote, "have pointed out that most of the mistakes made by British officials (such as Indian civilians) are due to their ignorance of the habits and customs of the natives.

It has been in the past a maxim of British politicians that in the English constitution and in English law there is a panacea for every political and social difficulty in any race under the sun. Only let us give, it is urged, this or that state a representative parliamentary system and trial by jury, and all will go well. The fundamental error in this doctrine is the assumption that a political and legal system evolved during many centuries amongst a people of North-western Europe, largely Teutonic, and that, too, living not on the mainland but on an island, can be applied, cut and dried, to a people evolved during countless generations in tropical or sub-tropical regions, with social institutions and religious ideas widely different from those of even South Europeans and still more so from those of Northern Europe. We might just as well ask the Ethiopian to change his skin as to change radically his social and religious ideas. It has been shown by experience that Christianity can make but little headway amongst many people in Africa or Asia, where on the other hand Muhammadanism has made, and is steadily making, progress and acting distinctly for good, as in Africa, by putting down human sacrifice and replacing fetish worship by a lofty monotheism." I offered as a suggestion to account for this fact that Muhammadanism is a religion evolved amongst a Semitic people who live in latitudes bordering on the aboriginal races of Africa and Asia, and that it is far more akin in its social ideas to those of the Negro or the Malay than are those of Christianity, more especially of that form of Christianity evolved during the last twelve centuries by the Teutonic peoples of upper Europe, who are of all races furthest in physical characteristics, in religious ideas, and social institutions from the dark races of Africa and Asia. "This great gulf is due not merely to shallow prejudice against other people's notions; it is as deep-seated as is the physical antipathy felt by the Teutons for the Negro, which is itself due to the very different climatic conditions under which both races have been evolved."

Mr. Houghton is very irate with these doctrines, but he does not challenge the main facts. He simply states that "Christianity is equally a Semitic religion as is Muhammadanism." But, as I pointed out, the Christianity brought into Africa, especially by the various Protestant denominations, is not Semitic Christianity, but a Semitic religion long adapted and improved in upper Europe amongst non-Semitic peoples. He asks me to account for the existence of "the Christians in tropical America, not to mention the flourishing churches in Uganda and South Madras, or contrariwise the Muhammadan races such as the Tartars or Kirghis on the mid-Asian plains. Buddhism, evolved amongst Indian races in a tropical climate, yet now receives the adoration almost solely of Mongoloid peoples, the bulk of whom live under temperate skies."

Is Mr. Houghton really serious when he would have us believe that what is called Christianity is the same everywhere and never undergoes any modification, and that the same holds true of Muhammadanism and Buddhism? Does he really hold that the Christianity of North Germany is the same as that of Italy, and that Scotch Presbyterianism is identical with Spanish Roman Catholicism? He would fain have his readers believe that when Negroes take over Christianity from any Christian sect, they discard completely all their inherited beliefs and assimilate every idea of their Christian teachers! Yet the facts are all against this. Any open-minded missionary—and there are many such—will tell him that there is very great difficulty in eliminating the substratum of old beliefs and immemorial customs. This need not surprise us. The early Christian Fathers had the same difficulties. In the Christianity of every country there is a solid element derived from pre-Christian times. In order to comply with the feelings of their neophytes and to keep them from relapsing, the Christian Fathers had to make the birthday of Christ coincide with that of Mithras and with the ancient midwinter festivals, such as the Saturnalia. In the so-called Christianity of Mexico there are numerous Aztec and pre-Aztec survivals, and the same holds true of the Christianity of the Maoris of New Zealand. The schisms in the early Christian Church were due in no small degree to the different religious ideas amongst the various races, Jews, Greeks, Latins, Africans, Goths, who embraced the main tenets of Christianity.

So too is it with Muhammadanism and Buddhism. If Muhammadanism is the same everywhere, why do so many different sects exist? It is almost incredible that Mr. Houghton, himself an Indian Civil servant, in daily contact with Indian Muhammadanism, which he must know has taken over the Hindu Caste system, should be under the delusion that this religion—Hinduism with a varnish of Islam—is absolutely identical with the Faith as professed in Arabia, or Persia, or Turkey. There can be no better proof than this of the need for giving Indian Civil servants some training in religion and ethnology before they are permitted to undertake the administration of Indian races. So too the Muhammadanism professed by Malays and Negroes is but a thin veneer, for each race brings over into Islam almost all its ancient ideas and practices.

Nor is it otherwise with Buddhism. In that religion the burning of the dead is a great feature. Yet the Burmese Buddhists have always refused to burn their dead, as do also many Tibetan professors of the same religion. Moreover, the best authorities on Lamaism maintain that in the Buddhistic religious dramas we have survivals of pagan dances and ceremonies. Thus once more Mr. Houghton has fallen into the fatal error of his whole school: he takes the shadow for the substance and never realises that a mere term may have many various connotations. Nor is he more happy when he points out that the Egyptian changed over completely to Christianity, and with equal readiness to Islam. In the graves of the Coptic Christians we find the same amulets as were buried in the pre-Christian graves. There was a veneer of Christianity, but the practical religion of the people and their real objects of devotion were but little changed. Then came Islam, but the amulets remained as before. The old eye-charms, the cowries and the like, are as much worn in Egypt and are as much believed in to-day as in the time of the Pharaohs.

Mr. Houghton next asserts: "Nor can polygamy, trial by jury, parliaments or such-like be, on any right conception of the facts, the heritage or result of any climate or locality. There were parliaments in Rome." Once more he is the victim of terms. There was a Senate in Rome and also Centuriate, Curiate, and tribal Assemblies, of which rhetorical and slipshod writers speak sometimes as parliaments. But there was no Parliament in Rome or Greece or Egypt or any other country in the world, ancient or mediæval, in the sense connoted by the term "British Parliament." By a parliamentary system we mean a distinctly *representative system*, a system unknown to any ancient state, a system evolved (*pace* Mr. Houghton) in England and nowhere else. But once more he gives away his own case by admitting that "new institutions introduced say into Japan often go awry and misfit" (pp. 275, 276). This he thinks he has sufficiently explained by the statement that the women are uneducated, forgetting that the position of women is one of the most fixed elements in the social doctrines of every people. I pointed out that the food supply at hand in each region may be an important element in the variations of race, while the nature of the food and drink preferred there may itself be due in no small degree to climatic conditions. Each zone has its own peculiar products, and beyond doubt the natives of each region differ in their tastes for food and drink. "The aboriginal of the Tropics is distinctly a vegetarian, whilst the Eskimo within the Arctic region is practically wholly carnivorous. In each case the taste is almost certainly due to the necessities of their environment, for the man in the Arctic regions could not survive without an abundance of animal fat. It is probable that the more northward man advanced the more carnivorous he became, in order to support the rigours of the northern climate, whilst the same holds true in the case of drink. All across Northern Europe and Asia there is a universal love of strong drink, which is not the mere outcome of vicious desire but of climatic law."

Mr. Houghton urges that "such temperate races as the American Indians and the Esquimaux (before the advent of Europeans) and the Japanese, constitute awkward facts against climatic alcoholism; whilst the supposed need for flesh-food has not hindered the Russian and the Japanese peasantry and the Highlanders and Irish (until recent years) from enjoying on a vegetarian diet remarkably robust health." Yet it was not want of inclination, but lack of the means of providing alcohol that made the Eskimo sober. The climatic law acted as soon as they got the opportunity. So too is it with flesh-diet in the case of the Russians and Japanese, Highlanders and Irish. The Japanese, since they have become better off, have taken ardently to a diet of flesh. The Japanese Government in the late war, under the belief that the soldiers would fight better on meat than on rice and fish, purchased vast supplies of tinned meat for their troops. Because the peoples enumerated were healthy on a vegetarian diet, it does not follow that they would not have been far better and more vigorous if they could have added meat. The readiness with which these northern folk took to alcohol and flesh when they got the chance shows that my argument in favour of a climatic law is sound.

I pointed out that, although the world has been ringing with the doctrine of Natural Selection and the survival of the fittest for nearly half a century, no statesman ever dreams of taking these great principles into consideration when devising any scheme of education or social reform. On the contrary it is a fundamental assumption in all our educational and social reforms that all men are born with equal capacities; and that there is no difference in this respect between the average child of the labourer sprung from many generations of labourers and one born of many generations of middle- or upper-class progenitors; and it is held that all that is necessary to make the children of the working classes equal, if not superior, to the children of the bourgeois is the same food, the same clothing, and the same educational advantages. I urged that whilst work has been a main factor in the evolution of the middle and upper classes, especially in later times, though undoubtedly other qualities, such as superior physique and superior courage, have been very important elements in their earlier stages, the special quality which led to their rise was a superior self-restraint, that enabled them to resist the vices too often attendant on prosperity. This superior morale influences the offspring both by heredity and by setting up a better standard of life in the home.

I thus concluded that the middle classes are not the outcome of chance, but of a long process of Natural Selection and the survival of the fittest in the struggle for life—the two main factors in this evolution being Heredity and Training. The middle and upper classes are in the main sprung from ancestors with better physique, brains, courage and morale, and who have generation after generation been brought up in a better moral atmosphere than the children of the masses. Their ranks are also being continually reinforced by the best of the working classes. But the number of the good "sports" are very limited, for hardly more than five per cent. of the children of the working classes have at the age of sixteen the same amount of brain-power as the average children of the middle classes at the same age.

Mr. Houghton asserts that my fundamental error is ascribing to environment "those physical traits of mankind which have hitherto by all competent investigators been referred to racial causes—that is, to Heredity." But when he comes to criticise my social doctrines, his faith in Heredity suddenly fails. He thus involves himself in the awkward predicament that, while he denounces me for not assigning entirely to Heredity the physical traits, he denounces me no less loudly for assigning ethical traits to Heredity as well as to environment. He denies that the existing evidence shows that the "great bulk of the lower classes are truly inferior by Heredity to those above them in the social scale," but he does not venture to challenge the data which I have given, and which since I first wrote have been amply confirmed by further inquiry. But as usual, after his first confident and dogmatic denial, Mr. Houghton makes a fatal admission. "No doubt," he writes, "a certain amount of evolution or elimination of the unfit does take place even in civilised states—an evolution which sends the drunkard, the wastrel and the sluggard to join the dregs of the population, and confers somewhat tardy laurels on thrift, on forethought, and on intelligence." It will be noticed that he entirely ignores the great quality of moral self-restraint, on which the steady rise of individuals and families depends.

"In a herd of deer or troop of monkeys," writes Mr. Houghton, "each member enters the world under absolutely equal conditions, and his chance of producing progeny depends on the average on his adaptation to the environment common to the herd. Amongst civilised people, on the other hand, what can be more grotesquely unequal than the environment under which the progeny of different classes enter on the battle of life—between the surroundings of the slum child and those of one born in a ducal house? Even in the highly improbable event of their receiving an approximately similar scholastic education, the former will from his home environments have received constant suggestions to lie, to steal, and to drink." Mr. Houghton evidently has a much higher idea of the ordinary morality of ducal houses than is usually held by his Socialist brethren, according to whose utterances one would be compelled to believe that to be born in a ducal household means to be reared in the worst of all possible environments. Mr. Houghton censures me for giving any place to environment in racial development, but when he has to maintain that the law of

Natural Selection does not work in modern communities, he is driven to abandon the doctrine of Heredity and nothing but Heredity, trumpeted so loudly in all the fore part of his strictures, and he writes as though it were I who had denied the influence of environment and as if he were correcting me for my heinous blunder.

His statement (derived from Sir E. Ray Lankester) of the working of Natural Selection amongst the lower animals is equally faulty. It is not alone adaptation to its environment which preserves the one deer or monkey, when another's life is cut short. Chance may determine that one should be killed by a leopard, and that its companion should escape, not because the latter was the stronger or swifter or better protected by its colour, but simply because it was farther away from the leopard when he made his spring.

It would have been strange if Mr. Houghton had not ended up with the usual ravings about "Society," the chief stock-in-trade of all his tribe. "At present Society, after pushing the mass of the proletariat into the mire of base surroundings is apt, standing daintily aloof, to point the finger of scorn at them, as incurable heirs to a vicious Heredity." These be fine words, but Mr. Houghton as usual omits to explain the use of the term on which all turns, and leaves his reader in the dark respecting the nature of that "society" which pushes the proletariat into the mire. Yet he himself has just admitted that "even in civilised States there is an evolution which sends the drunkard, the wastrel, and the sluggard to join the dregs of the population, and confers somewhat tardy laurels on thrift, on forethought and on intelligence." Cannot he see that he himself in his last sentence is describing the formation of that "society" which he denounces so furiously? He has simply put into other words the doctrine respecting the rise of the middle and upper classes by Natural Selection stated fully in my address. But his knowledge of human society is on a par with his knowledge of Natural History. "Only when men and women start on life's race as nearly equal as human institutions will permit, shall we be able to point with any confidence to the lower class, and say these possess an undesirable heredity." But yet any labourer's family will afford us a test of the falsity of this rhetoric. I write with one before me. Three brothers, born of the same father and mother, went to the same parish school and grew up under exactly similar conditions. *A* was an excellent labourer, steady and sober. Emigrating to South Africa he became a ganger on railway works, and he has now by his steadiness, thrift and good conduct himself become a contractor, whilst all the time he has been a kind and liberal son to his widowed mother, and also helped his brothers. *B* too is an excellent labourer, kind to his old mother, with whom he lives, but has heavy drinking bouts at intervals and wastes his earnings. *C*, also an excellent labourer, but unsteady, was brought out to Africa by *A*, and given a good start. *A* and *C* have lately been home to see their mother. *C* brought home with him a considerable amount of money, with the only result that his stay in his native village has been one long debauch for himself and *B*. *A*, the steady railway contractor, is now really part of "society," but as all the time he has been kind and generous to his family, he cannot be accused of having pushed his brothers into the mire. The present condition of the latter is the result of their own conduct, which itself is probably due to bad heredity. Though equal in physique to their brother, they are lacking in that morale which has made him a good "sport" and is one of the chief elements, if not the chief element, in the evolution of the middle and upper classes. This example could unfortunately be multiplied by thousands, not only in the case of labourers' families, but also in those of the middle and upper classes. Mr. Houghton, and those who think with him, would do well to contemplate these facts before they write about the wickedness of "society" in pushing the poor into the mire.

I have now dealt seriatim with Mr. Houghton's strictures on my doctrines of racial origin and racial criteria; and in each case—whether it be osteology, pigmentation, language, religion, or sociology—the evidence adduced, which might be largely multiplied, shows that the fundamental assumption on which he bases his attacks—that Man is free from the laws which condition the rest of Nature—has no support in fact.

# STANISLAO CANNIZZARO

By M. M. PATTISON MUIR

The death of the greatest of philosophical chemists at Rome, on May 13 of this year, bids those who are engaged in building up the science advanced so greatly by him to consider the work which Cannizzaro did in clarifying and realising for himself and them some of the fundamental conceptions of chemistry.

The lifetime of Cannizzaro covered the greater part of the period of the development of modern chemistry. Born at Palermo in 1826, had he lived two months longer he would have completed his eighty-fourth year.

In order to understand the work of Cannizzaro, and to form a just estimate of the relations of it to the general progress of chemistry, it is necessary to consider briefly the position of each of the departments of chemical thought whereon his work shed light, at the time, 1858, when his greatest contribution to chemical philosophy appeared.

By showing chemists how to use them, Lavoisier gave definite meanings to three conceptions—chemical interaction, composition, element. Eighteen years before the birth of Cannizzaro, Dalton gave to chemistry an instrument for applying, minutely and intimately, to all particular cases of material change wherein composition and properties alter simultaneously, the ideas which Lavoisier had clarified and begun to use. Dalton laid hold of the atomic speculation of the Greeks, attached it to particular instances of a certain limited kind of material change, and added to it a method which changed it from a vague, qualitative view into a quantitative scientific theory. Dalton began the work of determining the relative weights of atoms.

The path opened by Dalton soon led him into a difficulty which neither he nor others could overcome. He saw clearly that before a value could be selected from the several possible values of the atomic weight of oxygen, it was necessary to determine the relative weight of the compound atom of water—the product of the union of oxygen and hydrogen—and the number of atoms of each of the elements which unite to form an atom of that compound. As Dalton failed to find a method of solving the first of these problems, he was unable to solve the second.

The year after the publication of Dalton's *New System of Chemical Philosophy*, Gay-Lussac announced, as the result of his experimental investigation of the volumes of gases concerned in chemical changes, that there is always such a simple relation between these volumes that all of them can be expressed as small whole multiples of the smallest volume. Dalton interpreted Gay-Lussac's generalisation as equivalent to an assertion that equal volumes of gases contain equal numbers of atoms. To make this statement, Dalton argued, was to declare that all atoms are identical in size and weight. As his theory rested on the assumption that the atoms of different substances differ in size and weight, Dalton refused to accept the generalisation of Gay-Lussac, and declared it to rest on insufficient evidence.

That he might overcome the difficulty that gravelled Dalton, and also accept the generalisation made by Gay-Lussac, the Italian naturalist Avogadro proposed, in 1811, to distinguish two orders of minute particles—to recognise the molecule as a group of atoms, the atom as a part of the molecule; and to think of most chemical interactions as separations of molecules into atoms, and rearrangements of the atoms to form new molecules. He made the hypothesis, now generally but erroneously called Avogadro's *law* (never so called by Cannizzaro), that equal volumes of gaseous elements and compounds, measured at the same temperature and pressure, contain equal numbers of molecules. Avogadro said that if this hypothesis is accepted, the weights of molecules are in the same ratio as the densities of the gaseous elements or compounds which interact. He proposed to take the weight of a molecule of hydrogen as unity. Avogadro determined the molecular weights of several elements and compounds by determining how many times a specified volume of each, in the state of gas, is heavier than an equal volume of hydrogen. Looking back from the position of to-day it may seem to us, that by using the Avogadrean conception of the molecule as the smallest portion of an element or compound wherein the properties of the element or compound inhere, chemists would have at once been led to a clear working description of the atom as the smallest particle of an element in any molecule whereof the element forms a part, and to the solution of the difficulty which stopped Dalton. But the history of chemistry tells us that chemists had to wait nearly half a century for the coming of a man of genius to show them the real meaning of Avogadro's hypothesis, by showing them how to use it as an elucidator of facts. That man of genius was Stanislao Cannizzaro.

The example and influence of Berzelius turned chemists away from grappling with the difficulty that the Daltonian theory left not overcome, to the ingathering of facts which bore directly on the fundamental assumptions of the theory. Before he received a copy of Dalton's book, Berzelius supposed that the founder of the atomic theory must have based his conception of chemical change, as interactions of atoms, on a foundation of carefully verified facts. Berzelius indeed supposed that Dalton had spent years in his laboratory amassing facts concerning the compositions of diverse classes of compounds, and had deduced from these facts the conclusion that all elements and compounds interact chemically in the ratios of certain constant and determinable quantities by weight, or in the ratios of whole multiples of these quantities. Berzelius saw that unless this statement were proved to be an accurate presentation of facts, the Daltonian theory would have to be abandoned. He therefore devoted himself for several years to an experimental inquiry into the justness of what he supposed to be the Daltonian law of chemical combination. When Berzelius found, from a perusal of his book, that Dalton did not state the fundamental facts of combination in the form of an experimentally established law, but assumed the law as a necessary deduction from the theory, he was more than ever confirmed in his determination to make his criticism of the Daltonian theory rest on work done in the laboratory, not in the study.

Other chemists followed the lines of inquiry laid down by Berzelius; and about ten years before the birth of Cannizzaro the law of chemical combination was firmly established by accurate experimental work. For some years many chemists almost abandoned the atomic theory. Proclaiming their determination to keep to facts, and facts alone, they attempted to express the facts of combination in formulas, wherein each symbol represented a combining weight of an element, that is, the smallest quantity by weight of it that combines with unit weight of the standard element. But they were soon stopped by the same difficulty which had stopped Dalton. They found it impossible to discover a principle which would enable them to select *the* value of each combining weight from the many possible values, all of which were in keeping with the facts.

Some chemists kept the Daltonian language, and spoke of atoms and particles, but could not form a clear, mental picture of either the atom or the particle; others tried to express all the facts about composition without a theory of the structure of matter, but found themselves in difficulties as great as those which confused and blocked the way of the atomists; another school thought to escape the stumbling-blocks that tripped up their companions by using the conception of equivalent weights, rather than either combining weights or atomic weights, but could not find a satisfactory working definition of equivalent weight. Chemistry was in confusion. In the midst of the confusion Cannizzaro was born.

During the years when Cannizzaro was receiving his training in natural science, chemistry was becoming more and more confused. The disorder reached its height in the fifties of last century. There were then many competing systems, each claiming the monopoly of chemical truth, each violently supported by men who as violently condemned all the others. Meanwhile, work in the laboratories went on, facts were gathered in, minor hypotheses were started and tried by their fruits, the science advanced, the times were ripening for the coming of the great interpreter.

In the year before the birth of Cannizzaro a gaseous compound of carbon and hydrogen was isolated by Faraday, and proved by him to have the same composition as olefiant gas, but to differ from that gaseous compound in relative density and other properties. Remarking on the differences which he observed between the interactions of chlorine with olefiant gas and the new compound, Faraday said:

"This is a remarkable circumstance, and assists in showing that though the elements are the same, and in the same proportions as in olefiant gas, they are in a very different state of combination. . . . In reference to the existence of bodies composed of the same elements, and in the same proportions, but differing in their qualities, it may be observed, that now we are taught to look for them, they will probably multiply upon us."

Faraday's expectation has been overwhelmingly realised.

In 1830 Berzelius constructed the adjective *isomeric*, and applied it to compounds which have the same composition but not identical properties. The isolation of many isomeric compounds seemed only to make chemical confusion worse confounded.

A question concerning the elements propounded by Berzelius in his memoir of 1830 widened the chemical horizon, and, while deepening the perplexities, also increased the zeal of chemical investigators. Berzelius recited facts which seemed to show that some elements exhibit a phenomenon similar to isomerism of compounds. To obtain some clear conception of the isomerism of compounds and the *allotropy* of elements, Berzelius suggested that the differences between isomeric compounds may be associated with differences of aggregation of their minute particles, and the differences between the allotropic forms of an element, with changes in the arrangement of the atoms of the element.

When Cannizzaro was six years old, Wöhler and Liebig published their research on oil of bitter almonds. Trying to bring into one point of view the many interactions and relations of the compounds they had isolated and studied, the two chemists introduced into organic chemistry the notion of "the compounded element," named also the compound radicle. They thought of the collocation of certain quantities by weight of the three elements, carbon hydrogen and oxygen, as a group of atoms of comparatively great stability, which acted in many chemical transformations in the same way as an element acts, that is, combined as a whole with other elements and separated as a whole from its combinations with other elements. They thought of this hypothetical compounded element, which they named *benzoyl*, as a "foundation" whereon many compounds were built, and they associated the observed similarities between these compounds with the assumption that they all rested on one and the same foundation. Berzelius rejoiced when he received a copy of their memoir from Wöhler and Liebig. He saw the coming of a new day in organic chemistry. He proposed to change the name of the compound radicle: it should no longer be called benzoyl, it should be called *Proin*, or *Orthrin* (πρωί = the beginning of the day; ὄρθρος = daybreak).

An examination of the chemical memoirs published in the thirties and forties of last century shows that chemists were constantly forced to use the conceptions and the language of the atomic theory, even when they tried to confine themselves to the skinniest descriptions of the facts, and more especially the facts about isomerism and allotropy, that were daily added to the science.

During the thirties of last century Dumas began his study of the replacement of hydrogen by chlorine in organic compounds, and the relations between the parent compounds and their derivatives, a study which tended towards the fixing of the vague notions about substitution that had for long been simmering in the minds of many chemists. Dumas tried to go no further than the assertion that, as a matter of fact, one equivalent of hydrogen can be replaced, in many organic compounds, by one equivalent of chlorine. Then he said that in many cases this replacement is accompanied by "maintenance of the chemical type." But what does "maintenance of the chemical type" mean? many chemists—notably Laurent—demanded. Dumas replied:

"I consider as belonging to the same chemical type those substances which contain the same number of equivalents united in the same manner, and have the same fundamental chemical properties."

He produced formulas of several pairs of compounds, each pair of which he declared to belong to the same chemical type. But as neither he nor any other chemist could give a clear, workable meaning to the word *equivalent*, nor form a mental picture of "equivalents united in the same manner," his formulas were sentences of a language which bristled with vague hypotheses while it pretended merely to express facts.

Some years earlier, Dumas had made a bold attempt to reconcile the notions of the simple atom and the compound atom; he had even tried to apply the notion of the compound atom to the elements. But the facts of gaseous combinations, many of which were captured for chemistry by Dumas himself, proved too much for his hypothesis. He had attempted to revive Avogadro's supposition, but failing to make it act as a knowledge-producing instrument, he tried to convince himself that his business was that of the mere fact-finder to whom hypotheses are useless. Despairing of the atomic theory, in his *Lectures on Chemical Philosophy*, delivered in 1836, Dumas made his plaint:

"Si j'en étais le maitre, j'effacerais le mot *atome* de la science, persuadé qu'il va plus loin que l'expérience; et jamais en Chemie nous ne devons aller plus loin que l'expérience."

Looking back, we see that Dumas had confused equivalent weights with atomic and also with molecular weights; none of these three conceptions had been clearly grasped, and distinguished from the others, at the time when he promulgated his notion of substitution and chemical types.

In the forties and fifties of the nineteenth century chemistry was waiting for one who should possess the condensing and realising powers of a man of science, and also the generalising and expanding powers of a philosopher. It was fitting that the man of genius should appear in the country which had been the first in Europe to break away from the deadening scholasticism of the Middle Ages, to re-discover the meaning of lucid thinking about realities, to welcome the return to earth of beauty, the great illuminer. In 1853, Cannizzaro published his first chemical memoir, on benzylic alcohol.

From 1853 to 1855 Cannizzaro was Professor in the Technical Institute of Alessandria; for a few years he was Professor in the University of Genoa; for twelve years he was Professor in his native town of Palermo. In 1871 he was called to Rome, as Professor of Chemistry and member of the Italian Senate. He died in Rome on May 13, 1910.

Cannizzaro did not publish many memoirs on purely experimental chemistry. He was essentially a thinker. His most important contributions to theoretical and philosophical chemistry are contained in *An Abstract of a Course of Philosophical Chemistry given in the Royal University of Genoa*, which appeared in 1858; various articles in *Nuovo Cimento* in 1857 and 1858, bearing on the fundamental conceptions of the atomic and molecular theory; and "The Faraday Lecture," delivered to the

Chemical Society of London in 1872, and entitled *Considerations on some Points of the Theoretic Teaching of Chemistry.*

A congress of chemists was called at Carlsruhe, in 1860, to endeavour to bring some order into the confusion which prevailed in theoretical chemistry. Copies of Cannizzaro's *Abstract* were distributed among the members of the Congress. Lothar Meyer has told how, when he read the pamphlet, after the Congress:

"Es fiel mir wie Schuppen von den Augen, die Zweifel schwanden, und das Gefühl ruhigster Sicherheit trat an ihre Stelle."

Cannizzaro begins by setting forth the historical fact that almost every chemist who came after the appearance of Dalton's book had been forced to think about the actions he studied in images suggested by the atomic theory. He then reasserts Avogadro's hypothesis—equal volumes of gases, whether simple or compound, contain equal numbers of molecules—and refuses to admit any exceptions to this statement, declaring that apparent exceptions will be found not to be real exceptions when more searching experiments shall have been made. By affirming the universal applicability of the Avogadrean supposition, Cannizzaro said that results are obtained which are in keeping with all the laws that have been formulated by chemistry and physics. Cannizzaro then restates Avogadro's method of determining molecular weights; it is only necessary, he said, to find how many times any specified elementary or compound gas is heavier than hydrogen. The values which had been determined for molecular weights by this method had been so out of keeping with those arrived at by chemical methods, and had led chemists into so many perplexities, that the Avogadrean method had been either abandoned or used only in particular cases and as subsidiary to other, less direct (and, as most chemists thought, less theoretical) methods. By a stroke of genius, Cannizzaro changed the unit to which molecular weights determined from measurements of vapour densities are referred. He said:

"Instead of taking for your unit the weight of an entire molecule of hydrogen, take rather the half of this weight, that is to say, the quantity of hydrogen contained in a molecule of hydrochloric acid."

Cannizzaro then gives a table of molecular weights, referred, first to the weight of a molecule of hydrogen as unity, and secondly, to the weight of a semi-molecule of hydrogen as unity; in other words, he tabulates the densities of certain elementary and compound gases, referred, first to hydrogen as unity, and secondly, to hydrogen taken as having a density equal to two.

Cannizzaro's table contains two values for the density, and therefore, for the molecular weight, of oxygen, and two values for sulphur-gas. The value given to the molecular weight of oxygen, referred to that of hydrogen as twice unity, is 32; and the value for "electrified oxygen" is 128. To the molecular weight of sulphur "under 1000°" is given the value 192, and to that of sulphur "over 1000°," the value 64. At the very outset of his research Cannizzaro recognised the coalescence into groups, which act as individual molecules, of different numbers of identical elementary atoms. He brought within the scope of the molecular and atomic theory some of the phenomena of allotropy; and, so far as his application of the theory went, he made it possible to think of these phenomena without using the shuffling phrase, "different forms of an element."

Cannizzaro's next step was to examine the compositions of molecules the relative weights of which he determined by using the hypothesis of Avogadro. This is the part of his work which seems to me most evidently marked with the sign-manual of a splendid clarity. He describes his method with admirable precision:

> "If the body is a compound, it is analysed, and the constant weight-relations of its constituents are determined; the molecular weight is then divided into parts proportional to the relative weights of the components, and the result is the quantities of the elements contained in the molecule of the compound, referred to the same unit [namely, the semi-molecule of hydrogen] as is used for the expression of all molecular weights."

In applying this method Cannizzaro first sets down the molecular weights of various gases, both elementary and compound, "referred to the weight of a semi-molecule of hydrogen as unity," and then the weight (again referred to that of a semi-molecule of hydrogen as unity) of each constituent of each gas contained in a molecular weight of that gas. He then makes similar arrangements of the analyses of several compounds of the same element. I re-produce his table of compounds of chlorine:

| Name of substance containing chlorine. | Weights of chlorine in the molecules, referred to the weight of a semi-molecule of hydrogen as unity. |
|---|---|
| Hydrochloric acid . . . . | 35·5 = 35·5 |
| Chlorine . . . . . . | 71 = 2 × 35·5 |
| Mercuric chloride . . . . | 71 = 2 × 35·5 |
| Arsenious chloride . . . . | 106·5 = 3 × 35·5 |
| Stannous chloride . . . . | 71 = 2 × 35·5 |
| Stannic chloride . . . . . | 142 = 4 × 35·5 |
| etc. etc. etc. | = $n$ × 35·5 |

Atomic weight of chlorine = 35·5

Having presented similar orderly arrangements of facts concerning gaseous compounds of each of several elements, Cannizzaro arrives at a conception of the atom which is perfectly lucid and contains in itself the method of determining the relative weights of atoms. As true genius always does, Cannizzaro goes straight to the centre of the problem. He grasped and held "the particular go of the thing," around which two generations of chemists had "hummed and buzzed" for half a century.

Here is Cannizzaro's statement of the *law of atoms*:

"By comparing the different quantities of one and the same element which are contained, either in the molecule of the free element, or in the molecules of its compounds, the following law stands out in relief: *the different weights of one and the same element contained in the various molecules are always whole multiples of one quantity, which is justly called the atom, because it invariably enters the compounds without division.*"

At last, forty-seven years after Dalton had started chemists on the path, the open clearing in the tangle of facts is reached; a survey can be taken of the wood; the directions which new paths should follow are clearly seen. Well might Lothar Meyer declare that Cannizzaro had made the scales fall from his eyes, and had replaced his doubts by a feeling of quiet security.

It was now easy to construct simple, self-consistent, intelligible formulas, which should express the compositions of molecules in terms of their constituent atoms, for all gaseous and gasifiable compounds.

> "The atom of every element," Cannizzaro said, "is expressed by that quantity of it which invariably enters as a whole into equal volumes of the simple substance and its compounds; this quantity may be either the whole quantity contained in a volume of the free element, or a fraction thereof."

The second part of this sentence opens the way to the exact study of what is now called the atomicity of elements.

Cannizzaro said:

> "In order to determine the atomic weight of any element, it is essential to know the molecular weights, and the compositions, of all or most of its compounds."

To know the molecular weights of compounds, it was, of course, necessary to determine the relative densities of them in the gaseous state. Even to-day compounds of not more than about two-thirds of the elements have been gasified. Cannizzaro showed chemists how to obtain approximate values for the molecular weights of compounds that have not been gasified, by studying the chemical relations between these compounds and other similar compounds the molecular weights of which have been determined by the direct application of the hypothesis of Avogadro.

I think one may say that every chemist who worked between the time of the appearance of Dalton's *New System* and the Congress of Chemists in 1860 had drifted away from the definite but too limited conceptions of the founders of the modern atomic theory, Dalton and Avogadro, and had tended, whether he would or not, to divide the theory into parts, held together by no common binding idea. Cannizzaro returned to Dalton and Avogadro. He realised the parts of the theory, showed the meaning of each part, and set before chemists a clear mental picture of the interrelations of all of them—that is, of the complete theory.

The definitions which Cannizzaro formed of the molecular weight and the atomic weight of a substance are strictly working definitions; they tell what must be done to determine the quantities which they define.

In 1852—that is, five years before the appearance of Cannizzaro's *Abstract*—Frankland applied the notion of equivalency to the atoms of the elements. He invited chemists to consider facts which led him to assign to each elementary atom a certain "combining power . . . which is always satisfied by the same number of [other] atoms." Frankland acknowledged, in 1877, that his hypothesis could not have been satisfactorily developed had not Cannizzaro "placed the atomic weights of the metallic elements on their present consistent basis." But Cannizzaro did much more to advance the subject of atomic equivalency, and to prepare the way for the overwhelming multiplicity of its applications in recent chemical work, than is indicated in the words quoted from Frankland's *Researches*.

In the *Abstract* of his Lectures Cannizzaro tabulates the molecular compositions of series of chlorides, and shows that one atom of each of certain elements combines, sometimes with a single atom of chlorine, sometimes with two atoms of chlorine: as one atom of chlorine combines with one atom of hydrogen, he concludes that a single atom of each of the elements in question is equivalent to one atom of hydrogen in some compounds, and to two atoms of hydrogen in some other compounds. He then shows that the weight of chlorine which combines with one atom of certain elements—call these, elements of Class I.—combines with two atoms of certain other elements—call these, elements of Class II.; and then, expanding his data, he finds that the weight of any element which combines with a single atom of an element of Class I. combines with two atoms of an element of Class II. He thus arrives at the conception that each atom has a definite capacity of saturation. Having defined unit capacity of saturation of an atom as the power of combining with a single atom of hydrogen, chlorine, bromine, or iodine, to form a molecule, he classifies many atoms in accordance with their saturation-capacities.

The consideration of all the facts which he had marshalled convinced Cannizzaro of

> "the unchangeability of the proportions between the atomic weights of the bodies which mutually replace one another, whatever be the nature and number of the other constituents of the compounds." "This is a law," he said, "which limits the number of possible compounds and more especially applies to all cases of double exchange."

Having thus got a firm hold of the *law of atoms*, following from the study of the compositions of molecules, and the *law of* *the equivalency of atoms*, following from the same study, Cannizzaro uses these laws in the examination of "the transformations of matter," which he says is "the true object of our science." He classifies reactions, and expresses them in general formulas, using the symbols R′, R″, R‴, etc., to denote monatomic, diatomic, triatomic, etc. (or, as they are now called, univalent, bivalent, tervalent, etc.), atoms, and radicles—that is, according to Cannizzaro, atomic groups which function as simple atoms in particular interactions.

Cannizzaro's method of studying interactions was to consider particular cases in detail, and then to generalise these in symbolic expressions, which included all the special interactions he had examined, and suggested others. He investigated direct combinations of simple and compound radicles; substitutions of simple and compound radicles in chlorides, bromides and iodides; substitutions of radicles in acids, and in salts; double exchanges of the radicles, classified in accordance with their saturation-capacities, in the interactions of pairs of compounds. In studying these transformations of matter, Cannizzaro brings out unlooked-for analogies between the metals of inorganic, and the radicles of organic chemistry; he develops the conception of substitution, and makes clear what Dumas dimly saw when he spoke of "the maintenance of chemical type"; he classifies acids in accordance with their basicities, and shows, by examples, the usefulness of this classification; he removes the difficulties about oxy-acids and hydro-acids which had been troubling chemists since the time of Lavoisier; he develops and uses consistent formulas for both inorganic and organic compounds, formulas which include and reconcile the conceptions sought after by Dalton, Davy, Berzelius, Gerhardt, Laurent, Dumas, Liebig, and many other chemists of renown.

All this, and more than this, was the goodly gift that Cannizzaro gave to chemists more than half a century ago, at a time when all other chemists were groping in the jungle of facts, and almost despairing of a way out. Cannizzaro did not show chemists a way out of the jungle of facts; he showed them how to bring order into the facts. Get out of the sensible realities, and you are lost; remain in the realities without guiding ideas, and you are lost too. The illuminating, controlling, order-producing, fruitful conception was near at hand; but no one seized it until Cannizzaro came.

It is surely high time that an English translation was made and published of Cannizzaro's *Sunto di un corso di filosofia chimica fatto nella Reale Universita di Genova.*

The work of Cannizzaro must ever remain an encouragement and an inspiration to all genuine students of nature, to all earnest seekers of truth. Wonderful is the influence of the man of genius on the advancement of knowledge, when the environment is suited to his growth. In 1857 the chemical environment was propitious: only the man was wanted. The man appeared in the person of Stanislao Cannizzaro.

# REVIEWS

**Outlines of Chemistry with Practical Work.** First Part. Second Edition. By HENRY JOHN HORSTMAN FENTON, M.A., Sc.D., F.R.S. [Pp. viii + 410.] (Cambridge: at the University Press, 1910. Price 9*s*. net.)

WE are grateful to Dr. Fenton for this book—it is more than refreshing to meet with a teacher who makes some attempt to be logical and who ostensibly advocates freedom and breadth of opinion, for the world to-day is in a strange state: owing to the influence secured by school-master and school-marm and by the press, one uniform tone of thought—perhaps it were better said of thoughtlessness—is beginning to possess the nations. Over the great North American continent, for example, the boys all wear clothes of like pattern and the girls all seem to be framed in the same pair of stays—there is no other way of accounting for the similarity of figure and deportment that meets the eye north, south, east and west; no matter how hot and sunny the weather may be, the straw hat is doffed on the first of October in obedience to a rigid public opinion. Little wonder that a people that suffers hand-me-downs and has developed no philosophy of clothes should lack the sense of freedom and the courage to deal with the social abuses by which it is hampered: hence perhaps the value of a Roosevelt and the admiration accorded to his masterful ways.

Science so-called offers no exception to social habits—civilised Europe and America too are at present all but dominated by fashions set by a certain clique of "scientific" leaders, who are as little guided by broad philosophical conceptions, such as the early fathers followed, as are the Worths of our fashionable world. In the days of Liebig, science was studied at the bench: to-day it is spoon-fed to students in lecture rooms and from text-books. An age of idolatry is once more upon us—we have discovered a new Baal in the Examination Demon and worship him furiously, the offerings being the fees paid by parents and the spoiled lives of examinees—sacrifices more real perhaps than those of olden times.

Dr. Fenton is a sort of chemical knight-errant, his attitude being one of protest against the modern spirit of idolatry. The manner in which he deprecates the development of dogmatic habits is certainly most unusual, his "pay-your-money-and-take-your-choice,-gentlemen" attitude being delightfully inspiring and in marked contrast to that of writers of the modern intolerant, unobservant chemical-physical school, whose books unfortunately flood the market at the present time and make all genuine "inquisitive" practical study of our science impossible.

"As regards those subjects about which there is any reasonable difference of opinion, the attempt is made to adopt a wholly impartial attitude. It will be generally admitted that one of the peculiar advantages of a chemical education lies in the opportunity and encouragement that it affords for originality and independence of thought": so writes Dr. Fenton in his preface; if such views were more widely held, our science would be on a far higher moral plane than that on which it now rests; it would be admitted to be of real value as educational discipline; as now taught, it is probably better avoided. It is perhaps open to question whether Dr. Fenton altogether lives up to his own profession—he somewhat spoils the effect of his prefatory statement by excessive advocacy of one hypothesis and he can even be unfair. The statement (p. 201) that "the theory of ionic dissociation met with violent opposition at first and is still bitterly 'resented' by certain chemists of undoubted reputation" is nothing short of improper. The use of resented, even in inverted commas, is not justifiable and it is not the office of the writer of a text-book to appraise the reputations of those who have dared to differ in a particular discussion—reputation has nothing to do with the matter; all that we are concerned to know is whether objectors have been actuated by honesty of purpose and whether they have looked facts in the face. Unfortunately, at the present day, owing to the ease with which literary advertisement is secured and the prevalence of log-rolling, "reputation" covers a multitude of sins and is too often a means of misleading public opinion.

Dr. Fenton's book is of particular interest as showing what has been taught in the University at Cambridge as chemistry and how those who are being trained as chemists have been led to regard the subject. The course developed in it is stated to be that in General and Physical Chemistry which it is customary to give to candidates for the Natural Science Tripos. If such be the case, we fear that far too much has been attempted; that no really chemical foundation can have been laid. As to the first of these contentions, Dr. Fenton admits in his preface that the teaching has been carried on at high pressure. One is reminded of Mark Twain's inimitable stories of racing on the Mississippi—how the hams put on board as cargo were burnt under the boiler while the pilot sat on the safety valve. Mr. Fenton stokes his students with ions instead of hams and raises their mental osmotic pressure far beyond bursting point by loading them up with all the higher problems of so-called Physical Chemistry, not omitting the Phase Rule and the Colloidal State: most appropriately perhaps, as a colloid state of intelligence must be developed by so heavy a course administered in so short a time.

The subjects dealt with are Molecular and Atomic Weights, Valency, Chemical Change, Electrolytic-dissociation, Acids, Salts and Bases, Hydrolysis, Solutions, the Phase Rule, Diffusion and the Colloid State. It is difficult to say where the art of chemistry comes into this scheme. Mathematical formulæ abound; this, that and the other "law" is duly advertised: in short, all that passes for mathematics and physics nowadays among students of chemistry is more or less thoroughly expounded—yet chemistry proper is but scantily treated. Several chapters are devoted to chemical change but the conditions which determine it are nowhere defined or discussed—yet work has been done on the subject of late years. Under Catalysis, Dr. Fenton does not actually tell the student that the function of the catalyst is merely to modify the velocity of a change, not to determine it: unlike most writers, he spares us the statement "Ostwald has said that this is the case, therefore it must be so"; yet he goes far towards fathering such doctrine. Early in the chapter the statement is made, "In all the above mentioned examples (including the hydrolysis of sugar by acids) it can be shown *experimentally* that the change in question does actually take place when the catalyst is absent, the only observed difference being the greatly increased rate when the catalyst is present!" This is simply untrue—no one yet has shown that cane sugar, for example, can be hydrolysed by water alone: strictly speaking, it is impossible to make the experiment. The statement is of the kind we are always accepting, however; we take what is offered us without question until, for some reason, doubt arises—and then only do we become critical. The fact is life would be unbearable if we were always critical—hence the difficulty of arriving at the scientific attitude of mind. Dr. Fenton might, however, have drawn attention to the many cases in which chemists of undoubted "reputation" as clean workers have shown that two substances A and B are without action upon one another which interact readily when brought into a system with a third component. Writers of text-books on Physical Chemistry rarely deign even to refer to such cases—Mellor stands alone in having drawn attention explicitly to them.

We chemists need to go back to the land we were once so proud to cultivate—to chemistry: to realise that the chief office of the chemist is to know chemical materials—their qualities, their affections; to lead them into new ways. We also need to bear in mind that the condition of solution is necessarily one of great complexity and that accurate mathematical expression cannot be given to many of the phenomena—that the seeming agreement between hypothesis and practice is arrived at merely because cross influences are at work which counteract one another. Our office should be to form definite ideas of things, even though these cannot be seen or handled: we should learn to think of what may be rather than to assume that we know what is.

The mathematics thrown at chemical students is often of a very misleading if not spurious character. They are rarely warned that the expressions used have not necessarily any particular meaning—that they may be fitted equally well to several totally distinct if not incompatible interpretations—that they more often than not serve to mask ignorance rather than to define knowledge.

We need to return to the good old times when we were not ashamed to deal with plain acids and alkalies: modern indulgence in an obscure jargon is carrying us more and more beyond the ken of the public; if science is to be helpful in education and to the public its ways must be mended and simplified; realities must be dealt with to a greater extent than has been customary of late years.

Dr. Fenton has made an admirable beginning: the example he sets should be of great value to all students who appreciate the spirit in which his book is written; if, in later editions, he will allow full play to his sense of logic and his desire to be consistent, he will make his book of extreme value. And we would beg of him, if possible, to put into it some of his knowledge of laboratory method, some of that wonderful dexterity of hand and preparative skill that he has displayed in the masterly series of studies associated with his name of late years, which have gained for him world-wide appreciation. As he is one of the few chemists left to us, we cannot spare him from chemistry.

The only safe plan in scientific work, when conclusions have been drawn, is to consider whether after all the facts have been properly stated—whether after all the interpretation given be justified—whether other points of view may not be adopted. The student who is trained to work on such lines, to use hypothesis but to use it warily and not allow it to degenerate into dogma, will be a reasonable being and of some use in the world: under the now fashionable system the doctrines of unreason prevail and the men turned out are of little use in practical affairs. The escape from Erewhon was made in a balloon: now that we have flying-machines at our disposal, the escape from the pseudo-mathematical, pseudo-physical fools' paradise in which we have too long been lounging should not be so difficult.

**The Simple Carbohydrates and the Glucosides.** By E. F. ARMSTRONG, D.Sc., Ph.D. [Pp. x + 112.] (London: Longmans, Green & Co., 1909. Price 3*s.* 6*d.* net.)

THERE is surely no chapter in the whole range of the history of the chemistry of carbon compounds that calls forth greater admiration than that which centres round the elucidation of the structure of *d*-glucose. A prince amongst investigators devoted himself to this problem and to others arising from it; and the editors of the series of monographs on Biochemistry are therefore fortunate in their selection of a pupil of Emil Fischer to write this all too brief account of the simple carbohydrates and glucosides.

In this *résumé* special attention is devoted to the chemical properties and constitution of *d*-glucose, in which connection ample justice is done to the arguments which in recent years have accumulated in favour of the $\gamma$-oxide structure. The chapter on the chemical properties might, however, have preceded with advantage that dealing with constitution. Further, since the whole problem of configuration of carbohydrates is so intimately based on stereo-chemical conceptions, one cannot help thinking that a brief preliminary chapter on these conceptions might have been incorporated with advantage in the present volume for the benefit of that section of readers whose interests are more physiological than chemical. In Chapter I. *d*-glucose is introduced at once without any reference to *l*-glucose and *dl*-glucose, and an acquaintance with the meaning of a projection formula is assumed.

Chapters on hexoses, pentoses, and disaccharides are followed by a particularly interesting account of the relationship between configuration and biochemical properties. Here the author is in agreement with Wohl, that glucose, fructose, and mannose have a common enolic, unsaturated form. The close attachment which must take place between an enzyme and the compound it attacks is duly emphasised. From the discussion which follows on the highly significant results obtained by the use of enzymes as hydrolytic agents, it must be clear to every one that the separation of enzymes is a problem which will engage the biochemist for many years to come. Finally, a chapter is devoted to the natural and synthetic glucosides.

A complete bibliography is appended. This will prove useful; but the doubt may be raised whether it is preferable to the usual method of quoting references in the text.

Chemists are indebted to Dr. E. F. Armstrong for this admirable work, especially since, in our country, we have so few books of this type dealing with special subjects and written by a man who is actively engaged in investigating certain of the problems which he discusses. The true spirit of research is displayed on every page—even the phrasing betrays in places its Armstrongian origin; but should "glucase" (p. 55) not be "glucosidase"?

ALEX. MCKENZIE.

**Physics.** By L. LOWNDS, B.Sc., Ph.D. [Pp. vi + 145.] (London: Macmillan & Co., 1909. Price 1*s.* 6*d.*)—**A Class Book of Physics.** By R. A. GREGORY and H. E. HADLEY, B.Sc. [Pp. viii + 487.] (London: Macmillan & Co., 1909. Price 4*s.* 6*d.*)

IT is no easy matter, even for the experienced teacher, to gauge carefully the point from which to start a student off on a new subject; for, to retain the interest of the beginner, it is as disastrous to assume his complete ignorance of allied studies as it is to commence at too high a level. This is particularly the case with such a wide and embracing subject as Physics.

Dr. Lownds's little book is quite a successful attempt as an introduction to the study of elementary physical principles, dealing as it does with the prominent facts of Heat—*e.g.* thermometry, expansion, and change of state, and the general introduction necessary for a qualitative acquaintanceship with these subjects. The distinction between mass and weight might well have been omitted from this elementary book. As an introductory first-year course the matter dealt with here should be very suitable, and the use of this book would make unnecessary a separate practical manual.

Gregory and Hadley's book should serve readily as a continuation second-year course. With the knowledge of general principles already gained, this other volume, which deals more cursorily (perhaps too cursorily in places) with the larger field of Heat, Light, Sound, Magnetism and Electricity, should prove straightforward work. The section on light, which would be the first new subject of the second-year course, is the best portion of the book, and the figures here are particularly clear and instructive. The last part, dealing with electricity, could have been, with advantage, entirely omitted and the space taken up with a little more detail in the other branches.

With this qualification, then, these two volumes should prove excellent company as a two-years' course in elementary physics. Both are quite non-mathematical.

J. C. NIXON.

**Physical Science in the Time of Nero, being a Translation of the Quæstiones Naturales of Seneca.** By JOHN CLARKE, M.A. With Notes on the Treatise by SIR ARCHIBALD GEIKIE, K.C.B., D.C.L., Sc.D., LL.D., P.R.S. [Pp. liv + 368.] (London: Macmillan & Co., 1910. Price 10*s*. net.)

THE progress of science is at the present day so rapid that few have time to pay much attention to the advancement in the past even of that particular branch which specially interests them. Yet a knowledge of the lines along which any science has advanced is likely to lead to some idea of the direction of the continuation of those lines in the future; furthermore, recognition of the causes of the errors into which our predecessors have fallen may give us indication of the pitfalls which we ourselves must avoid.

The neglect of past history will be minimised if the specialist in research has his task in studying it rendered easier, for few are able to advance their own science in a conspicuous degree, and at the same time by study of original authorities to summarise its progress in the past. There are, however, such men: prominent among them is the President of the Royal Society, to whom the present translation of Seneca's treatise owes its inception, to whom also the translator is indebted for critical and explanatory notes.

Seneca's work under notice is not a critical summary of Physical Science as known at the time of its publication. "It was not his aim to compose a systematic treatise on Natural Philosophy, but rather to take up some special subjects, and deal with them in the light of what had already been written upon them, and of what his own reflections suggested." The subjects dealt with appertain mainly to the studies now known as Astronomy, Meteorology, and Physical Geography. The book is incomplete; but with all its imperfections it is of great value; for, as Mr. Clarke states in his Introduction, it "is a landmark in the Progress of Physical Science. From Aristotle and Theophrastus there is a great gap until we reach Seneca; the gap is still greater between Seneca and the Renascence, from which the era of true science is to be dated. The Q[uæstiones] N[aturales] is the last word spoken on the subject by the classical world."

Ever useful is the impressive lesson conveyed by the author on the moral influence of the study of Nature: why "it will be profitable for us to examine the nature of the Universe. In the first place, we shall rise above what is base; in the second, we shall set the spirit free from the body, imparting to it that courage and elevation of which it stands in need."

We cordially recommend the book to all readers. In addition to the text we have an introduction and notes by the translator, and the explanatory notes by Sir Archibald Geikie to which reference has been made. Mr. Clarke must be congratulated on overcoming the many difficulties of translation. He has put the thoughts of the author into up-to-date language without "attributing to Seneca ideas that were unknown to him, and are due to modern analysis and discovery."

JOHN E. MARR.

**Indian Insect Life: a Manual of the Insects of the Plains.** By H. MAXWELL-LEFROY, M.A., F.E.S., F.Z.S., assisted by F. M. HOWLETT, B.A., F.E.S. [Pp. xii + 786.] (Calcutta and Simla: Thacker, Spink & Co. 1909. Price 30*s*.)

THIS splendid volume—its weight makes the handling and using of it almost a health exercise—is at once a monument of industry and a sign of the times in biology. No science can more easily justify itself on the applied side than entomology. When we remember that on continents abroad the destruction done by insects to crops—agricultural, horticultural and forest—is equal to the outlay necessary for the upkeep of an army and navy, or exceeds the money necessary for the organisation and working of a national system of education, the entomologist has no difficulty in justifying his subject. In India also great toll is levied by insects. As the author says, apart from diseases due to attack upon man or the domesticated animals by predaceous or disease-carrying insects, "locusts lay waste a district, the boll-worm takes a tenth of the cotton crop or three-quarters of it in an occasional year, the moth-borer kills one cane in three, the rice hispa causes famine or the rice grasshopper destroys the paddy [rice] over a whole division, no fruit industry can be established in places where fruit grows, etc., etc.," and all due to insects. Again and again *Homo sapiens* of the books has proved anything but efficient in his dealings with insects, and has had unwillingly to own, as the author of this book naïvely says, that "not the least among man's occupations is that of providing food and occupation for insects."

This volume—the sections on Mallophaga, Diptera, Cimicidæ and Anoplura have been prepared by Mr. Howlett, and the portion on Insects and Flowers by Mr. J. H. Burkill—has been written mainly for the use of students of entomology in India, but is certain to be very useful to entomologists everywhere. After an interesting though necessarily sketchy introduction on insect instinct and habit, and a glance at the difficulties attending present attempts at a scientific classification of insects, Mr. Maxwell-Lefroy gives a helpful résumé of the various insect families, grouped according to food and habit, under the headings: I. Land Insects—herbivorous, parasitic and predaceous; II. Salt-water Insects;

III. Fresh-water Insects; IV. Myrmecophilous Insects. Then each Order is treated in detail.

The treatment of the Order Hymenoptera may be taken as typical. A résumé of the general characters of the Order is followed by a scheme of classification. Under each of the main headings in the classification of the Order comes a notice of the main features of each family—the Woodwasps, the Sawflies, the Gallmakers, the Proctotrypidæ, the Chalcids and other parasitic families, the Solitary and Social Wasps, the Solitary and Social Bees, the Ants. Reference is made in the various families to Indian species, and notes on habit and life-history, and collecting are also given.

Interspersed among the main chapters are incidental chapters on topics suggested by phenomena that have chanced to be mentioned under a family. Thus we have: "On Cosmopolitan Insects," suggested by the notice of the Blattidæ; "On Aquatic Insects," suggested by the Dragon-flies; "On the Relative Duration of Life in the Insect World," suggested by the discussion of the Mayflies; "On the Size of Insects," suggested by the Proctotrypidæ. The Velvet Ants suggest an interlude on "Sex and External Sexual Distinctions." Under Lepidoptera come interludes on "Silk" and "How Insects protect themselves." Under Diptera special notes are given on Blood-sucking Insects, and amongst the Rhyncota, the Cicadidæ lead to a mention of "Song among Insects." These incidental chapters are both happy and suggestive, and form an interesting and helpful portion of the book.

The whole work is finely illustrated by over eighty coloured plates and a very large number of black-and-white figures. The labour required to bring together the wealth of information contained in this volume, supplemented as it is by very numerous first-hand observations, must have been great, and deserves acknowledgment. Mr. Maxwell-Lefroy and those working with him can be heartily congratulated on the production of a very useful book, and we hope that the assurance of this and the hearty "Well done!" of workers in entomology generally will be some reward.

R. STEWART MACDOUGALL.

**Charles Darwin and the Origin of Species: Addresses, etc., in America and England in the year of the two Anniversaries.** By E. B. POULTON, D.Sc., M.A., Hope Professor of Zoology in the University of Oxford. [Pp. xv + 302.] (London: Longmans, Green & Co., 1909. Price 7*s*. 6*d* net.)

IN this volume Prof. Poulton has collected a number of addresses delivered by himself at meetings held to celebrate the centenary of Darwin's birth, or the fiftieth anniversary of the publication of the *Origin of Species*. Three of the seven papers of which the volume consists have been published before in other collections of addresses of the same kind; the remaining four are printed for the first time. Prof. Poulton is known to all biologists as one of the most faithful disciples of Darwin. His own researches have consisted almost entirely in the study of new examples of the action of natural selection, and to his mind that theory still affords a complete explanation of the phenomena of evolution. A great part of the present volume is entirely free from controversy, much of it consisting of additional details, discussed with that literary skill which Prof. Poulton possesses in no slight degree, relating to Darwin's exceptional life, character, and personality. In the preface, in the first address on "Fifty Years of Darwinism," and in the appendices, however, the author fights vigorously against the view of the mutationists that the small individual variations on which the theory of natural selection chiefly depends are of little importance in evolution. There is no objection to controversy *per se.* So long as investigators form different conclusions on the same subject, controversy is unavoidable. It is not merely necessary, but advantageous ; even if opponents do not convince each other they help others to more correct judgment. Darwin himself did not abstain from controversy—he considered and replied to the objections brought against his theory. It is the introduction of the personal note in controversy which is to be deprecated ; and when Prof. Poulton compares de Vries, Bateson, and Punnett to builders of Babel who do not understand one another's speech, one cannot help feeling that he is exceeding the legitimate bounds of scientific argument.

The point at issue is the heredity of individual variations, or more precisely the opinion of de Vries concerning their heredity. De Vries termed such variations, which universally occur among the individuals of the same species and even of the same parentage, fluctuations. According to Mr. Shipley's address to the Zoological Section of the British Association at Winnipeg in 1909, fluctuations are due to external conditions, and are probably not inherited. Mr. Shipley's statement was adopted from Mr. Punnett's little book *Mendelism.* Prof. Bateson writes that de Vries for the first time pointed out the distinction between the impermanent and non-transmissible variations which he calls fluctuations, and the permanent and transmissible variations which he calls mutations. Prof. Poulton quotes passages from de Vries, and from Hubrecht's expositions of the views of de Vries, which prove, not that de Vries stated fluctuations to be hereditary, but that he believed they could not, however much selected, lead to progressive evolution, because as soon as the selection ceased the organism would return to its original condition. In Prof. Poulton's opinion this view necessarily implies a belief in the hereditary transmission of fluctuations. But when Bateson refers to fluctuations as impermanent and non-transmissible he does not appear to mean anything more than de Vries meant by saying that continued selection has no appreciable effect on them, or than Galton meant by regression towards mediocrity ; and Prof. Poulton accepts Galton's principle as true. Both Galton and de Vries agree that when an individual fluctuation is selected for breeding, the offspring present variations round the degree of greatest frequency, which may not be the same as the parental character. According to the mutationists, when a mutation occurs the offspring all present the particular character which constitutes the mutation. This is all that Bateson means by saying that a mutation is transmissible, a fluctuation not.

The most important paper in Prof. Poulton's volume as a contribution to science is that on Mimicry in the Butterflies of North America. In such a subject the author is thoroughly at home, and has an unrivalled knowledge of the facts—facts in themselves of the greatest interest ; but whether they support the conclusions the author draws from them is another question. Even after fifty years of Darwinism the main assumptions on which the Müllerian hypothesis of mimicry rests are as far from definite proof as ever. There is no conclusive evidence that the steps by which one distasteful species came to resemble another were small indefinite variations, though Prof. Poulton considers that a comparison of the mimetic *Limenitis archippus* and the non-mimetic *L. arthemis* supplies such evidence. In the next place it is difficult to understand how "the advantage of a common advertisement" could be sufficient to bring about the amount of selection required to produce the actual similarity of coloration between two species both inedible. According to Prof. Poulton's explanation the mimetic *L. archippus* has been evolved from its non-mimetic ancestor *L. arthemis* by such selection; but *arthemis* is still very abundant in Canada and the north-eastern States, together with *Danaida plexippus*, whose recent invasion of the region is supposed to be the cause of the evolution of the mimic. If the presence of the model caused the selection of the mimetic form, how does Prof. Poulton explain the fact that the non-mimetic ancestor is still very abundant?

J. T. CUNNINGHAM.

**Super-organic Evolution.** By DR. ENRIQUE LLURIA, with Preface by DR. D. SANTIAGO RAMON Y CAJAL. Translated by RACHEL CHALLICE and D. H. LAMBERT, B.A. (Oxon.). [Pp. xx + 233.] (London: Williams & Norgate. Price 7*s*. 6*d*. net.)

BY super-organic evolution the author of this book, of which the subsidiary title is "Nature and the Social Problem," appears to mean the future evolution of human society. The goal of this evolution is the attainment of extremely vague socialistic ideals; the author's conception of the processes of evolution betrays a very imperfect acquaintance with the recent advances of biological science. In fact, his knowledge of biology seems to be derived almost exclusively from Haeckel, Herbert Spencer, and Ramon y Cajal. His conception of the course of human evolution involves the asssumption of the transmission of somatic, especially cerebral, modifications; but he adopts the theory that this transmission is effected by means of the nervous system, evidently in ignorance of the recent discoveries which tend to show that the connection between the gonads and the soma is chemical and not nervous. In support of his views he quotes a Manual of Pathology, the authors of which, Hillemand and Petrucci, state that "the heredity of acquired characteristics is a cerebro-medullar reflex action upon the germinating cells, the impressions received by the reflex centres of the grey substance of the brain being transmitted to the genital centre of the medulla and finally to the ovoblasts and spermatoblasts by the nerve fillets which, starting from this centre, are distributed in the testicles and ovaries." Regarding heredity as a mode of motion, the developmental properties of the fertilised ovum as due in some way to specific vibrations, the idea that the character of these vibrations is modified by vibrations transmitted constantly from the soma while the gametes are within the soma seems very reasonable, but at present there are no direct observations which prove an actual connection between nerve fibrils and reproductive cells.

In all that Dr. Lluria says concerning the ideas and problems of evolution, although he is sometimes original and suggestive, there are the fatal defects of indefiniteness and confusion of ideas. For example, he deliberately states that "adaptation, selection and heredity are one and the same thing, studied separately for greater clearness; selection is the same adaptation but journeying towards a fixed end," and so on. In the last sentence the obscurity seems partly due to the translators, but obviously the man who says that adaptation, selection and heredity are one and the same thing does not understand what these terms mean. Dr. Lluria writes in a vague and lofty way of adaptation as the harmony of the organism with the cosmic forces, without considering, or, indeed, being aware of the various problems connected with the origin of adaptation or its limits; his fundamental proposition is that when the institution of private property is abolished, humanity will enjoy universal health and happiness, without any danger of excessive increase of population. He makes no attempt to show how society will then be organised, but merely asserts that the existence of property and the struggle for wealth cause the conflict of classes, but that when the economic question disappears, psychic adaptation will be fully realised and man will reproduce the harmony of the natural laws.

It may be inferred from internal evidence that the translators of Dr. Lluria's book, of whom Mr. Lambert, owing to the death of Miss Challice, is chiefly responsible, were qualified for the task by a knowledge of Spanish rather than by a knowledge of biology. They employ throughout the word neurona instead of the English term neuron; they refer to the zoospore of the Ascaris instead of its spermatozoon; the name of the species of newt *Pleurodeles waltlii* is perverted into the Pleuro of the Walti. On the whole the general impression left by the book in its English version is that both author and translators have attempted a task beyond their powers.

J. T. Cunningham.

**Metallography.** By Cecil H. Desch, D.Sc., Ph.D. [Pp. xii + 429.] (London: Longmans, Green & Co., 1910. Price 9*s*.)

The valuable series, *Text-Books of Physical Chemistry*, edited by Sir William Ramsay, has been enriched by this volume on metallography. The study of metals and alloys by thermal and microscopical methods has received serious attention during the last twenty years, and has now developed into an important science. The laws and doctrines of physical chemistry form the basis on which investigators have constructed their theories and made additions to our knowledge; therefore a book devoted to the study of the structures and properties of metals and alloys is justly entitled to a place in this series.

The preparation of a text-book on metallography is not an easy matter, the subject having two sides which need equal attention; for particulars of the methods of investigation require as much explanation as the physico-chemical phenomena, and the difficulty lies in selecting the place for the introduction of the former. In this work Dr. Desch has combined the theoretical considerations with the practical details of pyrometry and micrography in quite a satisfactory manner. The theory of mixtures is excellently explained; thermal equilibrium being gone into very clearly and completely. The chapter dealing with ternary systems (Chapter IV.) will be found particularly valuable, for such systems are assuming greater importance, since with industrial alloys advances will undoubtedly be made in the direction of a combination of three or more metals, and as a rule, this branch of the subject is only briefly described in text-books on physical chemistry.

Following the four chapters on "The Diagram of Thermal Equilibrium" are others devoted to a thoroughly practical description and sane criticism of instruments and methods used in metallographic work. The chapter on "Practical Pyrometry" finishes with some necessary and sound remarks on pyrometric investigations, among which are the following: "Determinations of freezing-point curves, if undertaken at all, should be performed with the utmost available accuracy. The knowledge of alloys has now reached a point at which there is no need for more rough preliminary surveys, especially as the points which are of the greatest theoretical importance are precisely those which are most likely to be overlooked in an incomplete investigation. . . . Metallography, more than most branches of physical chemistry, has suffered in the past from the accumulation of inaccurate data, and it is well that the investigator should take every precaution to avoid adding to their number."

It is satisfactory to note that due importance is given to physical properties as means of investigation. Unfortunately the metallurgist does not always value the conclusions which can be drawn from a study of such properties; while, on the other hand, few physicists fully understand or sympathise with the purpose of metallography. In this connection Dr. Desch's remarks are worthy of quotation. He says: "In addition to its value in providing auxiliary methods of research, the study of the physical properties of alloys is of importance in quite another direction. The practical utility of alloys is dependent on their possession of certain physical characteristics. . . . It is important, therefore, to establish such a relation between the constitution of a series of alloys—most conveniently expressed in the form of the thermal equilibrium diagram—and their physical properties, that the latter may be predicted for any given alloy by an inspection of the diagram."

At the present time the metallurgist and engineer are devoting much attention to the problem of the corrosion of metals, and the chapter in which this matter is considered expresses very clearly the chief theories which are now accepted on the subject.

It is to be regretted that the chapter on the metallography of iron and steel did not receive more adequate treatment. The subject, of course, is extensive, and in a book of a general character, such as this, an exhaustive account is not needed; moreover, as the views held on the thermal equilibrium of iron and carbon are still conflicting, the time has hardly arrived for a severe criticism on any attempt to give a brief and concise explanation. There is no doubt that when our knowledge is more exact, the metallography of iron and steel will require a separate volume. The chief defect in this chapter is in the photographs; there are too few and of too low a magnification: for instance, martensite is not represented at all, while the plate showing pearlite is not good. It would have been made more valuable if illustrated with photomicrographs of the chief constituents; photographs of grey cast-iron, annealed and quenched steels, and high and low carbon steels might, with advantage, have been inserted.

The chapter on industrial alloys may disappoint some, but, strictly speaking, it is beyond the province of this book; however, what has been given is very useful and quite in order, for it indicates the nature of such alloys and on what their value depends.

Ample references are given throughout the book showing that the author has spared neither time nor trouble in making his work thoroughly useful.

There is an unimportant misprint on page 12, line 4, which also occurs in the first line of the preface.

Dr. Desch is to be congratulated on writing a concise, yet, on the whole, comprehensive book on the science of metallography.

E. O. COURTMAN.

**Report on the Mines and Mineral Resources of Natal.** By F. H. HATCH, Ph.D., M.Inst.M.E. [Pp. xii + 155.] (London: Published by Order of the Natal Government and printed by Richard Clay & Sons, Ltd., 1910.)

THIS publication is the first result of the author's recent visit to Natal, during which he was able to make an extensive collection of the rocks and minerals of the colony, which has been presented in duplicate to the Pietermaritzburg Museum and the Imperial Institute, London.

The concise account of the geology of the colony with which the book opens will be all the more welcome that less information on this subject is available in the case of Natal than in that of any other settled portion of South Africa. The western border of the colony is marked by the escarpment of the great central plain of South Africa, formed of Karroo beds, of which the volcanic rocks of the Stormberg series constitute the summit, and the Glacial Dwyka conglomerates the base. The latter rests unconformably on the Table Mountain sandstone, which Dr. Hatch correlates with the Waterberg Sandstone of the Transvaal. Underlying all these is the highly metamorphosed Swaziland System, with great granite intrusions. In many cases it forms the valley bottoms, while the heights are crowned by Waterberg or Karroo beds.

The greater part of the book is devoted to the economic products other than coal, the mining of which is already an important industry. Some of these present interesting features. Iron, for instance, occurs in ferruginous schists and the less metamorphosed "calico rocks," exactly as in the Transvaal; and the beds of iron oxide which are met with in the coal-measures of the Karroo System can be paralleled from India. Nickeliferous pyrrhotite is found in a basic rock which may be classed with the Morites, exactly as in Nyasa and at Sudbury in Ontario. One of the most curious occurrences is a bed of sandstone over three feet thick in the Karroo System, containing a considerable proportion of marcasite and molybdenite; of equal interest are the "reefs" and nodules of phosphate in the Ecca shales of the same System, containing scales of the fish Acrolepis.

The author very properly insists on the importance of a geological survey—the absence of which in a colony of such an area and importance is very difficult to understand.

John W. Evans.

# SIR WILLIAM HUGGINS, K.C.B., O.M.

By H. F. NEWALL, F.R.S.
*of Cambridge*

The close of the life of a great worker brings with it many thoughts. The one on which the mind is perhaps most inclined to dwell is that a great hoard of knowledge and experience is taken out of life; a seat of judgment is destroyed. The death of Sir William Huggins calls forth this thought with unusual force; for the greater part of the activity of a long and strenuous life was devoted by him chiefly to the development of one subject along exceedingly fruitful lines.

There are those who hold that the meeting of a man with an opportunity is the decisive element in any great advance. Others, believing that both men and opportunities are numerous, look for some peculiar bent of genius as the determining factor in the achievement of success. Upholders of either view might reasonably claim the case of Sir William Huggins as a crowning instance. And so, whilst the two aspects of his career remain in view, we are left revering his great achievements and rejoicing in the splendidly happy completeness of a long life's work.

The great opportunity came to him, after some years of tentative work, in his thirty-fifth year; from that time onwards the rôle of student and that of pioneer and leader were blended in him without a break up to the time of his death in his eighty-seventh year. He had, only last autumn, drawn together the threads of his life's work by collecting and editing his scientific papers. He had the splendid satisfaction of feeling that these records, thrown off from year to year through five decades, held their position, with hardly more than twenty editorial foot-notes, as lasting steps of progress in the subject of his choice. In hardly a single branch of it could an historical summary now be made without dwelling on some of his work as of prime importance in the development.

Carlyle says, "Veracity: it is the basis of all; and some say, means genius itself; the prime essence of all genius whatsoever." But who shall say where we are to cut into the circle? Waive the question what is essential truth, and say that genius is the heaven-sent quality of mind that leads a searcher to it? Or start with genius and define truth as the goal to which constructive genius works?

Some will have it that a possible view of the operations of thinking consists in regarding it as a process of selection constantly exercised by the thinker from a continuous supply of suggestions. The active brain appears to be restless in suggestion. The judgment of the thinker incessantly controls the direction of thought by rejecting the irrelevant suggestions of the brain and correlating the acceptable. The power of discrimination between the fertile and the barren in thought and work is a gift that is distributed here with splendid lavishness, there with sad parsimony. Contrast the bad workman's "That will *do*" with the good workman's "*That* will do," and we find ourselves face to face with this exercise of choice. This quality of judgment is perhaps supreme amongst the talents that go to make up genius; the other gifts—receptivity, suggestiveness, skill of hand and eye and brain—seem but as tools in its service. The patient judgment of a good thinker is as it were a good presiding agent over a meeting to decide some important issue in the interests of the intellectual state. If the meeting is to result in good and worthy decisions, the chairman must be ready to let views declare themselves; he must be prompt to discern the good ideas and the bad; he must guide the debate by encouraging the consideration of some views and by sternly repressing others that unduly emphasise side issues.

It was this quality of judgment which Sir William Huggins possessed in so marked a degree. It led him not only to wise choice of subjects for investigation and to just apportionment of the experimental means to the philosophical ends, but also to sane discernment of the limits to which he might fairly press his conclusions. He has often been accredited with habitual caution; and his caution was of that commendable kind that came from a just recognition not only of the power of his methods but also of their limitations. And if the quality of mind that marked him out among his fellows has to be summarised in a single word, it would probably be generally conceded that he excelled in judgment.

It is interesting to read the early records of some of the meetings at which Huggins's papers were communicated. As an instance we may take one relating to a meeting of the Royal Astronomical Society in January 1866. Huggins had given an account of his spectroscopic observations of Tempel's comet, and had recounted how the coma gave a continuous spectrum whilst the nucleus exhibited bright lines indicating that it was self-luminous. He added that probably the continuous spectrum is only that of reflected sunlight and suggested that if this were the case we may hope to see some of the solar lines in the spectra of brighter comets. This hope, we may add parenthetically, was realised by Huggins himself in the case of comets that appeared in 1881 and 1882. A question was asked by one in the audience "whether the coma might not reflect the light of the nucleus and not that of the sun." Huggins's reply in the sense that "as the character of light is not changed by reflection, if the light of the coma came originally from the nucleus the spectra of both nucleus and coma would be alike, which they were not," drew forth from the chairman the explanatory commendation: "Mr. Huggins is very cautious in guarding the spectroscope from saying more than he can warrant." At the same meeting Pritchard asked whether there were any doubt that the coma had emanated from the nucleus. This question elicited the answer that the process generally appears to be that the nucleus sends out jets of light which form successive envelopes and probably cool down sufficiently to take other forms than that of gas. Pritchard summarised the situation by remarking that in his mastery of the subject Huggins proceeded with a caution that was as rare as it was commendable. One is perhaps too ready to attach a timorous significance to the word "caution," so that we may submit that the quality by which Huggins compelled our assent was his judgment rather than his caution.

But there is no need to labour the theme. No wonder that we reverence the gift and are filled with regret when death robs us of one who possessed it in full measure. It is not only that the worker has ceased from his labour, but also that other workers have lost the help of his wisdom and can no longer appeal to his wide experience. All that is now accessible is that small part of his knowledge which is committed to print. The *results* of his technique are with us permanently, but if any one would learn the *methods*, he must tread the path again, learning by rejection and selection that which has not been and perhaps could not be communicated. How much of such knowledge goes out of life with men like Stokes and Huggins! Yet we would not have it otherwise; the very beginnings of regret are hidden in the thought of the wealth they have left behind.

Let us look for a moment over the ground wherein Huggins sought his opportunity. Here we can avail ourselves of his own words, for in the *Nineteenth Century* for June 1897 he gave a short epitome of his life's work under the title "The New Astronomy: A Personal Retrospect."

"At the time that I purchased my present house, Tulse Hill was much more than now in the country and away from the smoke of London. It was after a little hesitation that I decided to give my chief attention to observational astronomy, for I was strongly under the spell of the rapid discoveries then taking place in microscopical research in connection with physiology." [He had joined the Microscopical Society in 1852 and the whole of his life's work shows how strongly his mind was attracted to the application of micrometrical methods to the elucidation of large problems.]

"In 1856 I built a convenient observatory opening by a passage from the house and raised so as to command an uninterrupted view of the sky except on the north side. It consisted of a dome twelve feet in diameter and a transit room. There was erected in it an equatorially mounted telescope by Dollond of five inches aperture, at that time looked upon as a large rather than a small instrument. I commenced work on the usual lines, taking transits, observing and making drawings of planets. Some of Jupiter now lying before me, I venture to think, would not compare unfavourably with drawings made with the larger instruments of the present day.

"About that time Mr. Alvan Clark, the founder of the American firm famous for the construction of the great object-glasses of the Lick and the Yerkes Observatories, then a portrait painter by profession, began, as an amateur, to make object-glasses of large size for that time and of very great merit. Specimens of his earliest work came into the hands of my friend Mr. Dawes and received the high approval of that distinguished judge. In 1858 I purchased from Mr. Dawes an object-glass by Alvan Clark of eight inches diameter, which he parted with to make room for a lens of a larger diameter by a quarter of an inch which Mr. Clark had undertaken to make for him. I paid the price that it had cost Mr. Dawes—namely £200. This telescope was mounted for me equatorially and provided with a clock motion by Mr. Cooke of York.

"I soon became a little dissatisfied with the routine character of ordinary astronomical work, and in a vague way sought about in my mind for the possibility of research upon the heavens in a new direction or by new methods. It was just at this time when a vague longing after newer methods of observation for attacking many of the problems of the heavenly bodies filled my mind, that the news reached me of Kirchhoff's great discovery of the true nature and the chemical constitution of the sun from the interpretation of the Fraunhofer lines.

"This news was to me like the coming upon a spring of water in a dry and thirsty land. Here at last presented itself the very order of work for which in an indefinite way I was looking—namely, to extend his novel methods of research upon the sun to the other heavenly bodies."

If we look back to the old records, from Newton's time to Kirchhoff's, it is a strange spasmodic history that unfolds itself; and when we realise the absorbing interest that has been put into the subject of the nature of light by the developments in which Huggins played so large a part, we are struck by the indirectness, even waywardness, of the line of advance.

Newton himself was led to study the spectrum mainly by his desire to fathom the causes of the poor results obtained when lenses were used to form images. In his *Optics*, published in 1675, he dealt with the nature of light and the general features of refraction; though he developed a method of forming a pure spectrum, he never studied in close detail the peculiarities of the spectrum; at any rate he never recorded any such studies. It was not till 1802 that the dark lines in the solar spectrum were noted by Wollaston, and he apparently only utilised them as in a sense forming boundaries between the usually named colours in the spectrum. The next step was taken in 1814 by that remarkable pioneer in optical practice and theory, Joseph Fraunhofer. He too was in search of knowledge that would help him to improve the performance of telescopes. With him it was not, as it was with Newton, a question between a reflector and a single-lens object-glass. For Dollond and others had shown how by utilising the phenomena of different intensities and types of dispersion in different kinds of glass an achromatic object-glass could be made by combining properly lenses of the proper sorts of glass. It was in the search for the *best* available combinations that Fraunhofer made his careful determinations of the refractive and dispersive powers of different kinds of glass. In the process he rediscovered the dark lines of the solar spectrum; having satisfied himself that they depended on the nature of sunlight and not on diffraction, he named the more prominent lines by the letters of the alphabet A, B . . . . H, I and utilised them to define the colours used in his measures of dispersion. It is clear that he felt justified in doing this only when he had assured himself that the lines were not due to any kind of diffraction. In his experiments with sunlight the light had to be passed through a narrow opening and Fraunhofer was evidently suspicious of diffractional effects, until his observations of Venus, made without any narrow opening, proved that the lines were indubitably connected with the constitution of the light and that the light of Venus was of the same nature as that of the Sun. He went further and even examined the light of Sirius, finding that its spectrum was different from that of the Sun and that it was marked by the presence of three broad bands, one in the green [F or $H_\beta$] and two in the blue [$H_\gamma$ and $H_\delta$ ?].

He moreover recognised the fact that the yellow line seen in the spectrum of the flame of his lamp consisted in reality of two fine bright lines which in intensity and separation were similar to the dark lines D in the solar spectrum.

The memoir in which he communicated his results finished with the words: "Da der hier mit physisch-optischen Versuchen eingeschlagene Weg zu interessanten Resultaten führen zu können scheint, so wäre sehr zu Wünschen, dass ihm geübte Naturforscher Aufmerksamkeit schenken möchten."

The next important advance was recorded in 1849 by Foucalt. He had observed the spectrum of the electric arc and found it "marked, as is known, in its whole extent by a multitude of irregularly grouped luminous lines." In particular he noted the double line at the boundary of the yellow, and rediscovered the circumstance observed by Fraunhofer in 1814, that the two components coincided exactly with the D lines in the solar spectrum. His ingenious method of throwing an image of the Sun upon the arc itself allowed him to observe simultaneously the electric and solar spectra superposed, and in the process he made "some unexpected observations." Firstly it proved to him the extreme transparency of the arc; and secondly it showed him

"that this arc placed in the path of a beam of solar light absorbs the rays D, so that the line D of the solar light is considerably strengthened when the two spectra are exactly superposed. When, on the contrary, they jut out one beyond the other, the line D appears darker than usual in the solar light and stands out bright in the electric spectrum, which allows one easily to judge of their perfect coincidence. Thus the arc presents us with a medium which emits the rays D on its own account and which at the same time absorbs them when they come from another quarter. To make the experiment in a manner still more decisive, I projected on the arc the reflected image of one of the charcoal points, which like all solid bodies in ignition gives no lines (only a continuous spectrum), and under these circumstances the line D appeared to me as in the solar spectrum."

Thus Foucault had hit upon the secret of the solar spectrum in 1849. He had however to express it in terms of the ambiguous word "medium." For, in spite of Sir John Herschel's studies of the spectra of coloured flames in 1822, in spite of Fox Talbot's study in 1826 of the bright lines in the spectrum of red fire, and in spite of W. A. Miller's investigations in 1845 of the spectra of the alkaline earths, it was not then clear that the yellow D lines were due to sodium. It was not till 1856 that Swan's experiments showed that the persistency with which that pair of yellow lines appeared in all spectra was in great measure due to the extraordinary delicacy of the spectroscopic test. An excessively minute trace of sodium was enough, as he then showed, to produce the "flame reaction," as it was called.

Stokes had been one of the few who had realised the importance of Foucault's work. When Kirchhoff's work was published in 1859, it was he who had called attention to this work by publishing a translation of Foucault's paper in the *Philosophical Magazine* in 1860. Stokes added to the translation a few paragraphs showing how he himself attached a dynamical significance to Foucault's "medium," regarding it as consisting essentially of numberless systems, vibrating or capable of vibrating with definite periods, which could make themselves felt either by the emission or by the absorption of ethereal waves.

Thus, ten years after Foucault's work, the secret of the solar spectrum was again hit upon by Kirchhoff and by his masterly memoir the foundations of the subject were now laid wide and deep. His joint work with Bunsen had resulted in a double triumph: Bunsen was able to utilise it in discovering two new terrestrial elements—cæsium and rubidium; Kirchhoff succeeded in proving that in the remote Sun there were present most of the elements known to chemists on the earth.

It was in the midst of this activity that Huggins found his opportunity: in the variety of courses that appeared feasible his judgment helped him again and again to the choice of fruitful openings. From the outset he saw the need of minute accuracy in obtaining the exact positions of the lines in the spectra investigated. He saw also that it could best be obtained by direct comparisons of the spectra of metals and other elements with the spectra of the stars and sun. This wisdom constituted him a recognised authority from the first. Beginning work in this field early in 1862, he and Dr. W. Allen Miller elaborated the necessary appliances for their research, and the next five years show a record of wonderful achievement. In this period Huggins had learnt enough about the brighter stars to be assured, in spite of their remoteness, of the wonderful result that their chemical composition does not differ essentially from that of the earth and the Sun. He had also by a single glance, after much careful preparation, decided a question at which Sir John Herschel had laboured many years; and Huggins's solution was in direct conflict with Herschel's, for he concluded that nebulæ were not star clusters inordinately remote, but masses of incandescent gas. His refined apparatus was in complete readiness when good fortune gave him the opportunity of studying the "new star" in Corona Borealis. He was also able to probe into the secret of the luminosity of comets, for his continued study of terrestrial spectra enabled him to identify some of that luminosity with the presence in the comets of compounds containing carbon.

These five lines of research would seem enough to occupy the thought and energies of a single worker. And yet we know that in addition to these studies he was engaged on two others which proved to be of even deeper importance. Each of these was no less than an actual foundation stone, on which the vast constructions of the new astronomy lie. In one of them, namely the development of the method of viewing solar prominences, he was hard pressed by other workers who were engaged in the study of the phenomena of the eclipsed sun. In the other, namely the establishment of the possibility of determining, by measurements of position of the highest refinement in stellar spectra, the velocity of approach or recession of the body from which the light comes, nothing short of the inextinguishable faith of Huggins could have carried the work through to a triumphant conclusion. It has been well said of him that with an enthusiastic heart he combined a cool head.

This earlier pioneer work was carried out with an equatorially mounted telescope with an aperture of eight inches, judiciously combined with a spectroscope of two small prisms. Only those who have had experience of work even with the more powerful instruments of modern days, built up upon the experience which Huggins had to gain for himself, can appreciate fully the skill and patience and delicacy of hand and eye which enabled him to make these early conquests. Not that the value of his investigation was unrecognised at the time. On the contrary, the Royal Society put at his disposal the Oliveira bequest which had come into their hands for the construction of a large telescope. And so it came about that just at the end of the period in which the limited appliances had been so well utilised and when they began to be inadequate, Huggins was able to improve his instrumental equipment enormously. The dome was enlarged, its diameter being made 18 ft. instead of 12 ft.; and in place of the 8-in. object-glass he was provided with a 15-in. object-glass of the short focal length of 15 ft., and also a Cassegrain mirror, of aperture 18 in. and of focal length 11 ft. Each of them was mounted in a suitable tube and they were arranged so that either one or the other, according to the needs of the observer, could be attached to the equatorial mounting. The work of construction was carried out by Sir Howard Grubb in 1869–70. It will give some idea of the activity in the new observatory, if a paragraph be devoted to summarising the modes in which the instrumental equipment was utilised for various researches.

During five years, 1870–75, the achromatic telescope was used in determination of the velocity of stars in the line of sight, by means of visual observations. The spectra of Uranus and Neptune and of various nebulæ were studied and some time was given to observations of Coggia's comet. The rotation of the Sun was also investigated. In 1876, which we may recall as the year following Dr. Huggins's marriage with Miss Margaret Murray, the reflector was mounted in place of the achromatic telescope. During the next six years, 1876–82, the reflector was used in the study of planetary spectra and, after preliminary work had been carried out to gain experience in photographing spectra, the spectroscope with a single prism of Iceland spar was constructed and got into adjustment for obtaining by photography the ultra-violet spectra of the stars and nebulæ. The comets of 1881 and 1882 demanded attention; and it may be surmised that it was the unexpected appearance of these bodies which led Huggins to have a change made in the equatorial mounting, whereby it should be possible to keep both reflector and refractor permanently in adjustment and ready for use in any emergency. These telescopes had previously been interchangeable on the mounting, either of them being counterpoised by a dead weight at the end of the declination axis. But in 1883 the dead counterpoise was abolished, and a new double declination axis was made, carrying the reflector at one end and the refractor at the other, one thus serving as a counterpoise to the other, and each being movable in declination independently of the other. With the new arrangement it was easy to pass from investigation of ultra-violet spectra with the reflector to visual observations of Nova Aurigæ with the refractor. New spectroscopes were designed and constructed for each; and thus either a dense glass prism or a grating was available with the one, whilst a spectroscope with two prisms of Iceland spar was arranged for the other.

This brief summary of the development of his instrumental requirements serves to show the activity with which Huggins moved ahead in his researches. It is curiously similar to the development which marked the advance a generation later at all points, with one large difference—namely, that the earlier advance must be regarded as due mainly to the pioneering efforts of one man, whilst the later gained its impetus from the confidence inspired by those efforts in a widespread movement of collaboration.

The results and records of Huggins's life's work are contained in two volumes, bearing the title *Publications of Sir William Huggins's Observatory*. The first volume, *Atlas of Representative Stellar Spectra*, was published in 1899 and contains "the later original work of the Observatory which had not been previously published elsewhere; and in addition, it offers theoretical discussions on this newer work in connection with the earlier observations contained in the separate Papers." The second volume contains a reprint of the published papers recording the work done in the Observatory since its foundation in 1856.

No one can take up these volumes without being struck with the touches of artistic finish and thought with which they abound. They serve to recall, to those who have experienced the pleasure of a welcome to Sir William Huggins's home, the many objects of interest and old-world beauty which he and Lady Huggins collected about them with full knowledge of their history and significance. When in such visits scientific discussion reached a natural pause, some beautiful old book or old carving rescued by careful hands from destruction would become the talisman wherewith other interests were aroused. Or if the day were tempting, the garden would claim attention, and the company would watch the bees at work, illustrating the little scroll upon their hive "nil nisi labore." The return to the house would seldom be made without a reversion to the scientific theme previously discussed, and without addition of some further considerations bearing on it which showed that in the enjoyment of the garden thought had been at work.

Out of the leading memoirs in these volumes we can but touch upon a few in this short notice. The earliest work, "on the spectra of some of the fixed stars," was published in 1863 and 1864. In this work Huggins had the co-operation of Prof. W. Allen Miller. In their aim at realising trustworthy accuracy they chose a procedure that was much more difficult and laborious than those adopted by their only predecessors in such work. As remarked above, Fraunhofer, in his desire to assure himself that the dark lines in the solar spectrum were not caused by some effect of diffraction, had studied the spectra of a few stars and planets in the period 1814–23. He had arranged a large prism in front of the object-glass of his telescope, so that by properly pointing the instrument he had succeeded in viewing the spectra of the stars. With an ordinary eyepiece the spectrum would in such a case be excessively narrow, and he therefore employed a cylindrical lens to give breadth to the narrow riband of colour. In this way—the method of the objective prism of modern days—he succeeded in discerning lines and detecting dissimilarity in the spectra of a few of the brighter stars. Forty years later Donati, doubtless incited by Kirchhoff's work, had utilised a large single lens with an aperture of sixteen inches, belonging to the Florence Museum, to gather the light of the brighter stars for examination with a single prism. Donati passed the light collected by his lens through a second lens of short focus and then through a single prism, and viewed the spectrum with a short telescope. He used a diaphragm with wide opening for the purpose of setting each star image in the axis of the collimating lens but did not employ any slit. With this apparatus Donati studied the spectra of about fifteen bright stars and published his observations in 1862. He found differences, some of which may be recognised now as really attributable to differences in the nature of the stars, whilst others are no less clearly due to imperfections in his method.

In both Fraunhofer's and Donati's methods there lies a great advantage, and also a great disadvantage. The advantage is that all the light collected by the object-glass is utilised and relatively brilliant spectra result. The disadvantage is that they were unable to get trustworthy fiducial marks, so that comparisons of measurements were in general nugatory.

Huggins and Miller adopted the plan of attaching to the equatorial a spectroscope equipped with a slit, in such a manner that the image of the star could be brought upon the slit and that the light passed down the tube of the collimator and so through the prisms and the viewing telescope. They also arranged small reflecting prisms over parts of the slit, so that the light from a terrestrial source, such as a coloured flame or an electric spark, could be passed from a lateral direction into the slit. In this manner they were enabled to see the spectrum of a star and the spectrum of the terrestrial source simultaneously. It was delicate, laborious work, rendered all the more difficult by the tremulous motion of the star upon the narrow slit (not more than $\frac{1}{300}$th or $\frac{1}{400}$th of an inch wide). But with unremitting patience and in spite of constant interruption of clouds and bad weather, they succeeded in making measurements which conclusively proved that the stars contain materials similar to those found on the earth and in the Sun.

Such conclusions depended on their employment of the fixed spectra of terrestrial sources as intermediary standards of comparison. The stars and the sun could be directly and independently compared with the standards, and thus the similarity or dissimilarity of their spectra could be certainly established. In the preface to the second volume Sir William Huggins says:

"Looking back with the knowledge of the more efficient and perfectly adapted instruments and methods of work which have been gradually introduced during the last forty years, no one can be more conscious than I am of the inevitable shortcomings of my pioneer instruments and methods of work, which had to be created under circumstances of no little difficulty. These shortcomings prevented the attainment of accurate results in some single cases, but time has shown that they did not affect the fundamental general correctness of my early work."

Whilst Huggins and Miller were carrying on their work of detailed examination of a few typical spectra, Secchi devoted himself to passing in review a vast number of stars, searching for information as to the general types of their spectra. He adopted the observational method of Fraunhofer and classified first of all 600 spectra, and later on 4,000 spectra of different stars. He found that for the purposes of a general classification the spectra of all the stars examined might be referred to one or other of four different types. The later work of Pickering and of Vogel and others serves to show that there are stars which exhibit spectra intermediate in character between Secchi's types. But such intermediate spectra are relatively few in number. It would accordingly appear that here are evidences of a process of evolution in which the typical spectra of Secchi indicate stages of stability of some kind. The relatively scarce intermediate spectra are those in which, if anywhere, we may least unreasonably expect to be able to detect change; but it is hardly necessary to add that the time-intervals in such a process of evolution are far too long for us to have discovered any sign of actual change in the few years in which observations have been made.

Huggins's detailed work and his series of photographs of stellar spectra, which form the splendid feature of his *Atlas of Representative Spectra*, have contributed enormously to the advance of the subject. This was the work that he devoted himself to from the year 1875, when he succeeded with splendid results in photographing stellar spectra. One of the firstfruits of his labours was the detection of the series of rhythmically related lines in the spectra of Sirius and other blue stars. The resemblance of the lines of this series to the other lines previously recognised as hydrogen lines in the more refrangible part of the spectrum led him to surmise that they were produced by hydrogen.

It was in 1885 that Balmer discovered that the wave-lengths 6562·1, 4860·7, 4340·1 and 4101·2 of the four hydrogen lines bore to the wave-length 3645·6 the ratios denoted by the fractions $\frac{9}{5}, \frac{4}{3}, \frac{25}{21}$ and $\frac{9}{8}$, or $\frac{9}{5}, \frac{16}{12}, \frac{25}{21}, \frac{36}{32}$, or $\frac{3^2}{3^2-4}, \frac{4^2}{4^2-4}, \frac{5^2}{5^2-4}, \frac{6^2}{6^2-4}$, so that the wave-lengths of these four lines may be deduced from the expression

$$\lambda = 3645 \cdot 6 \times \frac{m^2}{m^2-4}$$

if the whole numbers 3, 4, 5 and 6 are successively substituted for $m$. Balmer learnt that five years previously Huggins had discovered a series of lines in stellar spectra at wave-lengths 3887·5, 3834 and 3796. These lines were found to correspond with the wave-lengths deduced from this same series, when $m$ has the values 8, 9, 10. And thus the work of the terrestrial physicist is supplemented by that of the astrophysicist. Huggins later detected the members of the hydrogen series up to the value $m = 32$; and it may be surmised that this work gave the impulse to the search for the isolation of rhythmical series in the spectra of other elements, a search in which various spectroscopists engaged and which culminated in success in the work of Kayser and Runge, and of Rydberg.

In referring to this application of photography to the recording of stellar spectra far into the ultra-violet region, we are anticipating other remarkable work and we would now revert to Huggins's solution of the mystery of nebulæ which was achieved in 1864.

Probably few of Herschel's discoveries struck the imagination more than that series of observations in which by the use of increasingly powerful telescopes he was able to "resolve" into groups of separate stars or clusters many of those celestial objects which to the naked eye or in a small telescope appear as faint nebulæ. He passed no less than 2500 nebulæ and clusters in review, and was thus led to believe that all nebulæ might be found to consist of stars if instruments powerful enough could be made. Lord Rosse showed later that at the same time that the number of clusters may be increased by the resolution of supposed nebulæ, other nebulous objects are revealed; and thus it appears that however large the telescopes were made, the decision would only be pushed a few steps farther off. In 1864 Huggins made that cardinal discovery, which closed the battle between monster telescopes and disclosed an essential difference between nebulæ and stars. The riddle was solved by the use of a relatively small telescope, which yet collected enough light to enable the observer to judge of the *nature* of the light when analysed by the spectroscope. A glance was enough to show that the spectrum was entirely unlike that of a star. We can well understand Huggins's first shock of incredulity. "At first I suspected some derangement had taken place, for no spectrum was seen, but only a short line of light perpendicular to the direction of dispersion." In all the three previous years, during which he had passed star after star in spectroscopic review, he had never found a case in which the spectrum did not present the whole array of colours, broken only here and there by narrow dark intervals or lines. Here in the small planetary nebula in Draco was an entirely new case. The light was nearly monochromatic. A few more moments only were needed to assure him that the spectrum was that of a luminous gas, exhibiting but very few lines and those bright. The nature of this gas had to be found, and the work of observation had to be extended. In the course of the next four years Huggins had examined the spectra of about seventy nebulæ. About two-thirds of them gave a continuous spectrum, the rest gave a spectrum of bright lines. Recent spectroscopic observations suggest that nebulæ which are of spiral type have a continuous spectrum, and that those nebulæ which either have an irregular form, or are ring-shaped or disc-shaped like a planet, exhibit spectra of bright lines. Huggins's investigation of the nebula in Andromeda has contributed in no small degree to the care with which this large inquiry has been carried on.

In his search for the origin of the bright lines in the nebular spectrum, Huggins identified two of the lines with hydrogen and originally ascribed the brightest green line to nitrogen; but later work, to which he himself contributed in open-minded search, shows that the single gas which by its luminosity proclaims nebulæ in widely separated regions of the vast universe cannot yet be identified with any known element.

In his study of the spectrum of Nova Coronæ Borealis which blazed forth in 1866, Huggins was able to discover the peculiar feature which has been found to be characteristic of all the new stars that have since appeared. They are marked in the early stages of their appearance by a spectrum in which the characteristic feature is a number of bright lines each accompanied by a dark line on the side of shorter wave-length. The lines are in reality too broad to be called lines, and so free from structure that one hesitates to call them bands.

In the later stages of decline of a Nova, the spectrum changes and gradually assumes the characteristics of the spectrum of a planetary nebula. When the Nova appeared in the constellation of Auriga in 1892, Sir William Huggins was able to observe it with new apparatus of increased power and refinement; his observations contributed in no small degree to our knowledge of the peculiarities of the spectra of new stars. When the star had faded away to 14th magnitude and more, he had taken the opportunity to have certain necessary alterations made to the eye end of his telescope and so was debarred from taking part in the earlier observations of the second stage of brightness, which occurred in August 1892.

But when it was discovered by Campbell and others that the spectrum had changed and had taken on the characteristics of the spectrum of a planetary nebula, Sir William Huggins was able to sound a note of caution. For in February 1893, by visual observations of this star of 9th magnitude made with a grating spectroscope, he was able to detect structure in the lines which approximately coincided with the nebular lines. This structure extended over a range of wave-lengths more than a hundred times as great as the width of the ordinary nebular lines; and though time showed that the spectrum eventually reached a stage which justified Campbell's surmise, there were few astrophysicists who did not marvel at the delicacy and acuteness of observing power by which Sir William and Lady Huggins had succeeded in detecting visually features which even the photograph had difficulty in portraying.

In 1868 Huggins communicated to the Royal Society his memorable paper on the measurement of motions of recession and approach of the stars. Basing his method of attack on the correctness of Doppler's principle, on the theoretical aspect of which he received assistance from Clerk Maxwell, he sought for signs of failure in exact coincidence between the absorption lines in stellar spectra and the corresponding bright lines in terrestrial spectra. By the principle, there must necessarily be a change in the frequency of waves of light as perceived by an observer on the earth if they emanate from a star which is in motion, of approach or recession relatively to the earth. Such a change would declare itself in the spectrum of a star by a minute shift of the absorption lines towards the blue or the red, according as the star approaches the earth or recedes from it. After several years' work Huggins was able to assure himself of the existence of such minute shifts and succeeded in measuring them in the case of several stars. The practical difficulties of applying the method were very great; after Huggins and the observers at Greenwich and Potsdam and Rugby had done all that was possible in the refinement of visual observations, the work reached its limit for that time. Twenty years after the publication of Huggins's memoir, and closely following his success in photographing stellar spectra, Vogel made a great advance in this department by introducing photographic methods and after three years' work was able to publish the results of his labour in the shape of the velocities of fifty-one of the brightest stars visible at Potsdam. Since that time the work has been carried on by many observers in various countries and it is not too much to say that many of the most remarkable advances recently made in astronomical knowledge are the results of the development of Huggins's method.

There are many other points that should be referred to in a notice of Sir William Huggins's work. His investigations on the spectra of the Wolf Rayet stars, his success in photographing the ultra-violet regions of the spectrum of the great nebula in Orion, his studies of the spectroscopic phenomena of comets, his development of the method of seeing prominences at the limb of the uneclipsed sun, his analysis of the luminosity produced by radium and its connection with the surrounding nitrogen, are instances of an activity that was always at work in the forefront of scientific advance.

Many were the marks of distinction that were offered to Sir William Huggins in recognition of his scientific achievements

The Royal Society made him one of their number in 1865, and afterwards bestowed on him three of the medals which it is their privilege to award—a Royal medal in 1866, the Rumford in 1880 and the Copley in 1898. He received the gold medal of the Royal Astronomical Society in 1867 jointly with Professor Miller. The Paris Academy of Sciences awarded him the Lalande prize in 1872 and elected him to their ranks as a corresponding member in 1874. Another honour came to him from France in 1888, when he received the Prix Janssen of the Institute. The University of Cambridge gave him the honorary degree of LL.D. in 1870, and a few months later he received the degree of D.C.L. from the University of Oxford. He also held honorary degrees from various other universities both at home and abroad; many foreign learned societies enrolled him as an honorary member.

He rendered valued service to many societies. From 1876 to 1878 he was President of the Royal Astronomical Society; he was for seven years one of the secretaries and for thirty years the Foreign Secretary of that Society. In 1891 he served as President of the British Association at its Cardiff meeting. After being three times vice-president, he was chosen in 1900 to succeed Lord Lister as President of the Royal Society and filled the office for five years with marked dignity and distinction. In his seventy-fourth year, on the occasion of the Diamond Jubilee of Queen Victoria, he was made a Knight Commander of the Bath, and four years later (1901) he was one of the group of twelve who first received the Order of Merit established in that year.

Sir William Huggins married in 1875 Miss Margaret Lindsay Murray, the daughter of Mr. John Murray of Dublin, a lady in whom he found an able and enthusiastic collaborator and assistant. Her name appears as joint author with his own on many of the scientific papers published by him since that date. Sir William died on May 12, 1910, in his eighty-seventh year. An admirable portrait of him in his eighty-first year was painted by the Hon. John Collier; it hangs in the rooms of the Royal Society. Through the kindness of Lady Huggins a reproduction of it is here given. It also appears in photogravure, together with a portrait of Lady Huggins, in the opening pages of the volume of his collected scientific papers.

# A BRIEF REVIEW OF BACTERIOLOGICAL RESEARCH IN PHYTOPATHOLOGY

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The subject of plant-disease in general has attracted attention from very early times, owing to the disastrous effects consequent upon the failure of cereals and other important crops. Thus references to Rust of Wheat are found in very ancient writings; allusions to the "*Aspera robigo*" are known in several classical authors, including Horace and Ovid, the latter introducing a prayer for preservation against this murrain in connection with the festival of the Robigalia instituted in honour of Robigo, the goddess of rust. The loss occasioned by various diseases of a parasitic origin has for centuries been truly appalling, yet only in comparatively recent times has any systematic study of such epidemics been possible. It was not until de Bary, in 1865, had observed the actual entrance of the germ-tube of a parasitic fungus into the host and its growth in the tissues that any correct appreciation of the pathogenic nature of the attacks could be made; these invaluable researches established a new epoch in the study of plant pathology.

Enormous advances have since been made in the ætiology of plant-diseases and for those of fungoid origin the importance of the results of physiological and pathological researches have met with universal recognition. It is, however, somewhat strange that the study of bacteria in relation to plant pathology occupies only a small place in the estimation of English botanists. Of late years there has been much activity in America in this field of research. France, Italy, Holland and Germany have also yielded contributions of note to the mass of literature which is rapidly accumulating, and every year adds to the number of important papers bearing on this subject. In spite of this fact the ordinary English text-books continue to give very inadequate descriptions of the diseases of plants which are due to bacteria, so that some brief historical review of the work which has been accomplished in this direction may not be without interest.

The attitude of suspicion and neglect towards disease in plants attributed to bacteria was very general up to about the year 1896 and is the more surprising when we consider the enormous interest and enthusiasm created by the researches and discoveries of Pasteur, Koch and other workers in the domain of animal pathology. No doubt the human interest involved in the latter case accounted for much and the great names associated with such investigations overshadowed to some extent the important labours of the earlier botanists. But it is necessary to remember that valuable work was done in the study of bacteria by Cohn as early as 1850, and that it was the persevering and brilliant researches into the life-histories of parasitic fungi, by botanists such as the Tulasnes, Cohn, Brefeld and de Bary which laid the foundation of our knowledge of both animal and vegetable pathology.

Cohn was one of the earliest workers in bacteriology and we must certainly acclaim him the real founder of this branch of mycology and acknowledge that he has never been accorded his true place in the history of the development cf pathological research. It was Cohn who first advanced the theory of the plant nature of bacteria which formerly were known as Vibriones and from the year 1868 onwards he continued vigorously to prosecute his researches upon these micro-organisms. His work was of supreme importance and influenced in great measure the methods of scientific study of medicine, which at that time began to look for the causes of disease through a microscopic investigation of bacteria. He was for long the source of inspiration to many of the younger men who were eagerly treading the new path in biological research then opening before them. Koch owed much to his association with Cohn; all his training was gained under him in the botanical laboratory at Breslau and, as I have been reminded by Sir William Thistleton-Dyer, they are Cohn's drawings which illustrate the publication by Koch of the first proof of any disease of the higher animals due to a specific bacterium. This account of his researches on the life-history of the *Bacillus anthracis* appeared in the *Beiträge zur Biologie der Pflanzen* in the year 1876.

In the published work of Mitscherlich in 1850 which announced to the Academy of Berlin his discovery of the fermentation of cellulose, we find one of the earliest stages in our knowledge of the part played by bacteria pathogenic to plants, which later researches have so completely established. He demonstrated the complete breaking down of the cellulose wall by experiments upon the cells of the potato, and, as his material contained no trace of any fungus, he suggested that the "vibriones" which were present in great abundance must be the agents responsible for the phenomenon. In 1865 Trecul, in the course of his researches upon laticiferous vessels, observed the appearance of minute bodies in the tissues under examination which seemed to him to arise quite suddenly and spontaneously in the laticiferous vessels and closed cells. These bodies, which he termed Amylobacter, furnished him with an argument in favour of spontaneous generation, only disposed of when, later, van Tieghem showed them to be stages in the development of a bacillus, named by him *B. amylobacter*, identical with the "vibriones" which Mitscherlich had rightly supposed to be the active agents in the dissolution of cellulose. Later, in the year 1879, van Tieghem carried out some investigations important for that day upon the action of bacteria as agents in the destruction of cellulose, and he clearly showed that certain bacteria possess the remarkable property of dissolving cellulose, but he was undoubtedly working with mixed cultures and was mistaken in attributing his results specifically to *B. amylobacter*.

In 1878 Burril traced a disease known as Pear Blight or Fire Blight, which produced a blackening of the parts affected and a gummy exudation, to the attack of a micro-organism, *Micrococcus amylovorus*. He found no sign whatever of fungoid growth in the diseased tissues until after the death of the cells and he succeeded in communicating the disease by a series of inoculations by direct infection from the diseased to healthy tissues.

This paper was followed in 1879 by Prillieux's description of the Pink Discoloration of Wheat due to a *Micrococcus*; these two represent the very first accounts of any disease of plants definitely attributed to bacteria.

Prillieux made a very close observation of the microscopic features of the disease of the wheat, which was invariably associated with the presence of *Micrococcus tritici*. He noted their destructive action upon the elements constituting the grain, the corrosion first of the starch-grains, then the proteids, and also the dissolution of the cellulose but he made no cultures or attempts at inoculation.

Wakker's extended investigations (1883–89) into the nature of the Yellow-stripe of hyacinths led the way in the study of an interesting type of bacterial parasitism, involving a destruction of the tissues which advances along the vascular bundles. A feature of this disease is a blocking of the xylem vessels by a yellow, gummy substance, followed by the dissolution of the cellulose walls. Frequent inoculations always produced a recurrence of the same symptomatic characters in healthy plants; but these were the result of direct infection experiments without the intervention of culture media.

A knowledge of good cultural conditions was not wanting even at this early date. Klebs' and Brefeld's gelatine methods of preparing culture media were already in operation; the introduction, in 1881, of Koch's methods of isolation by means of plate cultures simplified the preparation of pure cultures and afforded further facilities for bacteriological research. Koch's dicta, in 1883, established the recognised procedure necessary for the definite determination of a disease due to a specific organism, viz.: (1) it is essential that the organism be present in the diseased tissues; (2) that it be grown artificially in suitable media for several successive generations; (3) that inoculations from the pure cultures so obtained should produce the same manifestations of the disease in healthy tissues; and (4) the same organism must be again isolated from the artificially infected tissues.

The knowledge which was available was, however, not generally applied and unfortunately much of this early work rested upon evidence which could not be regarded as conclusive, owing to imperfect methods of experiment and the absence of proper precautions to ensure pure cultures. The unsatisfactory character of some of the work in this sphere no doubt contributed to maintain a disbelief in the part played by bacteria pathogenic to plants. But when many careful investigations of a later date and an exact study of the life-history left nothing to be desired, great reluctance was still shown to admit the truth of these conclusions. Much doubt and opposition continued to be expressed towards the whole principle of bacterial plant-diseases, by reason of established preconceptions which were founded upon a misapprehension of the true nature of the problem.

Hartig considered the plant-organism protected from bacterial intrusion, owing to its peculiar structure and the absence of circulatory channels which would serve for the distribution of micro-organisms, and that serious obstacles to their passage were presented by the impervious character of the non-nitrogenous cell-walls. Further, that the acid reaction of the cell-sap operates unfavourably for their growth. This latter view was also shared by de Bary. The reasons advanced by Hartig are merely theoretical and when submitted to actual experiment have been shown to break down. The citation of recent work upon the secretion of a cytolytic enzyme by bacteria and their penetration through the softened cell-wall, that showing the entrance of the bacteria through the water-pores and their power of living and travelling in the xylem vessels, is sufficient to indicate how completely his conception was at fault. Though the influence of the cell-sap has an important bearing upon the tendency to disease, it is now well known that the nature of the cell-sap offers no absolute resistance to the active growth of bacteria. It has been proved that the reaction of the parenchymatous tissues is by no means always acid, and moreover certain bacteria have been found to flourish best in distinctly acid media; while others possess the property of producing alkaline secretions which assist their penetration into the cells.

In 1882 Hartig summed up his convictions in the statement that there was no such thing as diseases of plants due to bacteria. In 1884 de Bary asserts that they have scarcely ever been observed; again, in 1885, in the *Lectures on Bacteria*, he assumes that present knowledge justifies him in regarding "parasitic bacteria as of but little importance as the contagia of plant-diseases." The whole subject is dismissed in some two pages with the mention of Wakker's Hyacinth, Burril's Pear and Apple Blight, Prillieux's Wheat Disease and Wehmer's Wet Rot of Potatoes; and while admitting that saprophytic bacteria may, under special conditions, attack the tissues of living plants as facultative parasites, he concludes by a repetition of the statement that bacteria are not objects of great importance in diseases affecting plants.

In spite of the weight of such authority a mass of literature gradually accumulated in favour of bacterial parasitism.

Pear Blight.—Burril's results were subsequently confirmed by the more definite cultural experiments of Arthur (1885) and ten years later (1895) Waite fully substantiated the origin of the disease by isolation of the bacterium (*Bacillus amylovorus*) and successful infection with pure cultures.

Yellow Disease of Hyacinth.—Abundant proof was forthcoming in support of Wakker's conclusions with regard to *Pseudomonas hyacinthi*; the parasitic nature of this organism was fully confirmed by Smith in 1896.

The Canker of the Olive—*Bacillus oleæ*—which produces galls or irregular tubercular swellings on the branches of these trees, was investigated by Archangeli and Savastano in 1886 and definite proof given by the latter in 1889 and by Cavara and Prillieux in 1890.

The Corn Blight—*Bacillus zea*—was described by Burril in 1889. The plants are stunted and yellow in colour, the disease first appearing on the lower leaves and brown spots on the surface of the roots.

The Soft Rot of Hyacinth—*Bacillus hyacinthi-septicus*, by Heinz—appeared also in 1889. The flowers either fail altogether in the bud or they open in very irregular order and soon tumble off. At the same time the axis of the inflorescence falls over and rots and the bulb degenerates into an evil-smelling slime.

The Potato Wet Rot—*Bacillus solaniperda*—was published by Kramer in 1890–91. He showed that the bacteria break down first the soluble carbohydrates, then destroy the intercellular substance and attack the cell-wall; the starch is not attacked but at a later stage the proteids also are destroyed.

The Bacteriosis of the Vine—*Bacillus uveæ*—was the work of Macchiati in 1894. After the flowering the young fruits and peduncles turn brown, shrivel up and become a completely dry, brittle mass.

The Cucurbit Wilt—*Bacillus tracheiphilus*. In this investigation Smith describes a peritrichiate bacillus, more particularly occupying the spiral vessels and later the tracheids. The most marked symptom is the wilting of the leaves; afterwards the bacilli pass through the conducting channels to the stem and eventually all the internal tissues become more or less destroyed.

The Brown Rot of Cruciferæ—*Pseudomonas campestris*—was established by Pammel in 1895 and fully confirmed by Smith in 1896. The organism is pathogenic for various Cruciferous plants, which are affected through the vascular system. The brown rot extends along the medullary rays, thus creating in the root a characteristically radial structure, with a central hollow in which alternating portions of the woody strands of the vascular cylinder alone persist. Externally the root may appear quite sound, the disease producing a kind of dry rot internally.

The Potato and Tomato Disease—*Bacillus solanacearum*—was investigated by Smith in 1896. This is early recognised by a shrinking of the stem and wilting of the foliage, due to the penetration of the bacilli through the vessels of the xylem, which become filled with an extraordinary number of these organisms. This bacillus is at first confined to the vascular system but ultimately the parenchyma becomes invaded and broken down. The organism makes its way through the stem into the tuber, where again the vascular cylinder is first traversed and only in very advanced stages does the starch-bearing parenchyma become affected.

Each one of the diseases enumerated above had been conclusively established, the evidence resting upon much carefully conducted work, based upon Koch's four premises, in which the organism had been studied in pure culture; repeated inoculations from pure cultures produced always the characteristic pathogenic symptoms and the reappearance in the tissues of the plant of the same specific organism.

In 1896 E. F. Smith published, in the *American Naturalist*, "A Critical Review of the present state of our knowledge upon the Bacterial Diseases of Plants." He drew attention to the need for full descriptions of the various forms, including a study of both morphological and biological peculiarities, at the same time emphasising the importance of the strictest cultural technicalities and rigid tests of pathogenesis, which have too often been disregarded. His review of thoroughly investigated examples up to that date leaves no doubt that certain well-marked plant-diseases owe their origin solely to a specific Bacterium.

Migula, in his *System der Bakterien* (May, 1897) still considered that the impenetrable cell-wall of plants presents great difficulties to the entrance of bacteria and that stomatal infection is generally impossible, yet he allowed that these objections were not of universal application. He admitted that a number of bacterial diseases had been established and devotes considerable space to the discussion of many well-known cases. Migula's attitude upon this question is in great contrast to that of Dr. Alfred Fischer at the same date. In the *Vorlesungen über Bakterien* (July, 1897) Fischer, in spite of the evidence available, expressed complete disbelief in the existence of bacterial diseases of plants. With the exception of the root-nodules of Leguminosæ, he professed to know of no single instance where bacteria invaded the closed, living cells of plants; he states that the uninjured plant is "quite impregnable to their attacks." He maintained that bacteria live metatrophically only in diseased plant-tissues "that have already been disintegrated and decayed by parasitic fungi." That the bacteria may "assist these subsequently in their work of destruction and modify perhaps more or less the character of the disease, but except for injuries from frost or insects the first attack on the plant is always made by fungi. All the cases of so-called bacteriosis of plants from the 'gommose bacillaire' of the vine down to the 'schorf' of the potato, are primarily diseases of non-bacterial origin in which the bacteria are present merely as accidental invaders." He even goes so far as to state that "infected wounds are dangers that have no existence for plants," owing to the development of wound-cork, which would cut off the provision of moisture and supplies of nutriment to the exclusion of the further progress of any pathogenic bacteria. As will be seen later, the rapid destruction of the cells, due to the activity of a bacterial parasite, as a rule precludes this protective tissue being formed; and the idea that fungi are always responsible for the primary attack is not in accordance with the cases described in which no trace of a fungal hypha was present. It is not possible here to enter in detail into a discussion of the points at issue; but Fischer's whole conception of the case showed such ludicrous ignoring of demonstrated facts in bacteriological research and such retrograde notions of the general physiological aspects of microbial infection, that some refutation was necessary.

E. F. Smith, to whose investigations in this branch of plant pathology we owe so much, took up the challenge and had no difficulty in showing the completely erroneous nature of Fischer's statements and "unwarranted assumptions." Smith has proved that in the case of the Black Rot of the Cabbage fully ninety per cent. of the infections take place through the water-pores, which provide in the epithem tissue all the elements in solution necessary for the growth of bacteria. This ready entrance through the water-pores has also been confirmed by H. L. Russell. As shown by Gardiner the water-glands are continuous with the termination of a fibro-vascular bundle, which thus furnishes a readily accessible channel for the progress of the attack. Kramer found that *B. solaniperda* works its way into the tuber through the lenticels. Stomatal infection has also been observed by Smith in a disease of Japanese plums, caused by *Pseudomonas pruni*. Waite proved by his experiments on Pear Blight that a large proportion of the infections take place naturally by means of the floral nectaries. The stigma is another part of the plant which presents an unprotected mode of access; and Kissling's work on the biology of *Botrytis cinerea* supplies an instance of very facile infection of the gentian through the anthers and stigmatic surfaces.

It is necessary to allude to Fischer's theories and misstatements, as in the English translation of his Lectures, issued by the Clarendon Press in 1900, the same errors are reiterated. This is all the more striking as this translation was published under the author's sanction and enjoyed the advantage of a proof-revision by Marshall Ward. Ward in general held the view that bacteria in association with plant-diseases were but a secondary accompaniment of the malady; and in his treatise upon *Disease in Plants* this author makes no allusion to the destruction of cellulose by bacteria, which plays such an important rôle in the penetration of the cells and the rapid disintegration of living vegetable tissues.

Some of the earlier observations demonstrating the existence of bacteria which exercise a fermentative action upon the cell-wall have already been mentioned. In 1890 van Senus attributed the fermentation of cellulose to a cellulose-dissolving enzyme produced by the symbiotic action of two bacteria, one aerobic and the other anaerobic. Later (1895) Omeliansky isolated, from the mud of the Neva, an anaerobic bacillus which entirely dissolved filter paper with the greatest rapidity. These investigations, however, dealt with organisms acting saprophytically. The question of the destruction of the cell-wall of living plants by the action of parasitic bacteria was first definitely established by the researches, published simultaneously, of Laurent and myself (1899).

Laurent, in his investigations upon the potato and the causes of its greater or less resistance to bacterial disease, established the existence of a cytase which dissolved the middle lamella, rapidly softened the cell-tissues and caused the disaggregation of the cells. The organism which was the chief subject of Laurent's researches, *B. coli communis*, is very rarely capable of living as a parasite upon potato tubers and other plants. It was necessary for the tubers to be deprived of resistance, by means of exceptional cultures, to enable the bacillus to develop upon the potato. From that point its virulence was increased by successive cultivations upon tubers of slight resistance, until varieties at first highly resistant ended by becoming invaded by the parasite. The virulence disappeared as soon as the microbe ceased to be cultivated on a living tuber, cultures in nutritive solutions served to suppress the aptitude of the parasite and henceforward it could only be restored after special preparation in alkaline solutions. In this he demonstrated a complete parallel with Kissling's researches on *Botrytis*.

The Bacterium causing the White Rot of turnips, which was the subject of my special research, belongs to the genus *Pseudomonas* and illustrates a very virulent form of parasite. It was isolated from turnips attacked in the fields and, unlike *B. coli communis*, flourished on nutritive media, and even after many cultivations could readily be inoculated from these on to pieces of living turnip, producing all the effects of the white rot in about twelve hours. This organism was grown in pure culture from a single bacterium and, both when living in a nutrient solution and on a living turnip, was found to secrete an enzyme which has the power of dissolving the middle lamella and of causing the softening and swelling of the cell-wall. I also demonstrated the production of oxalic acid by the bacterium and that this acid acts as a toxin in plasmolysing and killing the protoplasm. This proof of the secretion of a cellulose-dissolving enzyme introduced a new factor and finally disposed of the "impassable barrier" supposed to be offered by the cellulose membrane to the entrance of bacteria. As the result of further researches I was able to trace, by continuous observation, the actual penetration of the bacterium through the cell-wall. The observation of the movements of the bacteria, though difficult and very trying, was yet considerably furthered by the difference of refractive index between the cell-wall and the bacteria, which enabled the course of the latter to be distinctly followed.

It is not until the protoplasm has been killed by the toxin and the cell-wall very much softened by the cytase that the bacteria have the power of perforating the walls and passing into the cell-cavity. It would hardly be supposed that a single bacterium, through its own exertions, could soften the cell-wall and pierce it at one definite point after the manner of a fungus germ-tube. The extreme minuteness of the bacteria and the rapidity of their multiplication lead them to act, as it were, in concert and the wall becomes softened by the cumulative action of many bacteria before the penetration of a single individual.

The old and fully developed cuticle is apparently proof against the action of the enzymes excreted by *P. destructans*; but this parasite can readily effect an entrance into its host through the undeveloped epidermis of young and tender structures. It is incapable of manipulating the hard and tough rind of the sound turnip; but when brought into contact with a wounded surface it at once flourishes as a saprophyte upon the remains of the injured cells; very soon the number of bacteria becomes largely increased and the toxin and cytase have sufficiently accumulated to kill the first cell. With the death of its protoplasm the cell-contents are liberated and an additional supply of nutriment is thus provided; the bacteria continue to multiply, cytase and toxin continue to be set free, and thus each cell succumbs in turn.

A comparison of the parasitism of *Botrytis cinerea*, as demonstrated by the investigations of Nordhausen, presents an exact parallel. He has shown that the spore of this fungus excretes a powerful toxin in its initial stages of germination, before any trace of the germ-tube can be detected. Its manner of effecting an entrance into a host-plant is first to kill the cell by the emission of the toxin; the germ-tube then penetrates the dead cell and is nourished saprophytically upon it; with the vigour thus gained it destroys the neighbouring cells and passes from one to another without difficulty. The fungus hypha has the power of perforating the cuticle but only in young and tender structures; old and hardened membranes could only be entered when the cuticle had been injured, or when it had gained strength by special saprophytic nutrition.

Thus the bacilli play here a part absolutely comparable to that of the fungi and a complete homology is established between them.

This form of parasitism is apparently typical of a large class of bacterial diseases, in which there is a rapid degeneration of the cell-wall and complete destruction of the parenchymatous tissues. Van Hall's researches in 1902 demonstrated the action of a toxin secreted by *Bacillus omnivorus* when attacking *Iris florentina*; and three years later (1905) Jones isolated a cytase in the case of the soft rot of the carrot and other allied plants, due to *B. carotovorus*. In 1906 Harrison also described the action of a cytase in his investigation of a disease of cauliflowers, caused by *B. oleraceæ*, which exhibited symptoms identical with those produced by *P. destructans*. Jones has since shown that all these three organisms must be considered as representative of one species. This author has recently published a very interesting comparative study of the group of organisms producing the bacterial white soft rots (1909) and he shows that *Bacillus carotovorus*, as well as certain other soft-rot organisms, excrete a cytolytic enzyme which he determines to be a pectinase. In all probability a cytase is also concerned in certain of the potato rots, though in these last instances it has not been isolated.

A species of *Pseudomonas* producing a brown rot of the turnip, which I have had under investigation, belongs to a group working in a totally different manner; the action is very much slower and the rapid swelling of the cell-wall is not a conspicuous feature. The tumerous diseases, such as the canker-knots of the olive and the Aleppo pine, caused by *Bacillus oleæ* and *B. pini* respectively, also exhibit a comparatively slow development.

Another type of bacterial disease is that in which the xylem vessels are primarily attacked and become filled with numerous bacteria. As a consequence the transpiration current is unable to flow along these channels, the supply of water is cut off and hence a withering of the shoot occurs. In the bacterial disease of sweet-corn a variety of *Zea Mais*, described by F. C. Stewart, the organism is confined exclusively to the fibro-vascular bundles and never pervades the cells of the parenchyma. In many other cases there is subsequent invasion of the parenchymatous tissues and total destruction of the cell and cell-contents ensues. Examples of this type are found, as we have seen, in the wilting of various Cucurbitaceæ traced to *B. tracheiphilus*; in the bacterial disease of the tomato, egg-plant and Irish potato; the Yellow Rot of hyacinths; the Bacteriosis of *Dactylis glomerata*, etc. In this category must also be included the Brown or Black Rot so prevalent in species of the genus *Brassica* and other Cruciferæ. A striking symptom of this disease is the blocking of the lumen of the wood-vessels and also the neighbouring intercellular spaces with a kind of gum or mucilage. This gum is scarcely soluble. It stains red with phloroglucin and reacts to thallin sulphate but remains uncoloured under the phenol-potassium-chlorate hydrochloric acid test, thus bringing it within the vanillin group. It is a substance probably derived from the soluble carbohydrates.

There are many "gum-diseases," such as the Gummosis of the beetroot, sugar-cane and vine, and the Gummy Flux of the Amygdaleæ and other trees, which have now been traced to the activity of certain definite bacteria. In the "Gommose bacillaire" of the vine, Prillieux shows that the alteration of the tissues of the wood, which is at once recognised by the black dots, consists in a production of gummy matter in the interior of the wood. All the elements, the vessels and above all the ligneous cells of the parenchyma become filled with a brown matter of a gummy appearance in which are found quantities of bacteria. An early paper by Cobb (1893) gives an interesting description of the gum-disease of the sugar-cane in New South Wales, the chief features of which are a dwarfing of the canes and rot of the growing point, accompanied with an accumulation of yellow slime or gum which, when the stem is cut, oozes out in a gummy mass from the vascular bundles. Cobb ascribed this disease to bacteria and named the species *Bacterium vascularum*, but he did not succeed in obtaining any definite results from his infection experiments. On the contrary, R. Greig Smith, and in a later investigation E. F. Smith also, proved by inoculations with pure cultures of the bacteria that they were undoubtedly the primary cause of the infection. The organism is named by E. F. Smith *Pseudomonas vascularum*.

This last example affords but another illustration of the history of bacteriology in relation to plants, which has shown that more complete investigations have generally served to establish the pathogenicity of doubtful cases, and one is led to believe that in certain obscure diseases which appear to indicate a bactericidal origin perfected methods of research will yet discover the suspected organism.

It is impossible to do more than briefly indicate the characteristic features of attack in a few cases which have been worked out; it will be understood that no attempt is made to present a complete account of all the bacterial parasites which have been recorded.

In addition to their rôle of parasites, the bacteria function very actively as saprophytes, following in the wake of parasitic fungi and completing their work of destruction; this observation has no doubt led to the scepticism as to their aptitude as true parasites. Thus in discussing the disease of the potato (1898) Ward, while disclaiming any implication that no bacterial disease existed, expressed the conviction that alleged cases were not convincing and showed "that the way into the tuber is prepared for bacteria by fungus hyphæ and the open passages of destroyed vascular bundles afford them ample space." This last statement is incontestable; it is but natural to expect that bacteria should be present in many fungoid plant-diseases, and that their part is only secondary may be true in certain cases, but undoubtedly the converse is also true, viz. that the bacteria are often the prime agents in paving the way for the fungus hyphæ. In cases where bacteria and a fungus are associated together in a plant-disease, it is necessary to isolate these organisms and grow them in pure culture. As regards the bacteria, modern methods of culture have rendered this a fairly easy task, but to obtain the fungus free from any bacteria is a matter of the utmost difficulty. The definition of a pure culture in the case of fungi must be extended. It can no longer mean that no other fungus is present but must include the conception that bacteria are also entirely absent.

A promising field of inquiry awaits the investigator into the relation between bacteria and fungoid parasites and their association one with another in plant pathology. In the disease known as Finger and Toe, bacteria are always present in conjunction with *Plasmodiophora*, and hitherto, I believe, a culture of *Plasmodiophora* free from bacteria is unknown. Pinoy considers that they play an active part in this disease. The parasitism of *Fusarium* affords a further illustration. A species of *Fusarium* commonly met with on turnips and swedes

A

B

Impression cultures of leaves upon nutrient gelatine, incubated for three days (Sept. 4–7), showing the colonies developed.

A, upper surface of the leaf of an artichoke (*Helianthus tuberosus*); B, under surface of the leaflet of a potato (*Solanum tuberosum*).

(Reproduced by kind permission of the British Mycological Society.)

in Northumberland is apparently responsible for a large number of diseased roots in the fields. But in the decaying roots bacteria in abundance are invariably associated with the hyphæ, and so far, considerable difficulty has been experienced in obtaining a culture of this fungus entirely free from the bacteria. Until this has been accomplished the question of its parasitic nature cannot be decided in this case. Wehmer and Frank claim to have grown *Fusarium solani*, from a pure culture, as a parasite upon the potato; but these and other published accounts still leave a doubt as to the absolute exclusion of bacteria throughout the entire experiments. Under natural conditions of infection at least, it remains an open question whether the bacteria or the *Fusarium* is the secondary factor or whether the destruction of the host-cells is due to their combined influence. In the Erysiphaceæ again one would certainly expect innumerable bacteria to be present on the leaves together with the fungal hyphæ but nothing is known as to their action; apparently these organisms have been entirely left out of consideration here, as in the case of many other fungoid parasites.

Burri (1903) has shown that an actively living bacterial flora is ordinarily to be found on leaves and that these bacteria form a special class quite distinct from those normally present in the air or soil. The number of bacteria actively existent upon the surface of leaves may be several millions per gram. of leaf, whilst the number of those in the resting condition (presumably accidentally deposited) is always relatively very few. No relationship could be established between the number of bacteria and the atmospheric conditions. Düggeli has also shown that certain bacteria accompany dry seeds or fruits and on germination find their way on to the leaf-surface.

In addition to the well-known epiphyllous fungus *Apiosporium* (*Fumago*), I have found that other fungoid and bacterial germs are extensively present upon the surface of healthy leaves under the ordinary conditions. This was strikingly exemplified by impression cultures of leaves made upon the surface of a nutrient gelatine, in petri capsules. While still attached to the plant the leaves were lightly pressed upon the gelatine in the capsule, which was only momentarily opened for this purpose, and an impression of the leaf-surface was thus obtained. In every case incubation after two to three days showed numerous colonies of bacteria; and fungi, chiefly represented by species of *Penicillium* and *Botrytis*, were also met with. The colonies were confined to the área of the leaf impression, which was distinctly outlined in this way, and no growths appeared on the surrounding medium (see Plate facing p. 204). The organisms were equally abundant on both upper and lower surfaces of the leaf the species apparently varying with the season and the kind of leaf.

Since this epiphyllous flora is always present upon the surface of green plants, it becomes a matter of considerable interest to determine the part played by the micro-organisms in the ordinary fungoid diseases. Are these bacteria at all concerned in the problem of immunity? Do they in any way modify the life-histories of other bacteria or fungi with which they come in contact? I would merely throw out a suggestion that without being in any way harmful germs, they may yet profoundly influence existing conditions in some unsuspected way.

The question whether non-pathogenic micro-organisms are normally present in plant-tissues and can maintain their existence in the intercellular spaces is another interesting speculation. It must be remembered that for the purposes of respiration, etc., the intercellular system of plants is in constant communication with the surrounding atmosphere, and thus an easy entrance is afforded through the stomata or lenticels. Whether having gained an entrance the bacteria can actively live, or persist as spores, in the intercellular spaces requires further elucidation. In 1887 Gallipe's experiments led him to the conclusion that the soil micro-organisms enter the roots or tubers of many plants, and in 1888 Bernheim announced that micro-organisms are to be found in the Indian corn and other cereals. These conclusions have been very adversely criticised, and neither Buchner, Lehmann, nor Fernbach and Di Vestea have been able to confirm the results. Lominsky, however, finds that the soil bacteria can pass into the root tissues; and Fernbach and Di Vestea, though considering the interior of healthy, uninjured tissues to be free from bacteria, yet grant the fact of their presence in the interior of cut plants exposed to a damp atmosphere. As far as the evidence goes it indicates a possibility that the intercellular spaces of storage organs in the natural state may harbour living bacteria, but that they would almost certainly be present in detached portions of plants subjected to a damp atmosphere or other abnormal conditions.

Pasteur has determined that bacteria are not present in the normal healthy animal tissues. This view is also generally held with regard to vegetable tissues, but some more conclusive experiments are needed to decide this point in the case of plants.

In many physiological experiments connected with plants the existence of bacteria, both on the external surface and possibly in the intercellular spaces, is ignored but the action of the various micro-organisms present must have contributed in some measure to the effects recorded. That a neglect of such considerations may lead to serious misconception is exemplified by the observations of Stoklasa. The generally accepted view that an injured plant organism breathes more actively than an uninjured one is shown by him to be incorrect. The experiments by Stich which claimed to demonstrate this point were not conducted under conditions free from bacteria and when repeated by Stoklasa under sterile conditions were found to give opposite results. Under proper precautions the respiratory activity of injured cells proved to be less than that of uninjured tissue and the increased production of carbon dioxide at a wound was traced to the activity of the bacteria living upon the injured cells.

The external conditions to which any plant is exposed have an important bearing upon its general health and render it more resistant or more susceptible to parasitic attack. *Phytophthora infestans* may be cited as a familiar instance. It is generally recognised that light, and the temperature and vapour pressure of the air, influence in a marked degree the destructive action of this fungus; and presumably also of other fungoid and bacterial parasites. Again the temperature, air and moisture-content of the soil, and the nature of its food constituents, are all forces necessarily affecting the general vigour of any host-plant. There is considerable evidence that susceptibility to disease is influenced by manurial treatment and that abundant fertilisation, especially with nitrogenous manures, renders the host less resistant to microbial invasion. Laurent has shown how the susceptibity of a given variety of potato was related to the manurial treatment under which it was cultivated. Thus the variety Simpson, when grown in a soil manured with phosphorus, maintained a high degree of resistance to certain bacteria, which was totally lost when grown on the same soil liberally manured with lime. This he attributed to the action of the lime in liberating ammonia.

The direct relation of the character of the cell-sap to the question of immunity is also a well-ascertained fact, though Laurent proved that the *total* acidity bore no relation to the degree of resistance. Experiments showed that the resistance of tubers of potato to bacterial invasion was due to soluble substances which exist in the cell-sap and that the immunity could be destroyed by subjection to alkaline solutions. In this connection it is interesting to recall that in the attack of the potato by *Bacillus solaniperda* Kramer found that the bacteria first broke down the soluble carbohydrates with the production of carbon dioxide and butyric acid, and in the first stage of destruction the tuber shows an acid reaction. Later when the proteids came to be destroyed ammonia, methyl- and trimethylamine were formed, and after these bases had neutralised the butyric acid the second stage of the disease was reached, in which the tuber shows an alkaline reaction. The tubers rich in sugar were more liable to attack than those rich in starch.

The effect which a difference in chemical composition of the plant-tissues can exercise upon the development of a virulent form of parasitism is strikingly exemplified by Laurent's experiments with different forms of *B. coli communis*, *B. fluorescens*, *B. enteridis*, *B. typhique*, etc. All of these species were found capable of living as true parasites on the potato after special treatment had first diminished the resistance of the cells, the typhus bacillus showing the most surprising results in power of virulence.

May we not consider that the different forms of *Botrytis* raised by Kissling, as also the different forms of *B. coli communis* of Laurent, are "biologic forms" and that the foundation of this theory was laid by Kissling in his work upon *Botrytis*?

The past few years have been remarkable in considerably extending our knowledge of parasitic diseases and in opening out new avenues of research. The parasitism of bacteria has been established equally with that of the fungi and much confusion has been cleared away. But the mutual relationship of these parasites in certain plant-diseases still demands attention and their action upon the living cell requires much further elucidation. With a knowledge of the fact that nutrition may so alter a facultative saprophyte that it becomes a virulent parasite, while through other nutritional changes its virulence may be entirely lost, and further that the same influence operates in rendering the host more or less susceptible, we have the key to one of the important determining factors in the epidemic diseases of plants. It is no longer sufficient to trace life-histories or to prove parasitism under certain special conditions. The nutrition of both host and parasite must be taken into account, together with other factors which tend to disturb the nice balance existing between health and disease. We must look to further investigation in the botanical laboratory and in the field, for a solution of these and other problems connected with Plant Pathology.

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# THE RELATIONS OF INSOMNIA TO TYPES OF SLEEP

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It is admitted by all writers on sleep that more than one factor is operative in producing the condition, but I do not think that sufficient emphasis has been laid on the relationships between the sleep-producing factors and the various types of sleeplessness. We ought to have as many types of sleeplessness as we have types of sleep.

It seems to me that just as there are several causal factors productive of sleep, so there is a type of insomnia referable to each of these factors. Undoubtedly normal sleep does not ensue as the result of the operation of only one physiological state, but rather as the consequence of the simultaneous presence of several conditions, at least four of which are as follow:

1. Relative diminution of the vigour of the cerebral circulation.
2. Relative diminution or absence of sensory stimulation.
3. Relative diminution of psychic activity.
4. Relative increase or concentration of certain so-called "fatigue-toxins."

Now it is quite correct to say that each of these factors is responsible for a particular type of sleep and is thus related to a particular type of insomnia. Thus as regards the first factor, enfeeblement of the cerebral circulation is the cause, for instance, of the sleepiness which precedes sea-sickness, a condition distinctly related to cerebral anæmia.

Mosso,[1] and Durham [2] long before him, demonstrated experimentally that the cerebral cortex in sleep is in a state of relative bloodlessness. There is some difference of opinion as to where the short-circuited blood is accommodated, Hill [3] maintaining

[1] "Sulla circulazione del sangue nel cervello, dell' uomo," Rome, 1880.
[2] "Physiology of Sleep," *Guy's Hospital Reports*, 1860, vol. vi.
[3] "Mechanism of the Circulation," *Text-Book of Physiology* (Schäfer). Pentland, 1900.

that it is in the veins of the splanchic area, whilst Howell [1] says that it is in the vessels of the skin. The sleepiness of persons exposed to great cold, as in balloons and on the top of high mountains, belongs to this type; the excessive withdrawal of heat depresses the entire organism, the cardiac mechanism not excepted. Relative cerebral anæmia is again the cause of the sleepiness after a Turkish bath and after a full meal, when the skin and the alimentary canal are respectively flushed with blood. The sleepiness of a person who has suffered from severe hæmorrhage is circulatory in origin. Clearly the converse of this is insomnia due to an abnormally rapid or powerful heart-beat, a very familiar form. The sleeplessness of an excited child and of the feverish patient is of this type. We may then allude to these factors in sleep-production as "circulatory," whether the diminution of cerebral circulation be the result of cardiac enfeeblement or of a such a degree of vaso-dilatation as to reduce the cerebral blood-pressure to that point at which unconsciousness supervenes.

The view of sleep advocated by Howell is a circulatory one. He attributes the fall in cerebral blood-pressure to a cutaneous vaso-dilatation due to fatigue of the vaso-motor centres, this fatigue being periodic and normally occurring once in the twenty-four hours.

A second factor predisposing to sleep is unquestionably the absence of stimulation of the sense-organs, which, of course, ultimately means of the brain itself. The famous case of the boy described by Strümpell,[2] blind in one eye and deaf in one ear, who could be put to sleep by having his seeing eye bandaged and his hearing ear closed up, is an extreme example of the onset of sleep as due to the cutting off of sensory stimuli. The corresponding insomnia, that from the presence of sensory stimulation—the unextinguished light, the railway whistles, the neighbour's poultry—is too familiar to require more than mention. The variety of insomnia belonging to this class with which the physician has most frequently to deal is that arising from pain which is but a form of excessive sensory stimulation. The sleeplessness arising from cold feet is also a familiar example of this type. The sleeplessness from the painful sensations that are due to overworked muscles after too long

[1] *Journ. Exper. Medicine*, 1897, vol. ii. p. 313.
[2] *Deut. Archiv f. klin. Medicin*, xxii.

a walk or bicycle-ride in those not in "training," certainly belongs to this second type. One may be very tired through some unusual form of exercise and yet quite unable to sleep on account of the discomfort or pain arising from the strained muscles or joints.

The third factor contributing to the onset of sleep is the presence in the lymph of certain katabolites or fatigue-producing substances. Although these as a group have not yet been isolated, there cannot be any doubt that the products of the daily activity both of muscles and of the nervous system are responsible for some of the objective and all the subjective signs of fatigue. Preyer[1] thought that lactic acid was responsible for somnolence, and Weichart[2] more recently has carried out an investigation into the chemical nature of fatigue-toxins. The conditions of fatigue and those of toxæmia have much in common. MacDougal,[3] in his interesting paper on fatigue at the meeting of the British Association at Dublin in 1908, gives due prominence to the presence of katabolites as a factor in the production of fatigue—the normal precursor of sleep. He writes—"It seems probable that the resistance of the synapses is liable to be temporarily increased not only locally by the transition of the nervous excitation across them, but also generally by the influence of the waste products of metabolism brought to them in the blood—that they are, in short, very subject to chemical influences of many kinds." To this he adds, "There is something to be said for the view that they (synapses) are the seats of the primary and principal influence of various drugs, possibly of alcohol, chloroform, strychnine and others." This is the now widely accepted theory of the chemical origin of bodily fatigue, which manifests itself in general tiredness and sleepiness. "Sleep or general quiescence of the brain," MacDougal continues, "is the most important of the modes in which the organism protects itself against exhaustion."

Sleep has, then, a chemical factor; the katabolites (die Erdmüdungs-stoffe) have accumulated so much that the resistance of the synapses has been raised to such a degree that nerve-impulses can no longer pass over them; especially is this

[1] "Uber die Ursache des Schlafes," 1877, and "Schlaf," *Eulenberg's Encyclopädie*, Bd. xvii.

[2] "Uber Ermüdungstoxin," *Münch. med. Woch.* No. 1, 1904.

[3] *Proc. Brit. Assoc. Dublin*, 1908.

true of the synapses within the sensory centres in the cortex cerebri, regions which therefore enter on a state of functional rest the psychic correlative of which is the unconsciousness of sleep. The view of Duval[1] and Cajal,[2] that the arborescent end of the neurone at the synapses retracts in consequence of this poisoning, is extremely difficult of histological demonstration; if a fact, it supplies a physical basis for the increase of synaptic resistance.

It seems clear that there is an insomnia due in excessive fatigue to the very virulence of the toxæmia. The man not in "training" knows well its symptoms: headache, slight nausea. slight fever, feeling of general discomfort which together effectively banish sleep. Children frequently exhibit this type of insomnia, describing it by saying that they are "too tired to sleep." Of course the chemical factor is not alone causal in this case, for the heart is certainly beating faster partly by reason of the fatigue-products circulating through it and partly by reason of the slight increase of temperature of the blood, the fever in its turn being the result of the general toxæmia. The insomnia may be more directly due to the sensations of discomfort from muscles and joints, but the chemical factor in this case is certainly present. This view undoubtedly involves the idea that fatigue-toxins when in slight or what might be called "normal" amount can cause sleep—that is, depress cerebral activity; whereas if in greater concentration they act in the direction of stimulating and so keeping awake the cerebral cells.

Pharmacologists are quite familiar with similar examples of the difference of action between drugs in small and in higher concentration in the blood.

That this chemical factor is most potent in sleep-production there can be no manner of doubt, for the fatigue-induced sleep can reach a degree of profoundness of unconsciousness that is exhibited by no other physiological state of the cerebrum. We have authentic accounts of soldiers suffering from the excessive fatigue of forced marches, as in Kitchener's dash on Khartoum, rolling off the camels and falling sound asleep while the rest of the army thundered past. In the coaching days

[1] "Hypothèse sur la physiologie des centres nerveux; théorie histologique du Sommeil," *Soc. de Biol.*, 1895, p. 85.

[2] *Archiv f. Anat. und Phys.*, 1895, Heft 4-6, p. 375.

postilions used to fall asleep in the saddle and yet maintain their position, and more than once Holbein, the cross-Channel swimmer, has been found swimming asleep. All this means that sleep induced by extreme fatigue can supervene even although there be present the most penetrating sensory stimulation; the synaptic resistance has been so raised that the impulses from even violently stimulated sense-organs are prevented from arriving at the sensory centres.

The fourth factor productive of sleep is the absence of mental activity in every form—the absence of strong emotion, of worry, of intellectual problems—everything that can "keep the mind awake." Nothing is more familiar than the fact that intellectual occupation, especially if with distinct emotional colouring, can effectually banish sleep—a psychic insomnia.

Doubtless mental exaltation is accompanied by increased

| Type of Sleep. | Nature of Causal Factor. | Related Pathological or other variety of Sleep. | Related Insomnia. |
|---|---|---|---|
| I. Chemical | Positive: accumulation of normal fatigue-toxins. | Coma: uræmic, diabetic and other auto-intoxications; narcosis from drugs. | From excessive fatigue. |
| II. Vascular | Negative: diminution in the velocity of cerebral blood-flow. | Fainting (syncope): "Trance"; sleep from compression of carotids, after a meal or bath or both; hæmorrhage, in balloon, great cold. | From over-active heart. |
| III. Sensory . | Positive: increase of synaptic resistance in sensory cortical centres; or negative: diminution of conductivity at these synapses. | Coma from concussion, compression; sleep of Strümpell's patient; "Hypnotic trance." | From noise, light, heat, cold, pain, or country quiet. |
| IV. Psychic . | Negative: absence of mental activity (ideas, emotions, etc.). | Sleep of infants, very young animals from absence of cerebral differentiation; sleep of persons of low intelligence. | From problems, worry, grief, joy, etc.; or mania (continued). |

vigour of the cerebral circulation, and in many cases must be regarded as directly due to it; but as the heart is not in every case of nocturnal mental activity correspondingly excited, we are justified in regarding mental occupation as in itself a sufficient cause of insomnia.

While, then, there is a type of sleep referable to a definite physiological condition—negative or positive—either the absence of vigorous cerebral circulation, or of sensory stimulation, or of mental activity, or the presence of fatigue-toxins—yet normal somnolence is the result of the simultaneous co-operation of all these factors in different degrees of relative intensity.

We must not hold an exclusively vaso-motor theory of sleep, nor an absence-of-sensation theory, nor an absence-of-mental-activity theory, nor the presence-of-fatigue-toxins theory, but must regard physiological sleep as a periodically recurring phenomenon due usually to all of these four co-operant factors; and similarly, therefore, we must recognise the existence of an insomnia referable to each of the types of sleep—a circulatory insomnia, a sensory insomnia, a psychic insomnia and a toxic insomnia.

# AGRICULTURAL PROGRESS IN THE TROPICS[1]

## PART II

By J. C. WILLIS, Sc.D., F.L.S.,
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We have now to deal in very brief outline with the actual progress that has been or is being made under the various heads indicated in the preceding article.

*Land.*—It would lead too far to go into the questions of systems of settlement and tenure; we may merely point out that they are engaging the attention of most tropical governments. It is necessary for any progress that land should be held under a well-defined system; preferably that tenure should be individual and not joint. Probably the best existing system, from the agricultural point of view, is that of the Federated Malay States, where the land is nationalised and is sold to purchasers for a small premium down and an annual rental, the government resuming possession if the land is not kept in cultivation.

To render the land available, drainage and irrigation must be attended to in most cases; this is often, in the case of irrigation nearly always, the work of the government. Splendid works of irrigation have been carried out in India and in other tropical countries.

Suitable crops are also a necessity before the land can be said to be truly available; here science, strictly so called, comes in and has for long been used. In the early days of mankind there would be, in each country of the tropics, a fair general knowledge of the native plants available for food; but it is probable that intercourse between the different countries was extremely limited, and consequently that each country would have to depend on its own resources. Only when the trading nations of Europe appeared in the tropics were the plants of one country transferred to another. The Portuguese

[1] The previous article appeared in SCIENCE PROGRESS, 1910, v. pp. 48-59.

were very active in this respect and distributed large numbers of useful plants, which are now firmly established in various countries, taking the place in many instances of the native plants. Under Dutch rule botanic gardens were opened, to place this work of plant introduction upon a proper and more systematic footing; these were continued under the English. With the rise of the planting industry they became of great importance, and many of the largest of the cultivations carried on by planters are due to the gardens, which first introduced the necessary plants, *e.g.* tea, cinchona, rubber, cacao and many others. But gradually the usefulness of gardens has decreased, so far as this line of work is concerned, for obviously the number of useful plants to be introduced steadily diminishes, while at the same time private agencies for the supply of seed have a much wider scope than formerly and can generally get seeds in large quantity of any new plant of importance. In recent years, therefore, except in newly opened tropical countries, the botanic gardens have become centres of research along other lines, as we shall point out later.

*Capital.*—This subject is in general the most important that requires attention at the present time. In the majority of tropical countries, progress is held back more by lack of money than by any other cause. The peasant population is almost to a man in the hands of the local moneylenders, whose rate of interest is very rarely less than 50 per cent. It follows that they cannot afford to experiment but must keep to the old and well-tried lines of agricultural practice, however inefficient they may be. To get the peasantry out of the hands of the moneylender is a great undertaking and apart altogether from its bearing upon progress in agriculture very desirable. The most successful method so far introduced, and one which has a great vogue in the north, is the institution of co-operative credit societies, first begun by Raiffeisen in Germany and now widespread. They have been started on a large scale in India and a few are in operation in Ceylon. In Europe such societies are generally purely co-operative, each member contributing so much a week; in a tropical village this would in general be impossible and the aid of some local philanthropist, or of the government, is required. Once in possession of some funds, the society lends through a committee of its own members, well acquainted with the circumstances of applicants, to people situated within its own range of operation: outside loans are not allowed. A lower rate of interest than that of the moneylender is charged on loans; in India, for instance, 12½ per cent. is usual. From the profit thus obtained, the society first pays back any loan it may have had for a start and then pays dividends to its members.

There can be no doubt that such societies, properly managed, can do a very great deal to free the peasant from the clutch of the moneylender—the first thing necessary for agricultural progress. This alone is not enough to secure progress at the greatest possible rate. The problem can be attacked in other directions, by rendering it more easy for the peasant to obtain the necessary seeds, manures, tools, etc. and by providing markets in which he can easily dispose of his produce. All these things may be done co-operatively, as in fact they are done in many countries, *e.g.* France and Denmark. A very good method of helping the peasant is the co-operative seed supply store, which is in operation in several places in Ceylon. The money for a start must, as in the case of the credit society, be found in other ways. Having supplied itself with seed of good kinds the store advances it to the peasantry at a low rate of interest, paid in kind at harvest-time, in all other respects acting like a credit society. A co-operative society may also be started for the supply of manure; it finds out what each member wants, gets the whole in bulk from the manufacturers and then distributes it. In this way the cost to individual purchasers is reduced. A society of this kind is successfully at work at Baddegama in Ceylon, and elsewhere. A curious difficulty has cropped up: the local dealers in manures adulterate theirs so much that they can undersell the better manures of the society; but as the latter give better results, the effect of this action is not very marked.

Provision of markets is another very important aspect of this subject of capital, for if there be no market the peasant cannot dispose of his produce. The more readily he can sell, at a steady price, what he grows the more will his cultivations be differentiated. Hence it is obvious that the market should be a large one; in a small market, such as is provided by the village or district, the fluctuations in demand, supply, and prices obtainable will be very much greater than in a large market. So long, therefore, as the market is confined to a small district, the agriculturist must grow practically everything he requires and simply sell enough to provide a little money with which to purchase the small amount of clothing, tools and other necessities that he cannot make for himself. In other words, it was only when the white nations were represented in the tropics that any important amount of differentiation became possible in the crops grown by the people.

The market open to the peasant is in general afforded either by the travelling middleman or by the existence of a local market in which he can himself or by aid of his women-folk sell his produce. Such markets are easy to establish where there is a population, say of fishermen, who do not cultivate for themselves, or where the local population is fairly dense, but in thinly peopled countries they are very difficult if not impossible to maintain. In these cases, and also to a very large extent in the countries with local markets, the sale must be through travelling middlemen. These men are very common in many countries of the tropics; they buy up large quantities of produce but do not in general give the very best prices. Where possible, co-operative sale is probably the best way of disposing of produce, but it presupposes the existence in a village of some public-spirited person with more intelligence than the average villager. The produce of the villagers is collected from them, and they are paid say three-quarters of its value on the spot. It is then sent by the co-operative society to the market in some large town, which may be at a great distance, being there sold through an agent. After paying the cost of freight the balance is distributed among the producers; it is found that in this way they usually get a higher price for their crops, cattle, etc. Such societies have been fairly successful in some parts of Ceylon and India, and of course they are worked upon a large scale in Europe.

Yet another way in which a market may be provided for any produce that the peasant may grow, provided that this produce is exportable, is through the medium of any capitalist estates growing the same thing. For instance, if there be an estate in a tea-producing district the small grower of tea may sell his picked leaf to its factory, which can then make it up into a grade fit to sell on the open market, and by combining the crops of many small proprietors, can turn out enough to be worth selling in such a place. For it must always be kept in mind that the small man does not grow enough of anything to be worth sale in a large market, and that only by the aid of co-operation, of middlemen, or of an estate factory can enough produce be got together. Once the large market is open, of course the prices tend to be more constant than in the market of a single village or small district.

In these and other ways, then, the great problem of supplying the villager with available capital may be attacked. The tropical peasantry being in general simple and ignorant folk, must be protected from the rapacity of the markets as much as possible; this is probably best done by the encouragement of the tendency to socialism which exists among them and shows itself, for instance, in the joint villages so common in some countries. To expose the peasant to the exactions of the moneylender and the chances of a fluctuating market, as is so commonly the case at present, is to prevent him from making progress. His best chance is through co-operative work, but he is not as yet sufficiently intelligent to be able to make proper use of this without guidance; consequently the government will have to help him, and to do so without demoralising him—a somewhat difficult problem. In general, the government may help by aiding in the establishment of local markets, in the various ways we have considered above, and by making loans on good security to allow of the proper establishment of credit societies.

This almost total lack of capital is the greatest existing stumbling-block to the advancement of tropical agriculture. It cannot be too strongly insisted upon that if agriculture is to progress among all the people of the tropics and not only among the capitalist agriculturists, then the matter must be handled in a logical manner and this important question of the supply of capital thoroughly dealt with. Until this has been done, the scientific improvement of crops, of cattle, of tools and of methods can have no effect.

*Transport.*—Finance and transportation, said a President of the United States, are the keynotes of progress; of nothing is this more true than in reference to agriculture and more particularly to that of the tropics. A British Viceroy once said that the first principle of civilisation was roads, the second roads, and the third more roads. This expresses part of the same thought, and also one of the guiding principles of British policy in the tropics. The various countries of the tropics, especially those under British and Dutch rule, have been opened up by the continual formation of roads, a process which is still being carried out with great vigour.

Under native rule, apart from water carriage, there seems to have been but little possibility of the transport of goods, except by means of coolies or pack animals. The first step toward modern transport facilities is the opening of roads, along which wheeled vehicles can be driven. This cheapens transport, and makes it possible to grow crops for extended markets. This obvious policy has been so thoroughly carried out that there are but few tropical countries where transport is not sufficiently available for any progress likely to be attained within a good many years. It is therefore unnecessary here to go into the subject in much detail, beyond again drawing attention to the fact that the future lines of transport should now be marked out upon the map as reservations, so that when roads come to be required they can be constructed at far less cost than if the country is allowed to become filled up and land has to be bought back for their construction.

*Labour.*—If anything larger in agriculture than the smallest kind of peasant's garden, worked by his own labour and that of his family, is attempted, hired labour is a necessity, unless of course a village works upon co-operative lines and gives a part of its land to the cultivation of crops for sale. But in general hired labour will be a necessity in most places, if there is to be any progress. Now so long as the population remains sparse, so long will such labour be impossible to obtain, unless in very special cases; this has led the capitalist planters—for of course this cannot be done without capital—in many otherwise suitable countries to bring in labour from other and more densely peopled places, as, for example, Ceylon and the Malay States from South India, the larger West Indian islands from the smaller and more thickly peopled ones, and Hawaii from Japan. India and Java, on the other hand, do all their own work with their own inhabitants, though in India at any rate these have to move from one part of the country to another to get work.

Now this importation of labour renders the population artificially dense and if the labour could be induced to settle down in and colonise the country, the result would in general be good. In actual fact, however, this does not happen, except in individual cases; the imported coolies have always in view the chance of getting back to their own country with their savings. In British Guiana and in Mauritius the coolies are largely settled down and these countries have now quite a large population of East Indians, but are almost the only exceptions to the general rule.

Every effort should be made to get the foreign labourers to settle down in the country; by the system of road demarcations we have indicated it will be possible to divide up the country into villages, one of which may be reserved for one and another for another race or caste, whilst if a third is reserved for capitalist industry, the latter will be provided with labour within easy reach.

So long as a country has to depend for its agricultural prosperity upon imported labour, and upon foreign capitalists, so long is it in an insecure position and the profits made in agriculture will be largely taken out of the country by the planters and the coolies.

*Education.*—This, the last of what we have termed the preliminary factors, is now being largely attended to, while general interest is being aroused by the results of past educational systems in the East, if not elsewhere. It is being recognised that it is possible to give a general education which shall fit a man for the practice of agriculture and at the same time dispose him to regard more favourably newer methods of cultivation and treatment.

It is now fairly well agreed that the best system of education is one which is independent of agriculture until the pupil is about twelve years old and then becomes tinged with agricultural ideas until he is about sixteen, when boys who show a decided bent may be drafted off to an agricultural college. In the elementary stages, up to twelve years old, the scholars should have a school garden, with nature-study lessons upon its contents; after twelve these lessons should be more and more tinged with the principles of agriculture.

The school garden is proving to be one of the most successful means for the purpose of getting in touch with the natives of tropical countries. At first the people objected to their children doing what they termed coolie labour; now opinion is strongly in favour of the gardens, and there are signs in Ceylon, for instance, that they will very soon be "the fashion" and be demanded at all the schools. At present there are only a few hundred schools which possess them.

The general principles which govern the management of these gardens are twofold. The garden is intended to supply the scholars with elementary lessons upon the cultivation of plants and with nature-study lessons upon their cultivation and growth. For this purpose it is worked by the labour of the children under the supervision of the masters. At the same time it is intended to be what we may call a village experiment station, in which new crops and new methods of treatment may be tried, where every one may see. If the lessons in the garden were to be given upon crops which are already staples in the district, the master would at once invite comparison with the results of the local villagers and would almost of necessity meet with much ridicule, for he cannot be expected to know as much as they do about the practical side of the work. But if the garden be supplied with plants which are as yet unfamiliar in the district, the children can learn the elements of gardening practice just as well without inviting comparison and there is a very fair chance that these plants may get a place in the local agriculture. This, as a matter of fact, has already happened to a very large extent in Ceylon.

As the children pass the age of twelve, their lessons in the garden may become more and more tinged with definite agricultural practice, and they may learn the virtues of rotation of crops and of many other things. Then by the age of fifteen or sixteen the best boys will be ready to go to an agricultural college, in which the staff can be selected for their knowledge of agriculture; there they can learn actual agricultural practice.

We have now considered in very brief outline the preliminary factors of progress in agriculture. Until these have been fully put into action, it is hopeless to look for any effect from the later and more strictly scientific factors we shall now proceed to consider, which as yet have only produced an effect among the capitalist planting community. This in many countries is almost entirely foreign but in others has a large admixture of natives and in some places consists entirely of the latter. As we have pointed out, it is desirable to increase the number of native capitalist planters of all grades, and to put the peasantry so far into possession of capital that they have no excuse for lack of progress. If after the preliminary factors have been put into action in their case, they still do not progress, then progress is practically hopeless among them, and we must fall back on the two ideals first mentioned. We have no right, however, to do this till we have given the peasantry every chance to go ahead.

It is in these later factors that science, strictly so called, is mainly of service. They may be said to be the applications of the natural sciences to improvements of existing crops, methods, cattle and tools. Out of all these sciences will develop a subsidiary science of tropical agriculture, just as one of the agriculture of the temperate zones has grown up.

Thirty years ago the applications of botany to agriculture were confined in the tropics to the introduction through botanic gardens of new plants for culture, and to an investigation of the local flora, in general confined to the identification of the actual plants of which it consisted. We have dealt above with the introduction of plants and shown how in this respect the usefulness of a garden must continually diminish. In some places this fact was the only one to be recognised but in others the old botanic gardens have expanded into large modern departments of agriculture, forming very important centres for the carrying out of the researches upon which in the long run progress depends. Especially has this been the case in the famous gardens of Buitenzorg in Java. Not only has the garden itself been instrumental in carrying out many researches in pure botany, but numerous laboratories have been established in it for the study of the scientific problems that arise in special reference to single cultivations. There is a laboratory for tea, one for cinchona, one for tobacco and so on. In each of these there is usually a staff of three scientific men, who are expected to devote the bulk of their time to research.

The applications of botany to agriculture are many nowadays, not only as formerly in the introduction of useful plants. The study of fungi has yielded valuable results, so has that of vegetable physiology, of systematic botany and of plant-breeding. It will be well to deal with these in order.

The first great recorded attack of a fungus upon a cultivated plant, and the worst, was the attack of *Hemileia vastatrix* upon coffee in Ceylon; between 1880 and 1888 it reduced that industry to abject ruin. During its continuance the late Prof. Marshall Ward was appointed cryptogamist to investigate the disease; but though he made out its life-history, he was not able to recommend any satisfactory treatment. In 1897 the attack of a *Nectria* upon cacao in Ceylon led by one or two stages to the appointment of a mycologist upon the Ceylon staff; similar appointments have since been made in many tropical countries. It is now generally admitted, as the result of many years' work and experience, that a proper scientific study of disease-causing fungi and of methods of treatment will often give results of much value, whilst at the same time the agricultural community has grown to realise that its best policy is to give early notice of any attack of disease and get advice regarding treatment, instead of waiting, as used to be the case, until the disease had spread so far that it was practically out of hand, before asking for aid.

The success of mycological investigation and, of course, of the parallel investigations of hurtful insects and modes of dealing with them, has now led to the enactment of laws for the compulsory treatment of disease. It has in general been very difficult to get even the capitalist planters to adopt any treatment that costs more than a few pence and impossible to persuade the peasantry to adopt such. With a view to dealing with such people for their own good, laws have been passed in Ceylon and elsewhere under which, when the government is convinced that there is a bad outbreak of serious disease, which may if unchecked lead to grave consequences for the agricultural industry, regulations for its compulsory treatment may be issued.

Two cases have so far occurred in Ceylon of applications of this law—the destruction of hundreds of thousands of coconut trees at Batticaloa by a cyclone and the outbreak of the stem-bleeding disease of the coco-nut all over the island. In the first case the dead trees formed a favourable breeding-ground for beetles, which would soon have destroyed the trees that survived the cyclone. Instructions were therefore issued that they should be destroyed completely within a certain time. The people being reduced to great straits, compensation was paid for every tree destroyed; but this is not usual. Within six months every dead tree had disappeared, and the beetles had only their normal breeding-grounds left.

In the other case the disease was due to a fungus, which bored into the stem of the tree and whose presence was indicated by a bleeding upon the outside. The only treatment necessary was to cut out the diseased part and burn the wound. This was made compulsory, inspectors being appointed whose duty it was to go round the villages and show the headmen and people what to look for. The headmen had then to make lists of all affected places, and notices were served upon the occupiers to treat the disease within a certain time, or it would be done at their expense by the inspector. In the great majority of cases the notice was enough, the treatment was carried out, and the disease is now confined within reasonable limits.

Another direction in which work of this kind is being carried out is in the compulsory fumigation at the port of entry of plants or fruits likely to be carrying dangerous insects or other complaints capable of treatment by fumigation. Yet another is the prevention of entry of plants coming from countries in which they are subject to dangerous diseases.

Vegetable physiology is becoming a fruitful line of study in recent years and several very important researches have been carried out in the tropics, from which and from others it would appear as if our knowledge of growth and the factors affecting it may undergo very important changes.

The study of plant-breeding upon the newer lines which have come into existence with the rediscovery of the work of Mendel is likely to become of the very greatest importance in the tropics. Hitherto the improvement of crops in a country has consisted in importing new varieties or new crops from other places, but, as already explained, this work is decreasing in importance. It is now more urgent to improve the actual local crops, and for such work plant-breeding is of course an absolute necessity. The great success which has attended efforts in this direction in colder countries, where breeding has always been carried on, shows what we may expect in the tropics when once it is properly taken up. Disease-proof coffees, long-stapled cottons suitable to the country, rubber trees with larger yields, and innumerable other things are among the possibilities of the future.

Lastly we come back to the more strictly systematic botany, which in the form of introduction of plants from abroad was the chief *raison d'être* of a botanic garden some years ago.

Under this head the great duty of a garden is to introduce all the useful plants possible, to collect all the plants of the local flora, and to have at least two living specimens of each. This will as a rule take up all the available space of a garden. For further work an experiment station must be opened, in which any plants that have been acclimatised in the botanic garden may be further worked at, and experiments carried out aiming at improvements in the existing crops, methods, tools and cattle. In general at least two-thirds of the area of such a station will be devoted to the latter class of experiments and only a small portion to the experiments with new products.

It is in this direction, perhaps, that the most important advances have been made in recent years and every well-equipped agricultural department has now at least one experiment station. It would lead too far in this sketch to go into details of the actual work that is being done but a few of the experiments in progress on the chief station in Ceylon may be quoted.

The station having had to be planted from the start with all crops but cacao, the tea and other crops are only now coming into condition for experiments on preparation, and work has been devoted to the effects of different systems of manuring, in bringing them into good cropping condition. Tea has been grown with and without ordinary manures, with green manures, with and without "shade," and upon flat and upon sloping land. On the established cacao experiments have been tried with every kind and mixture of manure; as this crop was in bearing when the station was begun, experiments have been made on various methods of preparation of the product. Rubber, one of the greatest crops that have in recent times sprung up in tropical cultivation, is being tried both unmixed and mixed with various catch crops, which may perhaps enable the cultivator of rubber to gain some income during the period the rubber is attaining maturity without unduly delaying that period. Camphor, which has proved fairly successful at other elevations, is being tried, and manuring experiments are in progress with rice, coco-nuts, etc. Papaws have been tried for the preparation of papain, and many other of the established crops of the island have been experimented with. With new products the chief work hitherto has been with lemon grass, which has shown itself well suited to Ceylon, and with the new Manihot rubbers, lately discovered in South America.

This must serve as an indication of the class of work that is carried on upon such a station and the way in which it forms a continuation to the work of the botanic gardens. Extension of such work is being carried on in many countries by co-operative experiment among the planters, who are persuaded to try simple experiments upon systems of manuring and other matters and report results to headquarters, where control experiments are carried on. The great thing is to encourage in every possible way the experimental habit of mind among the agricultural community, for if this is once established and properly guided, the experiments at headquarters need not be so rough, and detailed scientific experiments —which must in any case go on if the place is to have any permanent value, but which do not appeal to the agricultural community in general—may obtain a larger part of the time and energy of the staff.

Plant-breeding work, which of course will be largely responsible for improvement of crops, may go on either at the experiment station or in the botanic gardens, according as space allows. In these various ways the botanic gardens and the experiment station between them will attend to the improvement of crops and methods of cultivation and preparation, whilst the improvement of tools and cattle calls for further aid.

To improve the cattle of a country is a more complex affair than might at first sight appear, for unless the food supply is improved at the same time the new cattle cannot be kept in good condition, and unless the tools they are to use are also improved the increased size and power of the cattle will be valueless. All must go together and it is obvious that the improvement of the crops, *i.e.* of the food supply, must lead the way by a trifle. But this, as usual, means greater demands upon capital, and unless this has been first seen to, it is useless to go in for the improvement of other things.

Apart then from the fact that crosses with greatly better races are in general weedy and unsatisfactory, it is obvious that the improvement of the cattle must be a very slow and gradual process, just like the improvement of the related matters. The same remark applies to tools. To introduce the greatly more efficient tools of Europe or America into a tropical country, except for purposes of comparison and study or for the use of the capitalist agriculturist, is to court failure. Enthusiasts without number have tried to improve tropical agriculture *rapidly* by the introduction of "good seed," of improved tools and what not, and have invariably failed. Nothing but progress of the most gradual kind is possible and not even this without attention to the proper order in which the factors of improvement work, as we have been endeavouring to set forth in this paper.

Tools require scientific study. All the native implements should be carefully investigated with reference to the work they have to do, and should be compared with the foreign tools of all kinds, obtained from other tropical countries as well as from Europe and America. When the principle of the whole subject is understood, the native tool may be modified slightly in the required direction and tested exhaustively against the old tools. If the new one prove more efficient in proportion to the cost of making and of using, then it may be recommended for employment in native agriculture. But at the same time the corresponding improvement of the cattle must not be forgotten, unless the improvement of the tool can be in such a direction as to make it more efficient without needing more power to draw it.

Chemistry, lastly, is a subject which is daily becoming of greater importance in connection with improvement in agriculture. All experiments on manuring require the aid of the chemist, and to decide the class of soil for a given product his help is also needed. Tobacco, for instance, needs a soil rich in lime and potash; it is far easier to decide whether this is the case by a simple analysis than by trial and error. The help of the chemist too is needed in working out methods of preparing products: for example, in distilling essential oils or in fermenting cacao.

These references to the scientific methods of improving agriculture which are in progress are necessarily very brief: the aim has been to point out some of the lines in which work is being carried on; in a later paper it may be possible to go into more detail. The chief object of the present articles is to show that the improvement of agriculture in the tropics, if it is to touch more than the capitalist planter, must begin with political measures, attention to land, capital, labour, transport and education, before the scientific factors of the amelioration of the crops, etc., can come in at all. Few people have properly thought out the position ; but when they have done so, they will in general come to much the same conclusions as those given here. The matter is one of the very greatest importance at the present time, when scientific departments for dealing with tropical agriculture are being formed all over the world, often it may be without a proper realisation of the fact that except in countries like Ceylon, Java or India, where there are large numbers of people properly supplied with capital, and where land, labour, transport and education have been and are being attended to, they can do but little if any good. It is not intended to imply that they are useless, but unless the government pays attention to the preliminary factors which we have indicated whilst the agricultural department works at the later ones, there will not be much progress.

# THE PHILOSOPHY OF MATHEMATICS

By A. N. WHITEHEAD, F.R.S.

The most recent work[1] on this subject suffers from the indeterminateness of its aims and also from its neglect of distinctions vital to the subject. In Part I., which is composed of the first three chapters, the interest is chiefly psychological. The relation of Thought to Language and Language as an instrument of Reason are considered. Then this general subject is narrowed to a consideration of the psychology of mathematicians, and of the reasons why mathematicians tend to lapse into what the author calls mysticism. But in the rest of the book the psychology gradually fades, and the substantial investigation appears to be concerned with the logical correctness of the statements and explanations of various mathematicians, intermingled with the author's own expositions. These two investigations should be kept sharply distinct: logically it is indifferent whether the error of a mathematician be due to mysticism or to indigestion, nor does a medical knowledge of these two diseases help us in determining the truth or falsehood of his statements. But the facile imputation of mysticism, like the invocation of the supernatural to explain events, has, I think, seriously misled the author by preventing him from apprehending the real points which the mathematicians criticised are endeavouring with more or less success to explain.

In Part II. the concepts of Algebra and of Imaginary Loci in Geometry are considered. Part III. is headed "Metageometry" and deals with the principles of Geometry. The book is written from the prevalent standpoint of fifteen years ago, apparently in complete unconsciousness that great discoveries have since been made by which the subject has been revolutionised. The author has read Mr. Bertrand Russell's *Principles of Mathematics*, but has missed the point that the theory of the "variable" is the key which unlocks the whole subject. Indeed,

[1] *Mysticism in Modern Mathematics*, by Hastings Berkeley, Oxford University Press, 1910, pp. xii, 264; 8*s*. net.

a lack of penetration as to what the authors quoted are really driving at is a defect of the whole work; and even when the passages quoted are wrong, they are generally at least as satisfactory as the criticisms passed on them.

I agree, however, with the main conclusion which seems to follow from the critical examinations in the book—namely that a thorough reconstruction of the philosophy of mathematics was badly wanted at the end of the last century. The book under review cannot by any stretch of imagination be said to have supplied it. A slight sketch of the problem to be solved will show how the doctrine of the variable has enabled a satisfactory philosophy to be developed.

In any consideration of the principles of mathematics two distinct subjects should be kept separate—namely (1) the nature of mathematical propositions considered in themselves apart from any admixture of particular application and (2) the discussion of the groups of particular facts which are special cases of mathematical truths. These two subjects are respectively the problem of the nature of pure mathematics, and that of the applications of mathematics. That the addition of two and two make four is a mathematical theorem; that the dropping of two apples into an empty basket and then of two more apples, with none taken out, will leave four apples in the basket is a theorem of applied mathematics. This distinction in its grosser forms has been known and recognised for centuries, probably ever since mathematics has been seriously studied. The recent progress in the philosophy of mathematics has shown that it cuts deeper and is of wider import than our predecessors imagined.

The problem of disengaging mathematics from its applications is not so easy as it looks. In popular thought it has been done most completely in the arithmetic of integers, and with a decreasing measure of success in the case of fractions, negative numbers, real numbers and complex numbers. Geometry has been the field of prolonged controversy over this point. The necessity for a solution of the problem becomes urgent when it is found that practically identical types of reasoning occur in connection with widely different material. The mathematician then seeks to disengage the abstract train of reasoning between hypothesis and conclusion from the variety of materials to which it can be applied. A fable will best illustrate the probable course of thought as soon as this process has commenced in any important branch of mathematical knowledge.

A people once existed, civilised and addicted to learning, who, as it happened, had only thought of number in connection with fishes. They would say two-fishes and three-fishes make five-fishes; but never once had they thought of two and three and five, and never had it occurred to them that two stones and three stones make five stones. Their philosophers, erudite men steeped in the classical languages and ancient wisdom, had elaborated subtle theories of the *a priori* nature of fish existence. Also the fish-thinkers, as the mathematicians were termed, were wont to commence their more complete treatises with some consideration of deep-sea dredging. In time other aggregates were considered but at first without reference to fish-arithmetic. Some fish-thinkers elaborated a disconnected theory, rather curious than important as it appeared to them, of deux-stones, trois-stones, quatre-stones, etc.; and, finally, in connection with fish-arithmetic a bold man denied that a fish was an ultimate thought-unit and introduced the notion of fractions—an idea which he utterly failed to make intelligible, as it was conclusively proved by philosophers that any portion of a fish was not a fish, and that it was illegitimate to apply to it the fish-concept.

Unfortunately the narrow specialism of a mathematical education left many mathematicians utterly unable to appreciate the crushing force of this philosophic refutation. Also the confusion was increased by the effort of an eminent mathematician to explain the idea of fractions in some popular lectures. He asked his hearers to imagine a universe in which it was impossible for any being to see at one and the same time the head and the tail of a fish. For more than a generation afterwards all philosophers and many mathematicians imagined that the theory of fractions was inextricably bound up with the existence of such a fantastic world. In short, no sound thinker—of the sort that carries weight—ever treated fractions seriously; and in standard philosophic treatises they were habitually dismissed as mathematical fads without any real import for serious thought.

At this point we may dismiss the fable and return to the central problem, How can mathematical truths be completely disengaged from all adventitious ideas? Twenty years ago, and even now in many cases, mathematicians who considered this question were driven to an extreme formalism. They laid the emphasis on the rules adopted conventionally to produce equivalent collocations of symbols. This formalism was always —and rightly—severely criticised by those who were philosophers first and mathematicians afterwards, if at all. But perhaps such critics did not always understand that an important problem remained which the mathematicians were endeavouring to solve.

But meanwhile a notable discovery has been made by the joint and partially independent work of three men: Frege, Peano and Bertrand Russell—a German, an Italian and an Englishman—by which a flood of light has been let in upon the whole question, so that the problem has received its solution in all essentials. The discovery is that of the generalised conception of the variable and of its essential presence in all mathematical reasoning. This discovery empties mathematics of everything but its logic. For the future mathematics is logic, whereas according to the old formalism mathematics is logic plus conventions as to marks, and according to the older tradition mathematics is logic applied to the domains of number, quantity and space.

A very cursory examination of one of the many ways in which the abstract science of Geometry can be presented will form the best explanation of the position of the doctrine of the variable in the philosophy of mathematics. Instead of thinking of the class of straight lines of actual space, we start by considering *any* class $\kappa$ whose members are also classes. We may, if we like, call the members of $\kappa$ the straight lines; but this is a detail of nomenclature and does not alter the fact that $\kappa$ is *any* class whose members are classes.

Then, in the place of the old axioms of Geometry which were stated as true of physical space, certain propositions involving $\kappa$ are *considered*, not stated as true but merely enunciated for inspection. In this new sense we will call them the "axioms," as a convenient short title. For example three such "axioms" are:

"If $\lambda$ is a member of $\kappa$, $\lambda$ is a class with at least three members;"

"If $\lambda$ and $\mu$ are distinct members of $\kappa$, $\lambda$ and $\mu$ cannot possess more than one common member, if any;"

> "If $\lambda$, $\mu$, $\rho$ are distinct members of $\kappa$, and any two of them possess a common member but there is no member common to all three, and if the same is true for $\lambda$, $\mu$, $\sigma$, then $\rho$ and $\sigma$ possess a common member."

In all about ten or a dozen such "axioms" are required to produce a subject interesting from its apparent relevance to the space of experiencc.

Then the subject consists of the hypothetical propositions which can be proved concerning $\kappa$, *whatever class of classes $\kappa$ may be*, where the hypotheses of the propositions consist of all or some of these "axioms" concerning $\kappa$. These hypotheticals are true for any value of $\kappa$, whether the "axioms" which from the hypotheses be false or true for that value. And further if for some special determination of $\kappa$, we know that the "axioms" are true, then the conclusions of the propositions are also true for this value of $\kappa$. These further propositions, arising from a special determination of $\kappa$ for which the "axioms" are true, belong in general to applied mathematics.

Accordingly the modern mathematician withdraws, or should withdraw, from the apodictic certainty of his results the whole question of fact involved in any application of his reasoning. What then are the properties of space as known to us in our common human experience? The mathematician should leave the answer to the physicist, or to the psychologist, or to the plain man in the street, or to the metaphysician, or to whomever is the proper person to deal with this question. It is true that mathematics forms an essential element in the inductive proof of the properties of space, if the proof is inductive; an interesting chapter could be written upon this point. But the result arrived at has not the shadow of a right to masquerade as possessing the certainty of mathematics; it may be as certain as you like but it is not mathematical certainty. This rapid account, taking geometry as an example, must suffice to show how the discovery of the doctrine of the variable has enabled mathematical philosophy to be reconstructed.

Another distinction, which the book under consideration ignores in practice, is that the problem of the presentation of mathematical ideas to students in the initial stages of knowledge must be entirely separated from the discussion of the true "principia" of the subject. Elementary mathematics and the elements of mathematics are widely different subjects, though a confusion still exists and has survived from bygone times when the psychology of education was not understood so well as it is at present. It seems natural that the learner should begin at the beginning of the subject. The truth is entirely the opposite. The learner's natural beginning is the study of the complex facts of some application of the subject. The simple abstract ideas are very difficult to grasp, because in practical life we never consider them directly; for beginners they require some particular embodiment.

In one instance, however, Mr. Berkeley has not made this error—namely when he criticises parts of the introductory chapter of my *Universal Algebra*. I am glad to be able to close this article on a note of agreement with him. I think the formalist position adopted in that chapter, whilst it has the merit of recognising an important problem, does not give a true solution, which is to be found, as explained above, in the doctrine of the variable. To that extent I agree with Mr. Berkeley's criticisms, though I hardly think that he always interprets correctly the meaning of my statements. His own solutions of the group of problems presented by the ideas of algebra, which ignore the doctrine of the variable, appear to me to be entirely inadequate, lacking in coherence, clearness and definition.

# THE GREAT STAR MAP[1]

## II. STAR COUNTING

By H. H. TURNER, D.Sc., D.C.L., F.R.S.,
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The map is to be a record of the positions of all stars brighter than a certain standard and will indicate the approximate brightness of each star. Before following the history of the project further, it is desirable to consider what are the problems which may be solved by the accumulation of such information and cannot be solved without it.

What can we learn, it may well be asked, of the great universe of stars from observations made under the severe restrictions which limit astronomers? We are permanently bound to a small satellite attendant upon one of the stars; other stars are at distances so vast that their movements are only discernible with difficulty: can we learn anything at all about their arrangement in space?

At first sight the inquiry might seem well-nigh hopeless but with a little persistence we find that the chances of learning some essential facts are not to be despised: some, it is true, can be learnt only after centuries of labour but there are one or two which have been established without very much trouble. For instance, it does not take long to satisfy ourselves that the stars are not scattered simply at random through space: it may take a long time to find out what their particular arrangement is but we feel confident that there is some arrangement for reasons which may be indicated as follows.

The stars have been divided into classes according to their brightness, such that each class (or "magnitude") is fainter than the one above it in a constant ratio. There is of course no sharp distinction obvious in the sky between one class and the next: the brightnesses vary by imperceptible steps,

[1] The previous article appeared in SCIENCE PROGRESS, 1910, v. pp. 1–14.

the abruptness of class division being entirely artificial. But it will make the argument simpler and obscure nothing if for the moment we suppose these class divisions made quite abruptly: let us imagine all the stars in each class to be exactly of the average brightness of the class, instead of grading off by small stages into the classes above and below. Now there is overwhelming evidence that these differences in brightness are partly actual differences in the stars themselves and partly the effect of distance. It is certain that the stars are not all at the same distance from us; it is just as certain that, if they were, they would not appear of the same brightness. Taking any particular star of magnitude 2 say and distance 10, we could make it appear of the 3rd magnitude by removing it to distance 16, of the 4th magnitude by removing it to distance 25, of the 5th to distance 40 and so on. Let us suppose spherical surfaces described about the earth with radii proportional to

10 16 25 40 63 100 160 250 etc.

[This series is determined by the convention about star magnitudes and we need not stop to explain it: but it will be noticed that after five terms it is repeated on ten times the scale; there is no difficulty in continuing it indefinitely both ways by means of this principle.] And now let us suppose all the stars in the neighbourhood of these successive surfaces to be actually collected upon them, which will save us the inconvenience of intermediate grades. Then if the stars had happened to be all of the same intrinsic brightness, those on the first surface (with radius 10) would appear to us of the 2nd magnitude; on the second surface (16) of the 3rd; on the third surface (25) of the 4th and so on. The difference in magnitude would be purely apparent and simply an effect of distance. This, as already remarked, is far from being the case; but before dismissing the possibility we will consider an important consequence of it.

The number of stars on the successive surfaces will increase rapidly outwards. The surfaces themselves increase in area and the distances between them also increase: so that if the stars are scattered through space impartially, the number due to each surface will increase from both causes. A little calculation shows that the resulting increase is as the cube of the radius, so that if there were 1,000 (or $10^3$) stars on the first shell of radius 10, we should find 4,096 (or $16^3$) on the next shell of radius 16, which is about 4 times as many: on the next shell of radius 25 we should find 15,625 (or $25^3$), which is again about 4 times the number. Had we taken more accurate figures for the successive distances, instead of only approximate values, we should have found a constant ratio, slightly less than 4, for the numbers on successive surfaces: that is to say, that on this erroneous hypothesis of stellar brightness being merely an effect of distance, we should expect to find 4 times as many stars of the 3rd magnitude as of the 2nd: 4 times as many of the 4th as of the 3rd: 4 times as many again of the 5th: and so on continually. Now this expectation is not fulfilled: the ratio is nearer 3 than 4, as the following figures (taken from Newcomb's *The Stars: a Study of the Universe*, p. 54) show:

| Magnitude. | Number of Stars. | Ratio to Preceding. |
|---|---|---|
| 2 | 52 | — |
| 3 | 157 | 3·01 |
| 4 | 506 | 3·22 |
| 5 | 1740 | 3·46 |
| 6 | 5171 | 2·97 |

[We begin with the second magnitude because stars brighter than this are so few that the numbers have an accidental character.]

What reason can be assigned for this discrepancy between expectation and observation? The one first to be suspected is that the considerable assumption just made, that the stars are all of the same intrinsic brightness, is not correct and is answerable for the discrepancy. But on examination we very soon find that error in this assumption can only increase the discrepancy and is without effect in diminishing it. Suppose for simplicity there were two kinds of stars, one much brighter intrinsically than the other. It will remind us that the difference is in the stars themselves, not an effect of distance, if we use two special words such as "brilliant" and "glowing" to distinguish them. Then in the first shell there will be say 50 brilliant and 50 glowing stars (the numbers are only illustrative). Of these the brilliant stars will appear of the second magnitude say, the glowing stars of the third. We shall thus recognise 50 stars only of the second apparent magnitude, for the more distant brilliant stars will be fainter than this and the glowing ones fainter still. Coming to the second shell, we should expect to find $4 \times 50$ or 200 brilliant stars which would now appear as of the 3rd magnitude; and $4 \times 50$ glowing stars appearing of the 4th. Thus altogether we should recognise as of the 3rd magnitude the 200 brilliant stars of the second shell and the 50 glowing stars of the first, making 250 or 5 times the 50 of the 2nd magnitude. Splitting up the stars into two classes has thus enhanced the expected ratio 4 in this instance and made it 5. If we go to the next magnitude we shall find that the ratio returns to 4 and remains at 4 ever afterwards: it is therefore only altered for one step but this alteration is an increase; there is no diminution available for explaining the observed drop towards 3.

We have taken a very simple case: but its characteristics are maintained in the most complex cases we can devise. They may be stated thus: just as the ratio 4 was disturbed for the first two magnitudes by dividing the stars into two classes, so if it be assumed that there are $n$ classes of diminishing intrinsic brilliance (according to steps of a magnitude each), the ratio will be disturbed for the first $n$ magnitudes, after which it will return to 4. In whatever way it be disturbed, it is increased and not diminished.

This avenue of escape is therefore closed and another must be found. Perhaps the figures used are wrong? It is not likely that the counts are wrong for they have been gone over many times; but is it certain that the drop of a magnitude in brightness is identified correctly? Accurate measures of brightness are not easy to make, as we find in everyday life in connection with candle-power tests: they are harder still for faint lights such as the stars and the difficulties increase as we pass to fainter and fainter stars. We shall presently have to consider these difficulties in connection with the project of the Great Star Map itself. But for the moment it need only be pointed out that it seems unlikely that the discrepancy under investigation is attributable to such a cause. It is easy to calculate what must be the error in estimation of a whole magnitude if such were the case: to make the ratio $3\frac{1}{4}$ instead of 4 we should have to be 20 per cent. wrong in the measure of magnitude, so that we should be estimating erroneously as a difference of 6 magnitudes what was really only 5. No human measures are perfect but, for reasons which it would take too long to give here, it is practically certain that our estimate is not so wrong as this.

We must go back to an earlier assumption that the stars are scattered impartially through space; this cannot be the case, at any rate in the neighbourhood of our sun. We have so far been considering only the brighter stars (roughly speaking those visible to the naked eye) and these must be nearer to us (other things being equal) than the fainter. It is after all not unnatural that in the neighbourhood of our sun the stars should not be scattered at random: for we see in the sky many "clusters" of stars and it is not unreasonable to suppose that our sun may belong to such a cluster or cloud of stars. The result would be an excess of stars near us and therefore bright: and to see that this will explain the observed facts we have only to turn our argument round. Hitherto we have argued from the number of bright stars how many faint ones there ought to be and found the estimate deficient: if we start with the observed number of faint stars and calculate how many bright ones there should be we shall find them in excess and the excess is due to the solar cluster. As an illustration, suppose we start with the number of stars of the 6th magnitude in the table given above and divide continually by 4, we get:

| Magnitude. | Number of Stars. | | Excess due to Solar Cluster. |
|---|---|---|---|
| | Calculated. | Observed. | |
| 6 | (5171) | (5171) | (0) |
| 5 | 1293 | 1740 | 457 |
| 4 | 323 | 506 | 183 |
| 3 | 81 | 157 | 76 |
| 2 | 20 | 52 | 32 |

and we have accordingly assigned 748 stars to the solar cluster. We have not much guidance as to the accuracy of this crude supposition but it is certainly well within the limits suggested by other clusters. On a photograph taken at the Yerkes Observatory of the great cluster in Hercules, Mr. W. E. Plummer

North.

The Globular Cluster, Messier 3, *Canum Venaticorum*, photographed by Prof. Ritchey with the 60-in. Reflector of the Solar Observatory, Mount Wilson, Cal., U.S.A. Approx. scale, 1 mm. = 10″.

measured over 2000 stars clearly belonging to the cluster; and just as this particular plate recorded more stars than others taken with inferior instruments, so a further improvement on the great Yerkes telescope would probably show an increase in the number of members of the cluster. There is every chance that the advance has already been made. Within the last few months we have seen the first results of the new 60-inch reflector of the Solar Observatory established by the Carnegie Institution on Mount Wilson, Cal., U.S.A. They are wonderful examples of what may be done in a really fine climate by a master in the construction and use of instruments. For the moment the latest photograph of the cluster in Hercules is not available. But we are indebted to Professor Ritchey, who made the telescope and took the photograph, for permission to reproduce his picture of the globular cluster in *Canes Venatici*, which admirably illustrates our text. There is no reason why a single cluster should not contain millions of stars; from this point of view, instead of the limits of our cluster being reached at about the 6th magnitude, there is no reason why they should not extend to the 7th, 8th or even much fainter magnitudes. But there are two considerations which make us hesitate to extend very far in this direction. The first is that by doing so we diminish the resemblance to other observed clusters in an important particular. In the clusters which we see in the sky the stars are thickest in the central portions. The law of condensation towards the centre has not been exactly formulated (there is room for an interesting research here): but something has been done, for instance, by Mr. Plummer in his paper[1] on the Hercules cluster; from the figures he gives we can infer that if an observer could be placed at the centre of the cluster to count the number of stars of successive magnitudes (as we have been doing for the stars visible from the earth), then the numbers would increase very slowly indeed. The ratio, instead of being 4 or 3, would probably be less than 2. Now, if we look at the numbers assigned to our "solar cluster" by the crude supposition just made, we shall find that the ratio is greater than 2. We can reduce it by reducing the dimensions of the cluster (supposing the cluster to extend no further than the 5th magnitude, say) but the more we extend the cluster

[1] *Mon. Not. R.A.S.* lxv. p. 812.

the greater we make this ratio. Hence this is, so far as it goes, a reason for moderation, though it must be admitted that the first argument is not very conclusive.

The second and more serious consideration is that, however far we extend the "solar cluster," we do not remove the chief difficulty. The ratio of the number of stars of any magnitude, to that of one magnitude brighter, obstinately refuses to rise up to 4, however far we count. The counting soon becomes very laborious, as may be seen from the figures already quoted: we have over 5,000 stars of the 6th magnitude, which means approximately 20,000 of the 7th, 80,000 of the 8th and so on; two more steps take us into the millions. It will cause no surprise that the counting has then to be done by inference from samples in different parts of the sky and is no longer complete; but even the imperfections of the counting fail to suggest any escape from the conclusion that the ratio is sensibly less than 4. Does then the "solar cluster" extend indefinitely? This would be only another way of saying that the whole universe is arranged with reference to our sun and its system. A few centuries ago it was natural to put ourselves at the centre of all things and to regard the universe as a mere appendage: but we have out-grown this instinct and we now feel suspicious of any suggestion which assigns special importance to our own position. The evidence of the star counts is very striking: but before accepting it as conclusive we feel bound to inquire whether it may not be susceptible of another interpretation.

One such interpretation at least is open to us and our familiar experiences in a fog are enough to suggest it. We know how a moderate fog limits our visible universe in all directions: in front, behind, to the right, to the left, upwards—downwards the earth anticipates the limit but from a balloon the exception would be removed—in all directions there seems to come an end to our surroundings at about the same distance. If we move about, objects appear suddenly within this charmed circle in front and leave it as suddenly behind us. Were it not for our independent knowledge, we might believe that we were the centre of all things: as it is, we attribute the appearance of centrality to the fog. Even if the fog were not in other ways obvious—if, for instance, it were night-time and the fog were too thin to irritate our nostrils—we might infer its existence from the fact that the street lamps seemed only to extend to a certain distance, instead of being visible indefinitely.

A closely similar explanation can be given of the appearance of centrality suggested by the star counts: the universe may be filled with a slight fog. It must, of course, be so extremely tenuous that the name fog is completely unsuitable; for that name suggests to us something which quenches light very rapidly, so that within a few yards (sometimes within a few inches) the brightness of a light would be reduced to one-half. The "fog" in space must require at least thousands of billions of miles to effect the same reduction to one-half. The size of these figures does not mean that they are hopelessly vague: indeed we are almost in a position to say that the number of thousands of billions must be greater than 4 and less than 40, for various independent discussions of this most important matter have been made recently, and they all point to figures within the limits just specified.

From the star counts alone we could not infer the existence of this light-extinguishing medium, which we may continue to call a "fog" for brevity. At any rate the alternative of a limited universe would have equal claims to consideration. But the evidence for the fog has been steadily growing. In the first place we have had before our eyes for centuries the spectacle of finely divided matter being driven off into space—in comets' tails and in the sun's corona. There are various interpretations of both these phenomena but the facts cannot be accounted for completely without some hypothesis of the escape of matter into space. Again, it has been realised that particles must be continually escaping from planetary atmospheres such as our own. There is, in fact, no doubt of the existence of matter in the spaces between the stars: the only question is as to its amount. And, as a second line of evidence, the spectroscope seems to indicate that the amount is appreciable. Professor Newall of Cambridge was so much impressed with the accumulated evidence of the spectroscope that he devoted to the subject a special Presidential Address to the Royal Astronomical Society in February 1909. "Here, then," he summarised, "are a few reasons for looking into possible practical ways of justifying the belief that in space, especially in the neighbourhood of suns, there must exist matter forming extended atmospheres." The phrasing is evidently that of a cautious reasoner; those who care to read the whole address will find ample confirmation of the suggestion that it is no idle speculation that is being put before us but a conclusion towards which we are urged from more than one side. Thirdly, there is a direct test for the existence of a fog which has been applied to the depths of space with apparent success. We all know that the sun looks red in a fog, because the red rays of light can penetrate a fog better than the more refrangible blue rays. For a similar reason our electric lights, being bluer than gas, suffer obscuration more readily in a fog. If, then, there be a fog in space, the more distant stars ought to appear redder than the nearer. The test, however, is not so easily applied to faint objects: for one thing we lose the sense of colour when the light is very faint. But there is one characteristic of red light that is familiar to all photographers: it takes longer to photograph it, unless we use special plates. We have then merely to ask the question, does the exposure required for the more distant stars increase in an unexpected way? The answer is certainly in the affirmative, though we must be careful that there is not another possible interpretation. From the very beginning of the work on the Great Star Map it has been a serious and fundamental difficulty that, when the exposure was doubled, the gain of faint stars on the plate was not so great as visual observations would lead us to expect. The expectation was founded on laboratory experiments, which show that, within proper limits, a light half as bright as another will give the same photographic effect if the exposure is doubled. "Within proper limits"—here is the need for care: the law breaks down when the light is very faint indeed and we must be careful not to mistake a breakdown from this cause for a cosmical phenomenon. The "proper limits" are still under investigation but they have already been subjected to careful scrutiny: a considerable research by Dr. C. E. K. Mees and Mr. S. E. Sheppard (to quote a single instance) indicates that the limit is reached, for such plates as are used in the Great Star Map, at about fifteen minutes of exposure.[1] Now well within this limit—for exposures of a few minutes only—we find that the difficulty of photographing faint stars is out

[1] See *Investigations on the Theory of the Photographic Process* (Longmans), p. 214.

of proportion to our visual expectations: and it is a fair conclusion that the difficulty arises from the characteristic property of a fog. There is room for difference of opinion as to the intensity of the fog, for the observations are difficult to interpret and even treacherous: but two separate discussions indicate as rough limits the figures which were given above. A discussion by the present writer,[1] assigning the whole of the difficulty to the fog and thus giving probably a maximum density to it, made it extinguish half the light in about 4,000 billion miles. A more conservative estimate by Professor Kapteyn of Groningen, who allowed for other possible contributing causes, makes the density about one-tenth as great. We must hope that further research will narrow the trail but it will be surprising indeed if we find that we are altogether on a false scent.

This rather long digression has not taken us so far from the topic immediately concerning us as might at first appear. Without some such explanation it would not have been easy to realise the importance of mere counts of the number of star images of a certain size. They might have been regarded as of academic interest merely, whereas we now see that they furnish evidence on two fundamental questions: firstly, is our Sun merely an individual star or is it associated with other stars in a family or cluster? secondly, is there an extremely tenuous "fog" of matter pervading the spaces between the stars and if so what is its density? We shall find that the first of these questions is presented again in another connection when we come to the movements of the stars: but the existence of some sort of solar cluster is established by simple numeration combined with measures of brightness.

How are we to measure the brightness of stars photographically? In approaching any measurement of differences we must first satisfy ourselves that we can recognise equality. Let us define as of equal photographic magnitude two stars which impress the same plate equally in the same time and we need go no further to encounter trouble. Suppose we pick out two stars by this rule, will they remain equal if we substitute a different plate? The answer is in the negative: for if one of the stars be a red star and the first plate be isochromatic, we shall

[1] *Mon. Not. R.A.S.* lxix. p. 61.

find the image of the red star much fainter on substituting an ordinary plate. If our photographic magnitudes are to mean anything, we must keep to the same kind of plate. Strict uniformity in this respect has not been possible : the sensitising of films is an art rather than a science and when a firm of plate makers changes its artist, the plates undoubtedly change in character. But it is hoped that the variations in character of plate throughout the work have not been serious enough to introduce large errors, though this is a point on which our information is not very complete. Moreover this is not the only trouble. Two stars showing similar photographic images on the same plate may be made to give dissimilar images by slightly turning the telescope so that the images fall on different points of the plate : or by refocussing the telescope. The apparent photographic magnitude may thus depend upon the distance of the star from the plate centre and upon the particular focus selected for the plate. The difference is so slight that it might escape detection by the direct process of comparing images : but it can be made unmistakably manifest in a very simple way. It has been already mentioned that the plates of the Star Map are ruled with a series of cross lines, called a reseau, dividing up the plate into equal squares. Let us count the number of star images in each of these squares for a large number of plates and add together all the counts for each particular square : then if stars photograph equally well all over the plate, the total numbers for each particular square ought to tend to equality : the stars are scattered sufficiently at random for this purpose. Now it is found that on the plates of the Star Map the numbers do *not* tend to equality : at the edges of the plate, the totals per square are considerably less than nearer the centre—the inequality may be as great as 1 to 2. The increase, however, is not maintained up to the centre : it reaches a maximum and then falls off again, unless the plate happen to be focussed in such a position that the centre is most favoured. The phenomenon depends, in fact, on the focussing of the plate : if it be focussed for the centre, then the total per square will be greatest at the centre and will fall off steadily towards the edges ; but it is customary to push the plate a little further in than this, so that the region of best focus is a ring intermediate between the centre and the edges and on this ring the total-per-square is greatest, falling off both towards the centre and towards the edges.

Photographers accustomed to ordinary cameras will read these words with some surprise for they may not have noticed in their experience any corresponding phenomenon; but the reason of this is simply that they use a different kind of lens—a "doublet" made up of two lenses separated by an interval, and with this combination the trouble does not occur. In sketching the early history of the Star Map it was pointed out that the selection of a lens was one of the important decisions taken by the Conference of 1887, and that it was decided not to use a doublet lens—chiefly because of the expense. But in other connections stars have been photographed with doublet lenses and it has then been found that the inequality of distribution of images disappears or is very considerably reduced. With the lenses used for the Star Map, however, the inequality is marked. By noticing where the total-per-square is a maximum we can ascertain to a nicety how the plate has been focussed and whether the focussing has been changed from one time to another. The position of the maximum changes slightly with the season of the year, doubtless owing to the expansion and contraction of the lens and the tube with variations of temperature. We can even tell whether the plate is tilted slightly to one side, for then the position of maximum will be further from the centre on one side than on the opposite.

Hence it will be seen that the determination of the photographic magnitudes of the stars is beset with difficulties from the outset. We must take into account the kind of plate used and the position of the star on the plate if we are to get comparable and accurate results. Nevertheless much information can be obtained by very simple means if these essentials are attended to. Suppose we take two plates from the same batch and expose them for the same time on the same night, one to a region in the Milky Way and the other to a region far from it: and that we then count the total number of stars upon each. There will be many more on the former and we can find definitely what the ratio of the two numbers is. Repeating the comparison with a longer exposure (still the same for both regions), we shall get another ratio. From the study of these different ratios for different lengths of exposure, we get information bearing directly upon the two great questions we have been studying—the existence of a solar cluster and of fog in space: for we must ascertain whether the Milky Way is in any way related to these possibilities. Hence it will be understood why counting all the stars on a plate, trivial as it might appear at first sight, has been an important operation in connection with the Star Map. It is usually done with a "billiard-marker." Those who play billiards have various devices for marking the score, one of which is a small apparatus, held in the hand, provided with two or four little springs. On pressing one of these springs one of the numbers shown on the face of the apparatus changes and by a series of clicks the score is registered as required. Now one of the astronomers who was taking a share in the chart had "misspent his youth" and accordingly knew of this apparatus and saw that it would be useful for counting star photographs: for the plate can be passed in review under a microscope with one hand, while with the other hand a click can be made, without removing the eye from the microscope, whenever a star is seen. The success of the performance has been sufficient to cause "billiard-markers" to be exported to distant astronomical observatories, where there is, as a matter of fact, no billiard-table.

But how comes it that different exposures are given in the actual course of the work on the Star Map? Is not uniformity one of the essential features of the scheme? It was certainly the original idea that a particular length of exposure should be selected and adhered to throughout: fifteen minutes found most favour. It was considered, in the light of the experience available, that fifteen minutes would give stars as faint as the 14th magnitude, and was not so inconveniently long as to be a tax on the observer. After this had been practically settled it was remarked that the bright stars would, with so long an exposure, form very large images, the centre of which could not be accurately determined. Now it is important to measure these bright stars accurately, for we already know their positions in the sky with some precision, and they are therefore useful reference marks for the others. Hence it was decided to take another series of plates with a shorter exposure (ultimately fixed at six minutes), on which the images would not be so large. This bifurcation of the enterprise was subsequently developed in both prongs. To the shorter exposure, two others shorter still, of 3 minutes and 20 seconds, were afterwards added, for good reasons which we need not stop to notice: and the longer exposure of 15 minutes was extended to 20 minutes, then to 30 minutes, then to 40 minutes, and ultimately to one hour, subdivided into three separate exposures each of 20 minutes. We need not follow the reasons in detail, it is sufficient to remark that for the originally projected uniform exposure of 15 minutes a series has been substituted, for reasons more or less good, of four or five exposures ranging from 20 seconds to 40 minutes. The ratio of 120 to 1 between the longest and shortest corresponds roughly to 7 stellar magnitudes and hence we have material for studying the variation of total stars per plate over a considerable range.

The particular proposal to give three separate exposures of 20 minutes each has had an unexpected result of an interesting kind. The three exposures are arranged in the form of a little triangle, so that each star is represented by three little dots in this figure. Now these long exposure plates are not to be measured but are intended for reproduction as charts. They represent the most expensive part of the project, each of the eighteen shares costing some £10,000 in all. At Oxford and at some of the other less wealthy observatories it has not been possible to undertake this expensive part of the work, but the French are particularly interested in it, as a consequence of earlier national projects of the same kind; the French Government has most generously undertaken the reproduction of the "Charts" in the Oxford zone and in some others. These charts are being very beautifully and carefully reproduced in Paris by heliogravure. As each chart is printed it is patiently examined for possible defects, to see for instance whether any of the images on the original plate are missing or whether any accidental blots could be mistaken for stars. One test applied by M. Jules Baillaud, who has special charge of this work, is a comparison of the three separate images of each star to see that they resemble each other; if they do not there is usually some defect in the reproduction calling for correction. But what was his surprise one day to find a distinct difference in the images of a particular star which was not a fault of reproduction but was apparent in the original plate itself! The conclusion was forced upon him that the star had varied in brightness between the exposures. The interval is usually so short that such an occurrence would be startling, but in this particular case there had been, owing to an accident, a longer interval than usual. After two exposures had been successfully given, clouds had come up and it was impossible to give the third on that night. But in these days of "dry plates" such an occurrence does not mean the loss of the work already done; it was only necessary to close up the plate securely until the next fine night when the telescope was again pointed in precisely the right position and the third exposure given. But meantime the star had changed in brightness a little, and so M. Baillaud's careful scrutiny enabled him to discover that it was variable—a success which he afterwards repeated in another instance. Indeed the accident was suggestive; just as an accidental effect at rehearsal is often deliberately adopted subsequently in the play itself, so it was made clear by the unavoidable separation of the exposures in question that there would be a distinct advantage in separating them deliberately. M. Baillaud made this suggestion at the recent meeting of the Permanent Committee and the wisdom of it was at once recognised.

The detection of variable stars, however, is not a regular part of the work on the Map. These objects are few and exceptional: we are concerned chiefly with the many and the average. The many are counted by thousands and millions, thousands of stars on a single plate and millions on the whole collection of plates. Perhaps a few definite figures may be given here: not too many of them, for they are apt to be tiresome; but one or two representative figures will give a crisper idea of the magnitude of the work.

There are eighteen observatories concerned: the share of each is about 1,200 plates, taken twice over with short and long exposures. Fixing our attention on the short exposures, there are on the average 400 to 500 stars on each, the places of which are to be measured and recorded. But this average is struck from numbers which diverge widely; on some plates there may be 5,000, on others less than 100, the rich plates being of regions in the Milky Way and the poor ones of regions far from it. Each observatory has thus to measure about half a million star images and as the number of figures required to record each measure may be several dozen, it is easily seen that many millions of figures are used by each observatory. These measures took a staff of four or five people at Oxford some ten years or so to complete: and the printing of them another four years. The checking of so many figures in the proof sheets is no trivial matter, since it is important to avoid mistakes—astronomers know the trouble which may be caused by a wrong figure, once it gets into print.

In dealing with so many figures it is important to devise tests of their accuracy. In mathematical calculations, a whole series of operations can sometimes be tested from a single result; if that is correct, there cannot be a mistake in any of the operations, at any rate not one mistake alone; there may be two or more which exactly compensate one another but this risk is more remote. Just so in money accounts, if the totals check one another, it is usually fair to assume that the individual items are correct. In the case of the Star Map no such economical tests are possible; for no connection between the positions of the stars is known to us. We must be content to check each star by itself and for this purpose two measures are made of it under different conditions. After all the stars on a plate have been measured, the plate is taken out of the measuring apparatus, turned round through 180° and put in again for remeasurement. The second set of measures gives an independent check on the first; in this way nearly all the unintentional slips are detected: so that when the results are printed and compared with both sets of measures, they are substantially correct. This is proved in the following ways: according to the decisions of the Conference the plates of each series are to cover the sky twice, with a certain overlap of adjacent plates, so that every star appears on at least two plates. After the measures have been printed off the two independent measures for each star have been compared in many thousands of instances; the number of errors found is remarkably small. Again, some of the plates accepted in the first instance did not come up to later standards and were repeated: here again comparisons between old and new measures have detected a few mistakes, but not many. These checks have given confidence in the general accuracy of the work.

*(To be continued)*

# THE TRANSLOCATION OF CARBO-HYDRATES IN PLANTS

## PART I

By S. MANGHAM, B.A.

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In the early days of inquiry into the phenomena of plant life one of the problems which investigators attempted to solve was that of the movements of "plant juices." Observation of the growth of crops and of the value of manures had shown that materials were taken in by the plant from the soil and it was obvious that as growth proceeded these materials must move about. But the nature of this movement was not easy to understand.

It was known that the veins of animals contained moving blood and this served to suggest that the juices of plants also travelled in tubes, a view taken by Cesalpino, who wrote in 1583. He called attention to the existence in the roots of certain fine threads which pass through the stem and spread into the leaves, where they branch repeatedly. These he thought to be the food passages of the plant. The actual motion of the juices "to the place where the principle of internal heat is placed" was compared by him to that of the oil traversing the wick of a lighted lamp.

Little progress was made towards a clearer conception of the movements of the absorbed nutriment until the beginning of the seventeenth century, when Harvey's discovery of the circulation of blood led to the suggestion that there might be an analogous *circulation* of the fluids inside plants. As the more minute structure of plant stems became known, this circulation theory took an improved form. This was expressed about 1670 by Malpighi, who held the opinion that a crude sap ascended from the roots through the fibrous portion of the wood to the leaves. There it became elaborated into a formative sap, which spread through the rind, and probably the bast bundles, to places of growth or storage. He was led to this view by observing that the seed leaves of a vegetable-marrow plant developed into green leaves not very different from the ordinary ones and that if they were cut off the young stem did not grow. Hence he supposed that leaves in general were organs for changing the crude sap brought up from the roots into substances fit for promoting growth.

But Malpighi's work was frequently overlooked and the older form of the circulation theory continued to prevail. Not until long after his time was a knowledge gained of the processes carried out by green leaves in sunlight; previous to this their importance in the question could not be realised. At the beginning of the seventeenth century Van Helmont had put forward the view that plants manufacture the greater portion of their substance from water alone. He had found that a willow twig planted in a weighed pot of earth had increased in weight by 161 lb. after five years, although nothing but water had been supplied and the dry weight of the soil had only decreased by two ounces. However as chemistry progressed such an idea became altogether untenable.

Towards the end of the same century Mariotte, better known for his discovery of laws relating to gases, applied chemical notions to plants. The various incombustible substances remaining after plants were distilled were held by him to have been formed by the combining together in various proportions of certain simpler substances absorbed from the soil. Hence Mariotte credited the plant with ability to change the absorbed materials into others. Still this did not explain the fact that plants derive only a small fraction of their total substance from the soil, as shown by Van Helmont's experiments. It remained for Stephen Hales, in 1727, to suggest in his *Statical Essays* that air too was a constituent of vegetable matter. He observed that air could enter through the leaves and through certain openings in the rind; connecting its presence in living plants with the evolution of "air" on distilling them, he concluded that the atmosphere actually entered into the composition of the plant. For half a century after Stephen Hales' work appeared no real advance was made. But the discovery of oxygen by Priestley in 1774, and of the composition of "fixed air" (carbon dioxide) and of water by Lavoisier, 1776–83, served to prepare the ground for further investigations into the functions of leaves. In 1779 Priestley found that oxygen was often exhaled by the green parts of plants. In the same year Ingen-Houss showed that this only occurred during illumination and that in darkness the green parts produced carbon dioxide. Later on Ingen-Houss concluded that the oxygen exhaled in light was derived from the atmosphere, which he thought to be the main source of the carbon in plants.

A further step was taken by De Saussure at the beginning of the nineteenth century. By quantitative analysis he found that the increase in dry weight could not be wholly accounted for by the amount of carbon gained, but that the elements of water were retained by the plants and fixed simultaneously with the carbon. From this time, until the discovery by Wohler, in 1828, that "organic" could be prepared from "inorganic" compounds, the comparatively slow progress in the theory of nutrition of plants was mainly due to the prevailing belief in the action of an unknown "vital force" in all the phenomena of life.

But with the rise of "organic" chemistry and Dutrochet's discovery of endosmose it became possible to find chemical and physical explanations of some of the hitherto incomprehensible processes carried out by plants. For example, it was suggested by De Candolle, in 1832, that a kind of gum was formed in the leaves from the union of water and its contained salts with the carbon derived from the atmospheric carbon dioxide. He thought, too, that this gum was easily alterable into starch, sugar and lignine, and travelled in the rind and wood, apparently through the spaces between the cells. Except for the path assigned to the assimilates this view of De Candolle's approximates very nearly to the theory at present in vogue.

The chemical activity of leaves was still more clearly recognised after 1840 when Liebig wrote his *Organic Chemistry in its relation to Agriculture and Physiology*. In this work he showed, as the result of calculations, that the atmosphere contained enough carbon dioxide to support the vegetation of the world for countless generations. Until this time botanists had always felt some difficulty in believing that a plant could obtain the carbon forming so large a proportion of its substance from the air, which only contains about ·03 per cent. of carbon dioxide. But the experiments of Sachs, Garreau, Boussingault, and the critical work of F. F. Blackman have shown that the carbon *is* so derived and that the amount assimilated bears a direct relation to the number of "stomata" (minute openings leading from the external air into the interior of the leaf) per unit area of the leaf. Further, the work of Brown and Escombe (1901) upon the passage of gases through "multi-perforate septa" removed the objection that the stomata were inadequate to take in the large volume of air required to furnish the carbon dioxide in sufficient quantity. They showed that a septum with holes at not more than a certain number of diameters apart allowed diffusion to proceed almost as freely as if no septum existed.

But before these more recent results were obtained it became evident that during exposure to sunlight, the leaves form organic compounds, and a movement or translocation of these had to be assumed in order to explain growth in distant parts.

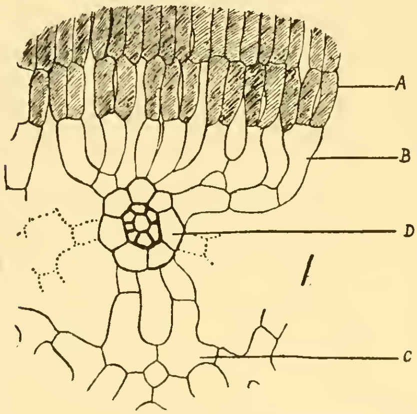
The problems remaining after the importance of the leaves was realised were to ascertain the chemical nature of the assimilates, the conditions and places of their formation, the causes of their movements and transformations and the paths followed by them in their passage from the leaves. Here the only problem to be considered in detail is the last one, which became more interesting when microscopic studies revealed to Theodor Hartig, in 1837, the existence of sieve-tubes—long chains of cells whose contents communicate by means of pores in the transverse walls. (Figs. 3–8.)

The actual substances translocated consist of albuminous and of non-nitrogenous compounds. The latter are for the most part carbohydrates and will here receive particular attention.

It has been supposed by some that to a great extent these two classes of compounds travel from the leaves by separate paths. The less diffusible albuminous substances were thought to pass along the sieve-tubes, while the more easily diffusible compounds were thought to be conducted adequately by the less specialised, thin-walled cells forming the general tissue in which the vascular strands of the conducting organs lie. When such cells are elongated the tissue has been called "conducting parenchyma." (Fig. 2.)

Nägeli first enunciated this view as to the function of sieve-tubes (1861) and the theory of separate paths has on the whole been the favourite one.

The anatomical studies of Haberlandt (1882) led him to put forward this view more definitely and the same theory was supported by Schimper (1885). The latter endeavoured to show, by microchemical and other means, that the sugars travel from the leaf by way of the parenchymatous sheath surrounding the vascular bundles of the veins (figs. 1, 11–13), the vascular bundles themselves being unessential for this purpose. Schimper's work was generally regarded as imparting an air of finality to this view, until in 1897 Czapek published the results of some experiments devised to investigate the sugar-conducting function of the sieve-tubes. He stated that although a certain amount of *diffusion* goes on through such a tissue as Schimper's bundle sheath, yet for comparatively rapid *translocation* over long distances the sieve-tube part of the bundle furnishes the sole



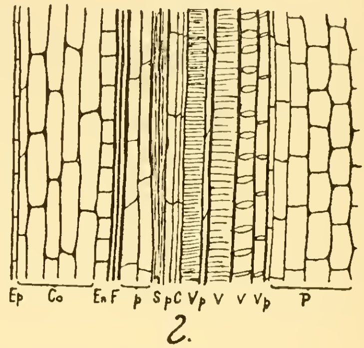




FIG. 1.—Transverse section of leaf of *Ficus elastica*.

A, palisade cells; B, collecting cell; C, cell of spongy parenchyma; D, parenchymatous bundle sheath surrounding small bundle.
(After Schubert.)

FIG. 2.—Diagrammatic longitudinal and radial section of a Dicotyledonous stem to show relations of tissues.

Ep, epidermis; Co, cortex; En, endodermis; F, fibres; p, parenchyma of the bundle; S, sieve-tubes and companion-cells; C, cambium; V, vessels; P, pith.

path for the *assimilates as a whole*. This conception of the sieve-tubes as the elements concerned whenever elaborated materials travel more rapidly than is possible by simple diffusion from cell to cell, was criticised by Haberlandt, the chief living exponent of the theory of separate paths. He devoted a page of the third edition of his well-known *Physiologische Pflanzenanatomie* (1904) to a criticism of some of Czapek's work. In this he showed how some of the results might be made to support his own views, laid stress upon the anatomical features of the assimilatory and conducting tissues which he himself had so carefully studied, and referred to an experiment made by Schimper upon Plantago in terms which imply that it may be regarded as a critical one. However in another part of the book he admitted that as the sieve-tubes often contain sugars, they may under some circumstances exercise the subsidiary function of conducting sugar, but he held that the main mass of the sugar is undoubtedly translocated through the "conducting parenchyma." Hence the question as to the exact paths followed by the sugars, after passing from their places of formation, cannot be regarded as generally agreed upon. The very formulation of the problem was confused prior to Czapek's work, for apparently little account was taken of the fact that movement may occur in two ways. The first of these is brought about by differences in the concentration of the contents of neighbouring cells, differences depending upon rates of production and consumption or transformation of the material in question. By such means diffusion is promoted and this may go on in the less specialised cells through any of their walls, as far as can be gathered from their structure. Haberlandt, however, thinks it highly probable that in the case of elongated cells the permeability of the end walls is greater than that of the lateral walls and that elongation indicates the direction of conduction of cell contents.

[In addition to simple diffusion, it is probable that a good deal of adsorption of sugars goes on at the surface of the protoplasm lining the cell and this doubtless assists in transferring these substances.]

It is clear that for such diffusion no highly specialised cells are required and also that its operation may be adequate to transfer materials over short distances, as through the undifferentiated cells near growing points, through endosperm and parenchymatous tissues. But whenever there is need for a comparatively rapid transport of assimilates over considerable distances, to meet the demands made by vigorously growing parts or by organs of storage, this slow diffusion must be supplemented. Just as the need for a rapid supply of water has called into existence a highly specialised water-conducting tissue, so, it may be held, has the need for rapid translocation of assimilates been instrumental in evolving a specialised food-conducting tissue—the phloem—by means of which both proteids and sugars are distributed.

*A priori* there is no reason to suppose that the sieve-tubes refuse admittance to sugars; indeed analyses made by Kraus of the contents of active sieve-tubes of the vegetable marrow have shown that more than a third consists of carbohydrates. Fischer, Briosi, Lecomte and others found starch in the sieve-tubes, and more recently Czapek found sugar present in them in most of the plants he examined. In the course of experiments of my own upon various plants I have repeatedly found sugar in the sieve-tubes, often in considerable quantities, even when apparently absent from the other tissues in their immediate neighbourhood.

Although the presence of sugars and other carbohydrates in sieve-tubes has been recognised for many years, their translocation by means of these elements, except in very small quantities, seems not to have been generally considered probable. Yet if it be allowed that proteids travel in the sieve-tubes, what is more likely than that the more abundant sugars also follow the same paths during their well-known rapid translocation?

There is no satisfactory experimental proof that sugars do *not* travel in the sieve-tubes and it is simply the neglect of these elements in the past which has led to a rather one-sided treatment of the problem. The difficulties attending investigations upon the changes in the amount of sugar in sieve-tubes at various times are not easily overcome by the methods formerly used; progress has depended on the advent of a more suitable test for sugar than Fehling's solution. Fortunately this is now to hand.

Meanwhile some account may be given of the kind of evidence hitherto adduced in support of the theories advanced. This has taken various forms, which may be classed as observations of the structure of plant cells and the carrying out of physiological experiments. These will be considered separately, the present article being mainly concerned with cell structure. Before experiments upon the passage of substances within plants can be carried out successfully, it is essential to become acquainted with the internal structure of the parts under investigation. The study of this may suggest *possible* paths for assimilates, but the actual function of any organ or tissue can hardly be ascertained with certainty until experiments have been performed. Hence it is that much of the evidence available, resting as it does mainly upon anatomical data, cannot be regarded as more than indicative of what *may be* rather than of what *are* the actual paths of translocation of assimilates.

## The Phloem

*Sieve-tubes.*—The phloem is characterised by the presence of sieve-tubes, which, as their name implies, are formed from elongated cells whose end walls are perforated in the mature state. Their development, in the case of certain Dicotyledons, has been studied by A. W. Hill in great detail. He found that in the young state the end walls of the cells are pierced by numerous protoplasmic threads arranged in groups in thin portions or pits (fig. 3). As the cell develops the wall thickens except in the pits and changes occur in the connecting threads, probably brought about, as Gardiner has suggested, by the action of an enzyme. This appears to travel from both ends of the thread towards the middle lamella of the wall. In some way or other this ferment changes the nature of the central core of the protoplasmic thread and the wall through which the thread passes, but leaves a tube of unaltered protoplasm (figs. 4, 5). The central strand which is enclosed in the protoplasmic sheath is known as a "slime-string," while the substance into which the cellulose wall has been changed is called "callus." By the continued action of the enzyme the slime-strings are thickened, until finally the whole of the membrane at each of the thin portions of the transverse wall becomes converted into callus and is disorganised, each original group of fine slime-strings being replaced by a single thicker one, which with its sheath of protoplasm entirely fills the resulting callus-lined perforation in the end wall (figs. 6, 7). On the completion of the process only the thickened portions of the wall remain, forming a sieve of cellulose, which during the active period of the sieve-tube is thinly coated with callus. Hence the protoplasmic lining and the slimy contents of the mature cells are connected through these pores and a tube with transverse sieves results, extending continuously through the plant (fig. 8).

When young each element contains a large nucleus, which generally disappears later on, though not in all cases according to some investigators. Embedded in the parietal protoplasm denser granules called "leucoplasts" are frequently to be found.

These are starch-formers and may contain grains which are coloured wine-red, instead of blue, by iodine, owing to the formation of dextrin by enzyme action.

Within the protoplasmic lining of the active sieve-tube elements is a solution of albuminous substances and sugars, together with minute granules of proteid nature. The fluid may be quite thin and watery or, as in the case of Cucurbita, it may be

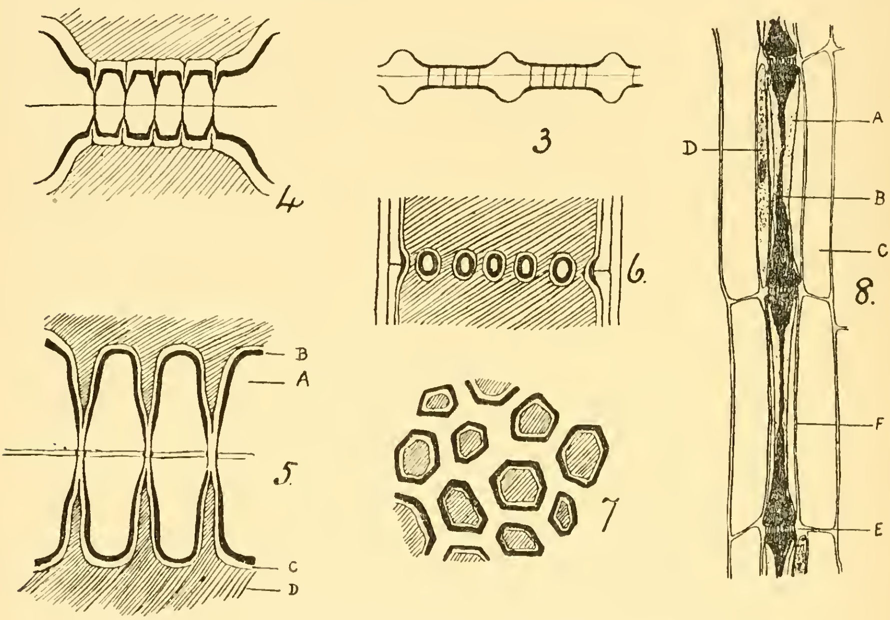



FIG. 3.—Young sieve-plate.

FIGS. 4, 5.—Diagrams to show formation of callus-tubes and slime-strings.
A, cellulose wall; B, callus; C, protoplasm; D, slimy cell contents.

FIGS. 6, 7.—Diagrams to show mature sieve-plates in section and surface view.
(Figs. 3–7 are founded on Hill's paper on "Sieve-tubes of Angiosperms.")

FIG. 8.—Diagrammatic representation of phloem elements.
A, protoplasmic lining of sieve-tube; B, slimy contents, contracted, as seen in spirit material; C, phloem parenchyma; D, companion-cell; E, sieve-plate; F, wall of sieve-tube.

slimy and gelatinous. In the latter case clots of jelly are produced on treatment with alcohol, due to the coagulation of the albuminous contents. The actual composition of the sieve-tube contents varies greatly from time to time and is dependent upon external conditions. When actively functioning a transverse cut through the sieve-tubes causes a partial exudation of the contents. In the case of the gourd stem, in which the sieve-tubes are of large diameter, Fischer found that this partial emptying extended over one or two internodes from the cut. This could be seen by comparing the distribution of the contents of the sieve-tubes in such cut stems and in stems which had been dipped in boiling water for a few minutes to produce coagulation previous to cutting. After the latter treatment a fairly uniform distribution was found and the tubes were well filled. On the other hand in unboiled stems it was found that some of the contents had been expelled through the sieve-plates, which acted as filters and withheld the larger granules against their opposing surfaces.

Hence the contents of active sieve-tubes appear to be under pressure. Sachs drew attention to the pressure caused by the turgidity of the neighbouring cells of the parenchyma and thought that the driving power of translocation came from this pressure being less at growing points than at the places of formation of assimilates. But Czapek remarked in his paper that it is very improbable that this is the sole cause of the movement of the contained assimilates. He considered that the protoplasm itself in some way takes in the substances, combines with them and gives them out on the other side where the process is repeated in the next element. However, it appears impossible to explain satisfactorily the mechanism of translocation until much more is known about the physical chemistry of plant cells, in particular the phenomena of specific permeability of the protoplasm and adsorption of dissolved substances by colloids.

Previous to the observations of Fischer and Lecomte the aggregation of contents more towards one end of the elements than the other, as seen in ordinary spirit material (fig. 8), had been held to indicate the actual direction of motion in the sieve-tubes. But these two investigators showed that such effects were due only to the cutting of the stem and to the withdrawal of water from the sieve-tubes by the alcohol, which thus caused the protoplasmic lining to contract.

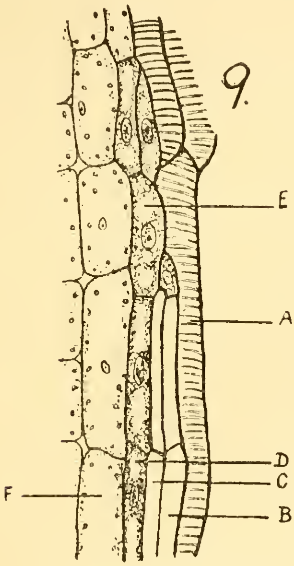
*Companion-cells.*—Associated with the sieve-tubes of the flowering plants are certain cells known as "companion-cells" (figs. 8–10). These are in reality sister cells of the sieve-tube segments, formed by the longitudinal division of a mother cell to give two unequal daughter cells.

In the phloem of the Gymnosperms and of the Vascular Cryptogams such sister cells are not found. In the former group of plants the companion-cells are probably represented by certain cells known as the "albuminous cells," on account of the great amount of proteid material in their contents. In the Vascular Cryptogams the phloem contains, beside sieve-tubes, a number of cells rich in proteid and these may correspond to companion-cells.

Unlike the sieve-tubes the companion-cells do not form continuous strands for any considerable distance and do not present any very obvious features which might be held to indicate that they form paths for *longitudinal* conduction. They are often situated between the sieve-tubes and the parenchyma of the phloem, the cortex or the medullary rays. This distribution, their dense protoplasmic content, comparatively large nucleus and intimate connection with the sieve-tubes by means of protoplasmic threads, suggest that they may be concerned in negotiating the exchange of materials which goes on between the sieve-tubes and the tissues surrounding them; but no direct experimental proof of this has yet been brought forward. Following the sieve-tubes and companion-cells out into the veins of the leaf, it is found that the diameter of each decreases. That of the sieve-tubes, however, does so more rapidly than that of the companion-cells, with the result that towards the ultimate endings of the finest veins the companion-cells are considerably larger in cross section than are the sieve-tubes (figs. 9, 10). Finally, sieve-tubes are absent and "transition-cells" occur, which are said by Haberlandt to arise by the failure of the mother cell to divide. Fischer thought that these transition-cells are the seats of albumen formation. As proteids are formed in the leaf and large nuclei such as the transition-cells possess are frequently associated with great metabolic activity, this theory is quite a plausible one, though actual proof is not available.

Investigators seem to agree that the companion-cells do not contain starch. In the course of my own observations, whilst I have repeatedly found sugars in sieve-tubes, it has never been possible to determine clearly their presence in companion-cells or albuminous cells, however distinctly visible these may be. The exact functions of these cells are by no means clearly understood.

*Relation of Phloem to other Tissues.*—As a rule the amount of phloem present (though not necessarily the number of sieve-tubes, as Lecomte has pointed out) is proportional to the amount of wood. In the case of Monocotyledons this proportion is more constant, since here the phloem consists almost entirely of sieve-tubes and companion-cells. In some Dicotyledons, however, as in Glycine and the root of *Lappa major*, although the phloem is fairly well developed, only a few small sieve-tubes occur. On the other hand, in Vitis, Cucurbita, Lagenaria, Impatiens, etc., the sieve-tubes and companion-cells form nearly



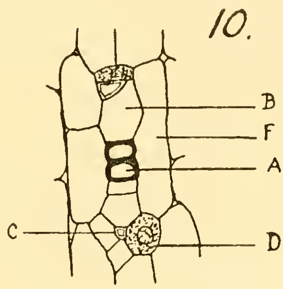




FIG. 9.—Bundle ending in leaf of *Fuchsia globosa*.
A, vessel; B, phloem parenchyma; C, small sieve-tube; D, large companion-cell; E, transition-cell; F, parenchymatous bundle sheath.

FIG. 10.—Transverse section of fine bundle of *Ecballium elaterum*.
Lettering as in Fig. 9.
(Figs. 9–10 after Haberlandt.)

the whole of the phloem and only a small amount of parenchyma is present.

Westermaier and Ambronne, in 1881, stated that the vessels and sieve-tubes of climbing plants are the largest in diameter and their opinion is quoted without qualifications by Haberlandt in the third edition of his *Physiologische Pflanzenanatomie*. Hérail, in 1885, confirmed some of the results of Westermaier and Ambronne and also showed that the diameter of the largest vessels in a climbing species is often very much greater than in a non-climbing species of the same genus. For example, in *Aristolochia Sipho* (climber) and *A. clematitis* (non-climber), the diameters of the largest vessels were found to be 200 $\mu$ and 70 $\mu$ respectively. But he found further that this rule could not be applied to sieve-tubes, for in Lonicera and in Clematis "the phloem is identical whatever the habits of the species." Moreover certain climbing plants were found to have very narrow sieve-tubes, while those of the succulent Stapelia, of the bogbean (*Menyanthes trifoliata*) and those of the rhizomes of Monocotyledons showed comparatively wide lumina. Unfortunately Hérail concluded from these observations that "the structure and particular position of the phloem elements have no physiological relations," a conclusion which Lecomte considered to be erroneous, since the habit of growth is not necessarily the dominating factor in the whole physiology of the plant. As a result of further investigations Lecomte concluded that the number and area of the leaves, the amount of phloem contained in the stem, the place of storage or utilisation of assimilates, the size and the affinities of the plant, all have to be taken into account when considering the diameter of the sieve-tubes. Generally, when there is a great production of assimilates, as in plants bearing many or large fruits, the sieve-tubes are well developed; when the assimilating and transpiring surfaces are large the diameters of the sieve-tubes and vessels are also large, especially if the stems are slender.

Kienitz-Gerloff, in 1902, remarked on the great amount of food material which had to be conducted in a short time through the tissues to form a Gourd and correlated this with the structure of the sieve-tubes, apparently so favourable to rapid conduction through the relatively slender stem.

Similarly, in the peduncle of the fruit of *Artocarpus incisa* (the bread fruit), the sieve-tubes are extremely large and numerous. This fruit is of considerable size and forms rapidly, so that here again there is a rush of material through the peduncle. An examination of the structure of this showed that the general ground tissue is composed of cells which are often flattened rather than elongated parallel to the axis. Their walls are not thin but are freely pitted, and it would seem that the chief function of this parenchymatous tissue is to provide the necessary mechanical support for the heavy fruit. Apart from the laticiferous tubes present, it certainly looks as if the only elements whose structure favours rapid longitudinal conduction are the sieve-tubes; in material collected in Ceylon by Mr. A. M. Smith at the time when the fruit was nearly formed, these contained a great deal of proteid material.

Turning now to leaves, in which the assimilates are formed, it has been shown by Lecomte that the phloem is more developed in comparison with the wood than it is in the stem and still more than in the root. He found too that relatively to the area of cross section of the phloem strands the number of sieve-tubes is greater in the leaf than in any other organ. Another way of putting this result of Lecomte's investigation of leaf phloem is to say that in tracing a phloem strand downwards from the leaf, the elements other than sieve-tubes (fig. 8) increase in amount. Now one of the most noticeable features of phloem is the absence of intercellular spaces—all the cell walls are closely in contact, even at the corners. Consequently the largest possible areas of cell wall are secured through which lateral exchange of material could go on. The relatively greater amount of parenchyma in the phloem of the stem and root than in the leaves can be correlated with the physiological functions of this tissue. The contents of the sieve-tubes have to be distributed throughout the various portions of the plant and presumably the phloem parenchyma plays a part in the lateral diffusion into the surrounding tissues. By way of these cells the sieve-tube contents pass into the parenchymatous cells of the cortex and of the medullary rays and into the wood parenchyma and pith. They may also be stored in the phloem parenchyma itself.

The distribution of the companion-cells suggests that they may be the active cells which initiate the transference, whilst the phloem parenchyma simply provides a pathway between the sieve-tubes and the other tissues. Hence it is to be expected that in storage organs, like stems and roots, the phloem parenchyma will be more abundantly developed than in organs like leaves, which are mainly concerned with producing and despatching the assimilates as quickly as possible. Accumulation of assimilates has been found to cause the stomata to close. Rapid translocation is thus a biological advantage.

Of the total assimilates which are removed from the lamina by far the greater part consists of carbohydrates, which are required to form the cellular framework of the plant, for storage as reserves and for respiration. This fact makes it seem all the more natural that the sugars should take the same paths during translocation as the less abundant proteid substances.

Another piece of anatomical evidence in support of this view is to be found in the presence of internal phloem in the "Lindsaya" type of Fern stele. Inside the mass of xylem a few sieve-tubes and parenchymatous elements occur, continuous with the phloem of the leaf trace. As pointed out to me by Mr. Tansley, it is difficult to understand the value of sieve-tubes in this position if they only conduct albuminous materials. If on the other hand sugars also travel in them, the centrally placed sieve-tubes are admirable contrivances for supplying the carbohydrate material needed to construct the large amount of thick-walled xylem elements present. In the case of *Matonia sarmentosa* these sieve-tubes apparently become lignified.

*Movements of the Protoplasm.*—The phenomenon of protoplasmic streaming calls for some consideration here. It has been held by some investigators, notably De Vries, that this plays an important part in mixing up the cell contents and aiding their transference. But this movement, thought at first to be of very general occurrence under normal conditions, has been shown by Pfeffer and by Hauptfleisch to be, in most cases, the result of the stimulus given on wounding the tissues and not to occur so widely as was previously imagined. That such movement of the protoplasm actually does increase the rate of diffusion has been shown by comparatively recent experiments.

Lecomte claimed to have seen protoplasmic streaming in sieve-tubes themselves not only in the young elements but also in some cases during functional activity. Strasburger only observed it in the young stages and Czapek has stated that his own investigations show that the streaming ceased in the sieve-tubes of all the plants examined as soon as the nucleus had disappeared and the glistening slimy contents had formed. Thus, whilst it is probable that the transport of assimilates in the parenchymatous tissues is aided by movements of the protoplasm, it is doubtful whether this phenomenon is of any importance when considering translocation in the sieve-tubes.

*The Closing of Sieve-tubes.*—It has been observed that in many plants, of which the vine is one example, a closure of each sieve-tube element is effected at the end of the annual growing period by the deposition of more callus upon the sieve-plate. In this way the pores become constricted and lengthened and ultimately a pad of callus is formed on either side of the plate. Traces of the pores may sometimes be seen, such as are figured by Hill, who suggests that in this manner the streaming of the assimilates is checked and the sieve-tubes sealed until the following spring. Then, by the dissolution of the callus, the pores are again opened and the sieve-tubes resume their conducting function. In other plants the sieve-tubes are only active during a single year, after which they lose their living protoplasm and are replaced functionally by new elements.

It will be seen from the above account of the sieve-tubes that their anatomical structure and their relations to the surrounding tissues fit them admirably for the rapid translocation of the assimilates as a whole; in the absence of any critical proof to the contrary it seems illogical to deny that the sugars follow the same paths as the albuminous substances, which it is admitted travel in the sieve-tubes. I hope before long to publish an account of experiments which have shown that sugars actually do travel in large quantities in the sieve-tubes.

### The Leaf

The structure of the leaf, in which assimilation is most active, may now receive closer attention. Essentially it is composed of layers of chlorophyll-containing cells so arranged as to expose a large surface of tissue kept extended by means of ribs, veins or nerves, as they are variously termed. The larger veins generally unite at the base of the leaf or pass into a midrib continuous with the leaf stalk, the tissues of which in turn are continuous with those of the stem, except in certain cases to be mentioned presently. Thus it is clear that the veins and stalks of leaves are particularly suitable for observations upon translocation, since all the assimilates must pass through them on the way to their destinations.

The lamina of the leaf shows structural indications of the path of the assimilates and was studied in detail by Haberlandt in 1882. Some ten types of leaf structure were distinguished by him and classified according to the degree to which they appeared to express the principle of removing the assimilates by the shortest possible path. Generalising from these types it may be said that the lamina is bounded by an upper and a lower epidermis in one or both of which stomata are present. Except for the guard cells of the stomata the epidermis is usually without chlorophyll.

Beneath the upper epidermis are the "palisade cells," elongated at right angles to the surface of the leaf, fairly closely packed and in one or more layers. They contain numerous chlorophyll granules and are the cells in which the formation of carbohydrates proceeds most actively. Frequently they are arranged in groups with their lower ends connected to a common cell termed by Haberlandt a "collecting cell," for the reason that such a cell must receive whatever products pass out of the group of palisade cells above it. (Figs. 1, 15, 16.)

Beneath this somewhat regular palisade are frequently to be found very loosely packed, irregular cells, which anastomose in all directions and form the "spongy mesophyll," a tissue full of large intercellular spaces. (Figs. 1, 16.) The collecting cells, when present, are in connection with this spongy mesophyll, which thus forms the next link in the chain of elements between the places of formation and of utilisation of the assimilates. When occurring in large amount the spongy mesophyll presents a maze of paths leading in all directions.

*The Parenchymatous Bundle Sheath.*—Many of the cells of the spongy mesophyll are united to the sheaths of thin-walled and often elongated cells enclosing the vascular bundles of the innumerable fine veins which ramify throughout the lamina. The cells of the sheath form a distinctly characterised tissue designated by Schimper the "Leitscheide" or *conducting sheath.* (Figs. 1, 9–13.)

The structure of such parenchymatous bundle sheaths of the veins of dicotyledonous leaves has been investigated by Schubert, whose results will be considered presently.

It is quite obvious that this sheath of the finer veins must *receive* the assimilates from those cells of the spongy mesophyll with which it is connected; but it was thought by Haberlandt and Schimper that the sugars and other easily diffusible substances were *conducted* from the leaf in the sheath and the parenchymatous tissues continuous with it.

This belief was based upon a number of anatomical observations and also upon the results of certain microchemical investigations made by Schimper. The latter, however, made one serious omission in that, save for an uncritical experiment upon Plantago, he apparently failed to take into account the possibility of the sieve-tubes sharing in the removal of the sugars.

As stated above, there is no *a priori* reason against their functioning in this way. But sieve-tubes are not easy to examine by Schimper's methods, especially when *changes* in their sugar content are in question. He was able to follow the passage of the sugars as far as the bundle sheaths and then drew his conclusions. The fact that sugars actually do pass in large quantities into the sieve-tubes as well could only be shown clearly when more suitable methods became available. However Schimper recognised that there were two main possibilities, either that the sugar left the leaf only in the tissue outside the vascular bundle proper or that the latter was necessary for the normal process of depletion. That he decided in favour of the first alternative was perhaps mainly due to the limitations of the methods he employed.

Turning to the anatomy of the bundle sheath, it is interesting to note that Schubert found this present in the leaves of all the numerous plants he examined, except members of the Crassulaceæ. The metabolism of these plants is of a sluggish nature and it might be urged that they have no need for any special contrivance for the removal of large quantities of assimilates. In the other plants Schubert found that the fine veins in the leaves, consisting of one or two tracheids or vessels, with a sieve-tube or transition-cell, are surrounded by a layer of cells markedly distinguished from the neighbouring parenchyma. Their inner walls are in close contact with the elements of the bundle; their lateral walls are placed radially and also show no intercellular spaces. The cells are often (but not invariably) longer than broad, have thin walls and frequently few or no chlorophyll granules, though there are exceptions to the last characteristic. Seen in transverse section they form a very distinct ring on account of their close approximation and regularity of arrangement. (Figs. 1, 12.)

The larger vascular bundles also are surrounded by parenchymatous sheaths, in which, however, more than one layer is formed by the tangential division of the cells and the volume of the tissue is thus increased. This increase in the number of layers of cells is chiefly noticeable on the upper and lower sides of the main veins, where it forms the tissue known as "nerve parenchyma." The structure of the cells of this tissue closely resembles that of the cells of the small sheaths and Haberlandt and Schubert considered that the nerve parenchyma is to be regarded as an amplified sheath.

Before dealing with this point the mode in which the small sheaths and the surrounding spongy mesophyll are connected may be noted. When the tissue of the leaf is closely arranged and the amount of spongy mesophyll is small, certain cells of the latter tissue unite with the sheath cells without causing any marked alteration in their shape. On the other

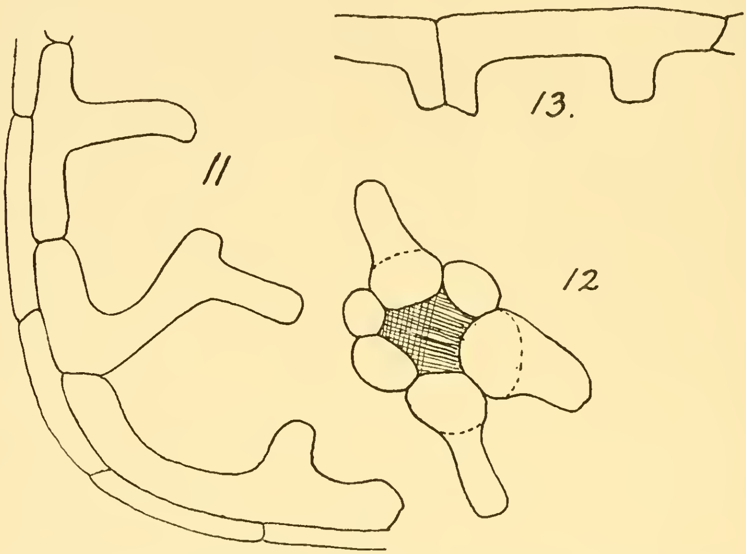



Fig. 11.—Tangential section of leaf vein of *Ruta graveolens* showing the long projecting arms of the sheath cells.

Fig. 12.—Transverse section of vein of *Ruta graveolens*.

Fig. 13.—Longitudinal section of a sheath in *Papaver Rhœas*.

(Figs. 11–13 after Schubert.)

hand, in leaves with comparatively loosely arranged assimilatory cells, it frequently happens that the connection is effected by means of tubular outgrowths from the sheath cells, which then present a less regular appearance in transverse section. (Figs. 11–13.)

It was found that the sheaths of the finest veins are provided with more numerous projecting arms than are the sheaths of the larger veins. This was accounted for by supposing that the cells of the small sheaths serve equally to collect and to conduct the assimilates, whilst those of the larger cells are mainly concerned with conduction.

The increase in the volume of the sheaths towards the bases of the larger veins was thought to be in accordance with the supposed greater need for conducting tissue to transport the assimilates concentrated in these veins from large areas of the lamina. But it may be pointed out that there is a similar increase in the absolute amount of sieve-tube tissue towards the base of the larger veins, owing to repeated additions from the confluent smaller veins. Thus the relation between the amount of conducting tissue and the amount of conduction holds quite as well in the case of the sieve-tubes as in the case of the bundle sheath and the nerve parenchyma continuous with it.

Schubert pointed out that the nerve parenchyma has in addition the mechanical function of providing a rigid framework for the leaf. Seen in transverse section its cells are frequently found to form a girder-like tissue and this is so arranged as to resist the bending which the weight of the assimilatory tissues would otherwise cause. Frequently the walls of the outermost layers of its cells are thickened, but undoubtedly the greater part of the support is derived not from this but from the turgidity of the cells, as is indicated by the flagging of leaves when the water supply is insufficient and their rapid recovery after the soil has been watered.

However Schubert agreed with the theory put forward by Haberlandt in 1882, that the carbohydrates are conducted from the leaf in the parenchymatous bundle sheath and "its homologues in the petiole and stem."

According to Haberlandt " . . . the morphological and physiological homologue of the single-layered bundle sheath of the leaf is the whole ground parenchyma of the stem, in which the parenchymatous vascular bundle sheaths sometimes present ('starch sheath' of Sachs, 'sugar sheath' of De Vries and 'endodermis' of De Bary) appear as newly differentiated products and so ought not to be homologised with the parenchymatous sheath of the bundle of the leaf." There certainly is a direct continuity between the single-layered bundle sheath of the veins and the many-layered cortex of the petiole, but the statement that the two are homologous in the *morphological* sense can scarcely be justified.

As the result of detailed anatomical studies Strasburger was led to consider the mesophyll of the leaf, together with the bundle sheath, as homologous with the whole of the tissue outside the vascular bundle proper in the petiole, etc. He would attribute to the bundle a conducting function and to the tissue outside it primarily an assimilatory function. This view seems a very natural one when the course of evolution of plant form is considered. In the lowest forms assimilation is carried out by the whole of the external surface, but in ascending the scale it is seen that there is a tendency to differentiate the plant body and to form, among other organs, leaves or localised areas of tissue specially fitted for carrying out photosynthesis. Together with this has gone the evolution of a special conducting tissue—the vascular bundle

Other parts of the plant supply supporting and storage tissues and to this category the cortex of herbaceous plants may be referred, though in addition it very frequently contains chlorophyll and can form sugars and starch. Similarly the homologous nerve parenchyma is primarily a tissue for the support of the assimilatory cells of the leaves. In the case of larger leaves it may also serve for the temporary storage of such reserves as may diffuse into it. A certain amount of the palisade and spongy mesophyll abuts directly on the nerve parenchyma in most cases and diffusion of sugars goes on through the lateral walls of its cells. Moreover there is nothing to prevent diffusion from cell to cell in the longitudinal direction. The main mass of the sugar, however, undoubtedly passes from the spongy mesophyll into the single-layered sheaths of the innumerable fine veins forming a close meshwork throughout the lamina.

These single-layered sheaths provide a tissue for the reception of the assimilates passed in from the surrounding cells and for their subsequent transference into the phloem of the vascular bundle, for translocation in the sieve-tubes. The structure of the small sheaths is in every way admirably adapted for this function. The absence of intercellular spaces between its inner walls and the cells of the bundle ensures that the largest possible area of surface is secured through which the transference may go on in the radial direction. The close approximation of the radial walls facilitates diffusion through them in the tangential direction, as may be necessary to reach the phloem.

The connections with the spongy mesophyll and the very great number of the fine bundle endings furnish an effective drainage system for the rapid removal of the assimilates, which however travel in the sieve-tubes and not in the sheaths themselves.

If the path of the water supply to the palisade is considered it is evident that after leaving the vessels of the finest veins the water must pass into the bundle sheaths. Consequently the sheath serves to distribute the water to the surrounding mesophyll and it is clear that its structure well fits it for this purpose. There is nothing improbable in the view that it also carries out the converse process of receiving substances from the assimilatory cells and passing them into the phloem for removal from the leaf. In this connection it is interesting to note that, in the English edition of Strasburger's *Text Book of Botany*, published in 1903, the passage of the sugars is described as ". . . out of the mesophyll cells into the elongated cells of the vascular bundle sheaths. The glucose and maltose are transferred in these CONDUCTING SHEATHS through the leaf stalks into the stem."

But in the English edition of 1908, the addition ". . . and from them into the phloem" is made after "bundle sheaths," while the words "IN THESE CONDUCTING SHEATHS" are omitted. Thus, although still left somewhat indefinite, a step is made towards restricting the choice of cells through which the sugars may be translocated.

*The Starch Sheath.*—Before leaving the bundle sheath and its homologues it may be mentioned that Sachs considered that, throughout the plant, the sugars travel more especially in the innermost layer of the cortex. As pointed out above, the innermost layer of the cortex, considering the plant as a whole (the "phleoterma" of Strasburger), forms in the leaf the single-layered bundle sheath of the finer veins, but in the stronger veins and in petioles and stems it is frequently differentiated as a definite starch-containing layer. Heine pointed out the improbability of this starch-containing tissue serving primarily for the conduction of sugars longitudinally. As a rule its cells are not markedly elongated and indeed are often almost cubical (fig. 2).. He was able to prove that interruptions made in the sheath in various ways did not produce any noticeable changes in the starch content, which, he considered, would not be the case were the tissue a path for the longitudinal conduction of sugars.

By observing the changes in amount and size of the grains of starch in the sheath at various stages in the growth of the plant, he was led to conclude that the main function of this starch was to form a reserve for the purpose of thickening the fibres usually present outside the phloem (fig. 2). When these were well developed he found that the starch content of the sheath had diminished and this in proportion to the amount of thickening. It would appear that Schimper accepted this view, but Czapek remarks that the starch sheath must have some other function as well, since ". . . adequate bast fibres can be developed without the presence of a starch sheath." He agrees, however, that it is most probably not concerned in the longitudinal conduction of sugars.

Accepting the above view that the assimilates enter the sieve-tubes of the finest veins directly from the single-layered sheaths, it will be evident that there is no demand for any *translocatory* tissue outside the bundles of the stronger veins. Their nerve parenchyma, though continuous with the small sheaths, has an obvious mechanical function, a function which the smaller sheaths are not required to perform. Thus, strictly speaking, the two tissues are neither morphologically nor physiologically homologous.

*Continuity of Leaf and Stem Tissues.*—There are a number of other anatomical facts which render it highly unlikely that translocation (as distinct from slow diffusion) goes on through the tissue outside the bundles of veins and petioles, at least in some plants. Strasburger pointed out that in the Palms the connection of the tissue of the leaflets with that of the main leaf stalk is such that all other translocation than through the tissue of the central cylinder is excluded. In the Conifers the leaves are so much constricted at their bases that only the tissue of the vascular bundles passes into the stem. Furthermore, in many Dicotyledons a layer of cork, extending to the vascular bundles, forms across the petiole early in the year, in preparation for the fall of the leaf. Such a transverse layer forms in *Æsculus Hippocastanum* early in July, whilst the leaf does not fall till the autumn. Also in *Veronica Hectori* and other species, according to information given me by Mr. R. S. Adamson, a layer of cork forms at the base of the leaf during the first year, while the leaf itself functions for three years or so.

Cases like these make it very improbable that in such plants the parenchyma outside the bundle can serve for the rapid translocation of sugars, which here at least are obliged to travel through the tissue of the bundle.

### Laticiferous Tissue

Another type of plant structure now claims consideration. In many plants there exists a system of "laticiferous tubes," so called from their contents, which are frequently milky in appearance. It has been held by some that these tubes, when present, provide a further path for the translocation of sugars from the leaf. Before giving any account of the structural and experimental evidence brought forward in support of this view, it may be noted that although a great many investigations have been made upon laticiferous tubes, it does not yet seem possible to point out what great advantage they are to the plants possessing them or to say what led to their evolution. Quite a number of functions have been ascribed to them by different workers.

*Structure and Relations to other Tissues.*—Two types of laticiferous tubes are distinguishable. In the one vessels are formed by the absorption or perforation of adjacent end walls and an anastomosing network results. Such vessels occur in Cichoreaceæ, Campanulaceæ, Papaveraceæ, Araceæ, Musaceæ and some Euphorbiaceæ. In the other type certain meristematic cells of the embryo develop into long, branching tubes growing continuously with the plant and passing among its cells. Tubes of this kind are found in Urticaceæ, Artocarpaceæ, Moraceæ, Apocynaceæ, Asclepiadaceæ and most Euphorbiaceæ. The walls are usually thin or they may be comparatively thick and are occasionally pitted. A protoplasmic lining, often with numerous nuclei, is present, and it has been stated by Kienitz-Gerloff that in some cases protoplasmic connections exist with the adjacent tissues.

The tubes often occur in close relation to the vascular bundle, being present in the pericyclic region or in the parenchyma of the secondary phloem. According to Lecomte they never occur in contact with sieve-tubes. In other cases they run through the cortex. They follow the bundles into the leaf, where their ultimate branches often turn up, pass between the palisade cells and end blindly beneath the epidermis (fig. 14). Haberlandt claimed to have seen a very definite relation of the laticiferous tubes to the assimilatory tissues. He stated that the tubelets "... often lie with their occasionally branched ends on palisade cells arranged as in a sheaf," and that "... when such a direct connection is not possible, funnel-shaped collecting cells negotiate the transference of the products of assimilation to the efferent laticiferous tubes" (figs. 15, 16). Schimper, however, made some criticisms upon this point in 1885, though his work

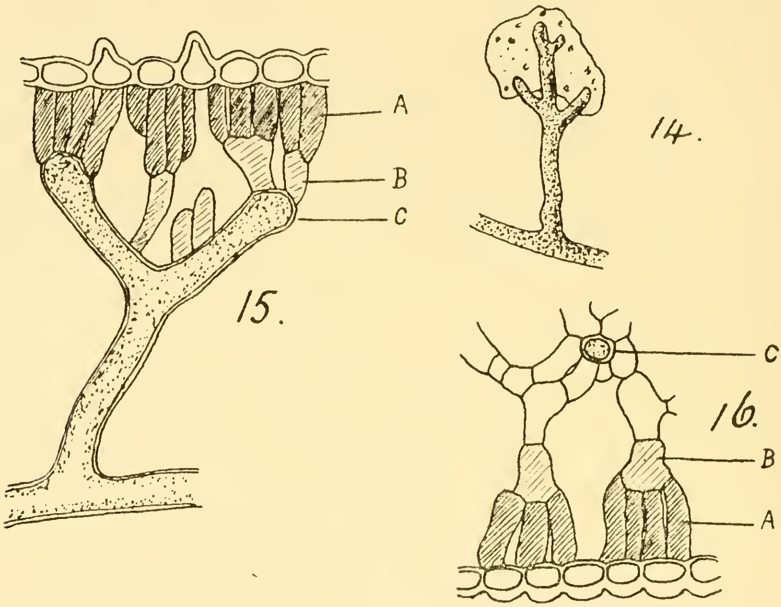



FIG. 14.—Laticiferous tube and portion of epidermis from *Hypochæris radicata.*

FIG. 15.—From leaf of *Euphorbia myrsinites* showing relation of laticiferous tubes to assimilatory cells.

A, palisade; B, collecting cell; C, laticiferous tube.

FIG. 16.—From leaf of *Euphorbia biglandulosa.*

Lettering as in Fig. 15.

(Figs. 14–16 after Haberlandt.)

upon laticiferous tubes appears to be entirely ignored in Haberlandt's treatment of the subject in his *Pflanzenanatomie* of 1904. Schimper failed to find such anatomical relations as Haberlandt described and wrote that the "statement that the laticiferous tubes branch under the palisade cells and that their ends are pressed to them, in order as it were to receive assimilates, is founded on an entirely isolated and exceptional case. I have never seen such even in *Euphorbia myrsinites* which, according to Haberlandt, shows it especially well."

The leaves were examined in chloral hydrate, by which the tissues were made transparent and the tubes could be followed in their entirety. They were observed to penetrate between the palisade cells like hyphæ, without in any way joining on to them. Schimper went on to say that Haberlandt attached great importance to the elongation of the mesophyll cells at right angles to the laticiferous tubes and upon their grouping in bundles above or below the tubes.

Such elongation indicated the direction of conduction according to Haberlandt, but Schimper considered that it is often possible to discover purely mechanical causes for the shapes and arrangements taken by cells during their development and insisted that it is unwise to argue from anatomical structure unchecked by experiment. He himself carried out a number of experiments and, as will be shown later, these gave results opposed to the value of laticiferous tubes as organs for the conduction of carbohydrates. In some plants (Cichoreaceæ, Papaveraceæ, Campanulaceæ) De Bary found that the sieve-tubes and laticiferous tubes were complementary in amount. Where the sieve-tubes were well developed the laticiferous tissue was comparatively small in amount and vice versa. From this it has been thought that in cases where the laticiferous tubes preponderate over the sieve-tubes the plastic materials are conducted in the former, a view accepted by Haberlandt. In 1889 Lecomte expressed doubts about the soundness of this idea. "The widespread belief that succulents and plants with latex possess smaller and fewer sieve-tubes than other plants, does not appear to me to be well founded; it has already been put in doubt by M. Vuillemin as far as the stem of the Compositæ is concerned. In fact, in this family it is not rare to find among plants with latex, sieve-tubes quite as large as in allied plants without latex. Lapsana, for example, has very good sieve-tubes although very rich in latex." The point was also dealt with by Strasburger, who agreed with Schimper on the subject and held that ". . . the laticiferous tubes ought not to be considered at all as substitutes for the sieve-tubes, as is often stated." So it seems that this question cannot be decided till further investigations have been made. Again, Haberlandt and after him Pirotta and Marcatili found that in many cases there appeared to be a relation between the amounts of laticiferous tissue and "conducting parenchyma" of the leaf veins, such that a large development of laticiferous tissue was accompanied by a comparatively small amount of parenchyma. Examples given are *Euphorbia myrsinites* and *E. glandulosa.* The conduction performed by these two tissues was accordingly held to be complementary in amount. But it is scarcely safe to conclude from the complementary development of these tissues that the function of conducting assimilates is shared by them. The relatively smaller development of parenchyma in the veins might arise equally well from a smaller need for the mechanical support which it provides, for such support may also be given by fibres, epidermis, shape of leaf, etc.

*The Latex.*—It is remarkable how many substances enter into the composition of the latex. An analysis of that of *Euphorbia cyparissus*, given by Wiesner and quoted by Kniep, showed it to contain 72 per cent. of water. Of the dry material the composition per cent. was resin 57·5, gum 13, caoutchouc 10, sugars and bodies soluble in ether 15, albumen ·5 and ash 3·6. Starch, oils, acids, colouring matters, mineral salts and enzymes are among other constituents of latex, and so it is seen that both substances of use in metabolism and others often regarded as waste products are to be found in the laticiferous tubes.

The presence of such materials as sugars and starch led some observers to conclude that these are probably conducted in the tubes, which appeared to be, in addition, receptacles for waste products. For instance, Haberlandt gave it as his opinion that ". . . the latex is primarily a formative sap" and that ". . . the tubes function as conducting organs for it."

Kniep, however, has pointed out that the plastic materials are present in much smaller amount than are such substances as resins, caoutchouc, etc. He considers that as these latter compounds are formed with great expenditure of energy on the part of the plant, it is only natural to suppose that their production continues because it is of some definite biological value to the plant. This value lies in the power of the latex to coagulate rapidly on exposure to air, owing partly to the presence of oxydases. In this way Kniep thinks that the chief function of the latex is an "ecological" one, since it helps to close wounds, a function thought by Haberlandt to be quite subsidiary to that of nutrition. The ramifications of the tubes beneath the epidermis certainly provide a well-distributed supply of the closing fluid and this is readily forced out when a hole is made owing to the turgidity of the tubes containing it. As the tubes have a protoplasmic lining it may be that the sugars and small amounts of mineral salts present simply serve to maintain turgidity osmotically and neither form a reserve nor represent assimilates in course of translocation. It has indeed been suggested by Parkin that the tubes are water-storage organs. However, it does not yet seem possible to estimate their real value to those plants possessing them or to say how much they assist in the conduction of food materials.

### Protoplasmic Connections

It will be gathered from the above account of the anatomical features of the various tissues dealt with, that in the cases of the laticiferous tubes and the "conducting parenchyma" it is difficult at first sight to pick out any one function as the primary one. The construction of the bundle sheath and the tissue continuous with it in the main veins is such that the functions of conduction, temporary storage and mechanical support are all more or less probable.

The sieve-tubes, on the other hand, cannot easily be conceived to be well adapted for any other purpose than that of providing a path for the rapid translocation of assimilates. Their structure it is clear would enable such a movement to take place under more favourable conditions than in the cells of the bundle sheaths, for example, owing to the enlargement of the protoplasmic connecting threads. However, protoplasmic threads exist between most cells in the plant and it will be well to devote some space to a consideration of the significance of their occurrence.

During the division of meristematic cells the nuclear substance of the two daughter cells appears to be connected for a time by a number of fine protoplasmic threads arranged in a more or less barrel-shaped bundle. These threads have been thought by Gardiner to persist during the subsequent formation of the cell wall between the two nuclei and so to unite the protoplasts of the two cells. Though not fully established, this theory of the origin of connecting threads is very plausible. Whether the threads consist entirely of protoplasm, or whether an interruption of another nature occurs at the "median node," is still uncertain; in any case the separating portion, if present, is very much thinner than the unperforated cell wall. Hence it is probable that the distribution of connecting threads materially affects diffusion from cell to cell. Unfortunately much of the work done upon connecting threads is open to question, owing to the nature of the methods employed. The question of technique has been studied by Walter Gardiner and by A. W. Hill and some of the results of the latter investigator, obtained by improved methods, may now be given.

Hill made a critical examination of the tissues of *Pinus sylvestris* with a view of ascertaining the nature and distribution of the connecting threads. Throughout the paper, which appeared in 1901, references are made to the probable value of the threads as paths for the conduction of food materials. For example, dealing with the threads of the endosperm, it is stated that ". . . it is by these paths that the dissolved food materials travel to the developing embryo." The arrangement of threads in the cotyledons was held to indicate ". . . the path of nutritive materials to the phloem," the albuminous cells being the gateway to the sieve-tubes. In the cells of the medullary rays ". . . the threads traversing the tangential walls . . . are especially well developed, thus indicating the value of these cells as passage ways through the xylem and phloem to the cortex." Moreover ". . . the distribution of the connecting threads appears to indicate the similar character of the starch medullary-ray cells, the bast parenchyma and the cortical cells." The assimilatory cells of the leaf are connected with the endodermis and ". . . by means of these threads the elaborated food material is passed in from the palisade cells." In the phloem itself the connections are such that ". . . the albuminous cells perform the important function of affording the only means of communication, and this an indirect one, between the sieve-tubes and the rest of the parenchymatous tissues which compose the bast and medullary rays."

Although Pfeffer has doubted the value of the threads for translocation purposes, yet the work of Brown and Escombe indicates that they must be of considerable importance in facilitating diffusion. It was found that given certain conditions ". . . the flow of diffusing substances may go on almost as rapidly through a 'multi-perforate' septum—such as that provided by a pit-closing membrane studded with threads—as if no membrane were present." In the above-mentioned paper, and in one on the sieve-tubes of Pinus, Hill supported the view that the cumulative effects of the threads are probably of great importance in translocation. But in a later paper, on the sieve-tubes of Angiosperms (1908), he considered it " . . . highly unlikely that they serve for anything more than the conveyance of impulses from cell to cell " but that " . . . owing to subsequent modifications in the special cases of the sieve-tubes, they become enlarged and are able to serve secondarily for purposes of translocation."

Taking into consideration the anatomical characters of the various tissues and the distribution of the connecting threads, where this is known, it would seem that the differences in the nature of the threads are correlated with the different motions of the contents of the cells in whose walls the threads occur. Normal threads appear to be adequate in those tissues in which the assimilates only travel over short distances, as in the assimilatory system, the bundle sheath and parenchyma in general. But where a comparatively rapid translocation occurs over considerable distances and in restricted channels, the fine threads seem to be inadequate and the sieve-tubes prove to have actual pores by which the translocation is facilitated. The actual mechanism of transport in the sieve-tubes is still obscure but it seems probable that the power of proteids to adsorb crystalloids in solution and the pressure to which the sieve-tube contents appear to be subjected are very important factors in the process.

From the foregoing account it will be seen that the anatomical and histological features of the tissues lend considerable support to the theory that the sugars pass from the chlorophyll tissues of the leaf into the bundle sheaths of the finer veins and are thence removed by the sieve-tubes. In these elements they travel from the leaf and are distributed throughout the plant body. Since nearly the whole of the assimilates enters the sieve-tubes near the bundle endings it is clear that there is very little need for any extra-stelar translocatory tissue. The nerve parenchyma is accordingly held to be primarily a mechanical tissue.

In the second part of this article I hope to show that this view is in accordance with the results of various physiological experiments.

*(To be continued)*

# WHEAT-GROWING AND ITS PRESENT-DAY PROBLEMS

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Few things in the whole realm of Nature have given rise to a greater number and variety of problems than wheat. The reason is probably to be found in its close association with man—an association that began before the dawn of history and has strengthened as time has gone on. More than anything else wheat is the food of civilised man, and there is no sign of it being displaced from its high position. Thus the wheat supply has to increase as the world's population increases; indeed it has to do more, for as nations that have hitherto lived on other cereals learn its use, they tend to forsake the oats, maize, rice or millet that their forefathers ate and take to wheat instead. Both in old countries and in new, investigators and practical men are at work trying to increase the yield or the quality of the wheat crop. Their results are published in a number of scattered journals, not always readily accessible; much of the work was summarised in the papers read at a joint meeting of the Botanical, Chemical and Agricultural Sections of the British Association arranged at the suggestion of Prof. Armstrong at Winnipeg last year.

Until recently, practically nothing was known of the origin of wheat; even the traditions of the Greeks, the Egyptians, and the Chinese got no farther than asserting it to be a gift of the gods. But during the last few years Schweinfurth, Kœrnicke, Aaronsohn, Otto Stapf, and others have done much to clear up this problem, and the broad outlines of the history are already discovered, although much detail remains to be filled in. Their work was ably summarised by Dr. Otto Stapf in his paper read at Winnipeg.

From this point of view Dr. Stapf divides the various wheats into four groups, each of which appears to have had a different origin. The first division consists of "Einkorn" (*Triticum monococcum*), the only wheat which possesses but one perfect flower, and therefore only one grain to each spikelet. It is grass-like in habit, and its compact bearded ear looks much like barley. The ripe grain adheres to the chaff and does not detach itself as in the commonly cultivated wheats. Einkorn is among the oldest of the wheats: Schliemann found it in the ruins of the second town of Troy (*c.* 2000 B.C.); it also occurs in neolithic remains in Switzerland and Hungary; it was known to the Greeks, but does not appear to have been cultivated by the Romans. Though still grown in parts of Spain, France, Switzerland, Wurtemberg, Thuringia, and the Balkan Peninsula, it is in no sense a modern bread wheat; further, the yield is so small that it can only be grown economically on poor soils. But it possesses one valuable characteristic that is likely to bring it into prominence in wheat-breeding experiments in the future—it is exceptionally resistant to rust. This wheat is now generally admitted to have arisen from *Triticum ægilopioides*, a wild form ranging from the Balkan Peninsula to Syria and Upper Mesopotamia. Dr. Stapf points out that the only obvious change is the great reduction, amounting almost to complete suppression, of the conspicuous long white hairs of the spindle in the wild form: other domesticated wheats have altered in the same way.

The second division comprises four groups that are now very distinct—the Hard or Macaroni wheats (*T. durum*), Emmer (*T. dicoccum*), the English or Rivet wheats (*T. turgidum*), and the Polish wheats (*T. polonicum*). Macaroni wheat has three or four fertile flowers to the spikelet, and has long bearded ears and hard pointed grains. It has been found in several Egyptian tombs, but not in the prehistoric remains of Central Europe. It occurs in Abyssinia and in India, accompanied in both cases by Emmer and forms intermediate between the two, and is grown in the European countries round the Mediterranean. It is used for the manufacture of macaroni and for blending with other wheats to make flour. Emmer is a bearded wheat, with two grains to the spikelet. It was one of the common cereals of ancient Egypt and occurs in quantities in Egyptian tombs of *c.* 2000 B.C., as well as in the Swiss lake dwellings of the Bronze age. It is cultivated in a few districts in South Germany, Switzerland, Spain, Italy, Servia, and in India and Abyssinia; it has also been introduced into Canada and the United States. *T. turgidum* (English or Rivet wheat) was another of the cereals of ancient Egypt, but little is known of its early history. The grains are large, short, plump, and may be as many as five on the spikelet. It is commonly grown in the Mediterranean district and is found in India. Although its quality is poor, it gives good crops and is therefore cultivated in England; it is the commonest bearded wheat we grow. Polish wheat is characterised by a marked development of the outer or involucral glumes, which are often an inch long and enclose all the flowers in the spikelet; the grains are very long and hard. It is probably ancient, as it is represented in Abyssinia by several marked races, but it is not mentioned till the seventeenth century. It gives poor yields, and is grown only in Abyssinia, Italy and Spain. Kœrnicke recognises it as a mutation from *T. durum*, while Schweinfurth shows that *T. durum* and Emmer had a common origin. Stapf has traced this origin to *T. dicoccoides*, a wild wheat found by Aaronsohn in Palestine.

The third division, economically much the most important, comprises all the common ordinary wheats grown for bread-making and grouped together as *T. vulgare*. Some of these are bearded, others not: bearded varieties usually predominate in hot, dry countries, and beardless ones in colder climates. The chaff may be red or white and the flour strong or weak. There is evidence that it is very ancient: some of the grains found among neolithic remains in Europe appear to be *T. vulgare*. This wheat has probably the same origin as *T. compactum*—the dwarf wheats, still grown in Germany, Switzerland, Turkestan, and India, the common ancestor being probably a small stout-grained wheat coming from Syria or Mesopotamia. So far, however, this ancestral form has not been discovered.

The fourth division consists of the Spelts. As was the case with Einkorn, the grains of Spelts do not readily detach themselves from the chaff; hence they do not thrash out like *T. vulgare*. Gradmann has adduced evidence to show that Spelt was the ordinary cereal of the Alamans, a group of Germanic tribes from whom the Romans obtained it. On the northern shores of the Black Sea there still occurs a species of *Ægilops cylindricum* or *Triticum cylindricum*, which seems to have been the original form of the Spelts.

### The Conditions Regulating the Growth of Wheat

In order that any plant may make satisfactory growth, six general conditions must be fulfilled. There must be sufficient food, water, warmth, air, light, and there must be an absence of poisons and pests. In the case of wheat there is a seventh special condition—the straw must be sufficiently strong to stand up or the plant will be beaten down by the rain and wind and cannot make proper growth or be harvested readily. As these conditions become more favourable the crop may increase, but any one of them may set a limit to its size.

One of the first problems investigated at Rothamsted was the food requirements of wheat. Experiments were begun in the Broadbalk field in 1843 to ascertain what substances must be added to the soil as manure. It was soon found that nitrogen and potassium compounds and phosphates were necessary, but even after sixty years there is no evidence that the other essential constituents — magnesia, iron, etc. — need be added, these always occurring in sufficient quantities in the soil. No great advance has since been made in this direction, and we have even yet but little direct knowledge of the part actually played by these various constituents in the metabolic processes going on in the plant. The general effects, however, are now fairly well ascertained. Thus it is known that nitrogen compounds tend to promote leaf development and vegetative growth, that phosphates promote root development in the early stages of the plant's life, while in the later stages they hasten the ripening processes; also that potassium compounds give the plant increased vigour, generally enabling it better to withstand adverse conditions of drought, wetness, rust attacks, etc. When no nitrogen compound is supplied there is of course no growth beyond the seedling stage, even though the other food-stuffs—phosphates, potassium compounds, etc.—are present in abundance, and the other conditions of growth are favourable. If a small quantity of an ammonium salt or a nitrate is added growth takes place, and as the nitrogen compound is increased, so the amount of growth increases. Some of the Rothamsted results are as follows:

Average Yields on Broadbalk Field, 1852–1907.

| | Plot 5. | Plot 6. | Plot 7. | Plot 8. |
|---|---|---|---|---|
| | lb. | lb. | lb. | lb. |
| Nitrogen supplied in manure . . | 0 | 43 | 86 | 129 |
| Total produce (straw and grain) . | 2315 | 3948 | 5833 | 7005 |
| Increase for each 43 lb. nitrogen . | — | 1633 | 1885 | 1172 |

The crop on Plot 5 has nothing beyond the nitrogenous material in the soil. To the other plots nitrogenous manure has been added, so that the crops get respectively 43, 86 and 129 lb. of nitrogen over and above what is present in the soil.

It might be supposed that the increased yields thus obtained should be simply proportional to the increased nitrogen supply, but the figures show that this is not the case, nor would it really be expected. The amount of food a plant takes up depends not only on the concentration of the food-stuff in the soil, but also on the extent of the absorbing root surface. The first increments of nitrogen increase the root system and therefore the absorbing surface, as well as the amount of material that each unit of this surface can take up. Hence the plant growth is more than proportional to the added nitrogen, and the second increment of nitrogen on Plot 7 produces a larger increase than the first increment on Plot 6. This increase, however, does not go on indefinitely. Some other factor, such as the water supply, that sufficed for the smaller crop no longer suffices for these larger ones, and thus sets a limit to the increase; further increments of nitrogen therefore give a smaller proportionate crop return. Still further additions of nitrogen introduce a new complication. The straw is not strong enough to carry these heavier crops, and they do not stand but become "laid," in which condition they are difficult and expensive to harvest.

The two general principles thus brought out hold not only for nitrogen manuring but for manuring generally. They have an important economic application and show that, up to a certain point, the better the farming the higher the profit; beyond this, the profit falls off. A scheme of manuring that pays when wheat is selling at 35*s.* a quarter may be unprofitable when wheat is only 30*s.* Certain complications may arise from interactions between the added manure and soil which modify the water and air supply, or from secondary actions of manures on the crop, but these details need not now be discussed.

The same general principles can be traced in the effects of water supply on the yield of wheat. Without water no crop is possible: with successive additions of water more and more crop is obtained; beyond a certain point, however, the increased yield is no longer proportional to the increased water supply and finally no further yield is obtained, no matter how much water is added, the limit being now set by some other factor. With excess of water there is an actual fall in the yield, not because the underlying law is different but because of secondary disturbing influences. A large amount of water in the soil necessarily diminishes the supply of air and keeps the temperature down, so that it is necessary to have some sort of balance between these three factors. Further, the plant itself does not always need the same amount of water. More water is wanted in the period of active growth than in the seedling stage or at the time of ripening. Thus, whenever wheat is sown in late autumn it has been known that wet winters are bad for the crop, while dry ones are good. "Under water, famine; under snow, bread," runs an old proverb. Shaw has recently shown that the relationship between autumn rainfall and crop-depression is almost mathematical. Over a period of years the average crop in England varies above or below a certain value in inverse proportion to the rainfall of September, October and November, the yield in bushels being—

$$39{\cdot}5 \text{ bushels} - \tfrac{5}{4} \times \text{rainfall in inches.}$$ [1]

The fatal effect of rain when the grain is forming or at harvest time has always vividly impressed itself on the farmer's mind. Anything indeed was better in old days than too much rain, and the dread of wet seasons reveals itself in a score of proverbs such as: "Drought never bred dearth in England." The depressing effect in both cases is complex but open to analysis: winter rains wash out the nitrates from the soil and thus militate against the development of a full root system, without which a heavy crop cannot be secured. Autumn rains keep the plant growing too long and retard the ripening process. The plant therefore becomes very liable to the attacks of various fungi; its straw does not stiffen, so that it becomes laid and is expensive to harvest; the grain, even when reaped, is not

[1] *Proc. Roy. Soc.* February 1905. The rainfall is for the 36th to 48th weeks inclusive. In *Proc. Roy. Soc.* May 1906 Dr. Shaw gives the formula for the eastern counties as 46 bushels – 2·2 × rainfall in inches over the same period. He further shows that in these counties the yield of wheat for the twenty-one years 1885 to 1905 is very nearly identical with the sequence given by a periodic variation of eleven years and its five harmonic components. This relationship is even closer than that shown by the rainfall.

fully mature and therefore loses in market value. It is not surprising, then, that the wheat regions of England are those of low autumn and winter rainfall—the eastern counties; here wheat has always been grown, even before history began.

The time when the wheat plant stands most in need of water is after the seedling stage is well passed but before the ripening processes begin. Thus in England a good rainfall coming in late spring to replenish the store of water in the soil is a not uncommon feature of a good wheat year. These water relationships are, however, best brought out in countries of very low rainfall, where the wheat is irrigated and where therefore the water supply is under control. The Punjab Irrigation papers contain a number of experiments showing that additions to the amount of water lead to crop increases up to a certain point but further water actually depresses the crop. It is upon this latter point that most Irrigation Departments conduct demonstration trials, since the cultivator almost invariably takes excess of water to the detriment both of the Department and himself.

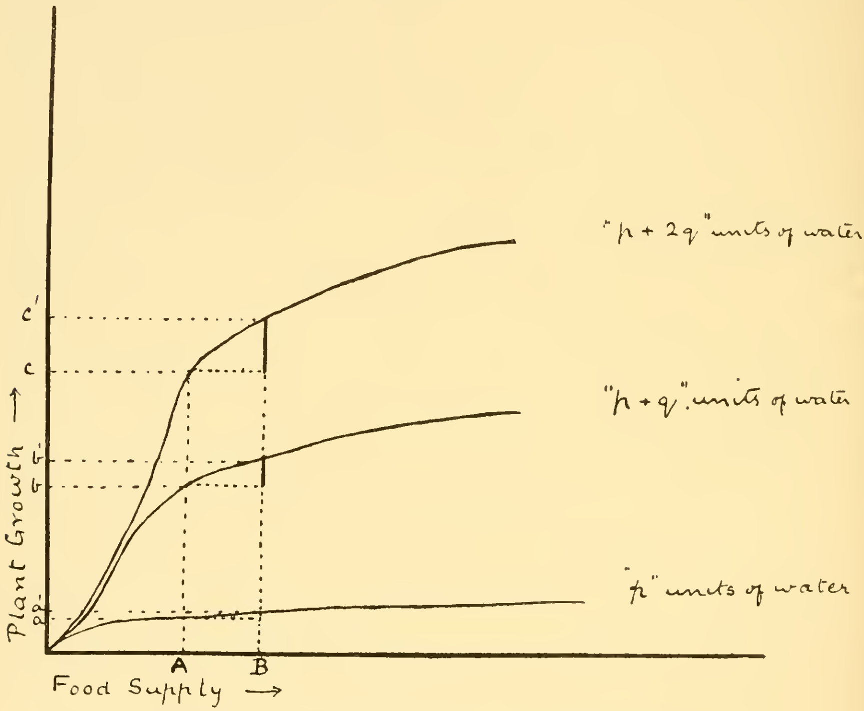
The extension of irrigation in India, Canada, the United States, South Africa and other countries is giving to these water-supply problems an economic importance they did not formerly possess. In three directions particularly is further information wanted from the plant physiologist. More detailed knowledge is required of the periods in the life of the plant when the need for water is greatest and of the effect produced on the plant by giving greater or less quantities of water. The relationship between water requirements and the supply of plant food should also be further investigated: it is known in general that manures by increasing the crop also increase the amount of water needed but not to a proportional extent, since it is found that manures economise the consumption of water by plants. Thus in Leather's experiments unmanured wheat transpired 850 lb. whilst manured wheat only transpired 550 lb. of water for every 1 lb. of dry matter formed. Lastly the effect on the grains of varying water supply requires to be studied; further reference to this will be made later on.

Perhaps even more important than the actual amount of water is the regularity of the supply, since plants will not tolerate great or sudden variations. Water stored in the soil is liable to loss by evaporation or by percolation, but both sources of loss can be diminished by suitable cultivation. The methods of doing this have been known in dry countries from very ancient time and have even in recent years been improved upon only in details. The Syrian peasant, no doubt following the practice of a civilisation long since passed away, adopts essentially the same methods as the modern "dry farmer" in the Western States. It cannot be said, however, that the underlying principles are yet thoroughly elucidated, the distribution and movement of water in the soil especially needing further study. From the economic point of view, however, the details of the methods are so far perfected that rain-water can be kept with comparatively little loss in the surface soil available to the plant, so that wheat is grown successfully in regions of 15 to 20 inches of rainfall or, if recourse be had to frequent fallowing, in regions of 10 to 15 inches only. Small dressings of phosphates are found to be extraordinarily effective in Australia, probably, as Hall has suggested, because they stimulate root production and thus enable the plant roots rapidly to get into the moist subsoil.

The temperature requirements of wheat, like the water requirements, differ at the various periods of its growth, but they have scarcely been investigated because of the experimental difficulties. In its early stages the plant will tolerate temperatures but little above the freezing point and it always survives a cold winter in England; it does not, however, survive the Western Canadian winter, so that wheat is always sown in the spring in that region, but at the time of ripening warmth is indispensable, and this circumstance, more perhaps than any others, sets a northern limit to the wheat belt of the world. Two methods are being used to extend the present limit. Early-ripening varieties are being produced that will be ready for harvesting before the short northern summer is over; much success is obtained by their use in Canada. In the north of England small quantities of nitrogenous manures are used in spring to cause early growth—for it is found that in presence of nitrates growth begins at a lower temperature than it otherwise would—and phosphatic manures are applied to hasten ripening. No serious efforts have yet been necessary to extend the southern limit set by high temperatures, but it seems probable that late varieties manured with potassium salts to enable them the better to withstand the adverse conditions could be grown much southward of the present line.

## The Part Played by the Soil

It has already been stated that the water, temperature, and air conditions of the soil are intimately bound up together, and that the effects of water supply and temperature depend on the food present. An ideal set of curves expressing this relationship in the case of water and food supply would resemble those in the figure: the temperature curves would probably be




Curves showing the nature of the relationship between plant growth, food supply and water supply.

similar. For a particular amount A of food-stuff present in the soil, the crop might be anything between *a* and *c* according to the water supply or the temperature; and an increase in food supply from A to B would lead in the first case to a small increase *aa′* and in the latter to a larger one *cc′*, corresponding to the vertical distances between the points *a* and *a′* and *c* and *c′* respectively. These varying increases in crop for the same dressing of manure may be taken as an indication of the "efficiency" of the various soils—a factor of great importance in determining their fertility. The following table may be given in illustration of this point, showing the yields obtained on a very good wheat soil in Sussex compared with two other soils less suited for wheat--one a light soil in Surrey, the other a loam at Rothamsted. The Sussex soil is not manured anything like as heavily as the Rothamsted soil and does not contain so much plant food, yet it invariably gives higher crops. The yields in bushels are:

| | Good wheat soil. | Soils less suited to wheat. | | | | |
|---|---|---|---|---|---|---|
| | | Rothamsted. | | | | |
| | A Sussex soil. | Unmanured. | Dung. | Complete artificial manures. Very heavy dressing. | Complete artificial manures. Lighter dressing. | A Surrey soil. |
| *Good Years:* | | | | | | |
| 1885 . . . | 54 | $15\frac{3}{8}$ | $40\frac{1}{8}$ | $36\frac{3}{4}$ | 22 | — |
| 1887 . . . | 50 | $14\frac{7}{8}$ | $34\frac{3}{4}$ | $34\frac{1}{2}$ | $23\frac{1}{4}$ | — |
| 1899 . . . | $52\frac{5}{8}$ | $12\frac{1}{8}$ | $42\frac{1}{2}$ | $39\frac{1}{8}$ | $18\frac{3}{4}$ | — |
| *Bad Years:* | | | | | | |
| 1879 . . . | 32 | $4\frac{3}{4}$ | 16 | $20\frac{1}{2}$ | $10\frac{1}{2}$ | — |
| 1892 . . . | 27 | $9\frac{3}{8}$ | $33\frac{3}{8}$ | $38\frac{1}{8}$ | 22 | — |
| Average of 5 years— | | | | | | |
| 1900–1904 . . | $43\frac{1}{2}$ | $9\frac{7}{8}$ | $33\frac{1}{4}$ | $38\frac{1}{2}$ | $19\frac{3}{8}$ | — |
| 1905–1909 . . | $41\frac{5}{8}$ | $12\frac{3}{4}$ | $38\frac{3}{8}$ | $40\frac{1}{2}$ | $24\frac{5}{8}$ | 35 |

It is characteristic of good wheat soils that they favour the production of stiff straw well set with corn. No amount of manure added to the Rothamsted or Surrey soil will give crops as large as those raised on the Sussex soil; the additional manure produces only a weak-strawed crop which cannot stand, and is therefore difficult to harvest. In countries of intensive culture, like England, the wheat-grower aims at producing, not the maximum crop that could possibly be produced, but the maximum crop that will stand up; the strength of the straw, therefore, commonly sets a limit to the size of the crop. It was at one time supposed that the strength of the straw was due to silica, but Lawes and Gilbert long ago disproved this view. Much further investigation is needed, since some of the most pressing of modern field problems centre round this point.

The exhaustion of virgin soils by continual wheat cropping has always attracted a great amount of interest, and has been carefully studied in several countries. It is satisfactorily established that the exhaustion is not due to the removal of material by the crop, but rather to bacterial or erosion changes that set in as soon as the soil is ploughed. So long as ground remains uncultivated and covered with grass or other vegetation that is not removed, the tendency of the bacterial change is in the direction of accumulating nitrogenous organic matter in the soil. Percolation of rain water and consequently loss of soluble matter is reduced to a minimum, and there is little surface erosion. But as soon as the land is cleared and ploughed erosion and leaching take place, and aeration becomes more thorough. The balance of bacterial change therefore shifts, the decomposition processes predominate, and the tendency now is for nitrogenous organic matter to decompose, giving off carbonic acid, ammonia, and, curiously enough, gaseous nitrogen. The ammonia is converted into nitrates, part of which is used by the plant, but part is lost. The annual losses of nitrogen from two virgin soils and the proportion recovered in the crop are as follows:

| | Total loss of nitrogen per annum, lb. per acre. | Nitrogen in crop. | Difference, being dead loss, lb. per acre. |
|---|---|---|---|
| Minnesota[1] . . . . | 170 | 37½ | 132½ |
| Indian Head, Sask.[2] . . | 100 | 32 | 68 |

Thus the cultivation of a virgin soil is a most extravagant process; it involves a great depletion of the stock of plant food, only one-third or one-quarter of which is utilised. This loss of nitrogenous organic matter reacts on the physical properties of the soil, and in course of time marked deterioration sets in. It is important to point out that no method of reducing these losses is known; they go on in any rich soil—in gardens, market gardens, heavily dunged fields, etc., and are the result of bacterial processes, not of crop growth. Of course as soon as cultivation ceases and the land is left in permanent grass and clover mixtures for a few years, the accumulation processes once more predominate and the soil is restored; the process may often be hastened by adding calcium phosphate. Thus it happens that soil deterioration ceases when, in the development

[1] Investigated by Snyder. [2] Investigated by Shutt.

of a new country, continuous wheat growing is displaced by a more general system of agriculture that includes the cultivation of grass and clover mixtures.

### The Quality and Composition of the Grain

The different varieties of wheats show considerable differences in the flour they yield. The exacting requirements of modern civilisation necessitate special sorts of wheat for special purposes. The baker, the confectioner, the biscuit-maker, all have their own requirements, and modern fastidiousness has put a price on subtle differences that were not recognised fifty years ago. Some very interesting problems have thus been opened up which are, as yet, far from being solved. In particular many investigations have been made to discover why certain flours—the so-called weak flours—only give small, squat, heavy-looking loaves, whilst others—the strong flours—will yield large, well-shaped, well-aerated loaves. The strong wheat commands the higher price, as the public insists on having the large loaf; whether it is intrinsically more valuable, whether it is more nutritive, has yet to be ascertained.

A. E. Humphries has set out the various properties which wheaten flour should possess in order to rank high in the miller's estimation. There are at least five involved: (1) "stability," the facility with which large masses of dough can be handled in the bakehouse; (2) the capacity for making a large quantity of bread from a given weight of flour; (3) the capacity for making large, well-piled loaves; (4) colour, it being desirable that both crumb and crust should be bright in appearance and not dingy; (5) flavour. These have not yet been correlated with the chemical composition of the flour, but a start has been made. An excellent critical summary of the present position of this problem was given by E. F. Armstrong at Winnipeg.

Perhaps most work has been done on the capacity for making large, shapely loaves which has usually been the definition of "strength" in wheat. This depends on two factors: (1) the gluten (the sticky, elastic part of the flour) must be capable of holding the bubbles of gas which give the necessary spongy texture; (2) there must be sufficient gas generated to distend the gluten to its utmost extent. The older hypothesis was that strength was regulated by the amount of gluten; the more gluten, the greater the gas-holding capacity, and therefore the greater the strength, but this view has proved inadequate. It is now known that the tenacity of gluten is conditioned by the presence of various salts or acids. Wood and Hardy have shown that gluten itself has no tenacity, this property depending on salts or acids contained in the flour or added in the process of working. Any salt confers cohesion on gluten; any acid or alkali, when sufficiently dilute, lessens it. The actual amount of gluten in flour is therefore only one factor in determining strength; account must also be taken of the acids and salts also present. No practical method has yet been devised for doing this but a beginning has been made by Wood.

The second factor in determining strength is the production of sufficient gas to keep the gluten fully distended. The gas is formed during panary fermentation, partly from the sugars already existing in the flour and partly from those produced by action of the diastatic enzyme of the flour on the starch. Three factors are here involved: the amount of sugar already present; the amount of diastase; the condition of the starch, especially as concerns its resistance to the attack of diastase. E. F. Armstrong points out that the sugar already present would not suffice to give the necessary volume of gas and in any case is of little importance, as sugar is found directly the flour is wetted. He shows also that ordinary flours contain more than enough diastase to produce the necessary sugar. There are flours, however, that do not, and these give better loaves when a little more is added as malt extract or in some other form. The condition of the starch must, therefore, be regarded as the most significant factor. The starch granules are each surrounded by an envelope which has to be disintegrated before the diastase begins its action. The larger the granule the greater becomes the liberation of starch when the envelope is destroyed; a flour containing 30 or 40 per cent of its starch as large granules—30 or 35 $\mu$—therefore more rapidly undergoes the necessary change than another in which only 10 per cent of the starch occurs in this form, the rest being present in smaller granules.

In the meantime while these problems are being cleared up, the percentage of nitrogen in the grain—which runs parallel to the percentage of gluten—is a rough indication of "strength" and in conjunction with other simple tests is taken as a first guide in selecting wheats for experimental purposes. But these indications are not regarded as final, and experiment stations working on wheat have arrangements for baking tests.

A parallel problem is also being investigated. How far does the "strength" of the grain depend on external conditions? It has been ascertained that strength is mainly an inherent property of the grain, Fife and its descendants being strong whether grown in England or in Canada. "Strength" cannot be imparted to a wheat which is normally weak by any method of manuring or cultivation. But strength is influenced by environment. Wheat grown on newly cleared scrub-land in part of North-west Canada is often more or less "weak," yielding a certain proportion of "piebald" grain, *i.e.* grain which, when cut with a penknife, shows whitish, opaque spots instead of the uniform, waxy brown appearance of strong wheat. This lowers its "grade" and its price. But when the soil has been in cultivation some time the "piebald" grain no longer appears and the market value improves. Weak "piebald" wheat contains less protein than strong hard wheat. F. T. Shutt has investigated this problem and obtained some very interesting and suggestive results. The wheat used as seed in one such district (Valley River, Dauphin district) was a strong wheat, grading No. 1 Northern and containing 11·11 per cent of protein. Grown on newly broken land it yielded a soft piebald grain of lower quality, containing only 9·93 per cent of protein. On land that had been in cultivation during nine years, however, it gave a hard grain, superior to the parent wheat, containing 12·62 per cent of protein. When in the next year the piebald grain was used as seed almost identical results were obtained; the grain grown on new land contained only 10 per cent of protein, whilst that grown on old land contained 13·5 per cent. Study of the soils *in situ* showed that the main difference lay in their water content: whereas the newly broken land contained 30–35 per cent of water between May and August, the old land during the same period containing from 13–23 per cent only; the lower amount during the period of grain formation. The deduction that high moisture content of the soil was responsible for the "soft" grain and low protein content was further tested on the irrigated land at Lethbridge, Southern Alberta. The original seed contained 15·95 per cent of protein; grown in unirrigated land the resulting grain contained 16·37 per cent of protein, but in that raised on irrigated land the percentage fell to 13·7 only. Howard has also found in India that over-watering gives rise to mottled grain and to samples very uneven in texture and therefore difficult to mill. In general long hot days and absence of excessive moisture during the later weeks of development increase the percentage of protein in the grain; where these conditions obtain wheat of high quality may always be expected.

The falling off in the percentage of protein in the piebald wheat from the moist soils is merely the result of an increased percentage of starch. It was long supposed that the protein moved into the grain rather sooner than the starch did; any prolongation of grain formation would therefore be accompanied by an excess of starch in the grain. This view explained very well a number of facts like those recorded above, but the recent investigations of Miss Brenchley and A. D. Hall threw grave doubts on its correctness. They found that the material transferred from the plant to the grain had much the same composition, both in the earlier and the later stages of ripening. The comparative poverty in starch of grain prematurely ripened by dryness, rust, or any other cause would have to be explained as the result of respiration continued after the filling-in process was completed, respiration of course leading to loss of starch but not of protein.

## The Economic Problem

The object of the grower is simply to produce the wheat giving the highest profit per acre whatever its intrinsic merits. In England the "weak" wheats are most profitable in spite of their lower price per bushel; "strong" wheats do not yield sufficiently heavy crops to pay. In Canada, on the other hand, "strong" wheats are the most profitable. Different varieties of wheat behave differently under different conditions of climate and soil, and perhaps the most pressing economic wheat problem of to-day is to find the varieties most suitable to each locality. Unfortunately nothing short of actual trial is sufficient, and so we find long tedious trials carried out in various countries to test the value of known varieties and to discover promising new ones. In the past some very striking improvements have been effected in this way. Square Heads' Master, for example, has proved so useful in England that it has displaced many of the older sorts; Fife, which was found quite by accident, is by much the most valuable of all known wheats for the northern parts of the great plains of North America; while Crimean or Turkey wheat has proved equally serviceable for the more southern part.

Newer varieties found at the Experimental Stations have not displaced these sorts from general cultivation but they have proved useful in regions where wheat would not grow before; thus Dr. W. Saunders and his son, working at Ottawa, have found early maturing strains of Fife and its crosses that promise to ripen farther north than present sorts and therefore to extend the northern limit of the Canadian wheat belt. Federation, now the standard wheat of South Australia, was bred by the late William Farrer.

At the present time vigorous attempts are being made to find wheats possessing "strength," heavy cropping power, early maturity, resistance to rust and drought. Two methods are being followed. The problem is being studied by Prof. Biffen at Cambridge on Mendelian lines and the mode of inheritance traced of the various characters involved. Elsewhere careful surveys are being made of the wheats already grown with a view to the isolation and study of pure types; such of these as possess important advantages over the ordinary varieties in use are carefully propagated. Mr. and Mrs. Howard are studying the Indian wheats in this way and Nilsson at Svalöf is similarly improving the Swedish wheats and other crops. Mass selection is of course given up; the unit for selection has to be a single ear. The process is tedious, because for a complete examination a considerable quantity of flour is wanted, but there is promise of simplification as it has been discovered that certain readily observed characters are correlated with others more difficult to determine. At Svalöf considerable use is made of these correlations and much time saved in consequence.

How far all this work will enable us to exceed our present yields of wheat per acre cannot yet be judged; it must certainly increase the dimensions of the present wheat belt and give greater certainty to farming practice. It is not too much to say that, when the virgin regions of the world are all inhabited, the total production of wheat will be limited only by the limit set to the plant-breeder's work.

# GIANT TORTOISES AND THEIR DISTRIBUTION

By R. LYDEKKER

From their abnormal bodily size, their restriction at the present day to two widely sundered groups of tropical islands, the number of species and the hosts of individuals by which they were represented a couple of centuries ago, the extent to which living specimens were carried by the old navigators to islands other than their own, the complete or impending extermination or extinction of many of the species and the great age to which they live, the land-tortoises commonly distinguished by the prefix "giant" have very naturally attracted, both on the part of naturalists and of the general public, an amount of interest and attention far in excess of that accorded to any of their smaller continental cousins. The interest attaching to these huge and heavily armoured reptiles is, however, by no means confined to the points indicated above. On the contrary, their study leads to questions of far wider and more important scope, such as the origin of races and species, the means by which island animals have reached their present habitats, and the division of islands into those of "oceanic" and those of "continental" origin. Nor is this all, for in Tertiary times, from the Lower Pliocene to the Eocene, giant tortoises, in place of being confined to one group of islands in the Indian Ocean and a second in the South Pacific, were distributed over all the great continents of the world, thus giving rise to the inquiry why they disappeared from the latter to survive in the former areas.

As a matter of fact, however, the term "giant tortoises" is a relative one, scarcely capable of very precise definition; for as regards bodily size one living African species, *Testudo calcarata*, approaches the smaller island forms; what is more important, it agrees very closely in the form of the forepart of the lower half of the shell, or plastron, which is produced into a pair of horn-like processes, with the huge extinct tortoise of the Siwalik Hills of North-western India, more fully referred to later. The aforesaid living African species is, however, of a light brownish yellow colour thereby differing markedly from all the modern insular giant tortoises, which, in addition to the length of their necks, are collectively characterised by their uniformly dark brown or black coloration. Whether the same funereal tint characterising the living species also obtained in the extinct Siwalik species cannot, of course, be determined; although from the similarity of its shell to that of the living *T. calcarata* it is quite probable that this was not the case. Other extinct tortoises, however, have shells more like those of the modern insular forms, with which they may therefore have agreed in colouring.

In modern times, that is to say during the historic and immediately preceding periods, giant tortoises, so far as can be ascertained, have been restricted to the Galapagos (Tortoise) Islands, situated on and immediately south of the equator, ten degrees west of the coast of Equador, in the South Pacific, and in the Indian Ocean to Madagascar, probably the Comoros, North and South Aldabra—small islands lying to the north-west of the northern point of Madagascar — the Mascarenes or Mascarenhas, situated to the east of Madagascar, and including Réunion, Mauritius and Rodriguez, and lastly the Amirantes and the Seychelles, which are the most northern of the whole assemblage, and only about four degrees south of the equator. The whole group is thus confined at the present day to the southern hemisphere.

From Madagascar the tortoises disappeared before the island was explored by naturalists; in the other islands of the Indian Ocean, as in those of the Galapagos group, most of the species survived till a very recent date and were in many instances abundant less than a century ago, as indeed some of them are in the Galapagos at the present day.

With the exception of Madagascar, where there were two probably contemporary species of hippopotamus, to say nothing of a small existing bush-pig and the half cat-like, half civet-like carnivore known as the fossa, the islands inhabited by modern giant tortoises were free from mammals of large size; and it has been suggested that this freedom from competition with the higher forms of life has been one, if not the chief, of the reasons why these sluggish reptiles should have survived and flourished in such situations long after their continental relatives had become extinct. Reasons are, however, given below for doubting this generalisation. It may be added, as not a little remarkable, that all the tortoises on these islands were of the giant type.

Giant tortoises were known long before the days of Linnæus, the first specimen to come to the notice of naturalists being apparently the upper half of a shell still preserved in the Paris Museum of Natural History, which was referred to so long ago as 1676 by Perrault, under the name of *la tortue des Indes*, and was believed to come from the Coromandel Coast. Later on the species represented by this historic specimen was named *Testudo indica*; whilst still later tortoises from both hemispheres were included under the same name. Eventually, however, it was ascertained that the real home of this earliest named member of the group was Mauritius, where the famous "Mare aux Songes" has yielded numbers of its semi-fossilised remains.

Testimony as to the numerical abundance of giant tortoises on the Galapagos, Aldabra and Mascarene Islands during the sixteenth and seventeenth centuries is afforded by the narratives of the navigators of those eras, from which a number of extracts have been given by Dr. Günther in the volume forming No. 5 in the list at the end of this article. In 1691, for instance, the French traveller, François Leguat, affirmed that in Rodriguez he saw droves of from two to three thousand tortoises, which were packed as close as sheep in a flock, so that it was quite possible to walk thirty yards or so by stepping from shell to shell without even touching the ground. So late as the year 1740 they were abundant in Mauritius, although not forming such closely serried ranks as in Rodriguez. By 1761, in which year Admiral Kempinfelt visited the last-named island, Mauritius would appear to have been well-nigh drained of its tortoises, for we read that a number of small vessels were employed in conveying these reptiles by thousands at a time to Port Louis, where their flesh was used as food in the hospitals. In addition to depletion by this means, the tortoises of all the islands in the Indian Ocean, as well as those of the Galapagos group, formed a valuable, and for a time inexhaustible, food-supply for vessels sailing in those latitudes. Such a continuous drain could have but one result in the case of the oft-visited islands of the Indian Ocean and all the tortoises eventually disappeared from the three Mascarene Islands. The Réunion species was the first to vanish, so that even its identification was for a long time a matter of difficulty; but of the Rodriguez forms a few shells are preserved, and there is the above-mentioned Paris example of the great *Testudo indica* of Mauritius.

That species, together with certain other less-well-known tortoises from the same island and Vosmaer's tortoise (*T. vosmaeri*) of Rodriguez, is distinguished by two specialised features of the horny plates on the front margin of the shell from all the other members of the giant group. Thus in the upper shell, or carapace, the small unpaired plate technically known as the nuchal shield, which is present in so many tortoises immediately above the neck, is completely absent, thereby permitting the first pair of large marginal shields to come into contact in the middle line. Similarly, the paired gular shields which in most tortoises cover the front point of the lower shell, or plastron, have coalesced into a single shield. Vosmaer's tortoise, which was first described in 1792 on the evidence of a shell in the museum at The Hague, is further peculiar on account of the extreme tenuity and fragile nature of the bony framework of the shell — a feature which has been attributed to scarcity of calcareous material in its habitat but which may well be due to the absence of need for protection against attack in island animals. This tenuity of shell reappears in certain Galapagos tortoises, all of which agree with the Mascarene species in their flattened heads and the absence of a nuchal shield to the upper shell, although they differ by the retention of paired gulars.

Of the Réunion tortoise, like those of the Comoro and Amirante Islands, very little is known; but it is noteworthy that certain remains from the Mascarenes indicate species with paired gular shields which were probably related to the long extinct Siwalik tortoise of India, of which more anon.

This, however, does not exhaust what I have to say in regard to the Mascarene species; for in June 1895 the Hon. Walter Rothschild received two living tortoises from the Seychelles, the smaller of which proved to agree with the Mascarene species in the absence of a nuchal shield, and probably came from one of the smaller islands of that group.

The Seychelles and Mauritius were indeed to a great extent "dumping places," to which tortoises were brought in former days from other islands, and maintained either as curiosities or for the sake of their eggs and young, if not for both reasons together; it is this "dumping" process which has rendered the unravelling of the history of the giant tortoises of the islands of the Indian Ocean such a difficult business.

By far the most famous of these transported specimens is the monster recently and, for all I know, still, living at the Royal Artillery Barracks, Port Louis, Mauritius. It formed one of a consignment of five brought to that island from the Seychelles in the year 1766 by the Chevalier Marion de Fresne, and accordingly known as Marion's tortoises—a name which, in the singular, may serve as the designation of the species they represent. At intervals three of these tortoises were taken to England, where they did not long survive, whilst a fourth, together with the one at the Artillery Barracks, remained in Mauritius.

In 1894 Mr. Rothschild obtained a photograph of the monster tortoise at Port Louis, which led him to suggest that it belonged to the aforesaid indigenous *Testudo indica.* This, however, had been previously shown by another naturalist, M. Théodore Sauzier, not to be the case, as this tortoise, although having no nuchal shield to the carapace, possesses divided gulars on the plastron; and it was accordingly described as a new species, under the name of *Testudo sumeirei.* In its divided gulars Marion's tortoise forms in some degree a connecting-link between the Mascarene and the under-mentioned Aldabra tortoises. This fact, coupled with the circumstance that five specimens were brought simultaneously from these islands, strongly suggests that Marion's tortoise is the species formerly indigenous to the Seychelles, where it was doubtless abundant in Marion's time. The Port Louis specimen has a shell measuring 40 inches in a straight line; since it is reported to have attained its present dimensions so along ago as 1810, it may be presumed to have been something like a century in age when brought to Mauritius nearly 150 years ago. When I last heard of it the veteran was reported to be blind but otherwise in good health and spirits. In the lack of a nuchal shield to the carapace, coupled with the presence of double gulars to the plastron, Marion's



The Giant Tortoise at Matara, Ceylon.

(From a photograph by Mr. Stanley Milliur.)

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tortoise resembles the great extinct Siwalik species, in which, however, the gulars and their subjacent bones are prolonged into horn-like processes.

Giant tortoises were also imported into Ceylon. Dr. Günther, in his *British Museum Catalogue*, quotes, for instance, the following paragraph from the *Ceylon Observer* of April 25, 1870, relating to one of these reptiles then living at Colombo: "We learn on good authority that the tortoise exhibited by Mr. Symons, of Uplands, the one which is so well known at the Mutwal end of the town, lived in the Uplands compound for between 150 and 200 years. It was sent from Java as a present to one of the Dutch governors here." In a later work[1] reference is made to a tortoise brought from the Seychelles to Ceylon in 1797 and stated to be living at Colombo. In this case, however, there is probably an error in regard to locality, and the tortoise in question is apparently the one still living at Matara, Ceylon, of which a notice, together with a reproduction from a photograph, appeared in *Country Life* of July 9, 1910. In this notice the reptile is stated to have been at Matara so long ago as about the end of the eighteenth century (a date which accords well with 1797); since then its continuous existence has been testified to by successive governors of Ceylon and other officials. In this tortoise the length of the shell is 53½ inches, only an inch and a half less than that of the great South Aldabra tortoise mentioned below.

By the courtesy of Mr. Mylius, I am enabled to reproduce (on the plate opposite) the photograph of the Matara tortoise, which shows that the species possesses a nuchal shield to the carapace and apparently paired gulars to the plastron. It appears also to have a shorter neck and a more rounded type of head than the Mascarene tortoises. All these features indicate that the Matara tortoise—which is distinguished from Marion's tortoise by the presence of a nuchal shield—came from the Aldabra group of islands and it is possible that it may be identical with the North Aldabra *Testudo gigantea*, although on a previous occasion I suggested its identity with *T. daudini* of South Aldabra.

From the preceding paragraph it will be gathered that the Aldabra tortoises are recognisable by their rounded heads and

[1] *Mostly Mammals*, by R. Lydekker, p. 337.

relatively short necks, combined with the presence of a nuchal and paired gular shields to the shell. To define the special characteristics of the two species from these islands would be out of place on the present occasion; but it may be mentioned that the North Aldabra *T. gigantea* (also known as *T. elephantina* and *T. hololissa*) has a very thick and massive shell, which may attain a length of 49 inches in a straight line and of 64½ inches over the curve. Of this species there are no examples now surviving in a wild state on Aldabra, but there are (or were recently) several living in the Seychelles, where they are kept in a semi-domesticated condition by the planters, and there is also a single well-known example in St. Helena; whilst, as mentioned above, the Matara tortoise in Ceylon may belong to this species. A fine specimen was also received in 1893 by Mr. Rothschild and kept for several years, at any rate, in the park and hothouses at Tring.

Daudin's tortoise (*Testudo daudini*) of South Aldabra, on the other hand, still survives in considerable numbers on its native island. This tortoise, which has a thinner shell, of somewhat peculiar shape, appears to be the largest existing member of the whole group, the shell of a specimen in the possession of Mr. Rothschild measuring 55 inches in a straight line. For a long period—it is said about 150 years, although there is some reason for doubting the accuracy of the statement—this tortoise, which is a male, lived on Egmont Island, in the Chagos group, lying to the south of the Maldives, whence it was carried to Mauritius and ultimately in 1897 to Tring. South Aldabra, which is a coral island and the largest of the group, is nowadays but little visited; it is also very difficult to traverse, so that it is no easy matter to see, let alone capture, the tortoises. Nevertheless, seven were taken and despatched to Europe in 1895, six of which reached their destination.

All the Aldabra tortoises present a curious structural peculiarity by which they are distinguished from those of the Mascarene and, with one exception, from those of the Galapagos Islands. This peculiarity consists in the third vertebra of the neck being biconvex, whereas in the other groups it is the fourth vertebra which is biconvex. Curiously enough, however, one of the Galapagos species, *T. galapagoënsis*, of Charles Island, as pointed out by Dr. Günther in the paper standing sixth on the list, agrees in this respect with the Aldabra tortoises. Such a peculiarity seems very unlikely to have been acquired independently in two widely sundered groups of islands; if indicative of community of descent, it is of great importance in regard to the mutual relationship of the species from the two oceans.

Turning to the tortoises of the Galapagos group, it may be premised that these have not suffered (or at all events had not done so a few years ago) to anything like the same extent from foraging expeditions by the crews of vessels as their relatives of the islands of the Indian Ocean. Nevertheless, some of the islands, such as Charles, Duncan and Chatham, have been denuded of their tortoises; although, after much trouble and no inconsiderable amount of more or less unavoidable error, it has been found possible to identify the species by which they were respectively inhabited. James Island, for instance, is known to have been the home of tortoises of a broad and flattened type, as distinct from the members of the saddle-backed group; it is therefore probable that it was inhabited by *T. nigrita*, while the allied *T. nigra* (= *elephantopus*) and *T. vicina* were natives of South Albemarle, and *T. microphyes* represented this group in North Albemarle. In one of his later papers Mr. Rothschild has described, under the name of *T. wallacei*, a large shell formerly in Bullock's Museum, Piccadilly, which there is every reason to believe came from Chatham Island. Of the so-called saddle-backed group, distinguished by the pinched-in form and extreme tenuity of the shell, which is scarcely thicker than paper, four specific representatives are known, namely, *T. ephippium* of Duncan Island, *T. becki* from North Albemarle, *T. abingdoni* from Abingdon and *T. galapagoënsis* from Charles Island, the last-named being the one referred to above as exhibiting a type of cervical vertebræ found elsewhere only in the Aldabra tortoises.

As a group, the Galapagos tortoises are distinguished by their long necks and limbs, the flattened head, the absence of a nuchal shield on the upper, and the paired gular shields on the lower shell. Except for the last-named feature, these tortoises are indeed very closely related to those of the Mascarene Islands, and therefore widely different from the round-headed Aldabra species; they are also distinguished, although in a somewhat less degree, from the double-gulared Seychelles species.

As regards the existence of three closely allied species on Albemarle, the largest island in the Galapagos group, it is important to mention that one of these, *T. vicina*, is found in the neighbourhood of Iguana Cove, on the south side of the island, whereas *T. microphyes* inhabits Tagus Cove, which lies to the north, separated from the first-named locality by a volcanic range absolutely unsurmountable by these reptiles. The precise portion of the island inhabited by the third species, *T. nigra*, does not appear to have been ascertained.

When Darwin visited Chatham Island, during the cruise of H.M.S. *Beagle*, he found tortoises abundant, and has given an admirable account of their habits.

Leaving the modern island species, both living and extinct, attention may now be directed to their continental relatives of earlier periods of the earth's history. The first of these made known to science was the gigantic Siwalik tortoise, *Testudo*, or *Colossochelys*, *atlas*, from the Lower Pliocene strata of the Siwalik Hills of India, described by Falconer in 1844. On a previous occasion I have expressed the opinion that the nearest living relative of this species is the Malay brown tortoise (*T. emys*) but I now believe this position to be occupied by the African spurred tortoise (*T. calcarata*), which is one of the largest existing continental species, and agrees with its gigantic extinct relative in the absence of a nuchal shield and also in the horn-like form of the front termination of the plastron. The Siwalik tortoise is by far the largest known member of the whole group, the length of the shell as restored by Falconer being no less than eight feet, although it is possible that there may be some exaggeration in the size of the restoration.

In those dark pre-Darwinian days, when the geographical distribution of animals was scarcely studied, the importance attaching to the discovery of the Siwalik tortoise was very imperfectly realised or, in fact, not realised at all. The discovery served, however to prove, firstly, that giant tortoises are not confined to islands; secondly, that during the later Tertiary periods they existed side by side with some of the largest land mammals the world has ever produced.

Later on remains of two giant tortoises (*T. leberonis* and *T. perpiniana*) were discovered in the Lower Pliocene deposits of the south of France; whilst those of other species have been obtained from the rock-fissures of Malta, from the Miocene of Nebraska and Patagonia, and other upper and middle Tertiary deposits. The Maltese species, it may be observed, is believed to have been nearly related to the extinct Mauritian *T. inepta*, while the two French species have been affiliated to the existing African *T. calcarata* and *T. pardalis*.

Last and most important of all is the giant extinct tortoise from the Upper Eocene strata of the Fayum district of Egypt described by Messrs. Andrews and Beadnell under the name of *Testudo ammon*, since this carries back the group to a much earlier epoch than any of the others. This species agrees with *T. atlas* in the absence of a nuchal shield and the divided gulars, although the latter are not produced into horn-like processes, a character in which the Egyptian species resembles *T. cautleyi*, another species from the Indian Siwaliks, and the modern *T. sumeirei* of the Seychelles. In the words of its describers, "it seems probable that in the present [Egyptian] species we have an early representative of a large group of tortoises, members of which occur at several horizons in the Tertiary beds of Europe, and of which [the African] *T. pardalis* and *T. calcarata* may be the modern forms. *T. atlas* and *T. cautleyi* of the Siwalik Hills and the existing *T. sumeirei* may also fall into this group."

From the foregoing survey it will be apparent that giant tortoises existed in Northern Africa in the later portion of the Eocene, and that during the Miocene and Pliocene divisions of the Tertiary period they were distributed over the greater part of the earth's surface. Further, these giant Tertiary tortoises, which were doubtless more or less nearly related to the modern insular forms, appear to have left two smaller descendants on the African continent, although these differ from the living island species by their variegated coloration.

Two points—one of which leads on to important problems in connection with the origin of the islands inhabited by modern giant tortoises—remain for consideration. Firstly, why did these reptiles disappear from all the continents, to survive in two widely sundered groups of tropical islands? secondly, how did they reach these islands?

As regards the first question, it is evident, as indicated above, that it was not the competition of large forms of mammalian life that led to the extinction of giant tortoises on the continents, since the largest of all—the Indian *Testudo atlas*—flourished alongside of a mammalian fauna comparable to that of modern

Africa in the palmy days of its animal inhabitants. Moreover, the Egyptian Eocene species, *Testudo ammon*, which was probably one of the earliest representatives of the entire group, lived in company with the gigantic horned ungulate *Arsinoitherium*, huge hyraxes, the ancestors of the modern elephants, and primitive carnivora as large as, if not larger than, wolves. Giant continental tortoises lived therefore throughout the whole of their known range in time in company with big mammals, including, during the Siwalik period, a tiger or lion fully equal in size to the existing species.

Of course it may be argued that the Siwalik tiger or lion, which was a larger and more powerful carnivore than any of the primitive species associated with the Egyptian Eocene *Testudo ammon*, may have killed off the gigantic contemporary tortoises, and that other big cat-like carnivora did the same for the other continental species. But it is inconceivable that such mail-clad reptiles, which could withdraw their heads with lightning-like rapidity into the shelter of their bucklers, could have been harmed in even the slightest degree by the biggest and most powerful carnivore that ever walked this earth. If, however, giant tortoises are safe from open attack by carnivorous mammals, it by no means follows that they are immune to the insidious operations of bacteria and trypanosomes; and in the absence of any other conceivable reason for their extinction it is possible, if not indeed probable, that their disappearance from the continental areas may be due to one or other of those minute but deadly organisms. It is true that it is somewhat difficult to understand how such diminutive enemies should have driven their attack home almost simultaneously throughout the world; but this is a difficulty which would occur in the case of every exterminating agent. It may be added that many naturalists now attribute the extinction of numerous groups of big animals, such as the ground-sloths of South America, to the attacks of parasites.

Whatever may have been the nature of the agency to which the old giant tortoises of the continents eventually succumbed, it is perfectly evident that their modern insular representatives, or rather the ancestors of the same, must have reached their habitats from the adjacent continents; this having been admitted by Dr. Günther in the case of the species of the Galapagos long before any extinct American members of the group were known. This being so, it remains to consider how the transit was effected.

Confining attention in the first instance to the Galapagos group, it has to be mentioned that both Darwin and Wallace are in accord in referring these islands to the "oceanic" type; that is to say, in regarding them as having been thrust up from the bed of the ocean, and therefore isolated throughout the whole period of their existence alike from one another and from the nearest mainland, so that the ancestors of their present scanty fauna arrived by crossing the sea.

Now the Galapagos group, which occupies an area of about 300 by 200 miles, comprises five large and twelve small islands composed of volcanic rocks, lying about 600 miles westward of the coast of Equador. With the exception of two or three species of rats and mice, their terrestrial vertebrate fauna consists solely of giant tortoises, with certain kinds of lizards and snakes of a South American type but in some cases representing peculiar genera.

To account for the presence of the tortoises, Dr. Wallace, on page 268 of *Island Life*, makes the following suggestion: "Considering the well-known tenacity of life of these animals, and the large number of allied forms which have aquatic or sub-aquatic habits, it is not a very extravagant supposition that some ancestral form, carried out to sea by a flood, was once or twice safely drifted as far as the Galapagos, and thus originated the races which now inhabit them."

This statement contains, I venture to think, one crucial fallacy, namely, that the ancestral Galapagos species was not a giant tortoise at all, whilst there appears to be a suggestion that it was not wholly terrestrial in habits. The fact that giant tortoises were distributed in Tertiary times over the greater portion of the warmer regions of the globe, coupled with the affinity—especially as regards colour—of the Galapagos tortoises, to some of those of the islands of the Indian Ocean, indicates, however, without reasonable doubt that the former were "giants" when they reached their present habitat. Further, the peculiarity in the structure of the cervical vertebræ of one of the species mentioned above—a peculiarity, it may be repeated, shared by the Aldabra forms—is highly suggestive that there were at least two ancestral species which reached the Galapagos from the South American mainland.

That at least one individual of two species of giant tortoises, which cannot swim in the proper sense of the term, although, as I am informed by my friend Mr. Pocock, they can float, which weigh several hundred pounds each when adult, could have been accidentally carried by currents across six hundred miles of sea, appears beyond the bounds of probability, not to say of possibility. If young individuals were transported on, say, floating timber, it would require two individuals of opposite sexes to be thus carried, before the race could be propagated.

This, however, is by no means all, for, as the late Dr. Baur observed in the paper cited below, on the accidental transport theory it is impossible to account for the modern Galapagos fauna. "We cannot explain why every, or nearly every island has its peculiar race or species, not represented on any other island. If some animals could be carried over hundreds of miles to the islands, why are they not carried from one island to the other? But besides that, how could we explain the presence of such peculiar forms as the gigantic land-tortoises, for instance? According to the elevation theory, we can only think of an accidental importation of these tortoises by some current, because they are unable to swim. After the islands had been elevated out of the sea, it happened once, by a peculiar accident, that a land-tortoise was carried over. Alone it could not propagate. This was only possible after a similar accident imported another specimen of *the same species*, of *the other sex*, to *the same island*. Or we could imagine that at the same time animals of both sexes were thus introduced. By this we could at least explain the population of a single island. But how did all the other islands become populated? To explain this we should have to invoke a thousand accidents.

"The most simple solution is given by the theory of subsidence. All the islands were formerly cônnected with each other, forming a single large island; subsidence kept on, and the single island was divided up into several islands. Every island developed, in the course of long periods, its peculiar races, because the conditions on these different islands were not absolutely identical."

In this passage it cannot fail to be noticed that in the line where he italicised several of the words the author has, no doubt inadvertently, somewhat unjustifiably strengthened his case. For it will be obvious that a female tortoise carrying fertilised eggs within her body would suffice to secure the propagation of her species on any land to which she might have been carried, were such carriage possible.

Nevertheless, the arguments of Dr. Baur appear conclusive as to the continental, as distinct from the oceanic, origin of the islands of the Galapagos group. That is to say, they formerly constituted a portion of the South American continent, from which they were subsequently sundered and split up by a great but gradual subsidence in the bed of the South Pacific. That Polynesia is a subsiding area is now generally admitted; and, without entering on the difficult question as to the existence of a land connection between South America and Australia during comparatively late geological times by way of Polynesia, it is quite probable that the severance of the Galapagos from Equador formed one of the later episodes of that subsidence. If I were asked to suggest an approximate date for the severance, I should select as the most probable the early part of the Miocene or the end of the Oligocene epoch.

Turning to the giant tortoises of the islands of the Indian Ocean, it may be premised that the arguments adduced by Dr. Baur in favour of a continental origin for the Galapagos will hold good also in this case. Madagascar, which, be it noted, was formerly the home of giant tortoises, is admitted on all sides to be a continental island, although opinions differ with regard to the probable date of its separation from the mainland. On the other hand, the Seychelles are regarded by Dr. Wallace as having been isolated from Madagascar since a very remote period, even if they were ever connected therewith, although they may have had ample opportunities of receiving from that island such immigrants as can cross narrow seas. Mauritius and Réunion (Bourbon) are, however, considered to be islands of antiquity which have never been connected with Madagascar, and it is probable that Aldabra would be included in the same category.

A later observer, Dr. F. H. Standing, whose opinions are based on the study of the subfossil, lemur-like mammals of Madagascar, has arrived at conclusions differing essentially from those of Dr. Wallace. He believes Madagascar to have been connected with the African mainland till the Miocene; also that during the early portion of the Tertiary period the connection between Africa and Brazil, which is indicated by several independent lines of evidence, was still in existence. He also quotes, on page 156 of the memoir cited, Messrs. Baron and Baker to the effect that botanical and geological evidence indicates not only the union of Madagascar with Africa during the Miocene, but likewise the inclusion of the Mascarenes and Seychelles, and doubtless, therefore, Aldabra, in the same land-mass. In other words the whole of the tortoise-islands of the Indian Ocean formerly constituted a portion of the African continent, just as the Galapagos group was at about the same date united to the South American mainland.

This, however, is not all, for Dr. Standing is of opinion that Africa (with which India was probably then connected) formed during the early portion of the Tertiary period a centre of dispersal, from which the ancestors of the South American monkeys and marmosets reached their present home. And it will be obvious that, if this theory be true, it will also serve to explain in a perfectly simple and thoroughly satisfactory manner the distribution and origin of the giant tortoises of the Galapagos and the islands of the Indian Ocean. This hypothesis will likewise account for the presence of species with a peculiar type of cervical vertebræ in localities so widely sundered as Charles Island in the Galapagos group and Aldabra in the Indian Ocean.

There is, of course, the alternative supposition that the giant tortoises of the Pacific and the Indian Ocean may have reached their present habitation by way of a land-bridge across Bering Strait, without travelling equatorially; but, in spite of anything I may have previously written to the contrary, I am now of opinion, in view of their comparatively near relationship, that radiation from an African centre of dispersal is the stronger and preferable explanation.

Whether, however, the first or the second explanation be accepted, it does not affect the opinion that all the modern tortoise-islands are of continental, as distinct from oceanic, origin; and that in each instance the tortoise-population has travelled overland from the adjacent continent, or rather that it is a remnant of the population which originally occupied each mainland (and, on the connection theory, the intermediate land-bridge).

To explain, on the theory of their continental origin, the absence of practically all terrestrial mammalian life from the Galapagos group and the smaller islands of the Indian Ocean is a task I am not on this occasion disposed to attempt. It may, however, be mentioned, that in the case of the Seychelles Dr. Wallace has suggested drowning out as a *vera causa* for the absence of mammals; but such a submergence would assuredly have been equally fatal to the tortoises which are now known to have been indigenous to that group, unless indeed they reached all the islands in the Indian Ocean at an epoch far too late to allow for their differentiation into insular species.

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# THE PROVIDENT USE OF COAL[1]

During my four months' stay on the American continent last year, the topic which came most prominently under my notice as of consequence was that of the Conservation of Natural Resources. It will some day be admitted, I think, that Mr. Roosevelt's most abiding claim to the gratitude of the world is that he has made this the subject of burning controversy in his country. To what extent are we alive to the fact that we have natural resources to conserve? Is the question ever asked here in Sheffield—in connexion with coal, for example?

I assume that it will be admitted by all who are capable of judging that we are improvident in our use of coal; only here and there are engineers and manufacturers being forced to economise on account of the constant advance in the cost of fuel. In far too many cases, coal is being consumed most wastefully—without any attention being paid to economy.

I do not propose to deal with the use of coal for industrial purposes generally. I shall confine my remarks to its consumption for domestic purposes and in the gas industry. And I shall be very brief, as my one object is to call public attention to a very simple issue.

In burning coal as we do in open grates, we not only burn it in the most wasteful manner possible but in such a way that we are a nuisance to ourselves and to our neighbours; the evil consequences are too apparent to need description. How are we to avoid them? Probably they are unavoidable as long as we burn bituminous coal.

But in burning bituminous coal we not only create a nuisance, we also waste much that is valuable: can we both save this and avoid creating a nuisance? The answer is, I believe, that we can—that in effecting the saving we shall also be taking the steps that will enable us to avoid creating the nuisance which now attends the use of bituminous coal as a domestic fuel.

During the early stages of combustion a variety of volatile,

[1] A communication read before Section B (Chemistry) of the British Association at Sheffield, September 1910.

inflammable substances are given off which burn with a smoky flame; by first coking the coal at a low temperature, we may remove and recover these and obtain a fuel which both takes fire and burns as readily as coal and on the average gives a better and hotter fire. By burning such soft coke in our towns we might get rid of the smoke nuisance, if not entirely, to a very large extent. In making such soft coke, we should separate from the coal substances of considerable value for a variety of purposes which are now wasted entirely. I am even prepared to go so far as to urge that such a policy be made compulsory at no distant date in our towns.

The subject has been in my thoughts during the past thirty years. In the early eighties I was led to pay much attention to the by-products of the manufacture of gas from oil, as practised by the various railway companies in making gas for compression to be used as an illuminant in railway carriages—an industry now somewhat in abeyance. I also studied the tars from the Jameson coke oven—in which coal was coked in such a manner that the volatile products were given off at a very low temperature compared with that prevailing in the gas retort.

In a note communicated to the Iron and Steel Institute in 1885 I ventured to insist that we knew practically nothing of what happens when coal is distilled or of the conditions most favourable to the production of the most valuable constituents of coal tar and that until we possessed accurate knowledge on such points the coking of coal and the manufacture of gas from coal could not be conducted scientifically. I urged that experiments should be made.

Nothing was done until the Coalite Company took the matter in hand recently. Coalite is simply soft coke formed by heating coal until all the volatile products which burn with a smoky flame are given off. I have availed myself of the opportunity which its manufacture affords to examine the products of the distillation of coal at temperatures perhaps not exceeding 800° C. The investigation is only in the early stage but I have already learnt enough to convince me that the tar obtained, as indeed was to be expected, is very different from ordinary gas-works tar, which is clearly a mixture of the end-products of numerous and complex changes undergone by the primary products of decomposition of the coal substance at high temperatures in contact with intensely heated carbon. The Coalite tar consists of the primary products of decomposition and of products of their interaction at relatively low temperatures.

It contains a not inconsiderable proportion of saturated hydrocarbons—in fact, of petroleum—together with unsaturated hydrocarbons of the olefine and acetylene series and a relatively small proportion of benzene and its homologues but no benzene-like hydrocarbons of higher series. All these hydrocarbons are most valuable solvent materials and may also be used in the internal combustion engine.

Phenols are present in far larger proportion than in ordinary coal tar, particularly the higher homologues of phenol. But little ammonia is produced during the distillation and basic substances appear to be less abundant than in gas tar.

To account for the presence of paraffins and unsaturated non-benzenoid hydrocarbons, it must be supposed that coal contains a very considerable quantity of "fatty matter" of some kind, as such matters are known to yield petroleum hydrocarbons on distillation. I am inclined to think that the benzenes are mainly synthetic products, formed by processes akin to those by which such hydrocarbons are produced in the manufacture of oil gas. The proportion in which the various homologues of benzene are present appears to be somewhat different from those in which they occur in ordinary coal tar. It is to be supposed that the phenols are derived directly from hydroxy-benzenoid compounds in the coal and it will be a matter of considerable interest to establish their nature. The greater proportion of benzenes in ordinary coal tar is probably due to the conversion of the phenols in large part into benzenes by the action of heated carbon.

The gas given off during the coking process is very rich. In the first place, I would advocate that this gas be substituted for the rubbish now produced by carbonising coal at very high temperatures so as to obtain the maximum possible yield of gas. It is absurd—no other expression is suitable—that the production of gas from coal in this manner should constitute a primary industry, especially as the coke which is produced is not suitable for ordinary domestic use.

It is time that the public realised that the article now supplied is beneath contempt and that a better article is at hand and can easily be supplied. Great improvements are in sight in the application of gas to heating purposes and it is to be expected that the use of gas will be largely extended in the near future; but if gas is to be more used it is essential that the quality be improved. Since the sulphur clauses in the Gas Acts were repealed, there has been a steady deterioration in the quality of gas and the damage done to furniture in consequence is now most serious. If the gas industry desire to retain its position within doors in competition with electricity, some action must be taken to supply a product free from the objections to which our present supply is subject.

Whilst low-temperature coal tar will yield a far larger supply of phenolic compounds suitable for use as disinfectants, it cannot well serve as a source of benzene and toluene, as these latter are mixed with so large a proportion of other hydrocarbons that it will not be possible to separate them economically.

Distinct uses must be found for the hydrocarbons in the lighter distillates. It will be possible, I believe, to make any class of solvent hydrocarbon that is desired from them without difficulty.

They should be valuable for cleaning purposes—far more so than ordinary petroleum spirit or petrol; as rubber solvents; and as substitutes for turpentine, the demand for which is now in excess of the supply. Much of the oil is also suitable for use in the internal combustion engine. It has been shown recently that the sterilisation of soil by means of toluene and similar substances has a most remarkable effect in increasing its fertility: should it prove practicable to sterilise the soil as a part of the ordinary farm practice, it is more than probable that a suitable agent will be found in the lower distillates from the Coalite tar.

My object in this brief communication is to direct attention to a provident use of coal which I believe deserves the most serious consideration by the public at large. I see no reason why the coal now used in the raw state by a community should not be first coked at a low temperature: the gas given off would be available as an illuminant and for heating purposes; the residual coke would be burnt with far greater efficiency than the original coal and without producing smoke. If washed coal were used, the sulphur would be largely eliminated and a still further improvement effected. A variety of by-products would also be obtained the sale of which should afford some if not considerable profit.

Science is of little public value if it cannot be brought to bear on such a problem; but it rests with the public to take action—some interest must be taken in the matter if the lethargy which now prevails and the vested interests which bar progress are to be overcome.

A brief but most instructive discussion took place after the above communication had been read.

Prof. Smithells agreed that the economical consumption of coal was a question of national importance. In the end, however, the question was one of economy, not in its large but in its small sense. The point to be proved was whether fuel of the type indicated could be produced and sold in competition with coal as put on the market under existing conditions. Amongst consumers of domestic fuel there was a widespread desire to contribute towards the abatement of the smoke nuisance. At present they were offered two fuels, practically speaking, coal and coal gas. If it could be shown that there was another fuel which had the advantage possessed by coal of giving a cheerful fire, which at the same time was not more expensive, he believed they would be ready to use it. But at present that had not been proved.

An ingenuous gas manager took exception to my objections to the quality of the gas now supplied. Complaints had not been more numerous since the repeal of the sulphur clauses.

Mr. Beilby pointed out that the estimated present annual consumption of coal for domestic purposes was about forty million tons and that the gas companies were distilling about fourteen million tons. It was obvious that if such large quantities were to be carbonised, not only must the gas industry be revolutionised but new uses would have to be found for the large quantities of gas produced and for the various by-products. On this account he thought that if coal were dealt with in the manner proposed ultimately only the fuel value could be realised for the greater part, as distinct uses could not well be found for so large a quantity of by-products.

Mr. Archbutt drew attention to Aitken's experiments proving that sulphurous acid caused the condensation of water from the vaporous state, so that smokeless fuel containing sulphur would still give rise to fogs, although these would be far less disagreeable. If people were aware of the evils caused by the sulphur in gas and coal, they would object strongly.

The explanation of the absence of special complaints of the quality of the gas now supplied by the gas companies is that people generally are too ignorant of the subject to appreciate the deterioration. If the reports which were made to the London County Council by their chemist on the gas supplied to the metropolis in the year or two following the repeal of the sulphur compound clauses are consulted, it will soon be realised to what an extent the sulphur in the gas was allowed to increase. Previously no effort was spared by those connected with the gas industry to remove the sulphur compounds. In making the change public interests were put aside for no good reason.

It is well known that the intrinsic illuminating efficiency of coal gas has been steadily reduced during the past few decades. The late Sir Edward Frankland, the premier authority on the subject, complained of this constantly; his son, Prof. P. F. Frankland, has also frequently taken exception to the quality of our modern gas supply. Burners have been improved, however, while the gas has been lowered in quality, so that the public have not been aware of the change; but as a consequence they have gained little from the improvements in burners. The situation has been saved for the gas companies of late years by the introduction of the Auer mantle and it has been the policy of many of the companies to encourage, if not enforce, its use, so that they might still further reduce the intrinsic illuminating power of the gas. Those who continue to use naked flame burners suffer considerably from the change. Now the lowering of the calorific power of gas is being advocated, which will involve the supply of a vastly larger quantity and the use of a large radiant for illuminating purposes.

To me it seems that the tendency is both wrong and impolitic. We need to copy the electric light and the acetylene lamp and use a small rather than a large radiant. And if gas is to be used more largely for domestic purposes, we shall need to minimise the size of the gas mains as far as possible.

If it be decided, on public grounds, to use gas of low intrinsic illuminating power, it will be easy to produce such gas economically by mixing the rich gas obtained by coking coal at relatively low temperatures with water gas; the present irrational method of manufacture should be abandoned, in any case. It would be necessary to make but simple alterations in the carbonising plant of our present gas works if such a change were to be made.

The production of coal gas is a chemical problem; the storage and delivery of gas is a mechanical problem. Unfortunately hitherto but scant attention has been paid to the chemical side of the industry; the control has been entirely in the hands of engineers who have not only not been conversant with the chemical problems and altogether destitute of chemical feeling but often actively antagonistic to the introduction of the chemist into the works. When admitted, the chemist has been kept in an entirely subordinate position. The industry is only now becoming alive to the fact that it must take some notice of chemistry if it is to progress. Gas managers have been led to look at matters entirely from the point of view of cost of production—questions of quality have been beyond their ken and understanding; they have been entirely in the hands of their engineers in such matters.

The issue I have raised is undoubtedly, as Mr. Beilby indicated, a very big one and one in which a variety of interests is involved. It is therefore essential that it be taken into consideration very seriously and in some public way. It cannot well be left to solve itself through the private operation of limited business interests.

In the first place there is the moral obligation we are under to be economical in our use of coal in the interest of future generations. Unfortunately such moral obligations count for little in our time. We can, however, appreciate the deplorable condition of smoke and fog to which our large towns are so often reduced through our use of bituminous fuel. The baneful effects are too well known to need statement but it may be pointed out that it is by no means improbable that, in a season like the present, much of the gloom which has affected us may have been conditioned by the products of combustion constantly passed into the atmosphere. It is only necessary to visit towns where smokeless fuel is used to appreciate the advantages it offers.

All the hydrocarbon oil we now use for internal combustion engines is imported. There is no doubt that by distilling coal at low temperatures we might obtain large quantities of material suitable for such purposes, of higher calorific value even than the petrol now used—far more than is to be had from coal tar as now made. Heavier oils, suitable probably for engines of the Diesel type, are also obtainable. Such products would have a considerably higher fuel value than coal. At no distant date, it may even be to the advantage of all large users of coal first to coke it at a low temperature and then to convert the coke into "producer-gas"—so as to separate all by-products of value, as well as realise the full fuel-value of the coal.

Low-temperature coal tar is not suitable for colour-making, as I have said. In the not-far-distant future, however, the tar obtained from coke ovens will furnish more of the materials needed by the colour-maker than is likely to be required. Distinct uses must therefore be found for the constituents of low-temperature tar.

We are enforcing a variety of sanitary provisions at the present day. I see no reason why one more should not be added—that of the sanitary use of coal, why pressure should not be brought to bear on the public to minimise the production of smoke and fog. But charity may well begin at home—one or other of the municipalities which has charge of its gas supply should set the example and secure powers to control the fuel supply of its district. We need bold experiment in this as in many other directions.

H. E. A.

# THE FUTURE OF THE BRITISH ASSOCIATION

The question is often asked, both among scientific men and by the general public, Of what use is the British Association—does it any longer possess functions which justify its existence? It is easy to suggest that it has become nothing more than a kind of glorified picnic, attended by some men of science as a shoppy holiday at which they can put in a certain amount of advertising, and by a miscellaneous camp following who look for cheap entertainments to support the boredom of keeping in touch with the development of modern thought. Real science, the critics say, is discussed elsewhere; they point convincingly to the fact that no investigator presents his discoveries to the world through the medium of its Annual Report. Of course the cynics have some justification: there is a cheap and nasty, even a greedy side to the British Association, just as there is to everything else that is big when viewed close at hand; but it is a narrow mind that can see nothing else in these annual meetings. Indeed it would be more correct to say that though this attitude was not uncommon among men of science a dozen years ago, it has been wearing away and has given place to a renewed sense of the value of a general gathering of men attached to all branches of knowledge. Science gets more and more specialised every year and in their working term men can barely find time to keep in touch with their own branch of the subject; they have found that it is good to exchange experiences with the inhabitants of other worlds which do interpenetrate, though for economy's sake in a too complex existence we habitually ignore their existence. It is the informal meetings that count, the hints and suggestions that are caught in conversation when botanist and chemist, physicist and physiologist gather together in smoke-room or lounge; even controversies which have become embittered upon paper begin to clear over coffee and a cigarette. It was ably argued at the Sheffield meeting that this turn in the opinion among scientific men has come since the inauguration of the overseas meetings; certainly no better opportunities for real discussion and exchange of ideas can be found than on these long journeys. To the youngster who is struggling to get some work done in science there can be no more fruitful or encouraging experience than one of these visits, and the British Association is not unmindful of its duty of helping such men to join its parties.

But if the scientific man is satisfied with the British Association the general public would seem to be blowing more coldly; the numbers of members do not grow as they might be expected to do and though the press still gives the meetings a front place and dredges the sectional proceedings for anything that can be trimmed up to the pitch of a headline, there are not wanting signs of criticism. *The Times* indeed has this year deliberately asked each section to justify its existence and show cause for the attention (and hospitality) demanded from the public, not without hints that the whole proceedings seem barbarously technical and pedantic. Now it is easy to repudiate the opinion that the public and especially the press has any voice in the matter, to maintain the rights of the man of science to manage his own affairs in his own way and insist on good honest doctrine with no backslidings towards the popular. Indeed the efforts of some of the newspapers to manufacture duly sensational copy make such a high and dry view only too congenial. But is there not something in this demand, are we not even in some degree responsible for the coruscations of the cheaper press, which must sensationalise out of sheer despair at not finding better pabulum? The public is pathetically anxious for information; the British Association, without in the least hurting its dignity or declining from its purist position, might on these occasions give it a great deal. The meetings are after all of the British Association for the Advancement of Science, the subscriptions are mainly provided by the general public and the great towns one after the other are drawn upon for their hospitality; it cannot be argued that we have no duty to the public. And while this is a positive duty, on the negative side we can say that the British Association is no place for papers by the pure specialist; every science has nowadays its own society, its own journals for every branch of the subject—these are the recognised gathering grounds for the expert and there is no suggestion that they do not function properly. But the British

Association brings together representatives of all the sciences and some of the arts, it invites the public to attend; it ought then to speak of science in its broad and general aspects where all interests meet. If we accept this point of view there are two directions in which reform is called for.

Let us begin with the Presidential Address, because here the question of duty to the public comes to a head. At the outset the reader must bear in mind that no person is being criticised, only a system. Now every one of us wants to see science of more account in the affairs of the nation; we believe that its results would be valuable, we believe still more that its method would be fruitful in the governing class just as its discipline is necessary to the members of a democratic state. Once in each year Science is put in the chair and given a chance of addressing the whole English-speaking people. Every newspaper of any standing, at home or in the Colonies, reports the President's Address to the British Association, and every newspaper will give it a leader if it possibly can make one up on the text supplied. It is an unrivalled opportunity; yet some unwritten tradition has impelled President after President to a minute and technical discourse on his own special branch of science. We know our President of this year has interests in other camps and has achieved distinction in very different fields than that of pure science, yet led by precedent he ignored the public and deliberately spoke to some score of geologists in the room and a few hundreds outside. No one wants mere popularising from our President, still less flap-doodle about science and religion and the Empire; there is, however, a plane on which all modes of thought meet and we may respectfully ask our presidents to set out to show us in some way how their subject merges in the general stream of opinion and how it bears upon life. If it cannot do this, science is either a tool for the service of some mechanic art or an intellectual blind alley, like learning Basque or constructing Greek verses, fascinating to its devotees but having no place in the history of the mind. Indeed the result of giving such prominence to purely technical addresses is disastrous; the man in the street may be impressed by the revelation of his own ignorance but those people whose opinions do count, who in this country be it remembered have received a literary training, are confirmed in their contemptuous belief that men of science are mere grammarians, useful perhaps in their sphere but having no contact with the greater things of the mind.

Of course we have among us scholars who are worthy of any honour Science can pay them but who have so bent every faculty to their special task that they have deliberately foregone any wider outlook on the world: let us honour such men indeed in our learned societies but not make them Presidents of the British Association. The Council cannot secure from any man an address that will be generally impressive but it can let it be understood that it expects its President to speak not to his immediate colleagues but to all men of science and to thinkers at large; it can also keep this desideratum in view when making its choice.

The other possible reform deals with the sectional meetings. It is well known that there are many cross-currents of opinion as to whether the sections are being run on proper lines. We know the general style: there is the regular frequenter of the Association who feels it his duty to contribute and either dishes up a survey of a series of old papers or gives a preliminary account of some new work, informally and without the responsibility of cold print to follow; this latter plan often leads to stimulating private discussions. There is the local man of science who welcomes the chance of a wider audience and the young worker who is getting himself known; also bores of various calibre and staying power. Sometimes there are too few papers and a general languor, more often they are too many and too long, until all discussion has to be stifled; anyhow the result is apt to be purely technical and the general public haunt Anthropology or Geography or Education; the other sections only when lantern slides are promised. Now it cannot be argued that any useful purpose is served by making B a second-hand edition of the Chemical Society: what can be done instead?

It is known that one party in the Association would like to see the sections very much reduced in numbers; it points to the success of joint meetings arranged to discuss a subject of common interest and it would like to impose some wide and permanent connexions of the same kind, as salutary to all concerned and appropriate to the British Association. The other party believes in keeping bright the sacred flame by walling it in; it declares that only the specialists can get up enthusiasm and that on the occasion of a joint meeting one or other section stays away *en bloc* except for its officers.

Now we must admit that the Society has always been organised by sections and that this plan has worked for more than half a century; it will not be well to destroy it until we are sure that we have something to put in its place which will also work. Like scientific men let us experiment; let the Council borrow one day or two days from the Sections, take a list of subjects—half a dozen or more which will unite several interests at each—and invite a set of speakers, who will know that something considerable is expected from them and that they are to play leading parts in a full-dress debate. The organisation would mean some work but suggestions might come up from the sectional committees and the results would not be without value both to the participants and to the rank and file. Thus a beginning could be made with the grouping idea, to see if it increases the interest of the professionals and attracts the public at large.

The next point is that we owe some duty to the local members; we might explain to them with the authority a stranger possesses how Science bears upon their own community. It is true that many Sections set aside one meeting for a discussion of some subject of local interest—such as steel-making at Sheffield—and that one of the public lectures is selected to the same end; much more might be done in this direction. Let the Council again pick out eight or ten men, not necessarily one for each section but thereabouts, who shall expound the local aspect of their subject simply and untechnically. The "Guide" is mostly a valuable production but how much more idea of say the geology of the district should we gain if it were also expounded to us *viva voce* by a master! These lectures need not be tawdry or clap-trap in any way, they need not even be elementary; they must be addressed to the layman, that sort of layman which each of us feels himself to be when he enters an alien section.

Of course it will be argued at once that this is not advancing science, but at least it is advancing the appreciation of science and any advancement of science in its narrowest sense that goes on at the sectional meetings can easily be compressed into half the time now given up to them.

Its expense may be urged against this proposal, for it might be desirable to pay for the address; yet if we owe a duty to the public we must face the expense even if some of the grants to committees be thereby curtailed; besides such a procedure is very likely to repay itself fourfold in an increased membership. We sneer at people joining the Association for its garden parties and yet we offer the ordinary educated man or woman very little more that is comprehensible.

The writer then ventures to suggest that the British Association may well consider some such policy of reform—a policy which shall recognise on the one hand that the special opportunity of the British Association is to emphasise the unity and common interests of the different sciences, on the other hand that it has still got a mission to educate the general public.

We agree that the Association is full of vitality; let us make provision for future growth both in the interests of the professional and of the layman. It would be unwise to attempt the amalgamation of the sections but the Council might well take command a little more and to some extent control their proceedings to a common end.

Finally, though the writer cannot claim any very long-standing membership of the Association he is only too glad to acknowledge how much he has gained thereby; it is in recognition of the obligations he is under and not in any carping spirit that this has been written.

"M. A."

# REVIEWS

**The Relations between Chemical Constitution and some Physical Properties.** By SAMUEL SMILES, D.Sc. [Pp. xiv + 583.] (London: Longmans, Green & Co., 1910. Price 14*s*.)

THE author of the work before us bears a name honoured in literary circles and he has executed the difficult task of compiling an account of researches on the relation between physical properties and structure in a way that must add to the reputation the name enjoys. Although somewhat costly and voluminous—too costly and too voluminous indeed to suit the requirements of the great majority of students—the book is one that should prove attractive on account of the clearness of the author's style and the impartiality he displays in setting out the conclusions arrived at by various workers; as an introduction to the study of a very difficult but all-important subject, it is a useful addition to our chemical literature—useful particularly in showing how very far we are, in most cases, from understanding the nature of the correlation between structure and properties, how careful we should be in interpreting physical properties.

The fault to be found with it is that from which all such works, as a rule, must suffer—the indeterminate character of the conclusions arrived at and the tendency to take arguments seriously which have been put forward by immature workers possessed by an idea but without breadth of knowledge and the saving grace of modesty. This is particularly noticeable in the section on the absorption of light, a section to be taken with many grains of salt by the student, although it is full of interest as a concise summary of the work done. It is obvious that Dr. Smiles has allowed himself to be somewhat biassed by environmental influences.

It is only after writing such a book and then spending years in repentance—in reflecting on what he has written and on the inadequacy of most of the opinions he has offered—that an author becomes competent to write a critical account of subjects such as are dealt with by Dr. Smiles. Far too much of the physical work of the day is in the hands of amateur specialists whose main qualification is their enthusiasm and their desire to achieve a reputation: naturally enough they rarely allow modesty to stand in their way; nevertheless we are forced to regard their efforts with a certain degree of approval, no other recognised way being open to them of educating themselves into efficiency, but we need not always take their conclusions seriously.

Literary work is of the very greatest value as training but it should be followed by a close time of penitence and devotion to practical exercises. This book is of such promise that we venture to express the hope that Dr. Smiles will now lay aside his pen for a time and take up the investigation of some one or other of the fundamental problems to which he calls attention; meanwhile he will probably find it of great advantage to take part in a few serious law cases, so as to acquire the habit of cross-examining every opinion offered before committing himself to its endorsement. Such training is invaluable and to be had in no other way: our present didactic system of teaching offers no opportunity of developing truly critical and logical habits of mind. The value of these habits, the need of exercising them, are only too obvious when our scientific text-books are studied.

If written in a more critical spirit, the book under notice might easily have been much shortened—perhaps to half its length ; the effect would have been at least to double its value—to make it really worth fourteen silver shillings, which it is not in its present form.

Almost every page of the book affords a subject for discussion. Perhaps one of its most striking features is the evidence it affords of the slowness with which ideas penetrate, of the difficulty we have in putting two and two together, of our unwillingness to draw general conclusions. The fact that, speaking generally, there are no such things as physical constants — that physical constants are usually dependent variables—should have been insisted upon ; and the improbability that we shall ever arrive at true atomic constants should have been made clear. Far too little attention is drawn to the effect of the extra-molecular unions conditioned by residual affinity ; the complexity of the phenomena to be analysed is in no way made sufficiently obvious. These are matters to which the attention of students should be directed from the outset, so as to guard them from the belief that it is likely that we shall ever be in the position to arrive at final conclusions.

It is stated in the introduction that the distinctive feature of the past twenty-five years has been the growth of dynamical theories of structure and the student is left to believe that such theories have been more or less justified. The exact contrary is the case. The fact is we are becoming daily more and more convinced of the structural stability of compounds if only they are in a pure condition ; and the conversion of isodynamic compounds into one another has been clearly proved to depend on their inclusion within a complex system. Even those who of late years have been most prominent advocates of "wobble" admit that their arguments have broken down—although they have not had the courage to withdraw their contentions publicly. Much of the difficulty arises from the fact that chemists are in the habit of interpreting "structural formulæ" too literally. Such formulæ are very largely symbolic of function rather than of absolute molecular structure. The objection to be taken to Kekulé's formula of benzene, for example, is that it *does not* picture the chemical behaviour of benzene, inasmuch as it represents it as an eminently unsaturated compound—which it is not. The centric formula is preferable from this point of view. The real reason why Kekulé's formula has been and still is popular is that it is the formula proposed by Kekulé ; chemists are human in respecting authority. The existence of two isodynamic forms of benzene was postulated to save the formula, not to satisfy the facts. Again it is absurd to say that "there are many substances which cannot be satisfactorily represented by the ordinary structural formulæ, since they react sometimes according to one structure and sometimes according to another." Such is not the fact. Nothing is easier than to represent the alternative behaviour of a compound such as ethylic acetoacetate, for example, if only it be recognised that—as pointed out above—interaction occurs in a complex system and that the passage from the one form into the other takes place with utmost facility, given the determining agent. These are matters which should have been discussed more carefully, as they are fundamental.

As another illustration of the manner in which the student may be misled, reference may be made to the section on the Nature of Colour. Exception is taken to the term "colour" as inexact. This is an illustration of the manner in which, at the present day, we are attempting to improve on ordinary speech and by so doing introducing confusion. Colour is something visible to the normal eye and no word has a more definite connotation. In discussing the "origin of colour," the object has been to discover what types of structure condition colour obvious to the eye—the problem being one of special interest from the physiological as well as the chemical standpoint. The importation of the all-seeing spectroscope into the discussion makes an already complex problem one of hopeless complexity, as every substance apparently has some absorptive power. Within the visible region, the spectrocope has enabled us to learn little, if anything, more than we have learnt with the aid of our eyes alone as to the "origin of colour"; at most we have ascertained with its aid that a few substances are visibly coloured, the colour of which had escaped detection owing to the fact that they are but slightly absorptive and had not been examined in sufficient thickness to permit of the eye appreciating their colour. Sufficient for the day is the evil thereof—to many investigators of colour problems; there is no reason to disturb their simplicity of belief in the human eye, nor to lay superhuman tasks before the contemplation of the student.

So long as the electron *is* but "a figment of the physicist's imagination" (p. 545)—it cannot well be claimed that it is more at present, and Dr. Smiles shows a surprising lack of sense of proportion in referring to the atomic theory in similar terms—it were better to refrain from advocating its adaptability to the problems of valency; the student should not at present be misled into believing that any real progress has been made in applying electronic conceptions to the explanation of chemical phenomena; he should rather be taught that what has been advanced is but superficial paraphrase of current chemical conceptions and that we are still in a position of "as you were" of knowing nothing of first causes in chemistry.

Read, mark, learn and inwardly digest—above all, inwardly digest—is the caution to be given to all who would ponder on the interconnexion of structure and properties. The present-day tendency is to bolt food; we need a return to the chewing habit in science perhaps more than in ordinary life.

**The Mutation Theory.** Experiments and Observations on the Origin of Species in the Vegetable Kingdom. By HUGO DE VRIES, Professor of Botany at Amsterdam. Translated by PROF. J. B. FARMER and A. D. DARBISHIRE. Vol. I. The Origin of Species by Mutation. [Pp. xv + 582.] (London: Kegan Paul, Trench, Trübner & Co., Ltd., 1910. Price 18*s.* net.)

IT is a satisfaction to have an English translation of the original documents of Prof. De Vries's *Mutation Theory*, and our feeling of satisfaction is heightened when we find a translation by two experts who have themselves contributed notably to the investigation of evolution-problems. They have not spared pains to give us in pleasant English a reliable version of this great, but somewhat difficult book, and they appear to us to have been singularly successful in their rendering of the original. In fact, we prefer the translation to the original. Any alterations that have been made have been examined and approved by the author. We offer our congratulations and thanks to Messrs. Farmer and Darbishire. To mix little things with great, we cannot say that we admire the contraction "Pro" which occurs in front of Professor Farmer's name on the title-page of the copy sent to us.

Prof. De Vries's *Die Mutationstheorie* began to appear in 1900 and it was soon recognised as a landmark. As the translators justly observe, it set forth "a successful attempt to bring the process of specific differentiation within the sphere of experimental inquiry." It need not be said that it is extremely desirable to have a detailed knowledge of a book like this, but we are not prepared to follow the translators' preface in its suggestion that this is a *sine qua non* for intelligent participation in a discussion of modern evolutionary questions. For life is short and a book in German is apt to be long, and we take it that Prof. De Vries had at least considerable opportunity of making his meaning clear in *Species and Varieties, their Origin by Mutation*, which appeared in English in 1905. On the other hand, the subject is one of extreme difficulty, the author has much that is new to contribute, he uses some terms in a sense peculiarly his own, so that it is of great advantage to have the detailed evidence before us. In any case, the evidence collected by De Vries up to 1901, bearing on the question of the origin of species and varieties by mutation, is now for the first time available to the student of evolution who cannot or will not read German.

The book before us is certainly an addition to the not very crowded shelf of great books on organic evolution, and we envy those who make its first acquaintance in its English dress. They will be interested in the story of the author's experiments, the inferences that he has drawn from them and from the behaviour of *Oenothera lamarckiana* in particular, his divergence from Darwin and the selectionists, and his fundamental contrast (so difficult, it seems to us, to work out) between mutations and fluctuations. Every here and there we must confess that we find some special point that we had overlooked before—for instance, the author's *obiter dictum* that it may be change of environment that pulls the trigger of mutations, for which "we can as yet assign no cause," which emerge from the arcana of the germ-plasm, seemingly independent of the environment. We may briefly refer to what the author singles out as the most important results of the experimental work in which he has taken such a large share. He declares that the principle of unit-characters "has gained almost universal acceptance, though there are still some authors, especially among zoologists, who are opposed to it." A clearer understanding of the processes of selection in plant-breeding leads to a recognition of the elementary species as the real material for artificial and natural selection, a view which has been "corroborated in convincing manner by the work of Nilsson and of Korschinsky." Secondly, De Vries points to his detailed experimental evidence, now made more available by the translation before us. He refers especially to "the observed origin of *Oenothera gigas*, which appeared suddenly in my cultures in the year 1895, and possessed, at its first origin, all the attributes of a new species, including constancy and even a double number of chromosomes in its nuclei." Thirdly, he refers to "the new light thrown by the principle of the unit-characters on the work of Mendel, neglected up to that time." Nowadays it seems rather the other way round, that the work of Mendelians throws light on the idea of unit-characters. "The work of Bateson and of his school, of Cuénot, Webber and many others, but above all that of Davenport, have since brought the principle of unit-characters to its now prominent rank in the study of hybridization."

J. ARTHUR THOMSON.

**Reports on the Geophysics, Geology, Zoology, and Botany of the Islands lying to the South of New Zealand,** based mainly on Observations and Collections made during an Expedition in the Government steamer *Hinemoa* (Captain J. Bellows), in November 1907. Edited by CHARLES CHILTON, M.A., D.SC. In two volumes. [Vol i. pp. xxxv + 388; vol. ii. pp. 389–848.] (Wellington, N.Z. Published by the Philosophic Institute of Canterbury, 1909. Price 42*s.* net.)

LYING to the south and south and south-east of New Zealand there are scattered several small, for the most part uninhabited, islands which, in the old days of the southern whaling and sealing industries, were the scene of some activity, but since the decay of the trade these remote islands are little thought of except as the rather frequent occasion of a wreck brings them before the public mind. The wreck of the *Dundonald* is sufficiently recent to be remembered by many; the vessel went to pieces on Disappointment Island in 1907, twelve of the crew being drowned; the remaining fifteen managed to subsist for seven months on the island, and then to cross over to Auckland Island, where a Government depôt of supplies and necessaries was found, and here they remained until, a month later, the *Hinemoa*, with the Scientific Expedition whose results we are about to examine, found and rescued them. In the historical sketch of the islands by Prof. Chilton in the first of the volumes before us, will be found an excellent account of the vicissitudes of the sealing industry and of the many vessels that have been wrecked on these stormy and rocky coasts. On many of the islands there are still remnants of these wrecks and of the huts built by the marooned sailors; the beautiful photographs scattered through the volumes before us of these remains and of the wild coast scenery and vegetation lend a picturesque and almost romantic interest to these scientific reports.

The islands visited by the Expedition were the Snares, Auckland Islands, and Campbell Island, the latter being the most remote from New Zealand, a distance of about 400 miles, but still within the 1,000-fathom line. The most distant outlier of all, almost half-way between New Zealand and the Antarctic shelf, is the comparatively large Macquarie Island, which was not visited on this occasion and is very little known either geologically or faunistically. This island lies in the 2,000-fathom limit and its thorough exploration is very desirable, as it is likely to throw further light on many problems of geographical distribution.

The excellent photographs and descriptions in Prof. Chilton's report enable us to gain a good idea of the general features of the islands. Wind-swept, damp, cold, and with very little sunshine, many of them are little more than barren, treeless rocks, the breeding haunts of albatrosses, penguins, and other sea birds; but Auckland Island, with precipitous cliffs to the west, affords good shelter on its east and south coasts to shipping (Carnley Harbour and Port Ross), while its inland slopes and valleys support a thick though stunted forest vegetation, and its hills rise to over 2,000 feet in height. The principal constituent of the forest is the stunted and twisted Rata, one of the Myrtaceæ, *Metrosideros lucida*, while in the undergrowth are tree ferns, which here reach their southern limit. A forest formation is also provided by *Olearia lyallii* mixed with tree Senecios. Some of the open meadow lands appear to be of great beauty owing to the large and conspicuous flowers borne by many of the plants, such as the large-leaved Pleurophyllums, which may carry large purple blooms.

Campbell Island is more treeless than Auckland Island, owing to its being more exposed and wind-swept, but a forest scrub is formed by a Dracophyllum. Some of the upland meadows support a rich tussock grass, Poa, and these meadows are browsed down by the sheep which are still run on this island. Of the other domestic animals that have been placed on the island from time to time, the principal survivors, besides rabbits, are the pigs, which have become feral on the Auckland Islands and appear to thrive. Of the endemic mammalia, which are of course all seals, whales, or dolphins, the most interesting is the sea-lion (*Arcto cephalus hookeri*), the habitual tracks of which are found passing through the forest, often some way from the shore. In Volume II. there are some excellent photographs of these creatures both on the shore and also in the bush, where their ungainly forms seem strangely out of place.

Of the other larger forms of life, the most conspicuous are naturally the birds, and Mr. Waite gives us some interesting particulars, accompanied with excellent photographs, of the albatrosses which he found nesting in large numbers on Auckland and Campbell Islands. The nesting habits of these wonderful birds can be easily observed, as they build large, mound-like nests close to one another among the tussocky grass and scrub on the tops of the cliffs.

But the naturalist, in looking through these fascinating, beautifully illustrated volumes, will turn with eagerness to the description of the geology and of the land fauna and flora, with the hope of gleaning some further information which may throw light on the geographical problems connected with these islands and their relationship to the Antarctic Continent, past and present. The principal mass of Campbell Island is composed of volcanic lavas and breccias and a small area of more ancient gabbro ; but on the west coastal area a fringe of Tertiary strata (probably Miocene) is exposed. The Snares are composed entirely of granite. The oldest rocks of the Auckland Islands consist of masses of granite and gabbro, overlaid and penetrated by dykes of trachyte, porphyrite and diabase. Next in order comes a conglomerate containing fragments of these rocks and of gneiss, granite, and contorted schists. The occurrence of this conglomerate is doubtless of great importance as indicating the presence of a continental area in the neighbourhood ; it would also appear that the outpourings of basalt which form the main mass of the islands were subaerial and not submarine. No fossiliferous stratified rocks were found. Signs of extensive glaciation, probably of Pleistocene age, were observed.

The geological evidence is favourable, therefore, to a previous greater extension of these islands, and it is not opposed to the theory of their having been once connected with New Zealand by a continuous land mass, though direct proof of this theory is not forthcoming.

Turning to the fauna of the islands Prof. Chilton in his summary of the results of the Expedition argues strongly in favour of a much closer connection in previous times with New Zealand. Evidence is drawn from land-shells, earth-worms, fresh-water Crustacea, and apterous insects.

Prof. Benham, speaking of the earth-worms, comments on the existence of the genera Rhododrilus and Plagiochæta in New Zealand, and also in the Auckland and Campbell Islands, and of the occurrence in the Auckland Islands of Leptodrilus, an endemic genus closely related to the purely New Zealand form Rhododrilus, and says : " The occurrence, then, of these three genera evidently indicates a former land continuity between the mainland and these subantarctic islands." Prof. Chilton records the occurrence of two New Zealand species of fresh-water Crustacea (*Idotea lacustris* and *Chiltonia mihiwaka*). These Crustacea do not lay resting eggs—immersion in sea-water rapidly kills them ; and it is practically inconceivable that they could have been carried across the wide stretches of ocean which at present separate the islands from New Zealand. Among the insects are many wingless forms, and forms with reduced wings, many of which are peculiar species with close relations either in New Zealand or neighbouring Antarctic countries.

It would appear to be established, then, that these outlying islands of New Zealand, probably including Macquarie Island from what little is known of it, were in previous times of much greater extent, and in close connection if not actually joined on to New Zealand. But Prof. Chilton deals with the further and wider problem as to the possible connection between the New Zealand thus extended and the Antarctic Continent. The theory of a more greatly extended

Antarctic Continent in past times putting New Zealand, Tasmania, and South America into communication, and permitting the interchange of inhabitants, has received a great measure of support from the discoveries by recent South Polar expeditions not only of continental and stratified rocks within the Antarctic circle, but also of coal, proving that the climate in past times has permitted life to flourish in these regions. These important discoveries afford a firm basis for the students of geographical distribution who have been accumulating in recent years a large body of evidence showing a striking community between the animals and plants inhabiting the temperate countries of the southern hemisphere, which are separated indeed to the east and west by wide and probably permanent oceans, but which almost impinge towards the south upon the Antarctic Continent. It must, however, be admitted that although all are agreed, both zoologists and botanists, as to the fact of Southern Australia, New Zealand, and South America having a considerable common element in their fauna and flora, yet there is considerable diversity of opinion as to whether this community necessarily implies a land connection across the Antartic Continent. The great majority of zoologists, including practically all those who have personally investigated the matter, are agreed that whether an actual land connection existed or not, at any rate the extension of land in the extreme southern hemisphere has been very much greater in times past than it is now, and that various forms of land and fresh-water animals have thus been enabled to pass, possibly by means of archipelagos of islands, from one region to another, with far greater facility than is at present the case. The botanists, however, are not agreed as to the necessity of taking this view, and in the report before us we notice a considerable divergence of view between Mr. Cheeseman and his zoological colleagues. The gist of Mr. Cheeseman's argument appears to be that although certain of the same and closely related species of plants exist in Fuegia, New Zealand, and Southern Australia, yet there are many whole families of plants characteristic of the one country entirely absent in the others, while many of the species that are common to the three regions are plants with special means of dispersal which are found all over the southern seas. He is therefore inclined to think that the dispersal of these plants has taken place under conditions very similar to those now existing, and that the radical differences in the flora of the southern continents are not consistent with the existence of a large continental extension connecting them together.

The facility with which plant seeds are rapidly dispersed over wide areas, despite barriers of mountains and seas, largely prohibits their use in the solution of geographical problems. It would appear that the determining factor for plant distribution is not so much the existence of continuous land, but rather the nature of the physical conditions, and the existing plant-associations in the countries to which they may be from time to time fairly universally dispersed, but in which they fail to find a footing. In the case of land and fresh-water animals which have not got special modes of dispersal, we are led to fall back upon a wider extension of land surface to account for their wide and discontinuous distribution; but zoologists should admit that until a great deal more is known about the possibility of dispersal across the sea, either by birds or in the water by currents, it is not safe to dogmatise on these questions. We are in need of systematic and careful experimentation as to the means of dispersal of special groups of small land and fresh-water invertebrates, concerning which very little is at present known. This is certainly desirable; but the systematic exploration of the habitable globe, and the careful description of its inhabitants, living and fossil, is an equal and perhaps more pressing need. As civilising man spreads over the world, he carries with him not only extermination but inextricable confusion among the plant and animal associations whose dominions he invades, so that it becomes increasingly difficult to disentangle the threads of the history of animal and plant distribution. It should be the aim of systematic zoologists and botanists to endeavour to reconstruct this history before it is too late, and as a valuable contribution toward this endeavour the reports issued by Prof. Chilton and his co-workers should be widely welcomed. GEOFFREY SMITH.

**Spark Spectra of the Metals.** By CHARLES E. GISSING, F.R.G.S. [Pp. 21.] (London: Baillière, Tindall & Cox, 1910. Price 7*s.* 6*d.* net.)

TO spectroscopists it is a constant source of wonder that metallurgical and mining analysts do not avail themselves more freely of spectroscopic methods. With a standard spectrograph once installed—not a costly or a difficult matter—the preliminary examination of, say, an ore becomes a matter of minutes in place of hours and the results are certainly no less sure or comprehensive. Even in some of our large Government laboratories the spectrograph is not recognised as an essential to speedy and searching preliminary examination.

Whilst welcoming Admiral Gissing's atlas for the reason that it may do something to disturb this apathy, we feel that it can only be recognised, in its present form, as a pioneer.

For general use we consider that "arc," rather than "spark," spectra would be more convenient; space will not permit us to argue this matter here.

Taking the author's spark spectra we find much to criticise. The wave-lengths given are often not sufficiently accurate, in some cases they differ from accepted standards by several tenth-metres and not infrequently, especially in the case of "blends," the difference of a fraction of an Å.U. draws the distinction between one element and another.

Then the complete omission of some elements is serious. In alloys and ores titanium, for example, often plays a not insignificant rôle and gives, in the spectrum, strong groups of lines to the confusion of others: yet titanium is not included. On the other hand we fail to see what useful purpose can be served by the introduction of the spectra of alloys; for example, one rarely requires to test a sovereign by spectroscopic methods. Besides, such an alloy as "brass wire" may vary so considerably in composition that the spectrum of one sample is quite useless, as well as redundant.

The descriptions of methods, of the characteristic features of each spectrum and the tabulation of the chief lines will be found useful in the practical employment of the fifty photographs of spectra reproduced on the ten folding plates. But here again there are omissions: surely the "triplets" of the iron spectrum are characteristic enough to warrant mention in the notes.

Finally we must protest against the arrangement of the spectra with the less refrangible radiations to the left. Red to the right is the British convention, now almost universally adopted, and any other arrangement must lead to confusion.

With these criticisms we welcome the volume as a pioneer, although there are of course the fine atlases of Hagenbach and Konen—translated by Dr. King—Eder & Valenta, Kayser & Runge, etc. Yet the volume under notice is a more direct appeal to our impractical practicians, and if it causes them to consider the spectrographic method enhanced efficiency will accrue.

WILLIAM E. ROLSTON.

**Principles of Chemical Geology.** A Review of the Application of the Equilibrium Theory to Geological Problems, by JAMES VINCENT ELSDEN, D.Sc., F.G.S. [Pp. viii + 222.] (London : Whittaker & Co., 1910. Price 5*s*. net.)

IT is only by slow degrees that geology is establishing its claim to be regarded as an exact science. As long ago as 1857 Sorby attempted to obtain quantitative determinations of the conditions under which igneous rocks were consolidated, and his last communication to the Geological Society in 1908, the year in which he died, was concerned with the application of similar methods to sedimentary strata. In Sweden the first steps to a definite geological chronology in years has now at last been taken ; but it is only recently that it has been generally recognised that the rocks of the earth's crust contain as a rule in their physical characters data which will serve as the basis of calculations with regard to the conditions that prevailed when they were formed.

In order, however, that these records should be satisfactorily interpreted, a competent knowledge of the principles of chemical physics and of their application to magmas, minerals and rock masses must be obtained. This at the present moment we do not possess. Much of the theory is still in dispute, and the determination of physical constants under conditions of high temperature and pressure is still very incomplete, though excellent work in this direction is now being carried out by Dr. Day and his collaborators at Washington.

A considerable body of literature has, however, already come into existence, much of it scattered through physical, chemical, mineralogical and petrological periodicals of Europe and America, and we are under very considerable obligation to Dr. Elsden for providing us with a handy digest of the researches that have already been carried out in this difficult field of research, and some discussion as to the conclusions to be drawn from them. In the present imperfect state of our knowledge he has acted wisely in endeavouring to make his book as comprehensive as possible, although in the result his exposition of important principles is sometimes all too brief. The full references which are furnished throughout will, however, always enable the worker to study the original authority at first hand if he wishes to do so. The time is not yet ripe for the preparation of a simple introductory text-book on the subject.

Among the numerous topics with which the author deals are crystallisation from fusion or solution ; diffusion ; viscosity ; surface tension in its manifold relations ; vapour pressure especially in connexion with hydration ; and the transformation of polymorphous forms into one another. Everywhere change is shown to proceed from a system of greater energy to one with less till a condition of stable equilibrium is reached, though millions of years may pass before the process even approaches completion.

In his anxiety to hold the balance true between different authorities, the author is sometimes unduly cautious. He declares himself, for instance, uncertain whether the presence of volatile fluxes would counteract the natural viscosity of acid igneous magmas ; though there are, I imagine, few facts so well established as the extreme fluidity of some of the more siliceous intrusions connected with granite masses, and there can be no doubt that this must be attributed mainly to the presence of water. The suggestion, on the other hand, that greisen represents the mother liquor of a granite magma is scarcely likely to be verified. The more usual view that it is the result of the action of residual volatile constituents on a portion which has already crystallised in whole or in part would seem to be much more probable. Again, the supposition that the separation of a marginal facies would be in inverse proportion to the viscosity of the magma seems exactly contrary to what one would imagine to be the case.

These are, however, minor points, and the author must be congratulated on the production of a valuable text-book in a department where one was much needed. It should find a place on the bookshelf not only of the petrologist and mineralogist but of the chemist and physicist as well. We can only trust that the demand may be sufficient to call at an early date for a new edition, in which the author will have ample space to deal at greater length with the fundamental problems of the subject, and at the same time his printer will show more technical skill in setting up the numerous mathematical symbols to be found in its pages.

JOHN W. EVANS.

**A First Book of Physical Geography.** By W. MACLEAN CAREY. [Pp. viii + 150, with 57 figures.] (London: Macmillan & Co., 1910. Price 1*s.* 6*d.*)

IN the hands of a capable teacher this little book should serve as a useful introduction to the study of physical geography. The general arrangement is good and the explanations are usually clear. Many of the suggestions for practical work are excellent; they require very little apparatus.

The book, however, attempts too much for its size, and some branches of the subject are treated with extreme brevity. The chapter on earth-sculpture occupies only fourteen pages. Under this head are included underground water, springs, rivers, glaciers, the effects of wind, the work of the sea, etc.; it is evident that so brief a summary of so large a subject can be of very little value. The chapter on the structure of the earth and on earth-movements is also too short to be of much use.

The mathematical and astronomical side of geography is more fully treated. The temperature, pressure, and humidity of the atmosphere are dealt with at some length. These are, accordingly, by far the most useful sections of the book. But before approaching this part of the subject, the pupil will require a little instruction in elementary physics. The description of the barometer is insufficient for those who do not already understand the principle of the instrument. There is no attempt to show why heat affects land and water differently. The effect of the earth's rotation on the winds is stated but not explained.

Under ordinary conditions in schools the study of climate and weather is more practicable and in many respects more useful, than the study of earth-sculpture. In a first course the latter may perhaps be omitted altogether, and for such a course this book should form a good foundation.

PHILIP LAKE.

**The Mineral Kingdom.** By DR. REINHARD BRAUNS; translated, with additions, by L. J. SPENCER, M.A., F.G.S. (In course of serial publication with 91 plates (73 coloured) and 275 text figures.) (Stuttgart: Fritz Lehmann; London Agents: Williams & Norgate. To be completed in 25 parts, price 2*s.* net each.)

THIS is an English edition of a series of plates which constitute one of the most successful attempts to represent minerals in their natural colours. It is somewhat strange that artists have so rarely attempted to reproduce the colour and lustre of minerals. One would have imagined that they would have welcomed an opportunity of displaying their skill and technique in conquering the difficulties that this class of objects undoubtedly presents, especially in the case of opaque minerals with metallic lustre. These difficulties are naturally still greater when the production of coloured plates at a reasonable price on a commercial scale is in question, as in the present work. The illustrations in a booklet of the Technological Museum, Sydney, on the *Building and Ornamental Stones of New South Wales* (second edition, 1909) are very successful, but do not include any representations of metallic lustre. The same is the case with the magnificent plates of the *Investigations and Studies in Jade* privately printed in New York, which was issued in a very limited edition.

The plates of the work now under review are accompanied by readable and instructive notes on the different minerals and the general principles of mineralogy, for the accuracy of which Mr. Spencer's name is a sufficient guarantee. He is somewhat handicapped by the text figures of the German edition, which show the Naumann symbols, now obsolete in this country, and are otherwise open to criticism. This, however, Mr. Spencer does not neglect to supply. Altogether the work may be recommended to all who wish to familiarise themselves with the appearance of the more important minerals and acquire some knowledge of their characters, but have not access to a good collection. It should find a place in every municipal library as well as in those of schools and colleges.

JOHN W. EVANS.

**Handbook to the Ethnographic Collections in the British Museum.** By CHARLES H. READ, Keeper. [Pp. xv + 304.] (London : Printed by Order of the Trustees, 1910. Price 2*s*.)

THE present work, the compilation of the Keeper of the Ethnographical Department of the British Museum and of his assistants, Messrs. Joyce and Dalton, aims, in spite of its title, at being something more than a mere handbook to the collections under their charge. It contains in effect a compendious ethnographic account of the more primitive peoples of the world (with certain exceptions), considered with special reference to the exhibits in the British Museum, and preceded by a short ethnology, in which the results of modern research are summarised with laudable care and precision. Since the total number of pages runs only to some 300, no small share of even this scanty space being allotted to illustrations, it goes without saying that brevity and conciseness form the salient features in the treatment of the subject-matter. It has thus been found possible to compress within the above limits the more material facts concerning the Geography, History, Dress, Dwellings, Food—an important item too often overlooked—Ornaments, Weapons, Social Systems, Musical Instruments and Religions of a large proportion of uncivilised mankind. Of course, all races are not described in detail under these heads; but information concerning them is given for the larger ethnic groups and for many of the more important tribes. The result is a very interesting and convenient manual, which may be usefully read by many besides the visitors to the British Museum.

The exceptions above alluded to are India, China and the countries comprised under the head Indo-China. Thus the descriptive matter under "India and Ceylon" relates mainly to Ceylon, the Andamans, etc., whilst Assam is almost the only country dealt with under the third heading. Of the peoples of China practically no account is given. These noteworthy *lacunæ* result from the poverty of the ethnographic collections available, though some of the arts and crafts of India are represented at South Kensington. An ardent love of science, it is to be feared, has never yet imbued the august councils of the Indian Government; indeed, its bitterest opponents—and, as we all know, it has recently come in for a good deal of criticism—have always abstained from charging it with an undue expenditure of funds on purely scientific objects. Let us hope that some enlightened Proconsul may, before it is too late, establish a complete and representative ethnographic museum, if not in London, then at least in Calcutta. China, it may be remarked, in spite of some excellent pioneer work, still remains a veritable Golconda of unworked material ready to the hand of the anthropological explorer.

As a general rule the authors have exercised a wise restraint in dealing with hypotheses which do not yet command a general acceptance. More caution might, however, have been exercised in the enunciation of the theories concerning the peopling of Africa and the origin of the Polynesian race. Experience in Europe bids us beware of sweeping if ingenious assumptions of origins put forth before the completion of the necessary spade-work in the way of anthropometrical statistics; in Africa and Oceania most of the latter is still to seek. In anthropology—as witness a recent discussion before the Society of Arts on the races of Burma—it seems especially difficult for the investigator to say, "I do not know."

The word "sacrificed" is wrongly used in the Introduction with reference to the killing of slaves for the purpose of serving the dead in a future life. Human sacrifices connote the gratification or else the interference in mundane affairs of a supernatural being, which is not here the case. Again, in view of Professor Frazer's remarks in his recent work on Totemism, it may be well, in a future edition, to modify the statement connecting exogamy with totemism and to substitute some such expression as "guardian spirit" for "individual totem" when discussing these beliefs of the North American Indians. "Mongoloid" would seem preferable for ethnographic purposes to "Mongolian." In spite of the distinction made in the text the latter is certain to be confused popularly with the Mongol people proper. The reference on page 118 to a "Caucasic" race is somewhat bewildering.

In a work such as this, covering in a short space so wide a field, some omissions are inevitable. Amongst matters inadvertently overlooked may be mentioned such items as the former prevalence of human sacrifices in Fiji and the existence amongst a minority of the Berbers of not only white skin but fair hair; Lord Curzon's military expedition into Thibet should have been included in the list of foreign visitors to that country; and more frequent references should have been made to exogamous customs.

The use of marginal paragraph headings would have much facilitated reference by busy students. Such sign-posts are particularly necessary in a work like the present, intended primarily for use whilst inspecting the exhibits in a museum. And it would have been no small boon—not only to the general public, but also to the student of anthropology—if a bibliography of at least the more important works had been inserted at the end of the different chapters. It is but natural that a visitor interested in an object or in the description of it in this Handbook should desire to refer to works where it and the notes and customs connected with it are described or elucidated at length.

The work is furnished with two complete indexes, the one general and the other tribal and geographical, whilst the illustrations, which form a prominent feature, are both well selected and instructive.

B. H.

**A School Algebra.** Part I. By H. S. HALL. (London: Macmillan & Co., 1910. Price 2*s.* 6*d.*)

THIS is the first part of a new Algebra by Mr. Hall, joint author with Dr. Knight of the famous "Hall & Knight." It is written in response to numerous requests for a thorough revision of that work. While differing to some extent in plan and detail, it resembles the earlier book in its characteristic features. The resemblance is so close that a long review is unnecessary. Part I. goes as far as Simultaneous Quadratics. Graphical methods appear early, and are freely used throughout the book: probably many teachers will think, as the reviewer does, that they are overdone. The examples are numerous, but there is still something to be desired in this respect. There are not enough $x$, $y$, $z$ equations of the absolutely straightforward type; there are not enough simple equations involving brackets and fractional coefficients; there are not enough simultaneous quadratics of the type $\left.\begin{array}{r} x^2 + 2xy = 3 \\ 3x + 2y = 5 \end{array}\right\}$; an unfair preference is given to those in which one coefficient of the linear equation is unity. Boys make endless mistakes in all these, and teachers need a larger stock of examples to draw upon. The examples on equations with literal coefficients are rather more numerous and better selected. A table of square roots has been added and should be found useful, and so should the hints which are given for the solution of harder equations. The book is excellently printed and paged, and preserves all the features which made "Hall & Knight" so deservedly popular.

F. G. CHANNON.

# MOLECULAR ARCHITECTURE

## THE RELATION BETWEEN CRYSTALLINE FORM AND CHEMICAL CONSTITUTION

By R. T. COLGATE and E. H. RODD

It is evident that we may expect many fruitful results for molecular mechanics (which forms a problem common to many provinces of natural science) from the further elaboration of the data concerning those variations which take place in crystalline form when the composition of a substance is subjected to a known change; and I therefore consider it useful to point out to the student of science seeking for matter for independent scientific research this vast field for work which is presented by the correlation of form and composition.—MENDELÉEF, *Principles of Chemistry*, 1905, i. 10.

### INTRODUCTION

TIME out of mind, the scientific imagination has been strongly impressed by the beautiful symmetry of the solids known as crystals. Haüy's great discovery of the rationality of crystal indices[1] led to this symmetry being traced to its source in the definite uniform arrangement of the ultimate units or molecules and was the first important step towards the fundamental conclusion since reached that crystals display symmetry because they are homogeneous structures as defined by the mathematician.

Recent investigation in the department of modern chemistry known as stereochemistry has shown that the homogeneity displayed is not alone a regular repetition throughout space of mere points or similar centres of force but that the chemical atoms of which the space units of a crystal consist are themselves definitely arranged in the molecule.

The conclusion has indeed been forced upon us that regularity of atomic arrangement characterises all matter and that, strictly speaking, the term amorphous (formless) is inappropriate as applied to molecular structure; the relative disposition of the

[1] The indices of a crystal face are the reciprocals of the intercepts of the face, supposed indefinitely extended, on the three crystal axes. The Law of Rationality of Indices states that these are always whole numbers. The occurrence of a digit higher than 3 in the indices of a crystal face is rare.

atoms in solids, indeed in liquids also, is always in a great degree regular and definite.

The most striking evidence supporting this conclusion that liquids and solids should in this respect be classified together is afforded by the recent discovery of liquid crystals, to which attention will presently be directed.

The term amorphous, in fact, must not be taken to imply irregularity of structure of a substance thus designated; it merely indicates that the orderly arrangement which obtains is uniform throughout small patches only or at least is not such that any distinguishable physical property is found to vary according to the direction in which it is determined. The definite orderly arrangement of some kind thus everywhere prevailing will, it is obvious, be attributed to the interaction of atomic forces as they attain equilibrium: whenever the conditions are sufficiently simple, as in crystals, equilibrium of similar sets of interacting forces evenly distributed throughout space is productive of homogeneity of structure. The test for the presence of this homogeneity is obedience to Haüy's law of rationality of indices.

The very important fact has long been known that crystals vary both in physical and geometrical properties according to the chemical composition and constitution of the material from which they are built up, this being most clearly shown in connection with those prepared from a variety of substances in the laboratory.

The sustained labours of a number of crystallographers and other mathematicians resulted during the nineteenth century in the attainment of a remarkably complete geometrical theory of crystal structure. As already intimated, the foundation of this theory was laid by Haüy's discovery; gradually perfected by a succession of mathematicians, it required more than half a century to reach finality.

The high degree of perfection of the theory, even before it had been finally completed by the later work of Sohncke, Barlow and Federow, is demonstrated by the following words of Stanley Jevons in his *Principles of Science*, published in 1879:

"Perhaps the most perfect and instructive instance of classification which we can find is furnished by the science of crystallography. The system of arrangement now generally adopted is conspicuously natural and is even mathematically perfect.

A crystal consists in every part of similar molecules similarly related to the adjoining molecules and connected with them by forces the nature of which we can only learn from their apparent effects. But these forces are exerted in space of three dimensions, so that there is a limited number of suppositions which can be entertained as to the relations of these forces."

Meanwhile many chemists had appreciated the importance of considering crystal structure in connection with the atomic theory; efforts were made from time to time to unravel the connection subsisting between crystal structure and chemical constitution but the vast accumulation of experimental details bearing on this connection remained uninterpreted at the close of the century. It has been left to the twentieth century to generalise from this rich collection of materials and to employ the crystal measurements of a generation of crystallographers and chemists in the discovery of the fundamental conditions and principles with which the sciences of chemistry and crystallography have to deal.

A comprehensive account of the accepted mathematical theory of crystal structure, including the history of its development, is to be found in the Report of the British Association for 1901 (Glasgow), p. 297. A homogeneous structure whose geometrical properties are found to coincide with those of crystals was ultimately defined as follows: "A homogeneous structure is one in which every geometrical point has an environment identical with that of an infinite number of other such points distributed throughout the structure supposed indefinitely extended." The number of types covered by this definition is 65 but when the variety introduced by discriminating between structures that are identical with their mirror images and those that are not is taken into account the number of distinct types increases to 230.

The properties of crystals which guided mathematicians in discerning the extensions and limitations of the fundamental principle of homogeneity that were necessary in order to make the theory coincide with the phenomena may be roughly described as (*a*) the various physical properties which in so many crystals vary regularly with the direction in which they are determined—the anisotropic condition of crystalline bodies thus variously revealed manifestly indicates the definite and orderly arrangement of the ultimate parts; (*b*) the constancy of the angles between corresponding crystal faces on any number of specimens of the same substance however the faces differ in size; (*c*) the varieties of symmetry indicated by the various kinds of cleavage and by the different relative dispositions of the crystal faces in different substances; (*d*) the way in which crystals grow.

The phenomenon of supersaturation is important in connection with the last. Solutions of many substances saturated at a certain temperature can be cooled far below that temperature without depositing crystals; crystallisation is as it were held back owing to the lack of a nucleus around which the structured edifice can be built up. A sufficient number of particles cannot get into position at the same time. If however a single crystal of the substance be introduced into the solution, the appropriate particles can instantly marshal themselves upon the pattern thus introduced among them and crystallisation proceeds. The supercooling of liquids is another phenomenon of the same order. The introduction of a particle of the solid substance into a supercooled liquid provides a nucleus upon which the liquid particles can arrange themselves. All such phenomena point to a definite arrangement of particles in crystalline matter.

The discovery of liquid crystals has been already mentioned. These substances, of which cholesteryl chloride is an example, give evidence of being arranged structures: while still in the liquid condition, they exhibit optical properties generally associated only with crystalline solids; and if drops of such substances be suspended in other liquids they assume not spherical but other geometrical forms. These liquid crystals are important as forming a link between the solid and the liquid states.

### The Growth of the Mathematical Theory of Crystal Structure

It is not possible in this short account to give more than a general idea of the salient points of the mathematical treatment of the subject just referred to.

The form of the theory of crystal structure, based on Haüy's great discovery, which obtained general acceptance and held complete sway for many years was that developed by Bravais. He supposed crystals to be composed of identical similarly orientated detached particles and attributed the symmetry observed, partly to the uniform distribution of these particles through space on parallel lines, partly to the shape of the particles. The theory was based upon the law that the distinguishing properties of a crystal depend upon the direction in which the property is determined and are alike in parallel directions whilst internally the conditions are similar at all points. Bravais showed that identity of environment and of orientation of the particles involved the distribution of these particles in the regular manner premised by the theory. A mental picture of a space-lattice (*Raumgitter*) whose figure is indicated by the distribution of the particles may be formed by imagining space partitioned by three sets of parallel planes into similar and equal parallelopipedal cells, the planes of each set being equidistant; the space-lattice consists of the points of intersection of the cell edges. In the most general case the edges of any one cell will be unequal and none of the angles will be right angles. The space-lattice then corresponds to a low type of symmetry in crystals.

The necessity for assigning a lower degree of symmetry to the particles than to the space-lattice marked by their arrangement, in order to obtain the hemihedrism or partial symmetry observed in some crystals, arose from the fact that no space-lattice of mere points displays symmetry of this nature, *e.g.*, every space-lattice of mere points possesses centres of symmetry and is therefore incapable of representing a case in which no centre of symmetry is present.

The objection to Bravais' theory is its lack of generality; whilst all the kinds of crystal symmetry observed can be represented by means of his flocks of particles, a number of forms of homogeneous structure which also display the symmetry of crystals are not represented by his method or only imperfectly.

In Bravais' work we see the presentation of the perfected methods of a number of previous workers; indeed the importance of his work consisted almost entirely in giving precision and completeness to conceptions already put forward by his predecessors. Much the same remark may be made regarding the achievements of Sohncke, who gave to crystallographers in an acceptable form the important generalisation based on the Bravais method which led to a mathematically complete definition of homogeneity of structure exactly corresponding to the facts regarding crystal symmetry.

The advance on Bravais' conception which has given lasting fame to the name of Sohncke consists in the removal of the limitations involved in supposing all the similar particles or units of which a homogeneous system consists or their similar environments to have the same orientation. When this important extension of the definition of homogeneity of structure had been achieved and the number of distinct types of symmetry thus reached had been shown by Sohncke to be 65, a further analysis of the geometrical features of these various types was soon made by Fedorow, Schönflies and Barlow; the purely geometrical investigation of the subject was then found to be practically closed.

The perfectly general definition of homogeneity which was ultimately attained is applicable to all kinds of strictly homogeneous structure however complicated the composition; any number of varieties of particles may be simultaneously present. The characteristic feature of such a structure is that it presents identity of aspect as viewed from a fixed point before and after being subjected to any one of a definite series of movements made by the structure as a whole (coincidence movements).

The complete crystal symmetry of the structure resides therefore in the group of coincidence movements characterising it, this group of movements possessing elements of symmetry strictly corresponding to those of some one of the 32 classes of crystal symmetry known to the crystallographer. For the sake of simplicity, however, the crystallographer supposes a crystal to have a geometrical centre to which he refers its elements of symmetry [1] and thereby reduces these elements to four kinds: (1) an axis or axes of symmetry, (2) a plane or planes of symmetry, (3) a centre of symmetry, and (4) a kind of symmetrical operation compounded of two of these which produces a change of orientation. To these four should in strictness be added the property of constancy of angular inclination of faces; this characterises all classes of crystal symmetry, including the lowest form of anorthic symmetry which possesses none of the

[1] The crystallographer uses the term "symmetry" to imply the orderly distribution of the parts of the crystal about certain planes or axes or about a centre. A crystal structure may be such that it is symmetrical about both planes and axes as well as a centre; it may also possess only one of these so-called "elements of symmetry." The crystal is said to be of a high or low order of symmetry according as its structure may be referred to many or few such elements of symmetry.

four elements of symmetry referred to. When the elements of symmetry of the types of homogeneous structures are compared with those of the crystallographer just referred to, it is perceived that the simplification introduced by the conception of the geometrical centre of the crystal is artificial; some of the coincidence movements characterising homogeneous structures possess no such centres, *e.g.*, the screw movements. In the case of movements referable to no centre of symmetry, the change of orientation or effect of the movement on direction produced by the less regular movement of the homogeneous structure is identical with that brought about by some centred movement.

## The Phenomena of Polymorphism, Isomorphism and other Morphotropic Relationships

From this necessarily brief sketch of the mathematical theory of crystal structure, we may now pass to the consideration of the mode in which the theory serves in the interpretation of problems which arise in the practical study of crystals. The net result of the work which has been described above is to show that an intimate relationship exists between the number of possibilities of homogeneous distribution of points in space as determined geometrically and the classes of symmetry observed in crystals. The conclusion to be drawn from this result is that crystals are homogeneous structures; a conclusion amply justified by many physical facts, some of which have already been cited. It is possible to imagine the crystal units distributed so as to form a homogeneous structure corresponding to one of the 230 types; but it must be observed that this purely geometrical conception does not restrict or define either the shape or size of the crystal unit in terms of the chemical molecule. No reason is offered for the adoption by the units of a particular type of symmetry and there seems to be no reason why the same units should not be able to arrange themselves in more than one way. That they can do so we have ample evidence, for many cases are known of a chemical substance assuming more than one crystalline form. This phenomenon is known as polymorphism. Sulphur affords a good example of a polymorphous substance: it is known in four modifications, one possessing orthorhombic symmetry (the common stable variety), two monosymmetric and one anorthic. Dimorphous organic compounds are of frequent occurrence.

The difference between the two crystalline forms of a substance is comparable in some aspects with that between the solid and the liquid states. Passage from one form to another is accompanied by change of energy content and each form is stable only throughout a certain range of temperature and pressure. At one particular temperature—the transition temperature—corresponding to the melting point of a solid, both forms are stable. It is clear then that the two crystalline forms of a substance are not chemical isomers, for no transition temperature is in general observed between such related compounds; the difference between the two forms must be merely physical and due to a different homogeneous arrangement of the same units, for the configuration of these units in each modification must be the same, as any differences here would be accompanied by chemical variations. Thus although at present we cannot lay down any definite rules governing the differences of arrangement of the structural units in two such modifications of the same substance, yet in consideration of the readiness with which in many cases one form is transformed into the other, we can prophesy that the types of architecture of the two forms are very intimately related.

It may be conceived that two types of units more or less similar to one another might assume similar homogeneous arrangements. Examples of this have been multiplying ever since the discovery of isomorphism among phosphates and arsenates by Mitscherlich in 1822.

It is reasonable to expect that only such substances which contain very similar groupings would be isomorphous, as isomorphism implies the same crystalline system with closely approximating axial ratios [1] and also the possibilities of forming mixed crystals of the isomorphous substances. As a rule isomorphism is only found among such related substances as salts of acids with equivalent metals or salts of the same metal

[1] Every crystalline substance is characterised (i) by its symmetry or the crystal class to which it belongs, (ii) by stating the ratio $a:b:c$ between the lengths of the three edges of the parallelopipedal cell from which the crystal structure may be regarded as built up. The ratios involved in the expression $a:b:c$ are termed the axial ratios of the crystal.

with similar acid radicles. In the sulphates and selenates of the metals potassium, rubidium and cæsium we have a typical isomorphous group, which has been most carefully studied by Tutton.[1] The crystals of two isomorphous substances, unless they belong to the cubic system, are dimensionally never identical but the differences are smaller the more nearly the substances be allied chemically. Thus potassium and rubidium sulphates exhibit axial ratios which are more nearly equal than are those of potassium and cæsium sulphates, apparently for the reason that potassium and rubidium are nearer relatives in the periodic family than are potassium and cæsium. In organic substances the displacement of equivalent elements by one another is frequently observed to be unattended by any marked change in crystalline form. The halogen elements, chlorine, bromine and iodine exhibit this property of being isomorphously interchangeable in a marked degree.

When it is known that a particular unit can assume two types of arrangement and that a second similar unit can assume one of these types, it might be expected to be able also to assume the other. That is to say, the two substances, the units of which we are considering, would be doubly isomorphous or in other words the two forms of the one would be respectively isomorphous with the two forms of the other. Instances of this phenomenon, known as isodimorphism, are indeed well known. The general phenomenon of bodies being isomorphous in more than one form is termed isopolymorphism. It frequently happens that this property of related bodies can only be inferred. For example two similarly constituted substances A and B may occur in totally distinct forms. In such cases it may be observed that B will form mixed crystals with A in the form characteristic of A until the mixed crystals contain a certain proportion of B. On the other hand A will form mixed crystals with B in the form characteristic of B until the proportion of A reaches a certain maximum. Thus although when alone each substance crystallises in but one form, it may be induced to assume the second form in presence of the other substance. The two substances may therefore be regarded as isodimorphous.

Certain relationships subsist between the crystalline forms of more or less related bodies which could not have been foreseen from the results of the work of the earlier crystallo-

[1] *J.C.S. Trans.* 1897, p. 846.

graphers and for which neither Sohncke's conception as ultimately modified by himself nor the later mathematical developments which followed give any adequate explanation. An interesting example of such a morphotropic relationship occurs amongst a group of minerals comprising chondrodite, humite and clinohumite. The axial ratios of these minerals are as follows:

| | | $a : b : c$ | $\Delta c$ |
|---|---|---|---|
| Chondrodite . . | $Mg_3(SiO_4)_2$ $2Mg(F,OH)$ | 1·08630 : 1 : 3·14472 | 1·25862 |
| Humite . . | $Mg_5(SiO_4)_3$ $2Mg(F,OH)$ | 1·08021 : 1 : 4·40334 | |
| Clinohumite . . | $Mg_7(SiO_4)_4$ $2Mg(F,OH)$ | 1·08028 : 1 : 5·65883 | 1·25549 |

The axial ratio $a : b$ is practically the same for each mineral, whilst a constant increment to the ratio $c : b$ accompanies each addition of the group $Mg_2SiO_4$. The need of an explanation of this and of many other similar relationships between crystalline substances has long been felt.

## Topic Parameters or Molecular Distance Ratios

With the object of attempting to explain such relationships as have been shown to exist between the crystalline forms of chemically related substances, a new method of investigating crystal structure was suggested simultaneously by Becke and Muthmann. The method consists in attempting to determine the dimensions of the space-lattice which can be regarded as the fundamental basis of the crystal structure. It was expected that interesting information would be obtained by comparing these dimensions for different allied substances. The axial ratios give only the relative dimensions of the particular space-lattice which is indicated by the principal faces of the crystal. The crystal structure being imagined divided into units, so that each unit contains a chemical molecule, the centres of gravity of these units are taken to form the points of a space-lattice which is the fundamental lattice. The volume of each parallelopipedal cell of this space-lattice can be expressed numerically by the molecular volume of the substance and is obtained by dividing the molecular weight by the density. The volume of the parallelopipedal cell, its relative dimensions and the angles between its sides (the interaxial angles of the crystal) being known, the absolute dimensions of the cell can be readily calculated. These dimensions are called "Topic Parameters" or by Tutton "Molecular Distance Ratios" and are represented by $\chi : \psi : \omega$. They are calculated from the formulæ :

$$x = \sqrt[3]{\frac{a^2 V}{c \sin A \sin \beta \sin \gamma}}, \quad \psi = \frac{x}{a}, \quad \omega = c\psi,$$

where $V$ is the molecular volume. The above formulæ are for the general case of an anorthic crystal.

It was hoped with the aid of these topic parameters to throw light on the internal crystalline structure of substances which were chemically related and to a limited extent these expectations were realised. To take an example, it is found that when in ammonium iodide, which exhibits cubic symmetry, the four hydrogen atoms are displaced by four methyl groups, the resulting compound, tetramethylammonium iodide, is tetragonal and one of its topic parameters is approximately equal to each of the three parameters of ammonium iodide. That is to say, the introduction of the four methyl groups has expanded the structure of ammonium iodide equally in two axial directions leaving it unchanged in the third. Substantially the same change is produced if ethyl instead of methyl groups be introduced into the molecules. The following table exhibits the relationships between ammonium iodide and its derivatives which are revealed by the topic parameters :

| | $NH_4I$ | $N(CH_3)_4I$ | $N(C_2H_5)_4I$ |
|---|---|---|---|
| $V$ | 57·51 | 108·70 | 162·91 |
| $\chi$ | 3·860 | 5·319 | 6·648 |
| $\psi$ | 3·860 | 5·319 | 6·648 |
| $\omega$ | 3·860 | 3·842 | 3·686 |

Similar investigations have been carried out with the sulphates of the alkali metals and with the double salts formed by these with the sulphates of the bivalent metals, with the alkyl substituted ammonium chloroplatinates and with several other series of related substances.

The study of molecular volumes and molecular distance ratios, however, was not found to lead to any considerable advance in solving the problems involved in the correlation of crystalline form and chemical constitution. The difficulty which had arisen was to find the means by which to reconcile the mathematical idea of points with the chemist's notions of the atomic occupation of space.

The first step towards the solution was taken by William Barlow in 1883.[1] In his paper of that date he develops the idea of representing the atoms by spheres and so pictures crystal structures as close-packed arrangements of spheres; he also points out that most binary compounds, *i.e.* those which consist of but two kinds of atoms, crystallise in the cubic system and that such compounds may therefore be represented by close-packed homogeneous arrangements of equal spheres. Barlow however, was, at this time unable to extend his ideas in an adequate manner owing to the lack of a clue to the relationship between the sizes of the atomic spheres of different elements.

In the Report of the British Association for 1901, which has been already referred to, there is an article summarising the state of the knowledge of the structure of crystals at that time. At the conclusion of this report the author states that there are three questions to be answered before the great problems of crystal architecture can be unravelled: (1) What are the parts of which a crystal consists? (2) How are they arranged? (3) Why are they arranged in this particular way?

The second question at that time could be regarded as having received a general answer; the parts of a crystal, whatever they may be, must be arranged according to one or other of the 230 types of symmetry. We are now in a position to answer the remaining two questions with a fair degree of certainty that the answer is correct so far as it goes. The solution of the problem is furnished by the joint work of Barlow and Pope with which we may next deal.

## The Barlow-Pope Theory

In their paper published in 1906 under the title "A Development of the Atomic Theory which Correlates Chemical and Crystalline Structure and Leads to a Demonstration of the Nature of Valency,"[2] Barlow and Pope have interpreted the results of Sohncke and his successors in quite a new light; they have put forward a treatment of these earlier results which furnishes a concrete interpretation of the chemical idea of the

[1] *Nature*, Dec. 20 and Dec. 27, 1883.
[2] *J.C.S. Trans.* 1906, p. 1675.

atomic occupation of space and in addition furnishes a geometrical interpretation of chemical valency.

Before it was possible to develop such a conception as the one under consideration, based directly as it is upon the atomic theory, it was necessary to make certain precise assumptions regarding the atoms themselves. It is necessary to assume that each atom in a crystal structure occupies a distinct portion of space; simple as this assumption appears to be, it is yet of great significance in that it recognises that the atoms retain their individuality in the molecule. The idea should prove quite acceptable to modern chemists and particularly to followers of stereochemistry. Each atom in the crystal structure is regarded as the centre of attractive and repulsive forces, the structure itself as the equilibrium arrangement of these centres of force. On the basis of this assumption, it can be foreseen that the structure will almost certainly be one in which the smallest distance separating component atom centres is the maximum compatible with the density of arrangement of the centres.

The portion of space commanded by each atom will indeed be as nearly spherical as possible under the conditions of distribution of the atom centres, since the attractive and repulsive forces are supposed to be exerted equally in all directions from these centres. The conditions prevailing in the structure may then be simulated by representing each component atom by a deformable but incompressible elastic sphere, the spheres being arranged homogeneously and with the maximum density of packing, the stacked mass being subjected to a general pressure sufficient to eliminate interstitial space and thereby convert each sphere into a polyhedron. Each polyhedron may then be regarded as the "domain" or "sphere of influence" of the atom of which it forms the habitat; within its own domain the atom exercises predominant influence.

A crystal structure may now be considered as a closest-packed homogeneous assemblage of atomic spheres of influence; or, as defined by Barlow and Pope, as the "homogeneous structure derived by the symmetrical arrangement in space of an indefinitely large number of spheres of atomic influence." By homogeneously subdividing such a structure, a space unit is eventually obtained; this unit is the domain of the chemical molecule. The question of the partitioning of an assemblage into units is one of considerable import and may sometimes lead to interesting developments; for instance it may happen that a particular structure may be partitioned in different ways so as to yield units of the same composition but differently constituted; and it may thus be possible from geometrical considerations to gain an insight into such purely chemical phenomena as that of isomeric change.

## The Interpretation of Valency

By a logical development of this fundamental hypothesis of crystal structure, the authors have come to some noteworthy conclusions which, when they become generally understood, cannot fail to influence the trend of modern thought. The most important of these conclusions concerns that debatable question, the nature of valency. From the consideration of the geometrical properties of close-packed assemblages of spheres, the remarkable deduction is made that valency is an expression of a volume relationship. The particular geometrical properties from which this deduction is made are those which relate to the conversion of one assemblage, by substitution of certain of its parts, into a related assemblage. Suppose that from any particular close-packed assemblage spheres or sets of spheres of influence be removed homogeneously so as to produce a number of cavities and that these cavities be then similarly filled by other spheres or sets of spheres of influence: then if the same density of packing is to be preserved without remarshalling the assemblage, the total volume of the sphere or spheres thus introduced into each cavity must be about equal to that of the sphere or spheres removed from that cavity. The bearing of this fact upon the question of valency will become clear if a concrete example, such as the derivation of triphenylamine from benzene, be considered. Without troubling for the present about the form of the assemblage characteristic of crystalline benzene, suppose it to be possible to form a cavity in that assemblage by removing three adjoining hydrogen spheres, one from each of three benzene units. It will be found that if a nitrogen sphere is to be squeezed into such a cavity so that only a slight readjustment is necessary to restore close-packing, the volume of the nitrogen sphere must be equal to that of the three hydrogen spheres or three times that of a single hydrogen sphere of influence. The mind is led instantly to connect this circumstance with the fact that the valency of nitrogen is 3, that of hydrogen being unity. And the conclusion at which Pope and Barlow have arrived from considerations such as the above is that, in any given crystal structure, the volumes of the spheres of atomic influence of which the structure consists are approximately proportional to the fundamental valencies of the atoms composing the molecule. For instance in the structure appropriate to triphenylamine, if the volume of each hydrogen sphere be taken as unity, each carbon sphere has a volume 4 and each nitrogen sphere a volume 3. Note the expression "fundamental valencies," for it has never yet been found necessary, in the numerous cases studied, to ascribe to the sphere of influence of any atom a different volume from that expressed by its fundamental valency. The volume of a nitrogen sphere appears always to be three times that of the sphere of a monovalent atom, even in the ammonium compounds, in which the element is usually considered to be pentavalent.

This new conception of valency has an important bearing upon the prevalent ideas, due to Kopp, of atomic and molecular volumes. It was Kopp's belief that each element had its own specific and constant atomic volume and that once these atomic volumes were determined, any molecular volume could be calculated by summing up the volumes of the atoms contained in the molecule. His results are, however, not compatible with the new idea of the proportionality of valency and atomic volume, as a single example will show. It has been demonstrated by Barlow and Pope that benzene and paradibrombenzene have in the crystalline form almost identical spatial arrangements; and on the new hypothesis this is only to be expected, since in p-dibrombenzene the spheres of atomic influence of hydrogen and bromine will occupy equal volumes, one-fourth that of a carbon sphere, just as in benzene each hydrogen atomic sphere occupies one-fourth the volume of a carbon sphere. This being so, the substitution of hydrogen by bromine need not be accompanied by any profound change in crystalline form. But Kopp ascribed to hydrogen and bromine atomic volumes equal to 5·5 and 27·8 respectively; and if these figures express the truth, it would be expected that substitution of hydrogen by bromine should produce considerable distortion in the crystalline assemblage of benzene. The fact that no appreciable distortion is observed is evidence in favour of the new doctrine.

An even more striking illustration of the difficulties involved in Kopp's notion is afforded by the isomorphism of potassium and cæsium sulphates. Passing from the former to the latter salt, there is an increase in molecular volume of about 30 per cent.; according to Kopp, the whole increase is due to the greater atomic volume of cæsium compared with that of potassium. If this were so, on substituting potassium by cæsium, the attainment of a stable equilibrium of the atomic forces, such as is indicated above, would involve so great a distortion that the crystalline form of cæsium sulphate would necessarily diverge widely from that of potassium sulphate. But, on the new hypothesis, since cæsium has the same valency as potassium, the volumes of the cæsium, sulphur and oxygen atoms in cæsium sulphate would be in the same ratio as those of potassium, sulphur and oxygen in potassium sulphate; and as a consequence close similarity of crystalline form in the two salts would be anticipated. In fact, but for some reservations which the authors of the new theory have found it necessary to make, the two substances should be crystallographically identical: it is necessary to call attention to these reservations in order to prevent misunderstanding of their conception.

In the first place, it is not to be supposed that the volumes of the spheres of influence of the atoms stand precisely in the ratio of the whole numbers by which we represent valency. These whole numbers do not exactly represent the relative atomic valencies or volumes of the spheres of influence of the atoms of the elements; the idea of the existence of residual valencies has gradually forced itself upon the minds of chemists and just as no two elements appear exactly to satisfy each other in combination, in the same way it is probable that no two spheres of atomic influence of different elements are exactly displaceable the one by the other.[1] All so-called univalent elements are not precisely equivalent; and observed crystallographic facts show that their atomic spheres of influence must differ to some small extent in volume. Secondly, it must be borne in mind that

[1] It might be remarked here that if valency expresses a volume relation, it can hardly be expected that nature would construct spheres of influence of atoms of the various elements in the strict volume relation of 1 : 2 : 3 : 4. Much rather would it be anticipated that the volume of any specific element would be any value up to 4 and that the fundamental valency is merely the nearest whole number to the volume referred to a univalent sphere as unity.

different elementary atoms in a structure behave differently as regards the volume changes which they undergo under varying conditions, such as changing temperature. Again, the absolute magnitude of the sphere of influence of a particular atom changes in passing from one compound to another; for instance the oxygen and sulphur atoms cannot occupy the same absolute volumes in cæsium sulphate as in potassium sulphate. The hypothesis states that *in any particular compound* the volumes of the spheres of influence of the constituent atoms are very approximately proportional to their valencies.

The problem of multiple-valencies has always proved a stumbling-block to every theory of valency; but even this difficulty yields to the new method of treatment. Another geometrical property of close-packed assemblages of deformable spheres explains the fact that when an element increases its valency it does so in steps of 2 units. The property in question may be illustrated as follows: Suppose that a number of sets of spheres of influence each of total volume $m$ can be removed homogeneously from an assemblage and that into each of the cavities thus formed be squeezed a sphere or spheres of total volume $(m + 1)$; then it is found that another sphere of volume 1 must be introduced into each cavity in order that close-packing may be restored without remarshalling. In general, if the volume of the substituting sphere be $(m + n)$, further spheres of volume $n$ must be introduced into the structure to restore close-packing. To give a concrete example, ammonium chloride must be regarded as derived from the ammonia assemblage by inserting into this an atom of valency 1, a gap being thus produced in the assemblage which can be filled only by a second monovalent atom. It is thus possible to give a hitherto unexampled explanation of this singular atomic property of matter. In the case however of elements which appear to possess valencies differing only by one unit (cf. molybdenum, which forms the chlorides $MoCl_2$, $MoCl_3$, $MoCl_4$ and $MoCl_5$), it can be assumed that the volume of the sphere of influence of the atom lies nearly midway between two whole numbers and that the element may form a close-packed assemblage with either the smaller or the greater number of univalent atoms, or with either number plus 2. The phenomena of multivalency are therefore not inconsistent with the interpretation of valency as a volume relation.

### Equivalence Parameters

The conclusion reached as to the internal structure of a crystal is then that it consists of a homogeneous closest-packed arrangement of spheres of atomic influence, the volumes of these spheres of influence being approximately in the ratio of the valencies of the respective atoms: all this is deduced from the fundamental hypothesis. We cannot learn anything about the internal structure of a crystal by direct observation, but from direct measurements of the interfacial angles of the crystal we can obtain, as it were, a numerical statement of its external form. And the next step is to discover a connecting link between the external form and internal structure of the crystal, in order that by observation of the former we may gain information regarding the latter.

On the assumption that the spheres of atomic influence fill space without interstices and that the volume of each sphere is proportional to the valency of the atom, the total volume appropriated by the molecule can be represented by the sum of the valencies of all the atoms comprising it. Further, since we know the relative dimensions of the crystal structure in the three axial directions of the crystal, these being given by the axial ratios, it is possible, by choosing suitable fractions of these axial ratios, to determine the dimensions of the molecule in these three directions in terms of the valency-volume unit. The sum of the valencies of the constituent atoms is termed the "valency volume" of the substance under consideration, and is denoted by the symbol $W$. The molecular dimensions referred to are called "equivalence parameters," and are denoted by the symbols $x, y$ and $z$. They are calculated from formulæ similar to those already given for calculating topic parameters, substituting $W$ for $V$.

$$x = \sqrt[3]{\frac{a^2 W}{c \sin A \sin \beta \sin \gamma}} \qquad y = \frac{x}{a} \qquad z = cy.$$

It will be readily understood that the equivalence parameters represent three translations in the three selected axial directions of the crystal structure. They may be defined as the dimensions of the sides of a parallelopipedal cell equal in volume to the valency volume, the sides of the cell being proportional to and parallel with the three axes. It may be inquired, of what greater service are these equivalence parameters than the topic axial ratios? This we shall proceed to explain.

In the first place, we know that the equivalence parameters represent definite translations in the assemblage of spheres of which the crystal is built up; that is to say, they are the distances between adjacent similarly situated points of the homogeneous structure in the three selected directions. It is of great significance that models of quite a number of crystal structures have been built up from spheres of appropriate sizes; these models not only conform in symmetry to the crystals whose structure they represent but the translations of the model correspond in dimensions to the equivalence parameters calculated for the crystal. In such cases as these—notably in that of benzene, with which we shall subsequently deal more fully—the architecture of the crystal can be regarded as very fully elucidated and the equivalence parameters, having a perfectly definite physical meaning, must in all cases be of great value for comparing the structures of different substances.

We have already referred to the minerals of the chondrodite group as exhibiting among themselves a most interesting morphotropic relationship. The minerals prolectite, chondrodite, humite and clinohumite differ in composition each from the preceding one by the increment $Mg_2SiO_4$, their respective compositions being given by the formulæ $MgSiO_4 2Mg(F, OH)$; $Mg_3(SiO_4)_2 2Mg(F, OH)$; $Mg_5(SiO_4)_3 2Mg(F, OH)$; and $Mg_7(SiO_4)_4 2Mg(F, OH)$.[1] The axial ratios of the four minerals are related in such a way that $a:b$ is practically the same for each, whilst the ratios $c:b$ are in the proportion 3:5:7:9. No explanation of this well-known relationship has hitherto been forthcoming. But if the equivalence parameters for the four minerals be calculated and compared, it is found that *two* of these dimensions are practically constant for each mineral and that the addition of the increment $Mg_2SiO_4$ leads in each case to a constant increase of the dimension $z$.

| | Axial Ratios. $a:b:c$ | $W$ | Equivalence Parameters. $x:y:z$ | Diff. on $z$ |
|---|---|---|---|---|
| Prolectite . . | 1·0803 : 1 : 1·8862 | 22 | 2·389 : 2·210 : 4·169 | |
| | | | | 2·851 |
| Chondrodite . . | 1·0863 : 1 : 3·1447 | 38 | 2·425 : 2·232 : 7·020 | |
| | | | | 2·877 |
| Humite . . . | 1·0802 : 1 : 4·4033 | 54 | 2·428 : 2·247 : 9·897 | |
| | | | | 2·858 |
| Clinohumite . . | 1·0803 : 1 : 5·6588 | 70 | 2·435 : 2·254 : 12·755 | |

[1] In these compounds the fluorine and hydroxyl are isomorphously mutually interchangeable.

This constant extension of the crystal structure in one direction with each addition of $Mg_2SiO_4$ indicates that the increment takes up its position as it were in the form of a slab on one end of the unit of which the equivalence parameters as they stand prior to the addition are the dimensions. But this is not all. A mineral is known—forsterite—having the composition of the increment under consideration, the crystals of which have been examined. A possible set of equivalence parameters for this mineral would be such that $x$ and $y$ were the same (say the mean of) those above, while the $z$ would be equal to the constant increment on the $z$ axis above. It is interesting to compare the axial ratios and equivalence parameters thus calculated with those obtained by direct measurement. The agreement is seen to be remarkably close:

| | $a : b : c$ | $x : y : z$ |
|---|---|---|
| Observed . . . | 1·0757 : 1 : 1·2601 | 2·449 : 2·277 : 2·869 |
| Calculated . . . | 1·0823 : 1 : 1·2775 | 2·429 : 2·245 : 2·867 |

The relation between all five minerals is admirably displayed by a series of models of which a photograph is reproduced in fig. 1. Rectangular blocks having as horizontal dimensions the $x$ and $y$ values and as a vertical dimension the $z$ value for forsterite, when superposed upon a similar set of blocks having the corresponding dimensions of prolectite, form a stack exhibiting the equivalence parameters of chondrodite; a second superposition of forsterite blocks gives a stack corresponding to humite; another forsterite block on the humite stack gives a representation of clinohumite.

After considering the numerical data and the figure shown, no one will be prepared to deny the utility of equivalence parameters as throwing light on obscure morphotropic relationships; many other instances could be quoted but for these the reader should consult the original memoirs of Barlow and Pope.

Before proceeding to demonstrate the lines on which the new theory has been developed and to show how it has been applied to the discussion of the crystalline form of a large number of substances both elementary and compound, we would call attention to a confirmation which has been put forward by Le Bas[1] of the new idea respecting the relation between valency and atomic volume. From a consideration of the molecular volumes,

[1] *J.C.S. Trans.* 1907, 112.

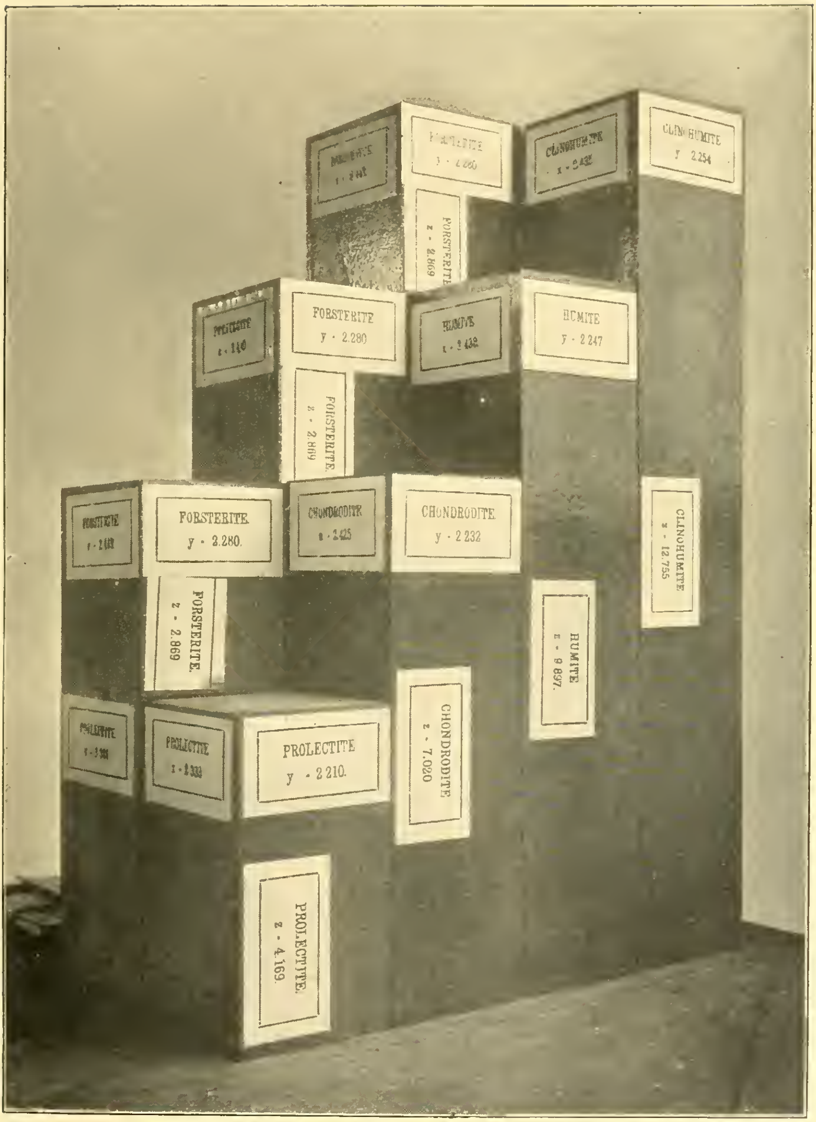


FIG. 1.—Model to demonstrate the relationship between minerals of the chondrodite group.

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determined near their melting-points, of the members of a number of homologous series of organic substances, this worker has come to the conclusion that in these compounds the molecular volume is proportional to the valency volume calculated according to the Barlow-Pope method, each carbon atom being supposed to occupy a volume four times that of a hydrogen atom; further that, at any rate in the case of higher members of such series, the absolute volumes of the atomic spheres are unchanged in the passage from one member to another. The molecular volume of any compound can be regarded as the sum of the volumes of the spheres of influence of the atoms contained in the molecule. It follows that if the molecular volume $V$ be divided by the valency volume $W$, the quotient obtained is the volume of a unit or hydrogen sphere. This unit has been termed a "stere"; the following table shows how remarkably constant is its value throughout the paraffin series of hydrocarbons. The mean value of the stere for this series is 2·970; by multiplying this number by $W$, the molecular volume for any hydrocarbon of the series can be calculated; the calculated values, as can be seen from the table, approximate very closely to those obtained experimentally.

MOLECULAR VOLUMES OF THE NORMAL PARAFFINS AT THEIR MELTING POINTS.

| Hydrocarbon. | $W$ | $V$ | Diff. for $CH_2$. | $\frac{V}{W}$ = stere. | Calculated $V$. |
|---|---|---|---|---|---|
| $C_{11}H_{24}$ | 68 | 201·4 | | 2·962 | 201·96 |
| $C_{12}H_{26}$ | 74 | 219·9 | 18·5 | 2·971 | 219·78 |
| $C_{13}H_{28}$ | 80 | 237·3 | 17·4 | 2·966 | 237·60 |
| $C_{14}H_{30}$ | 86 | 255·4 | 18·1 | 2·970 | 255·42 |
| $C_{15}H_{32}$ | 92 | 273·2 | 17·8 | 2·970 | 273·24 |
| $C_{16}H_{34}$ | 98 | 291·2 | 18·0 | 2·971 | 291·06 |
| $C_{17}H_{36}$ | 104 | 309·0 | 17·8 | 2·971 | 308·88 |
| $C_{18}H_{38}$ | 110 | 326·9 | 17·9 | 2·972 | 326·70 |
| $C_{19}H_{40}$ | 116 | 344·7 | 17·8 | 2·971 | 344·52 |
| $C_{20}H_{42}$ | 122 | 362·5 | 17·8 | 2·971 | 362·34 |
| $C_{21}H_{44}$ | 128 | 380·3 | 17·8 | 2·971 | 380·16 |
| $C_{22}H_{46}$ | 134 | 398·3 | 18·0 | 2·972 | 398·00 |
| $C_{22}H_{48}$ | 140 | 416·2 | 17·9 | 2·971 | 415·80 |
| $C_{21}H_{50}$ | 146 | 434·1 | 17·9 | 2·973 | 433·62 |
| $C_{27}H_{56}$ | 164 | 487·4 | 53·3 | 2·972 | 487·08 |
| $C_{31}H_{64}$ | 188 | 558·4 | 71·0 | 2·970 | 558·36 |
| $C_{32}H_{66}$ | 194 | 576·2 | 17·8 | 2·970 | 576·18 |
| $C_{35}H_{72}$ | 212 | 629·5 | 53·3 | 2·969 | 629·64 |
| | Mean Values . . . | | 17·83 | 2·970. | |

This investigation furnishes positive proof that in this series the volumes appropriated by the carbon and hydrogen atoms respectively are in the ratio of 4:1, *i.e.* in the ratio of the valencies of the elements.

It is worthy of note that the experimental data quoted in the above table were contributed by Krafft a quarter of a century ago and had not hitherto received a satisfactory interpretation. The very close agreement observed between the experimental and calculated values of $V$ is striking testimony to the accuracy of Krafft's work.

### The Architecture of the Crystalline Elements

It has long been known that the majority of elementary substances crystallise in forms exhibiting a high degree of symmetry, most of them belonging either to the cubic or to the hexagonal system. In fact 85 per cent. of the elements which have been examined crystallise in one or other of these systems. This observation can be readily accounted for by the new theory which regards crystals as homogeneous, close-packed assemblages of the spheres of influence of the component atoms. In the case of an element we have atoms of but one kind to consider and it can therefore be immediately assumed that their crystals may be adequately represented by close-packed homogeneous assemblages of equal spheres which stand for the spheres of influence of the atoms. Indeed, a suggestion to this effect was made as early as 1883 by Barlow.

It has been shown independently by the late Lord Kelvin and by Barlow that two modes exist of homogeneously close-packing equal hard spheres. The two modes give assemblages possessing respectively the symmetry of the cubic and hexagonal crystalline systems. Fig. 2 shows a number of spheres packed together to form the cubic assemblage. It may be of interest to the reader to trace out the connection between this assemblage of spheres and one of the 230 homogeneous point systems.

Suppose space partitioned into cubes by three sets of parallel planes at right angles to one another; place a particle at each cube corner and at the centre of each cube face and then discard the cubes, leaving only the particles; what remains is one of the 230 homogeneous point systems. Imagine next that each particle expands uniformly in all directions until it touches its next neighbours; when further expansion ceases, an assemblage of spheres is found to have been formed similar to that in the figure. There is no way of packing together equal spheres more

FIG. 2.—Closest-packed assemblage of equal spheres possessing cubic symmetry.

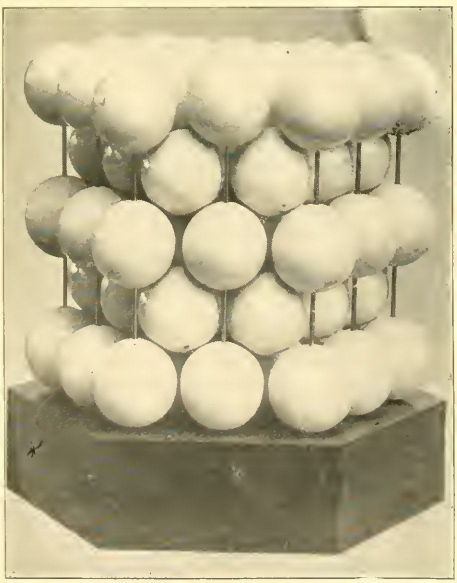

FIG. 3.—Closest-packed assemblage of equal spheres possessing hexagonal symmetry.

closely than the one of which we have traced the derivation. Being derived from a cubic partitioning of space and as it retains the high symmetry of the cube, it is termed the cubic closest-packed arrangement of equal spheres. The fragment shown in the figure outlines a cube; it can be seen that the three directions at right angles, those of the cube edges, are identical in kind; this identity in the three rectangular directions $a$, $b$ and $c$ is conveniently expressed by the ratio $a : b : c = 1 : 1 : 1$.

But we have said that there is another closest-packed assemblage of equal spheres which possesses hexagonal symmetry. This second assemblage is closely related to the cubic form already described, as we will demonstrate. When spheres are removed from one corner of such a cubic fragment as fig. 2, a close triangularly arranged layer is disclosed and it can be seen that the assemblage is built up of such layers superimposed upon one another in such a manner that the fourth layer is directly over the first, the fifth over the second and so on. But there is an alternative mode of stacking these triangularly arranged layers, in which the third layer lies directly over the first, the fourth over the second and so on, the structure thus obtained being just as close-packed as the other but exhibiting hexagonal instead of cubic symmetry. Fig. 3 shows a fragment of such a structure. In both the cubic and the hexagonal types of assemblage, we can regard the structure as built up of triangularly arranged layers and select as a horizontal dimensional unit ($a$) the diameter of a sphere drawn through two contacts and as a vertical dimensional unit ($c$) the distance separating planes drawn through the layer centres. The ratio of $a : c$ then becomes $1 : \sqrt{(\frac{2}{3})} = 1 : 0{\cdot}8165$.

Before we can translate our spheres into atoms and our close-packed assemblages into crystals, it is necessary to take one more step. Suppose the component spheres of each assemblage described above to expand uniformly in all directions until further expansion is checked by contact with neighbouring spheres, all insterstitial space having by that time been eliminated, the spheres are all transformed into twelve-faced polyhedra, those in the cubic assemblage having the form shown in fig. 4 and those in the hexagonal assemblage the form of fig. 5. The regular uniform expansion of each particle of the original point system to a regular dodecahedron is symbolical of the even radiations of forces from the atomic centre. Each close-packed assemblage possesses the property that, with a given density of distribution of the centres, a maximum distance prevails between nearest centres. This result has been arrived at by the geometrical artifice of employing spherical surfaces for the derivation of the close-packed assemblages. It must be clearly understood that the atomic domain in a crystal structure is not to be regarded as spherical or else we should be making a physical distinction between portions of space lying within the spheres and the portions forming the interstices between them; there is no justification for any such distinction. The portion of space dominated by each atom must rather be looked upon as polyhedral.

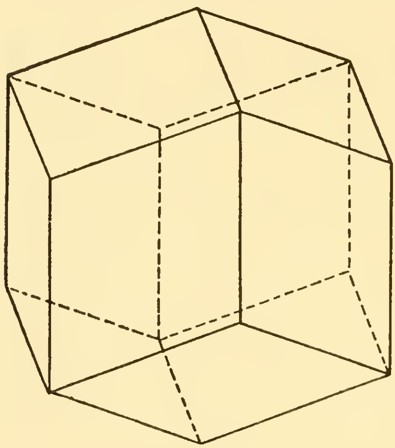

Fig. 4.

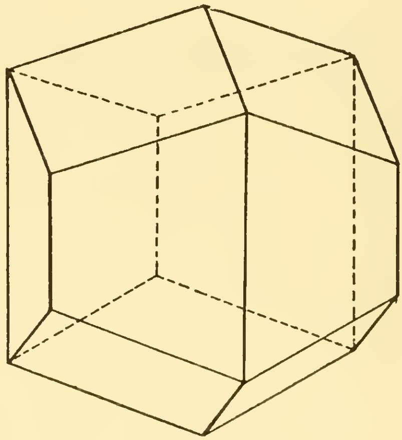

Fig. 5.

If, however, we consider the crystals of elements to be close-packed arrangements of so-called atomic spheres of influence, it is evident that we can expect crystals of but two types—cubic and hexagonal, the latter to exhibit the ratio $a : c : = 1 : 0{\cdot}8165$; as already stated, the expectation is realised to a great extent.

The following elements all crystallise in the cubic system: copper, silver, gold, carbon, silicon, lead, arsenic, vanadium, iron, platinum, iridium, osmium, palladium, titanium, thorium, germanium, mercury, gallium, chromium and nickel; and consequently their crystalline structure must be represented by the cubic closest-packed assemblage of equal spheres: 50 per cent. of the crystalline elements are thus accounted for. Another 35 per cent. crystallise in the hexagonal system with the axial ratios $a : c$ approximating to $1 : 0{\cdot}8165$ or $1{\cdot}6330$

or to an alternative value, 1 : 1·4142, this value depending upon which direction in the horizontal plane of the assemblage (fig. 3) is chosen for the "$a$" translation. The following are the known hexagonal elements with their crystalline constants :

| Element. | Actual Ratio. $a : c$ | Ideal Value. $a : c$ |
|---|---|---|
| Glucinum | 1 : 1·5802 | 1 : 1·6330 |
| Magnesium | 1 : 1·6242 | ,, |
| Zinc | 1 : 1·3564 | 1 : 1·4142 |
| Cadmium | 1 : 1·3350 | ,, |
| Arsenic | 1 : 1·4025 | ,, |
| Antimony | 1 : 1·3236 | ,, |
| Bismuth | 1 : 1·3035 | ,, |
| Tellurium | 1 : 1·3298 | ,, |

It can be concluded that these elements are correctly or very approximately correctly represented by the hexagonal closest-packed assemblage of equal spheres. But the authors of the theory are not content with stating that the observed axial ratios approximate to the ideal values in the crystals of the above elements ; they seek an explanation and make some suggestions to explain why the observed slight discrepancies occur. For example, after studying the case of glucinum, they discover that if one-fourth of the spheres in the hexagonal assemblage be considered slightly greater than the rest (these greater spheres being symmetrically situated) and the packing be made close by compressing the layers, then an assemblage is obtained having $a : c = 1 : 1·5802$, the value observed for glucinum. Of course, beyond the fact that the crystallographic constants can be thus explained, there is no evidence to show that such a state of affairs (*i.e.* the special treatment of certain atoms) may exist in the crystal ; it is significant that the digressions of all the other elements from ideal axial ratios can be explained by some such slight modification.

Among the elements the crystals of which are neither cubic nor hexagonal are sulphur, selenium, tin and iodine. Sulphur is remarkable in that it is known in four crystalline modifications, one orthorhombic, two monosymmetric and one anorthic, this last being rather doubtful. The axial ratios of one monosymmetric form are particularly striking, being $a : b : c = 0·9958 : 1 : 0·9998$, $\beta = 95°46'$.[1] The close approximation of these numbers to the cubic values $a : b : c = 1 : 1 : 1$, $\beta = 90°$, is obvious and it will be admitted that but a very slight distortion of a cubic assemblage

[1] Mitscherlich, *Ann. Chem. Phys.* 1823, 24, p. 264.

would suffice to produce one having the above constants of sulphur. The nature of this distortion cannot be definitely stated but Barlow and Pope show that a minute diminution in size of one-third of the spheres, selected in a particular manner, in a cubic assemblage of equal spheres will cause a distortion sufficient to give the assemblage the above constants. A similar shrinking of every third sphere in a hexagonal assemblage of equal spheres, followed by a slight distortion, gives a structure having axial ratios corresponding to those of orthorhombic sulphur, $a:b:c = 0{\cdot}8108:1:1{\cdot}9005$. The remaining monosymmetric form of sulphur can be accounted for in a somewhat similar manner; and by adopting such devices it is possible to mimic the divergence of the crystals of selenium, tin and iodine from the ideal cubic and hexagonal forms. It is interesting to note in this connection that whilst the colourless elements are all cubic or hexagonal, all the highly coloured elements belong to systems of lower symmetry. Since colour may be taken as conditioned by molecular complexity, it is not unjustifiable to conclude that the abnormality of crystalline form of coloured elements may also be the result of the formation of molecular aggregates.

It must be understood that the preferential treatment of a proportion of the spheres in an assemblage of equal spheres in order to explain the divergence of certain elements in their crystalline forms from the cubic or hexagonal symmetry indicated by theoretical considerations can at present be regarded merely as a geometrical device for obtaining the desired result. At most it is but a slight distortion which has to be accounted for; as was pointed out by Prof. Pope in a lecture recently delivered at the Royal Institution, such a distortion may arise from some want of symmetry in the individual atoms or in a reduction of the symmetry caused by some grouping of the atoms. From a chemical point of view the latter seems, perhaps, a more rational explanation, since some elements, as for example sulphur, are known to have a complex molecule in the gaseous state; but the former explanation is adequate to account for the observed geometrical facts. However, until there is more direct evidence regarding the internal structure of crystalline substances—and it is hard to say at present whence such evidence is to come—the matter must be allowed to rest.

(*To be continued*)

# THE IRON-ORE SUPPLIES OF THE WORLD

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Iron may be said to be the material in which modern civilisation chiefly finds its expression. Substitutes could be found for any other of the metals in use or the world could go on without it; even if our coal supply failed other sources of heat and power would still be available: but modern civilisation would inevitably wither in a serious iron famine, as cheap iron is required for the construction of the appliances used in the cultivation and preparation of our chief foods and in the manufacture of our clothing; it is also indispensable as a building material in our modern cities and in providing the means of rapid transport which render their existence possible.

The exhaustion of the world's supply of iron ores has been repeatedly predicted and one essential difference between the mode in which ores of iron and those of most other metals occur seems at first to give some plausibility to such forecasts. The lodes of gold, copper and tin met with at or near the surface of the earth continue to great depths beneath it. Thus gold is being worked in Bendigo at a depth of 4,700 feet, copper in the Lake Superior region at over 5,000 feet and tin in Cornwall at 3,000 feet; no doubt such ores occur at even far greater depths. The best ores of iron, on the other hand, occur only at comparatively shallow depth: instead of being derived from solutions rising from the deep interior of the earth, they have been deposited on the surface or by water percolating downward from the surface; on this account, oxide ores in sufficient bulk and purity to be of present commercial importance are practically confined to the layer accessible to water containing oxygen; as that depth is limited, it appears most probable that the high-grade ores of iron will not continue downward to depths below the earth's surface in any way comparable with those reached by mines of copper, gold and tin.

It is true that the vast barysphere within the earth is composed chiefly or largely of oxide of iron; that mass, however, is separated from the surface by so thick a layer of rock that the material is quite beyond our reach; whilst the accessible igneous rocks rich in iron contain constitutents which render the material either useless as ore or so refractory, that if it had to be utilised the exhaustion of the world's coal supply would be seriously hastened.

Mr. Andrew Carnegie in his Rectorial Address to the University of St. Andrews in 1902 declared that the British iron ores were nearly exhausted, as the Cumberland supply was almost finished and the mines at Cleveland, except two, would last only for another twenty to twenty-five years. The British ironmaster has been warned repeatedly that the rich Spanish oxides which are used so largely in our blast furnaces will soon give place, as they are followed downward, to poor carbonates; and the American steel-magnate has been assured that the Lake Superior ore, on which the great iron and steel industry of the United States was founded, will be exhausted a few years hence. In answer to various jeremiads numerous estimates have been made in recent years of the quantities of ore actually available. Much information was collected by the British Consular authorities in a report edited by Sir Llewellyn Smith and published by the Board of Trade in 1905; but as these accounts came from men who had no special knowledge of the subject, they were irregular and incomplete. Recognising that the supplies of Scandinavian ore were becoming of increasing importance to the British iron industry, I invited Professor Sjögren to read a paper at the Leicester meeting of the British Association upon the iron-ore reserves of Scandinavia. He kindly undertook this task and visited several of the more important areas to bring the information up to date; his valuable report was published in full by the Association.[1] Tornebõhm had previously prepared an estimate of the iron-ore supplies throughout the world but the information available was so limited and unsatisfactory that his optimistic estimates were unconvincing. Professor Sjögren, realising the importance of more reliable and comprehensive data, proposed that the International Geological Congress

[1] Hj. Sjögren, "The Iron-Ore of the Scandinavian Peninsula," *Rep. Brit. Assoc.* 1907, pp. 332–45.

should arrange for an authoritative estimate to be made by an international body of experts. The work was undertaken by the Congress and thanks to Professor Sjögren and the Secretary, Dr. Gunnar Andersson, a series of fifty-five reports has been collected dealing with all the iron-producing countries of the world. These have been issued by the Geological Congress in connection with its recent session at Stockholm in two large volumes and a folio atlas.[1] They form the most comprehensive attempt yet made to estimate the total available resources of any mineral, as they include the whole world in their range; they deal with an industry conducted on so large a scale that the unit adopted is a million tons and of such antiquity that some of the contributors refer back to the time of Alexander the Great or the still earlier dawn of the iron age.

The reports on the different countries vary greatly according to the local condition of the industry. The most important reports deal with Great Britain, Scandinavia, Germany, France, the United States of America and Russia, the long report by Professor Bogdanowitsch on this last country, owing to the comparative inaccessibility of some of the literature, being of special value. Some of the reports deal with countries which, like Belgium, once had important iron mines, the relics of these, M. Lespineux remarks, remaining as monuments of former folly. There are short notes on countries such as Holland, which contain only insignificant deposits of bog-iron ores far too small to be of commercial value; and there are general statements from some countries in which it has not yet been worth while to make an accurate survey of the ore, though the amount is known to be considerable.

The only continent of which the iron-ore supplies are adequately known is Europe. Professor Sjögren considers it improbable that any new deposits of the first rank will be discovered in this continent but even the European estimates vary greatly in value and it is probable that they may be considerably increased. In the case of the British Isles, as the iron ores belong to private land-owners and not to the State, it is no one's business to estimate the available supplies.

[1] *The Iron-Ore Resources of the World.* An Inquiry made upon the Initiative of the Executive Committee of the Eleventh International Geological Congress, Stockholm, 1910. Vol. I., pp. lxxix+552; Vol. II., pp. 553–1068, 22 pls., 142 figs.; Atlas, 43 maps.

Professor Louis's report on the British iron field is, therefore, of special interest, particularly as he takes a very pleasing view of the extent of our reserves. The British output of pig-iron is now over fifteeen million tons per annum and there is still, on the whole, a slight annual increase in the amount produced. Professor Louis concludes that the British Isles have in reserve 39,500 million tons, containing an average of from 30 to 35 per cent. of iron. If all this ore could be used, it would supply all our blast furnaces for nearly 3,000 years. Professor Louis, of course, recognises that a very large proportion of the ores cannot be smelted profitably under any conditions that may be reasonably expected, though it may be added that he has formed a much more favourable view of the extent of the Wealden ores than I formed after some inquiry into their quantity in 1907.

Dr. Sjögren has divided the world into sections A—D, according to the extent to which available estimates can be trusted. In division A, which includes 13·3 per cent. of the land of the world, the calculations are fairly trustworthy. In division B, including 10·3 per cent. of the land, only very approximate estimates can be made. In division C, including 51·6 per cent. of the land, no numerical estimates are possible. Division D includes the unknown countries and the Polar regions, covering 24·8 per cent. of the land area. Hence even general estimates of the iron-ore supplies are possible for less than a quarter of the land of the world. Professor Sjögren has recently proposed a formula which appears to yield the most probable approximation to the ore in the less-known countries. He takes the average iron-ore content per square mile of the lands in division A and multiplies this factor by the remaining land area; according to this method, the iron-ore supply in it is some 425,000 million tons. This cheering estimate may be exaggerated if Europe and the eastern states of America, by some fortunate circumstance, have been endowed with more than the average share of iron; and as the iron-ores were only discovered after the settlement of those countries, it seems probable that the extent of the ore supplies is due to the thoroughness with which search has been made for them.

No doubt in some cases fuller knowledge may involve a a reduction in the estimated quantities. Vague reports and traditions of mountains of iron ore may prove to be exaggerated;

stories of this sort in reference to China and India are discredited in this monograph. But there seems no reason to doubt that as countries now sparsely occupied become better known increased quantities of ore will be found in them.

As the monograph deals essentially with the quantity of ore, it does not often refer to the manner in which the ores have been formed. In some cases the genesis of an ore must be determined before the amount can be estimated. Iron ores can be classified according to their mode of origin into six main groups. The first, igneous ores due to magmatic[1] segregations in molten rocks, are represented by the titaniferous magnetites such as occur at Tåberg in southern Sweden and at Routivaara in Lapland; but few ores unquestionably of igneous formation are at present of practical value. It is true that this origin is accepted by some leading authorities for the great masses of magnetite in Lapland; but this view is not yet established and Mr. Lundbohm, the manager of the Kiruna Mines, both in his report in this monograph and in another submitted at the recent meeting of the Geological Congress, represents this question as still unsettled.

The second group includes ores formed as contact-deposits beside eruptive rocks; the present tendency is to transfer some ores, such as those of Gora Blagodat in the Urals, from the magmatic to this group. There is also a somewhat uncertain boundary between contact ores and those of the third group, the lodes and masses due to the replacement of other rock by ironstone. They are represented, for example, by the masses of rich kidney iron ore in the Carboniferous limestones of the north of England, which provide the only British supply of Bessemer ore. This group also includes the famous iron mines of southern and central Sweden.

The ores of sedimentary origin which form the fourth group include the most widely distributed ores, now of much economic importance. Amongst them are the Carboniferous and Mesozoic ironstones of the British Isles and of Spain, the Clinton ores of the United States and the vast beds of minette in France and Germany.

The fifth group comprises the banded ironstones and their

[1] Magmatic ores are those formed by the concentration in a molten rock of a metallic constituent that is normally scattered through it into masses sufficiently large and rich to be worked as ore.

associated ore bodies, which are ores of great importance in the United States and in all the existing fragments of the ancient continent of Gondwanaland. They occur in Brazil, South Africa, India and Australia. Hitherto it has only been in the Lake Superior region of North America that these ores have been of great importance; they have there furnished the main supply of material to the iron and steel industry of the United States. The iron has been concentrated into huge bodies of ore by water, which has dissolved the metal from the rocks above and deposited it where the descent of the solution has been stopped by some impermeable layer.

The last group includes the laterites which are so widely spread as superficial deposits in the tropics and sub-tropics. They extend occasionally farther into the temperate regions, occurring, for example, in workable quantities in the north of Ireland. Hitherto these ores have only been mined to supply local demands or for use as iron-bearing fluxes. Many primitive races still obtain their iron from laterite; the prehistoric smiths of the early iron age were probably mainly dependent on such ore and in the event of a serious rise in the price of iron ore laterite deposits would once again become of great industrial importance.

The estimates of the iron ore supplies throughout the world included in the various reports have been collected in tables compiled by Mr. Tegengren; the chief results are shown in the list on the opposite page.

The total reserves of ore are shown by the following list to be enormous and ample for all probable requirements. The estimates are the best hitherto made but they are inevitably indefinite. More precise estimates would hardly be worth the time and trouble it would cost to procure them. A final determination of the iron ore available is impracticable because there is no fixed test of what is ore, the availability of a material as ore varying with political and commercial changes. The percentage of iron in a material affords no absolute separation between iron-bearing rock and iron ore. Thus 60 per cent. of iron in a material may be useless when associated with much titanium, whilst another containing only 20 per cent. of iron may be a valuable ore. Commercial conditions also control the profitable working of ore and therefore affect the estimates hitherto made.

| Country. | Actual Ore Supplies. | Equivalent in Metallic Iron. | Potential Ore Supplies. |
|---|---|---|---|
| | Million tons. | Million tons. | Million tons. |
| France | 3,300 | 1,140 | — |
| Luxemburg | 270 | 90 | — |
| Spain | 711 | 349 | considerable |
| Portugal | — | — | 75 |
| Italy | 6 | 3·3 | about 2 |
| Switzerland | 1·6 | 0·8 | 2 |
| Austria | 250·9 | 90·4 | 323·2 |
| Hungary | 33·1 | 13·1 | 78·9 |
| Bosnia and Herzegovina | — | — | 21·9 |
| Servia | — | — | probably moderate |
| Bulgaria | — | — | 1·4 |
| Greece | 100 | 45 | probably moderate |
| Turkey | — | — | considerable |
| Russia | 864·6 | 387·2 | 1056·3 + |
| Finland | — | — | about 45 |
| Sweden | 1,158 | 740 | 178 |
| Norway | 367 | 124 | 1,545 |
| Great Britain | 1,300 | 455 | 37,700 |
| Holland | insignificant | insignificant | moderate |
| Belgium | 62 | 25 | — |
| Germany | 3,607·7 | 1,270 | enormous |
| Canada | considerable | considerable | probably enormous |
| Newfoundland | 3,635 | 1,961 | enormous |
| United States | 4,257·8 | 2,304·6 | 75,105·3 |
| Mexico | 55 | 30 | probably considerable |
| Central America | — | — | exist |
| West Indies | 1,903 | 856·8 | 1,007 |
| Columbia, Venezuela, Bolivia, Peru and Chile | 4·2 | 2 | considerable |
| British Guiana | — | — | exist |
| Dutch Colonies | — | — | exist |
| Brazil | — | — | 5,710 + |
| Argentine | — | — | exist |
| Western Australia | — | — | 26 + |
| South Australia | — | — | 21·6 |
| Queensland | — | — | 13·7 |
| New South Wales | 48·9 | 26·8 | 5·1 |
| Victoria | — | — | moderate |
| Tasmania | 23 | 15 | 2 |
| New Zealand | 64 | 32 | 0·2 + considerable |
| Asiatic Russia | — | — | 27 + considerable |
| British India | 100 | 65 | 400 + considerable |
| China | 100 | 60 | probably enormous |
| Japan | 55·6 | 28 | moderate |
| Rest of Asia | 4·8 | 2·5 | — |
| Algeria and Tunis[1] | about 125 | about 75 | — |

[1] The supplies in the rest of Africa are at present incalculable and are reported from the various states as considerable, moderate and enormous.

Material that in one place or time may be quite valueless may become a useful ore through some change in political or commercial conditions. For example, there are said to be large supplies of high-grade ore available in Honduras but it has not yet been worth any one's while to test these reports: no doubt, if Honduras were a great importing country the ore would be investigated at once, as it could then be exported owing to the cheapness of return freights. The working of British ores, on the other hand, is retarded because the ships that export our coal bring back ore at an abnormally low rate.

The conditions controlling the development of an iron ore are so complex that no simple rules can be stated and so some of the reports include ores containing as little as 20 per cent. of iron; others restrict the limit to a minimum of 30 or 40 per cent. or even 60 per cent. The lower-grade ores can be used if they form a good smelting mixture with richer ores or after the others are exhausted. Twenty years ago the ores used in the United States contained an average of 62½ per cent. of iron; at the present time cargoes containing over 60 per cent. are exceptional, the average being about 50 per cent.; even cargoes of 40 per cent. iron are used and in Alabama ores are mined and included in the reserves having on an average as little as 36 per cent. of iron.

Though the data collected by Dr. Sjögren show that the fear of an iron famine is idle, the supply of ores containing 60 per cent. of iron is acknowledged to be comparatively limited. The total supplies of such ore are estimated at 1,300 million tons of known ore and some 687 million tons of potential reserves. Of the known supplies, about 1,100 million tons or more than four-fifth of the total known amounts are in Sweden. In America, excluding ores too rich in titanium to be of present value, the reserves amount to only 58 million tons and are limited to Mexico and the West Indies; Australia reports 49 million tons in Westralia and Tasmania and an additional 34 million tons of potential reserves in South Australia and Queensland. In Asia there are 530 million tons, all belonging, however, only to the potential reserves.

At the present time about 60 million tons of pig-iron are produced every year; the production has on the whole more than doubled in every twenty years, so that if we were dependent on ores containing 60 per cent. or more of iron, the known supplies would only last another fourteen years, though the potential supplies, which have been crudely estimated, including 250 million tons in India, would last another six or seven years. But when the lower-grade ores are included, the total actual reserves of iron are estimated at over 10,000 million tons, and the potential at 50,000 million tons, exclusive of enormous supplies which are still unmeasured and may amount even to ten times that great amount. The known supplies, at the present rate of production, would last two hundred years. Including the potential reserves, they would last over a thousand years; by lowering the grade of ore that iron-masters are willing to smelt, ample ore supplies would be available for further millenniums.

These estimates fully confirm the opinion expressed to the Geological Section of the British Association in 1907, when I remarked that—

> "The geologist who knows the amount of iron in most basic rocks finds it difficult to realise the possibility of an iron famine; he can hardly picture to himself some future ironmaster complaining of 'iron, iron everywhere and not a ton to smelt.' There are reserves of low grade and refractory materials which the fastidious ironmaster cannot now use, since competition restricts him to ores of exceptional richness and purity. When the latter fail, an unlimited quantity could be made available by concentration processes. The vast quantities of iron ores suitable for present methods of smelting in Australia, Africa and India show that the practical question is that of supplies to existing iron-working localities and not of the universal failure of iron ores."[1]

The future demands on the world's supplies of iron ore will be affected by industrial changes and on those of separate countries by their financial policies. Thus the introduction of reinforced concrete must materially diminish the demand on the iron ores. The adoption of a protectionist policy by a country may lead to the development of its own iron mines and perhaps the closing of those of some of its neighbours. Thus the imposition by the United Kingdom of a tariff on imported iron ores would no doubt lead to the working of British ores that now lie unutilised and might close some Mediterranean iron fields which can only mine iron ore for export, having neither fuel supplies nor water power.

[1] Presidential Address to Section C, *Rep. Brit. Assoc.* 1907, p. 501.

Alarmists tell us that we are using an unfair share of minerals and that there will be none left for future generations. On the other hand, the well-known commercial principle, use an asset whilst you can, is applicable to many iron ores. For when South America, Asia, Africa and Australia smelt their own high-grade ores, iron produced from our poorer ores will not compete in their markets. Hence European ores which can be worked profitably at present may become valueless if not used before the newer countries establish iron industries of their own. If the present opportunity of working such ores be lost there may never be another.

The real danger to the future in regard to the iron-ore resources of the world is not the limited quantity of ore but the limited supply of coking coal necessary for its reduction. Even if the heat required for smelting iron be supplied electrically, one ton of coking coal is required for the reduction of two tons of iron ore. The supply of iron ore is unquestionably adequate; indeed, it is certain that there will be plenty remaining when there is no more coking coal left for its reduction.

[The following most interesting statement as to the discovery of the metal iron is copied from the South African Supplement of *The Times* published on November 5, 1910:

"Special interest has been aroused in the problem of the origin and history of iron-working in Africa. As is well known, Africa south of the Sahara came into its Iron Age without having passed through a Copper or Bronze Age and thus the development of the iron industry was not reached through the normal sequence of successive stages which characterised the culture-history of Europe, North Africa and elsewhere. 'Savage' Africa passed directly from stone to iron. The question arises—Did the native negro peoples acquire the art of iron-working from peoples who had already acquired and developed it or is the art indigenous amongst them and a product of native inventiveness? Recent researches and deductions by Dr. von Luschau and others have tended to support the latter hypothesis and speculations have arisen as to the possibility of the art of extracting iron from the ore and of forging it into useful appliances having not only been developed at an early period amongst the negro peoples of Africa but, further, of its having been transmitted through Egypt into Europe. If further archæological investigations support this view and if it can finally be proved that the relatively highly cultured Bronze Age peoples of Europe owe to the African native the suggestion that iron could be substituted for the inferior metals copper and bronze, it will be recognised that the world at large owes a deep debt of gratitude to the primitive indigenous population of the Dark Continent.

"In Egypt, the natural link between 'Savage' Africa and the Mediterranean, the early history of iron-working is obscure. Iron has been found in deposits reputed to belong to the sixth dynasty (*c.* 4000 B.C.) and even to the fourth dynasty (*c.* 4600 B.C.). In the Egyptian paintings of early Mycenæan times iron is represented in blue to distinguish it from copper and bronze (red and yellow). Still, the evidence of the commencement of the iron-using period in Egypt is very incomplete and fresh investigations will be required ere this point is cleared up.

"In Europe, as Dr. von Luschau urges, the Iron Age began at a comparatively late date, probably not earlier than 900 B.C. Hence it is improbable that Egypt received the idea of iron-working from Europe. For the same reason it is unlikely that inspiration was derived from Assyria and Babylonia, whose Iron Age does not appear to have commenced prior to 1000 B.C. In India, too, it was a development of comparatively late date. If we exclude the north and the east, there remains only the south (for the west may be disregarded) as a possible source whence Egypt, and through her Europe, acquired the art of manufacturing iron, unless we assume that in Egypt itself is to be found the fountain-head of the industry.

"A study of the present conditions obtaining amongst the native African iron-workers tells us several things of interest which bear upon this problem. First, we find that many of the native iron ores are produced with little trouble and are easily reducible at a relatively low temperature. From hematite, for example, malleable iron which can be forged into bars can be extracted by heating in a charcoal fire for a few hours with the help of bellows. The ease with which native ores are reduced renders it possible, if not probable, that the process of extraction of iron from the ore may have been accidently hit upon by a primitive people through pieces of the ore becoming repeatedly, though unintentionally, mixed up with the ordinary domestic fires. Some suggestive evidence is, moreover, acquired from a comparative study of the primitive methods employed by the natives in reducing the ore, more especially in Equatorial Africa. In some districts a very rudimentary process still prevails, the broken ore and charcoal being merely heaped up upon the ground. After the requisite heat has been raised with the help of bellows, the extracted iron falls to the bottom of the mass and is collected. As a slight improvement, in some districts the fire is built over a shallow pit or depression in which the iron collects. Elsewhere, again, the mass of charcoal and ore is bounded with lumps of clay, which form an incomplete confining wall. From this stage may have been evolved the idea of confining the fire within a complete encircling clay wall, whose height was increased as it was found that a more effective draught was obtained thereby. This probably led to the development of the tall, circular furnaces of clay employed by the famous iron-working tribes of the Bahr-el-Gazal region and elsewhere. If these several graduated methods applied to the reduction of iron ore may be regarded as survivals from various stages in the developmental history of the process, the probability of this industry having been arrived at by the native African negro independently of outside influence is greatly increased. The wide range of dispersal of these methods over Africa south of the Sahara also favours this view.

"The structure of the most characteristic and most widespread form of native African bellows, in which the flexible membranes are worked with a pair of sticks held in the hand, links the modern negro blacksmith with the ancient Egyptian metal-workers of the eighteenth dynasty, who employed the same type of bellows very slightly modified, a type which does not occur outside the confines of the African Continent. The famous eighteenth-dynasty painting at Luxor, representing the casting of the bronze temple doors, shows clearly the form of the ancient Egyptian bellows and establishes their identity with the prevailing Central African form of to-day, incidentally, moreover, proving an affinity between the metallurgical methods of the two peoples. Further researches may prove that the earliest experiments in iron-working took place in Egypt and that the early history of the industry had its home there ; but so far convincing evidence of this is not forthcoming and in the meantime a fairly strong case can be made out for the origin and early development of iron-working amongst primitive peoples of negro stock, probably in or near the equatorial region. It may appear revolutionary to suggest with Dr. von Luschau that the African negro may have been the real discoverer of the potentialities of iron and the inaugurator of the Iron Age, the introduction of which revolutionised culture in Europe and hastened to a phenomenal extent the advance towards civilisation ; but the supporting evidence is strong enough to call for a following up of the clues."]

# SOME SCIENTIFIC ASPECTS OF THE REPORT OF THE CANADA AND WEST INDIES ROYAL COMMISSION

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In recent numbers of Science Progress two articles have appeared on "Agricultural Progress in the Tropics."[1] This is a subject that has assumed considerable importance in recent years, and as the productions of the Tropics are daily becoming more and more necessary to the inhabitants of temperate countries it deserves further attention.

It is acknowledged that Great Britain is "mistress of the richest tropical possessions in the world" and it has recently been urged that this is a time when "commercial supremacy largely depends upon the control and development of the Tropics."

Speaking roughly it may be said that there are about three million square miles (1,920 million acres) of British territory lying within the Tropics. The total value of the exports yielded by this area is computed at not less than 230 millions sterling. An appreciable proportion of the exports are received in the United Kingdom, and supply not only foodstuffs but the raw material for the manufacturing industries on which the prosperity of this country depends.

So far, nothing has appeared in these pages on the considerable progress made in agricultural matters in the Western Tropics. The conditions existing there are so markedly different from those in the Eastern Tropics that they deserve separate treatment. Further, as the result of the labours of two Royal Commissions, there is an enormous amount of material accumulated in regard to the agricultural conditions in the British West Indian Colonies. It is not proposed here

[1] J. C. Willis, "Agricultural Progress in the Tropics," Science Progress, 1910, v. pp. 48–59 and 219–33.

to deal at all exhaustively with so large a subject. The exigencies of space will only permit of a brief review of the Report of one of the Commissions above referred to. This, however, will sufficiently emphasise the value of scientific methods in extending agricultural industries and also, to some extent, in securing favourable markets for the produce.

It is recognised that true agricultural progress cannot be confined to merely increasing the production. Many other factors have to be taken into account; among them may be mentioned efficient means of communication by steamers and telegraphs, and opportunities of disposing advantageously of agricultural produce, so that the cultivators may obtain adequate remuneration for their labours.

The first West India Royal Commission was appointed by her late Majesty Queen Victoria in December 1896. To this was entrusted an inquiry into the condition and prospects of the West Indies, and "to suggest such measures as appear best calculated to restore and maintain the prosperity of those Colonies and of their inhabitants." Sir Henry Norman was chairman. The other members were Sir Edward Grey (the present Secretary of State for Foreign Affairs) and Sir David Barbour.

The Commissioners reported in the autumn of 1897 that owing to various causes the West Indies, a purely agricultural community, were at that time in a very depressed condition, and they recommended the adoption of measures having for their object the improvement of the conditions under which the cultivation of the sugar-cane, the chief mainstay of some of the Colonies, was carried on, the introduction of other and more remunerative industries, suitable for the conditions existing in the West Indies, and the creation of an Imperial Department of Agriculture. The latter was organised in October 1898, and during the last twelve years, as will be shown later, valuable work has been done, with the co-operation of other agencies, in improving the general condition and prospects of the Colonies.

The second Royal Commission, appointed in August 1909 by the late King Edward, had not to deal directly with the improvement of agricultural methods and the increase of exportable commodities, but to secure a reliable market for West Indian productions, especially for sugar.

This Commission was charged to inquire into the commercial relations at present existing between the Dominion of Canada and the West Indian Colonies, and recommend steps which should be taken "in order to secure, encourage and develop mutual trading facilities, the improvement of transportation, and a cheaper and more efficient telegraph system, together with all other matters that may appear to be best calculated to strengthen and to extend commerce and communication between Canada and the West Indies."

For the purpose of this Commission it was understood that the term "British West Indies" would include Bermuda, the Bahamas and British Honduras, in addition to the British West India Islands as usually so-called, and British Guiana.

The members of the Commission were five in number, with Lord Balfour of Burleigh chairman. Two of the Commissioners (the Hon. W. S. Fielding and the Hon. William Paterson) were Ministers of the Canadian Dominion. In this respect the composition of the Commission was of a special character, indicative of the growth of common interests between the Mother-Country and the Oversea Dominions. The other Commissioners were Lord Islington (now Governor of New Zealand) and the present writer.

In fulfilment of the duties entrusted to them the Commissioners held sittings in the principal cities of Canada in the autumn of 1909. They travelled through the British West Indian Colonies in the spring of 1910, beginning with Jamaica and visiting in order Barbados, British Guiana, the Leeward Islands, Windward Islands and Trinidad. In the summer of 1910 the Commissioners received evidence in London on the subject of telegraphic and steamer communication; and from associations and individuals interested in the production of sugar in the West Indies, and its sale in Canada.

In spite of the complexity of the inquiry and the considerable labour involved in travelling to and from Canada and the West Indies to take evidence on the spot, the Commission brought its labours to a close in little more than a year, "an achievement," according to *The Times*, "so unusual in Royal Commissions, that it cannot be too warmly recognised."

The Report, which has been characterised as a remarkable one, in the sense that it shows a great advance in dealing with questions affecting the Oversea Dominions, opens with a summary of the events leading to the appointment of the Commission. Proposals for reciprocity between Canada and the West Indies have been repeatedly considered in the past. In 1890 the Minister of Finance in the Dominion visited the West Indies with this object in view; the project then met with little favour. At that time trade negotiations were being carried on with the United States, and it was pointed out that the Dominion could then consume only one-third of the West Indian sugar crop, of which the bulk went to the United States.

In 1898 the Canadian preferential tariff was extended to the British West Indies. The Canadian Government recognised the large opportunities that existed for the development of trade between Canada and the West Indies, whom they regarded as the natural customers of the Dominion. The West Indian Colonies were at that time suffering from a severe depression, so much so that their condition was a matter of serious concern to the people of the Colonies and a grave problem for the Imperial Government. Mr. Fielding, then, as now, Minister of Finance, claimed that Canada had some Imperial responsibilities in the matter; he stated that the Canadian Government, having the desire to assist his Majesty's Government in dealing with these problems, had decided to extend the preferential tariff to the West Indies without demanding any concessions in return.

At first the effect of the preference was disappointing, chiefly owing to the favourable market existing for West Indian sugar in the United States; but in 1903 the attractiveness of the Canadian market was increased by a singular concourse of events. These were: (*a*) the adoption of the Brussels Convention, which aimed at the abolition of bounties on beet sugar in European countries; (*b*) the surtax imposed on sugar and other commodities imported from Germany; (*c*) the preferential treatment accorded by the United States to sugar imported from Porto Rico, the Philippines and Cuba; (*d*) the increase of consumption of sugar in Canada. By the operation of (*b*) the importations of German sugar into Canada, which at one time amounted in value to over £600,000, have since nearly disappeared. The heavy duties imposed on West Indian sugar in the United States have practically closed that market, except under special circumstances, such as the falling off of supplies from their own possessions.

The combined effect of the four causes above referred to is shown in the statistics of the sugar imported into Canada. The importations from the West Indies in 1897 did not exceed 11,000 tons. Up to the year 1903 they had not much increased, but in the year 1909 they had reached a total of 133,000 tons, or about 60 per cent. of the total production of the West Indian Colonies.

The attention thus drawn to the Canadian market led to a renewal of the proposals for a reciprocal arrangement between Canada and the West Indies. In British Guiana in 1903, and in Trinidad in 1904, steps were taken in that direction, but with no practical result. As a consequence of efforts made by the Imperial Department of Agriculture, the Boards of Trade of Toronto, Halifax and St. John sent a Commission of three delegates to the West Indies to study trade conditions. The report which followed was of considerable interest and value. In the same year the Department initiated a more general movement, which resulted in the assembling of a Conference at Barbados in January 1908 to consider the steps that might be taken to encourage more intimate trade relations with Canada. This Conference was attended by representatives of all the West Indian Colonies. The Dominion Government also was represented by two delegates. Resolutions were adopted in favour of entering into negotiations for reciprocal tariff concessions, for the establishment of an improved and cheaper telegraphic communication, and for increased transportation facilities, both by a service of steamers to the West Indies and by rail in Canada.

The next step, which eventually led to the appointment of the Royal Commission, was taken by the Canadian Government, which, in view of the difficulties foreseen in the conclusion of separate reciprocity agreements with the several West Indian Colonies, proposed in a Report of the Committee of the Privy Council that the whole subject should be further considered by a Conference, organised by Imperial authority in the form of a Royal Commission or otherwise.

Since the Report of the Royal Commission of 1897 certain important changes have taken place in the condition of the West Indian Colonies. This Commission found the situation one of extreme depression, due to the low prices then prevailing for sugar products, which formed the largest share of the exports. The Commission drew attention to the danger that the West

Indian Colonies incurred in their dependence on a single industry, and their recommendations were largely directed to the establishment of other and alternative industries. The result of this policy during the last twelve years is shown in the fact that while the total exports of the produce and manufactures of the Colonies have increased from £5,625,000 to £7,195,360, the exports of the products of the sugar-cane (sugar, rum and molasses) have declined from £3,243,000 to £3,037,660. On the other hand, the exports of other commodities, such as cacao, fruits, cotton, logwood extract, tobacco and cigars, rice, coconuts and rubber, have more than trebled, viz. from £1,382,000 to £4,157,700.

The recent Commission entirely concurs with the Commission of 1897 as to the danger of dependence on a single industry, and strongly supports a continuance of the efforts that have been made with such signal success to develop other industries suitable to the soil and climate of the West Indian Colonies.

At the same time it is recognised that the sugar industry is still the dominating factor in such Colonies as British Guiana, Barbados, St. Lucia, Antigua and St. Kitts. In Jamaica the proportion of sugar products is 12·6 per cent. as against 18 in 1896; and in Trinidad 24·7 as against 57 per cent. in 1896. In Grenada, St. Vincent, Dominica and Montserrat the exports of sugar products are negligible. It is encouraging to find that, in colonies where the natural conditions are favourable for sugar-growing, recent events have tended to place the industry on a surer foundation. Slightly higher prices and more assured markets have brought improved credit, with the result that the central factory system is being extended in Jamaica, Antigua and elsewhere, and that more scientific methods of cultivation, including the use of artificial and other manures, and of new pedigree canes are being adopted in all the Colonies. It is admitted that an important factor that has contributed to this is the abolition of the continental bounties. To this must be added the policy of the Dominion Government which has provided in Canada a market for sugar more or less lost in the United States. It still remains true, as pointed out by the Commission, that for some important products of the West Indies, for example, bananas from Jamaica, cacao and asphalt from Trinidad, and fresh limes from Dominica, the United States remains the largest market.

A summary of the agricultural and commercial conditions in the several Colonies forms Part iii. of the Report. This deserves the careful attention of those interested in the productions of tropical countries. The details are too voluminous to be discussed here.

The British West Indies, in the larger sense in which they are dealt with in this Report, cover an area of 109,836 square miles or a little less than that of the British Isles. The population is estimated at 2,300,000. The annual total trade (imports and exports) is of the value of £20,662,348. Of the imports about 40 per cent., in manufactured and other goods, are obtained direct from the United Kingdom.

Attention is drawn to the extensive tracks of cultivable land still awaiting development in Colonies such as British Guiana, British Honduras, Jamaica and Trinidad. Where native labour is not available for developing new agricultural industries, a well-organised system of immigration which has been in satisfactory working for many years is carried on with the co-operation of the Government of India. The number of coolie immigrants now under contract or settled as free coolies on small holdings in the West Indies is estimated at about a quarter of a million.

The report proceeds with an inquiry into the value of the Canadian preference to the West Indies. The Commissioners found very early in their inquiry that there was considerable conflict in the evidence as to the division of the benefit arising from the preference given by Canada as between the buyers and the producers respectively. After careful consideration of all the facts placed before them they express the opinion that the preferential policy initiated by the Canadian Government has been of undoubted benefit to the West Indian producer of sugar. Taking one year with another the latter has received from "a third to a half, or approximately from 9*s.* to 14*s.* per ton above the price which he would have been able to obtain without the preference." On the other hand, the Commissioners hold that the Canadian refiner has also benefited, since "it creates in a large body of producers an interest in selling to him," and to a great extent it relieves him from the necessity of competing for his supplies in other markets. "It also, by reducing duties, probably stimulates consumption and improves his business."

In another aspect of the subject the Commissioners are of opinion that the permission given by the Dominion Government in the early months of 1909 to the refiners to purchase and import 20 per cent. of their consumption of non-preferred sugar upon preferential terms was rightly regarded by the West Indian producer with deep concern, as the effect of the concession was to reduce the value of the Canadian market to him to the extent of 20 per cent. It was also regarded that it might place a weapon in the hands of those interested in the Canadian refinery industry which would enable them to exercise such pressure upon the West Indian producers as would deprive the latter of any advantage from the preference.

The situation was a delicate one but the difficulty is likely to be overcome by a compromise suggested by Sir Nevile Lubbock and supported by Mr. Sandbach Parker, that, provided the concession to the refiners were withdrawn, a smaller preference than at present exists would attract West Indian raw sugar, and would be regarded as a material benefit by the growers.

The question of the continuance of the Canadian reciprocity is next discussed. It is admitted, as the result of the preference in the customs tariff, that the Canadian market has become of great importance to the sugar-growers in the West Indies. The grant of the preference was part of the Imperial policy of the Dominion Government, which neither demanded concessions in return nor even questioned whether the tariff systems in force in the West Indies imposed any burden on the trade of Canada. It is evident that from the point of view of the Dominion the existing position is by no means satisfactory, the Canadian Government gave the preference freely and they would be justified in withdrawing it if it suited their policy to do so. The Commissioners represent "that if this contingency can be avoided, or even deferred, by some present concessions on the part of the West Indian Colonies that concession ought to be made." The question then arises, What is to be the nature of the consideration which the West Indian Colonies can offer Canada in return for an undertaking that the benefits which they have so long enjoyed shall not be withdrawn? It was understood that any concession made to imports from the Dominion should also be extended to like commodities from the United Kingdom. This principle has obtained universal recognition in the West Indies. The Commissioners then arrive at a further stage, namely, that if the principle of the policy of Preferential Tariffs between Canada and the West Indies be accepted there remain three points to consider: (*a*) the selection of the goods on which a preference might be granted; (*b*) the amount of the preference; (*c*) the method of effecting the preference. Each of these is fully discussed, and conclusions are arrived at leaving little doubt that in the special case of the West Indian sugar colonies, at all events, a scientific tariff can be arranged beneficial to both countries, whilst, at the same time, the interests of the Mother-Country are fully provided for.

In the case of Jamaica, owing to the fact that its most important industry—that of fruit-growing—is dependent mainly for its market on the United States, the Commissioners recommend "that in any arrangement that may be discussed between Canada and the West Indian Colonies it should be understood that the position of Jamaica is entirely distinct from that of the other Colonies, but the way should be left for the subsequent adhesion of Jamaica if that Colony should afterwards so desire." British Honduras is held to be in similar case. Bermuda and the Bahamas at present trade almost entirely with the United States; the prospect of closer trade relations between these and Canada must depend mainly on better steam communication.

The Government of Newfoundland was represented at the investigation, and expressed its desire to share with Canada in any trade arrangement that might be made with the West Indies. This desire is regarded favourably by the Commissioners.

A matter of some importance that was brought forward in evidence was the possible attitude of the United States in the event of the West Indies entering into preferential relations with Canada. After full and careful inquiry the Commissioners are satisfied that "it may be regarded as a settled principle that trade arrangements between parts of the British Empire are to be considered matters of a domestic character which cannot be regarded as discriminatory by any foreign Power." The United States did not treat the Canadian preferential tariff as an undue discrimination, and "it follows that the granting of a preference by the West Indies to any part of the British Empire could not be so regarded. This is the logical conclusion to be drawn from the fiscal arrangements of other Powers, including the United States themselves, with different parts of their own tropical possessions."

In discussing the subject of steamer services the Commissioners are of opinion "that the development of the trade between the West Indies and Canada depends very largely upon the provision of adequate means of transportation. Having regard to the importance to which we refer elsewhere of retaining and extending as far as possible the market which Canada offers for West Indian sugar, and which in the future the Dominion may be able to offer to other products of the West Indies, we would represent that the maintenance and improvement of the steamship service between Canada and the West Indies is in the highest degree essential."

With this view it is recommended that "early steps be taken by calling for tenders to ascertain the cost of such a service as we have indicated and that very careful consideration should be given to the possibility of its inauguration."

Under telegraphs the conclusion arrived at is briefly summarised as follows: Already the British Government subsidises three telegraph companies to the amount of £27,420, and the earnings of the company are put at £24,000 a year. Thus the Government could at a cost little, if at all, exceeding the amount of the subsidies now paid, give the West Indies an incomparably better telegraphic equipment and probably at the same time considerably reduce the rates.

In the early part of the Report the Commissioners drew attention to the work of the Imperial Department of Agriculture in improving the sugar industry and extending the cultivation of alternative industries in order to obviate the danger of the dependence of the West Indies on a single industry. Later (p. 38) they state: "In the course of our inquiry into the possibilities of the development of the resources of the West Indies we were much struck by the excellent work that has already been done in this direction by the Imperial Department of Agriculture." This Department, as already mentioned, was established in 1898 on the recommendation of the West India Royal Commission of 1897. The whole cost during the first six years amounted to £17,400 a year. This was entirely provided from Imperial funds. Of this, about £5,000 a year was allocated to the maintenance of the scientific staff at the central office and £12,400 as grants-in-aid of the maintenance of botanic and experiment stations and on agricultural education in the several Colonies.

In 1906 the grants to Jamaica to provide the services of an agricultural lecturer, to British Guiana for carrying on scientific experiments to improve the sugar industry, and to Trinidad (for Tobago) for maintaining a botanic and experiment station were withdrawn. Since 1908 the grants to the other Colonies have been gradually reduced and the balance in each case has been provided by contributions from colonial funds. It is in contemplation that in 1913, or possibly earlier, all contributions from Imperial funds for the maintenance of the local agricultural departments will entirely cease.

The duties entrusted to the Department include the general improvement of the sugar industry, and the encouragement of a system of subsidiary industries in localities where the conditions are more favourable for the production of such crops as cacao, coffee, bananas, limes, cotton, rubber, coco-nuts, sisal hemp, rice, nutmegs and pineapples. Attention has also been devoted to the improvement of the breed and condition of cattle, horses and small stock, and to the extension of bee-keeping.

As the mass of the people in the West Indian Colonies must continue to be dependent on the products of the soil, a prominent position has been given to teaching the principles of elementary science and agriculture in the primary and secondary schools. The Department has devoted special attention to the scientific investigation of questions affecting the sugar industry. The investigations have included the raising of new seedling varieties of sugar-canes capable of yielding a larger return per acre and of withstanding disease; the testing the relative values of manures, and of different methods of tillage; and the introduction of modern agricultural implements.

The Department has carried on experiments in manuring cacao plantations and in dealing with fungoid and insect pests affecting cacao.

The Commissioners conclude as follows:

"The great extension in recent years of the cultivation of cotton in the West Indies is very largely due to the efforts of the Department, who took especial pains to supply at cost price large quantities of seed of the best variety of Sea-island cotton. Three fully-equipped cotton-ginning factories were erected and worked under the auspices of the Department in St. Vincent, Antigua and Barbados. The factories at Antigua and Barbados are now run on co-operative lines.

"The Department has purchased pedigree animals for the purpose of improving local breeds of cattle and horses in the smaller islands. A distinct improvement has been effected in some localities, especially in small stock.

"One of the most important services rendered by the Department has been the investigation and treatment of the pests and diseases which attack the staple products of the West Indian Colonies. The Department has, moreover, taken an active part in securing the fumigation of all imported plants, with a view of preventing the introduction of other pests.

"Popular agricultural knowledge has been disseminated among all classes of the community by travelling instructors, and assistance has been given by the officers of the Department to the promotion of intercolonial agricultural conferences and agricultural shows.

"The Department issues a popular fortnightly review (now in its ninth volume)[1] and a quarterly scientific journal (now in its eleventh volume).[2] Both these publications have a wide circulation. In addition a large number of pamphlets (sixty-five in all) on special subjects of local interest has been issued. The Department has an Imperial value as a training ground for scientific and technical tropical agriculturists. There is no other organisation in any part of the Tropics where such diversified work is carried on over so large an area and under such varying conditions of soil and climate.

"It cannot be doubted that the Department has been of enormous practical utility to the West Indian Colonies and has had a large share in the gratifying improvement in the condition of the Colonies which has recently been apparent. It may well be that the finances of the several Colonies will be able very shortly to bear unaided the cost of the local agricultural establishments. But we consider it of the highest importance that the Imperial Government should continue for some years to come to maintain the central office of the Department. The central office is necessary for the co-ordination and general direction of the work of the local departments. No single colony could afford to maintain a staff of officers of sufficient standing and reputation to continue its work. . . . We consider it essential that the central office should be continued, and we think that economy would result from an immediate decision to continue it for a definite term."

Lord Islington, before his departure for New Zealand, contributed a Memorandum which is attached to the Report. In

[1] *Agricultural News.* [2] *West India Bulletin.*

this he refers to the work of the Imperial Department of Agriculture in almost identical terms:

"I was deeply impressed during our visit to the West Indies by the value of the work done in the past by this Department, and by the greatness of the possibilities which still lay before it. The revival of the cotton industry and consequent restoration of comparative prosperity to some of the smaller islands; experiments with the sugar-cane; the discovery and destruction of insect pests,—these were in themselves great achievements. In my opinion, however, an even more valuable work has been done in diminishing the prejudices of agriculturists, and inducing them to try new methods, and in inculcating the value of science and co-operation. . . . Until after the Report of the Royal Commission of 1897 I can find no trace of a settled policy, calculated to place the West Indies on a healthy footing, beyond the need of doles, which, while of no permanent value, have in the aggregate imposed a heavy burden on the Imperial Government. The most conspicuous and successful fruit of that Report has been the work of the Imperial Department of Agriculture, which has beyond reasonable doubt saved the Home Government from appeals which could not wholly be rejected, and would have cost more than the total outlay on the Department. While urging the prosecution of that work with, if possible, even increased energy, I beg to observe that in my opinion the larger Colonies, which have profited and would continue to profit by the advice and example of the Imperial Department, should provide for their own expenditure."

In view of the strong recommendations made, as above, it is announced that the Lords Commissioners of the Treasury have agreed in principle to the continued maintenance of the Central Office of the Imperial Department of Agriculture in the West Indies for a further period of ten years from April 1, 1911.

# THE SUDDEN ORIGIN OF NEW TYPES

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Formerly the opinion was generally held by comparative anatomists that simplicity of structure necessarily implies a persistence of the primitive organisation of the race; this view is still not without some supporters at the present day. The theory, however, has not only proved unfruitful but has acted as a positive bar to the correct estimation of affinities and pedigrees in the organic world. It is by no means the least of the late Anton Dohrn's services to biology that he was among the first to make a stand against the view prevalent at the time, to the effect that organisms of relatively simple construction, such as Amphioxus or Ascidians, are necessarily archaic survivals or in any way represent the actual links in the ancestral chain of the higher or more complicated forms of the same group. This older view was all the more alluring to the laity because it was so easy to comprehend, but every new discovery in the field of palæontology tends only to discredit it still further and to render the position of its supporters more and more untenable.

The comparative study of languages, which has furnished so many interesting parallels to the processes of organic evolution, can also supply us with a close analogy to the case under consideration. The English language which has spread so successfully all over the world is far simpler and less inflected than its ancestral Saxon of King Alfred's day; whilst Sanskrit, the oldest known type of the Indo-European group of languages, possesses the greatest number of inflections. In Chinese, no less than in English, inflections have fallen extensively into disuse owing to the gradual adoption of a fixed word-order and a multiplicity of particles, but it is obvious that the resulting simplicity by no means implies that these modern languages are archaic survivals.

The main distinction between primitiveness and simplicity of structure in an organism would appear to lie in its capacity for variation. A primitive type, that is to say an ancestral type, must *a priori* have been pre-eminently plastic and variable, otherwise it could not have given rise in the course of evolution to a divergent series of descendants. On the other hand, a modern existing animal or plant showing a marked simplicity of structure has evidently reached this condition by an extreme degree of adaptation to its environment, and it will be found on examination to be due to a process not so much of degeneration as of specialisation by reduction from a more complicated condition.

Now it was noticed long ago by Isidore Geoffroy St. Hilaire[1] that when any part or organ is repeated many times in the same individual (*e.g.* the vertebræ in snakes, the arms of some star-fishes, the stamens in polyandrous flowers, etc.) its number is variable; whereas this remains constant in cases when the same part or organ occurs in few numbers. In other words, multiple parts are extremely liable to vary in structure. The high variability, which is necessarily implied by the capacity of a primitive type to give rise to a series distinctly higher in the scale than itself, indicates that the first step requisite to start the beginning of this new series would seem to be the multiplication or repetition of some special part of its organisation. This increase in the number of parts is correlated with an increased variability of the part or organ, and under the sifting influence of natural selection upon the numerous variations a greater efficiency and elaboration of the organ would be rapidly secured, leading to a correspondingly rapid rise and predominance of the new type.

Strong support of this view has recently been furnished by the investigations of Wieland[2] on American *Bennettiteæ*; his results have been still further amplified and pushed to their logical conclusions by Scott[3] and Arber.[4] In face of these

[1] *Hist. Gen. et Part. des Anomalies de l'organisation*, t. i. 1832.

[2] Wieland, G. R., A Study of some American Fossil Cycads, *Amer. Journ. Sci.* Ser. 4, vii. (1899), pp. 219, 305, 383, xi. (1901) p. 423; and *American Fossil Cycads*, 1906.

[3] Scott, D. H., The Flowering Plants of the Mesozoic Age, etc., *Journ. Roy. Microsc. Soc.* April 1907, p. 129; *Studies in Fossil Botany*, vol. ii. 1909; Adaptation in Fossil Plants, *Pres. Addr. Linn. Soc.* 1909.

[4] Arber, E. A. N., and Parkin, J., On the Origin of Angiosperms, *Journ. Linn. Soc.* xxxviii. p. 29, 1907.

remarkable discoveries it is no longer possible for systematists to lay stress on the simplicity of the trimerous flowers of Monocotyledons or the reduction shown by the flowers of the *Amentiferæ* or of other so-called *Apetalæ* as indicating primitive relics of the ancestral forms of flowering plants. Palæontology itself seemed to lend some countenance to the earlier view, which held the field for so long, for the earliest known remains of Angiosperms, occurring in the Neocomian of Portugal and the United States, were identified for the most part as members of the catkin-bearers, such as Myrica, etc.

Wieland's researches on the *Bennettiteæ* have clearly shown that the members of this group possessed strobilate flowers, which must have been closely allied in structure to those of the primitive Angiosperms, for they evidently stood very near to the direct line of descent. It is sufficient for the present purpose to adopt his generalised diagram of the flower which he considers to be typical of the *Bennettiteæ* or Pro-Angiosperms (as Saporta[1] with almost prophetic instinct named them at an earlier date on very insufficient material). Here, *e.g.* in *Cycadeoidea dacotensis*, we find a conical axis or receptacle on which the ovules (separated from each other by bracts) are ranged in dense order. These are surrounded by a whorl of 18–20 stamen-bearing fronds, which in turn are enveloped by a hundred or more, hairy, sterile bracts arranged in spiral phyllotaxis. The male fronds or microsporophylls each bear twenty pairs of alternate pinnæ, on which are ranged ten rows of sori or synangia (closely resembling the synangia of the eusporangiate fern Marattia), showing affinities with the fern-like Pteridosperms to a greater degree than the reduced and more specialised megasporophylls or ovules. It is, however, intelligible and only natural that the organs which are the most essential to the continuance of the race, *i.e.* the female elements, should have advanced more rapidly than the male organs; and, as a matter of fact, it is well known that the Pteridosperms had attained to the production of seeds at least as far back as the Carboniferous period. The great stride in advance that the *Bennettiteæ* had made upon the Pteridosperms was effected by the evolution of a strobilus, in which the male and female organs were grouped together and protected by spirally arranged foliar structures (a primitive perianth according to

[1] Saporta, G. de, *Plantes Jurassiques*, vol. iv. Paris, 1891.

Arber[1]) foreshadowing the hermaphrodite flowers of Angiosperms. It will be evident from the principle already enunciated above that the very fact of a repetition of protecting bracts (or primitive perianth) as well as a repetition of the male and female elements (microsporophylls and megasporophylls) must have induced a high degree of variability in all these structures and in the resultant strobilus as a whole. In this connection it is of importance to note that the American *Bennettiteæ*, according to Wieland's researches, show a remarkable variety both of form and structure.

A most significant result of his observations is that the male and female elements are not matured at the same time. This protandry would appear, in Wieland's opinion, to indicate (as it does in the Angiosperms of the present day) the dependence of these primitive flowers on insect-visits for fertilisation. The abundance of pollen or microspores produced by the pinnate microsporophylls renders it likely, as Arber (*op. cit.*) suggests, that pollen and not honey was the original attraction of these early flowers to insects. Dr. Scott[2] has in this connection remarked on the bright colours shown by living species of Cycas and Encephalartos; and it seems therefore not unreasonable to infer that these primitive flowers also displayed colours and laid themselves out for insect-visits. This entomophily must have been a most potent factor in the action of natural selection upon a plastic and variable group. The presence of protandry in itself implies a long-established succession of insect-visits to the ancestral *Bennettiteæ*, for it is a well-known law[3] that the primary effect of intercrossing is to enhance the size of the corolla, to give a preponderance to the andrœcium, and to cause protandry by checking the growth of the gynœcium. Now since this is already the state of things in the *Bennettiteæ* (excepting only that the numerous floral leaves are long rather than broad), it would follow as a matter of course that the agency of insect-visits must have been at work upon the ancestral forms for a considerable time previously.

Perhaps it might be travelling too far into the region of

[1] *Op. cit.* p. 58.

[2] *Journ. Roy. Microsc. Soc.* April 1907, p. 139.

[3] Henslow, Rev. G., *The Origin of Floral Structures*, etc. p. 20. London, 1888.

hypotheses to suggest that the insects, in their visits to the ancestors of the *Bennettiteæ* for the purpose of eating or collecting microspores (pollen), directly exercised a stimulus upon the strobilus, causing a greater flow of nutriment to this region of the plant and inducing an active multiplication or proliferation of its parts, which, as already indicated, is the necessary preliminary of variability. At any rate the phloëm in the peduncle of the Bennettitean strobilus greatly exceeds the xylem in thickness, and this is a clear indication of the heavy requisition which the very numerous reproductive organs made upon the supply of the necessary nutriment.

The reduction from an indefinite number of floral leaves to a pentamerous perianth must have occurred at a very early period, since pentamerism is so deep-seated a characteristic of Dicotyledons,[1] and the question naturally arises why *five* should have become the optimum, not a greater or a lesser number. The problem seems to be open to a plausible solution if we turn for a parallel to the early forms of Crinoids. Here the question is quite obviously one of obtaining the greatest economy of material consonant with the maximum area to be exposed by the food-gathering arms, which have also to enclose and protect a central vaulted dome, often prolonged into a high anal column, *e.g.* in Poteriocrinus, Botryocrinus. The number *five* in this case, and still more forcibly in the Blastoids, seems to me to be determined by the geometrical fact that the maximum plane-angle of a polyhedron is that of a pentagon, for the three angles of three adjacent hexagons cannot form a solid angle; while, on the other hand, four sides based on a square, or three sides on a triangle, would be quite insufficient to ensure the complete protection or space necessary for the full development of the central dome of Crinoids. Now the same reason can be held to apply to the flower of the early Angiosperms, for the greater part of the flower of the *Bennettiteæ*, which stand so close to the parent-stock, is occupied (as already described) by a central column composed of innumerable megaspores or ovules, surrounded by a whorl of fern-like stamens, and by a more external envelope of spiral perianth-leaves. Since in both the flower and the Crinoid the same amount of

[1] The fact of the *Bennettiteæ* possessing two seed-leaves is one of the strongest of the reasons for considering Dicotyledons to be the parent-stock of the Monocotyledons.

reduction to the optimum number of *five* has taken place in the floral leaves and arms respectively, it seems only consistent to infer that in both cases the same cause has been operative, viz. the use of a minimum amount of material consistent with the maximum amount of enclosed space.

It has usually been held that floral pentamerism is a result of the $\frac{2}{5}$ phyllotaxis, which is so frequent in Dicotyledons; but since it has now been proved that whorled flowers (at any rate flowers with staminate whorls) occur in the unbranched or only slightly branched *Bennettiteæ*, and since free-branching was a habit coincident with the rise of Angiosperms, it appears to me at least conceivable, if not altogether demonstrable, that the $\frac{2}{5}$ phyllotaxis and the consequent free-branching may be the *result*, not the cause of the floral pentamerism. The possibility of this view is supported by Rendle's[1] observations on a specimen of heather (*Erica cinerea*), in which the flowers were replaced by dark-red leaf-buds of about the same size as the flowers; in these the leaf-arrangement resembled that of the flower, *not* that of the foliage-leaves.

Dr. Scott[2] has cogently argued that evolution by reduction in the number of parts seems to have taken place with comparative rapidity and suddenness, so that it often "set in at a relatively early stage of evolution." Thus such groups as the *Amentiferæ*, with simple reduced flowers, may indeed be very ancient, though not primitive in the exact sense of the word.[3] Whilst in some cases reduction may occur mainly as the result of the natural tendency of organisms to economise material, it seems to me not impossible that in the case of the early *Amentiferæ* it took place partly as a result of a migration to the Temperate zone of their ancestral types from the Tropics, where variability is always greatest owing to the increased rate of growth induced by heat[4]; and partly as a

[1] *Proc. Linn. Soc.* Nov. 4, 1909.

[2] Adaptation in fossil plants. Presid. Address, *Linn. Soc.* 1909.

[3] A striking instance of a remarkably rapid and early reduction is furnished by the Chelonians, for the earliest known example of this class, viz. from the Upper Keuper sandstone of Würtemberg, is a complete shell (2 feet long) of Proganochelys belonging to the peculiarly specialised group of the Pleurodira. Yet this Triassic form cannot, geologically speaking, be very far removed from the ancestral Chelonian, considering that no reptiles are known in pre-Permian rocks.

[4] Blaringhem, L., *Mutation et Traumatisme*, etc. Paris, 1908.

result of their becoming adapted to wind-pollination, owing to the presumable absence of the insects necessary for cross-fertilisation.

Within comparatively recent times the catkins of the willows have reverted to pollination by insects; in order to attain this end the flowers now secrete honey and have become conspicuous by the increased yellow colour of the stamens. It seems not improbable that the extraordinary variability of *Salix* may be in some way correlated with this abandonment of wind-pollination in favour of visits by insects, whilst the poplars, which stand nearest to the ancestral type of the two genera Salix and Populus, retain the habit of anemophily and display none of the great variability of the willows.

With regard to the question of a reduction in parts being influenced by the necessity for an economy in materials, it is interesting to note that it is a distinct economy for the plant to have acquired the essentially Angiospermic character of the protective envelopment by the carpel of the ovule; for, as Arber[1] points out, the chance of pollination is thereby increased, since the insect has only to leave the pollen on one part of the carpel. The same author[2] indeed considers that entomophily by means of closed carpels will be found to be the real influence which called the Angiosperms into being.

The foregoing principle, which would seem now to be substantiated for flowering plants, viz. an increased variability and plasticity resulting from a repetition of similar parts, and a subsequent specialisation by rapid reduction to an optimum, can be applied equally well to the animal kingdom. As Smith-Woodward[3] has succinctly observed, "throughout the evolution of the organic world there has been a periodical succession of impulses, each introducing not only a higher grade of life, but also fixing some essential characters that had been variable in the grade immediately below," and again that "the progress of the backboned land-animals during the successive periods of geological time has not been uniform and gradual, but has proceeded in a rhythmic manner." I will

[1] *Op. cit.* p. 74. [2] *Op. cit.* p. 68.

[3] *Ann. Nat. Hist.* xviii. 1906, p. 312; see also his Presid. Address, Geology, Brit. Assoc. 1909.

now endeavour to explain the nature of these "periodical successions of impulses," or "expression-points" of E. D. Cope, by the application of the principle of this paper to some of the main classes of animals.

In the case of mammals it is obvious that their sudden rise and predominance in the Tertiary epoch, supplanting the reptiles in every branch of life, must have been due to the rapid development of some part or parts of their organisation, which gave them a distinct and overwhelming advantage over their reptilian competitors in the struggle for existence. It is true that the brains of the great Dinosaurians, so far as our evidence goes, are remarkably small in comparison to the size of the animals, and show a low and simple state of organisation; but the larger and more complex brain does not appear to be the primary factor, although doubtless it was an important element in assuring the predominance of mammals. The primary and determining point of difference between reptiles and mammals obviously lay in the adoption by the latter of mammary glands for nourishing their young; all their other advantages can be derived from this essential feature. The suckling of the young with a perfect food like milk entailed a longer association with the mother, whilst permitting a more gradual attainment of maturity. These factors, together with the greater maternal sacrifice entailed by lactation, would in themselves be sufficient to promote and foster a greater development of brain in mammals than in the case of reptiles, in which the young individual has no other start in life than the yolk within the egg prior to hatching. The acquisition of warm blood and of a hairy covering are probably to be regarded merely as secondary consequences of a general advance in organisation resulting from a novel mode of nutrition of the offspring and the concomitant advantages of a lengthened period of development.

Now the question naturally arises: were the mammary glands absolutely new structures, or were they modified from pre-existing glands? The latter hypothesis is *a priori* more probable and more consonant with the usual laws of development of organs. The mammæ form a repetition of similar structures, ranged in series along two convergent lines, extending from the axillæ towards the pubic bones. Even in man supernumerary nipples have been shown to occur along these mammary lines in 90 per cent. of the observed cases.[1] It is a curious circumstance that the abnormal presence of these structures is more frequent in men than in women; this fact alone would lead one to surmise that mammary glands were originally derived from structures which were at least as equally developed in the male as in the female. It is also noteworthy that the percentage of supernumerary mammæ is higher than would naturally be sûspected, for Bardeleben[2] found that out of nearly 3,000 recruits as many as 23·3 per cent. possessed additional teats. In many species of mammals the number of the mammæ is indeed very inconstant within the limits of the species.

The mammary glands are usually regarded as modified sebaceous glands, but this opinion does not go far towards solving the question of their origin from a phylogenetic point of view. At this juncture embryology comes to our aid and throws unsuspected light upon the problem. Thus O. Schultze[3] discovered that in the young embryos of several mammals (pig, rabbit, etc.) a dorso-lateral ridge is formed on each side, and that the mammæ and nipples are eventually developed at points upon this ridge. By subsequent changes of growth these two dorso-lateral mammary lines are finally brought down into the ventro-lateral position that is permanent in the adult. Now the dorso-lateral position of these lines in the embryo exactly corresponds to the position of one of the lateral lines in Amphibia;[4] it seems to me therefore within the range of probability that by the very common occurrence of a change in function the lateral line has become transformed into the mammary line in the course of evolution from amphibians to mammals, and has subsequently become differentiated into separate mammæ. The glands in both cases are similarly situated and in both cases arise in the Malpighian stratum of the derma.

[1] Leichtenstern, Ueberzählige Brüste, *Virch. Arch. f. path. Anat. & Phys.* 1878, lxxiii. 222.

[2] Quoted by Bateson, *Materials for the Study of Variation*, p. 183. London, 1894.

[3] *Verh. d. phys. med. Ges. zu Würzburg*, xxv. 1893, p. 171.

[4] Additional force is lent to my theory by the fact that there are several lateral lines in Proteus and all Amphibian larvæ (fig. 1), not only a dorso-lateral but also an axillary-inguinal, exactly corresponding in position to the mammary line.

Even at the present day the small, heterogeneous remnant of the Urodele Amphibians shows considerable diversity and variety with regard to the glands of the lateral line. In some cases these glands secrete a milky juice, although this appears to be of a poisonous nature. It is therefore conceivable that in some of their remote ancestors corresponding glands may have taken to secreting a milky fluid capable of nourishing their young. As soon as this change of function had become established, it would obviously be more convenient and advantageous for the nutritive organs firstly to travel from a dorso-lateral to a ventro-lateral position, and secondly to become reduced and concentrated in a few centres along these lines.

It will now be evident that the same principle which has been discussed in the case of the ancestry of flowering plants can be held to apply to the origin of mammals. Thus we find in the first place a lateral line (or more than one) composed of a repetition-series of similar glands—a condition which is in itself conducive to a high degree of variability of the members of the series. Under special conditions one or more of the variations might have progressed in the direction of the glands producing a nutritive secretion; when once this initial step had been taken all the subsidiary characters of the Mammalia could easily have followed as a natural consequence. Among the chief of these was the increase of intellect, which is well known to vary directly as the maternal sacrifice. As a necessary corollary to the absence of the lateral line in all reptiles, it is evident that—contrary to the received and prevalent opinion—the mammals must have taken their origin directly from Amphibia, not from Anomodont reptiles, which were already highly specialised creatures and should be more naturally regarded as a closely parallel side-branch from a common ancestral amphibian stock. The earliest mammals must have been smaller and more generalised in structure than the Anomodonts. Indeed most attempts at representing the true phylogenetic relationships of animals suffer from the stem-form not being placed far enough back in the line of ancestry. On the other hand, the lateral line was well developed in the adult Stegocephalan amphibians, with the exception of the Aïstopoda.

As an instance of the manner in which reduction in one direction may have an influence upon the growth and differentiation of quite another part of the structure of an organism, attention may be drawn to the dependence of the increase of brain capacity in mammals upon the adoption of a hairy covering consequent on the reduction and final abandonment of scales. To this may be attributed the formation of the neopallium, which G. Elliot Smith[1] explains by the fact that "in the immediate ancestors of mammals the number and variety of sensory paths which found admission into the cerebrum became enormously increased, and led to a further specialisation of the pallial formation, resulting in the birth of the *neopallium*—a cortical area where all the sensory impulses brought to the cerebral hemispheres along these new channels might be received, be blended in consciousness with those coming from other sense-organs, and leave impressions which might be stored, as it were, in this neopallium, and so influence other sensations and states of consciousness at some subsequent time. The neopallium is thus the organ of associative memory." And he also states that "the first appearance of a definite neopallium coincides with the transformation of the skin over the whole surface of the body into a highly specialised tactile organ." This conclusion may even be carried still further, as Friedenthal[2] has pointed out, to explain the great development of the brain which is obvious even in the earliest human skulls. Thus, by man becoming naked, owing to the hair being reduced to a minimum, the skin became more than ever before the seat of the reception of tactile impressions, and thereby the extent of the neopallium became increased, and the mentality of the human race correspondingly enlarged.

The theory of the evolution of limbs in the Vertebrata from continuous fin-folds (extending along the entire length of the body), which is now so generally accepted, furnishes another instance of the general principle of this paper. The paired fins as well as the unpaired fins appear first of all as longitudinal folds of the body-wall, converging towards the anus; in the Elasmobranch embryo each somite gives

[1] Some Problems relating to the Evolution of the Brain. Arris and Gale Lectures, Roy. Coll. of Surgeons, 1909.

[2] *Beitr. zur Naturgeschichte des Menschen.* Jena, 1908.

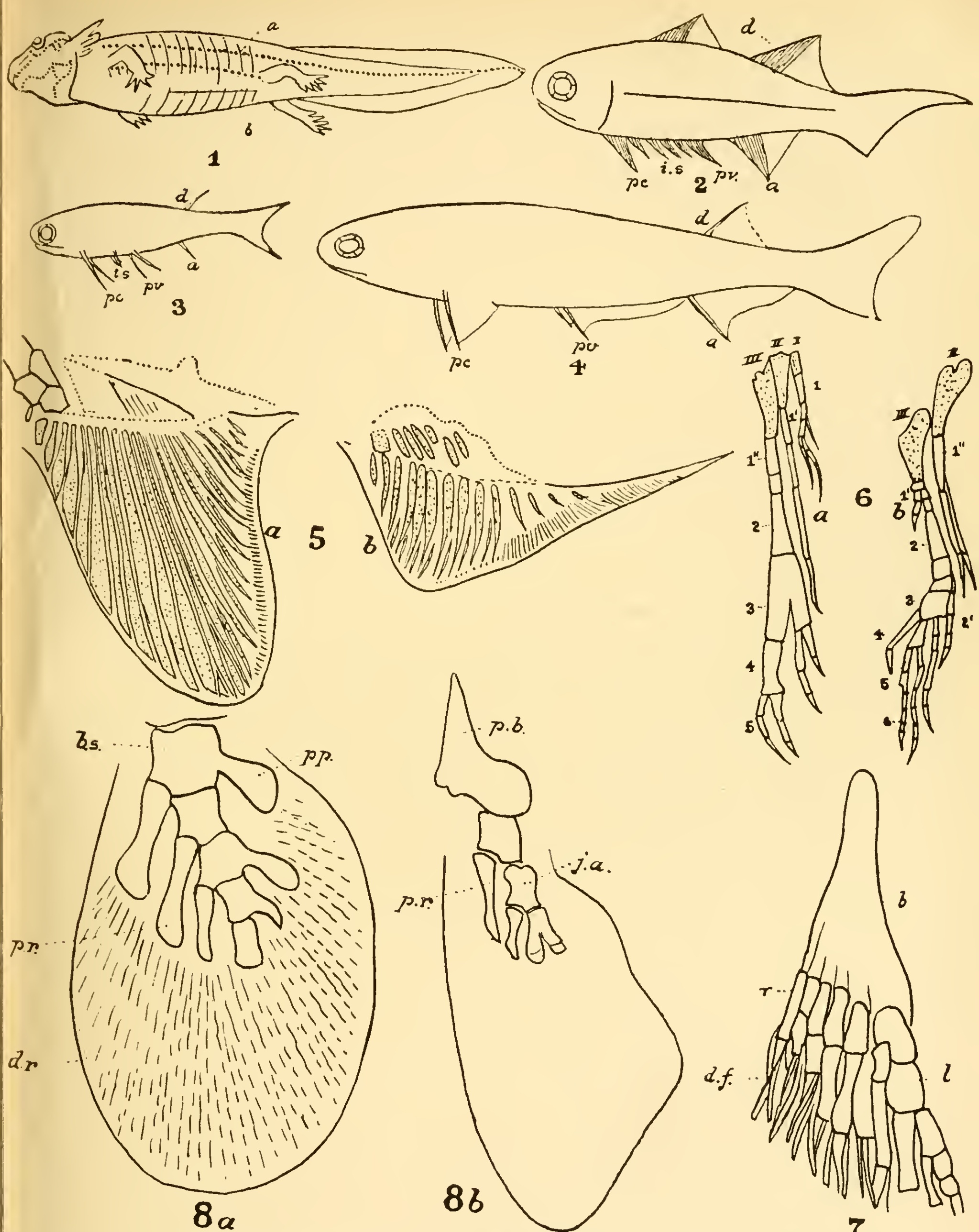


Fig. 1.—Salamander larva. *a*, *b*, lateral lines. (After Wiedersheim )

Fig. 2.—*Climatius scutiger*, Egerton. *pc*, pectoral ; *pv*, pelvic ; *a*, anal ; *d*, dorsal fins ; *i.s.*, intermediate spines.

Fig. 3.—*Mesacanthus mitchelli*, Egerton. Lettering as in Fig. 2.

Fig. 4.—*Acanthodes sulcatus*, Ag. Lettering as in Fig. 2. (Figs. 2, 3, 4 after Smith-Woodward.)

Fig. 5.—*Cladoselache*. *a*, pectoral ; *b*, pelvic fin. (After Bashford Dean.)

Fig. 6.—*Pleuracanthus* (*Henacanthus*) *Decheni*, Gf. *a*, *b*, skeleton of anal fins ; I, II, III, hæmal arches ; 1-6, segments of fin-radials. Right border pre-axial, left border post-axial. (After Fritsch.)

Fig. 7.—*Glyptolepis* (*Holoptychius*) *leptopterus*, Ag. Endoskeleton of the second dorsal fin. *b*, basal ; *r*, radial ; *l*, segment of a longitudinal axis ; *d.f.*, traces of dermal fin-rays. (After Smith-Woodward.)

Fig. 8.—*Eusthenopteron Foordi*, Wht. *a*, left pectoral fin ; *b*, pelvic fin ; *b.s.*, basal segment of axis ; *p.p.*, post-axial process (radial?) ; *p.r.*, pre-axial radial ; *d.r.*, dermal rays ; *p.b.*, pelvic bone ; *j.a.*, jointed axis. (After Goodrich.)

rise to a fin-element, consisting of two rods of cartilage (somactidia), with two dorsal and ventral bundles of muscles, each supplied by a spinal nerve. These fin-elements disappear except in the pectoral and pelvic regions, where a considerable number of these outgrowths are concentrated, and unite to form the pectoral and pelvic fins respectively. Now here we find an instance of a great number of similar structures (somactidia or segmental radials) in the fin-fold; as a result of this multiple repetition there naturally ensues a high degree of variability, and the first step in the successful variations consisted in the breaking up and reduction of the continuity of the fin-folds into a series of paired fins, forming a double series along the entire length of the abdomen. Some of these earlier fins, lying between the pectoral and pelvic fins, became reduced to mere spines, which Smith-Woodward[1] regards as the stiffened front-edges of such fins, *e.g.* in the Acanthodian *Climatius scutiger*, Egerton, and *Mesacanthus Mitchelli*, Egerton, of the Lower Old Red Sandstone (figs. 2, 3).

The next stage, following the differentiation of the continuous fin-folds into separate fins, was that in which the fin-flaps attained greater rigidity by the concrescence of the bases of the somactidia at four nodal points. Now the ceratotrichia are supported by tri-segmented radial cartilages (pterygiophores). This primary number *three* is of considerable significance, for the pentadactylate limb is essentially composed of three segments, the foot being regarded as the distal segment, which has become still further segmented secondarily. In Cladoselache—the most primitive form with regard to the appendicular limbs—this tri-segmented nature of the fin is clearly visible, but even here some reduction has evidently taken place in comparison with the simpler arrangement still visible in the unpaired fins of living Elasmobranchs. In the pectoral fin of Cladoselache[2] (fig. 5), the elements of the basal segment have become fewer and stouter, by fusion due to growth-pressure, so as to form a basal support (basipterygium), and the distal elements have become more slender and reduced in number. But no trace of pelvic girdle was developed in Cladoselache, and the pectoral girdle did not

[1] Presid. Address, *Proc. Geol. Assoc.* xix. 1906, p. 266.

[2] B. Dean, *Journ. Morph.* ix. 1894, p. 87; *Trans. New York Acad. Sci.* xiii. 1894, p. 115; and *Anat. Anz.* xi. 1896, p. 673.

proceed beyond the basipterygial stage.[1] The limb-girdles, which show great variations in form, are clearly adaptive structures, and were apparently segmented off from the proximal radials or basipterygium; and although limb-girdles are present in the more specialised, but still very primitive Pleuracanthus, the two halves of the limb-girdle remain distinct. In proportion to the exigencies of an increased demand for support and leverage for the fins, the limb-girdles increased in size by dorsal and ventral out-growths, and subsequent union in the median plane. It is, however, only in Elasmobranchs and Dipnoi, not in Crossopterygians, that actual union and fusion of the two halves takes place. Not until this stage had been reached, giving a suitable amount of support, could fins or paddles become adapted, by further specialisation and reduction, to the optimum necessary for crawling on the sea-shore.

It may be remarked parenthetically that in *Pleuracanthus* the anal fins, fig. 6 (which may have some use for crawling on the sea-bottom), show, if anything, a more superficial resemblance[2]—by a variable fusion of radials—to the pentadactylate limb than the pectoral and pelvic limbs do; and since this is the case in a fish which is so generalised a type that it might "with very little modification become either a Selachian Dipnoan or Crossopterygian,"[3] it furnishes us with a possible clue as to the *analogous* way in which the pentadactylate limbs could become differentiated from the paired fins. A marked reduction is also to be seen in the dorsal fin of the Crossopterygian *Glyptolepis* (almost handlike), fig. 7, in which the proximal radials are united at their base into a single piece—the basipterygium—but are free distally.[4]

In Elasmobranchs and Crossopterygians[5] the basal elements

[1] R. C. Osburn, "The Origin of Vertebrate Limbs," *Ann. New York Acad. Sci.* 1906-7, xvii. No. 2, Pt. ii. p. 419, has shown that in Cestracion the rays and basals begin to appear before the girdle, and this is supported by the palæontological evidence of Cladoselache. But all three structures are differentiated out of the same band or layer of mesenchyme.

[2] A. Fritsch, *Fauna der Gaskohle*, 1883–1901.

[3] A. Smith-Woodward, *Vertebrate Palæontology*, p. 32, 1898.

[4] A. Smith-Woodward, *British Museum Catalogue of Fossil Fishes*, ii. p. 335, 1891.

[5] In the Crossopterygian *Eusthenopteron Foordi*, Wht. Fig. 8, of the Upper Devonian of Canada (Whiteaves, *Trans. Roy. Soc. Canada*, vi. 1888, p. 77; and

of the paired fins remain three in number, pre-, meso- and metapterygium) but a rapid reduction must have been correlated with the adaptation to progression on land (doubtless due to the mechanical necessities of raising the body above the ground) so as to reach the typical reduction characteristic of the Tetrapoda, viz. to the single basal element, the double median element and the pentadactylate distal portion, which is to be seen, already in Carboniferous times, in such diagrammatic simplicity in the Stegocephalan limb, *e.g.* Seeleya. At any rate pentadactylism is a special instance of a very rapid reduction to a minimum optimum. The horny fin-rays would conceivably coalesce at the same time to form epidermal claws protecting the tips of the distal radial elements of the limb. Thus there was a steady system of progressive reduction until the Amphibian type was reached.

Just as a rapid reduction followed the adaptation of fins for progression on land, so conversely when higher Vertebrates have returned to an aquatic mode of life, the opposite tendency is exhibited of extra or supernumerary digits being formed adaptatively on the post-axial side of the limb. In the White Whale or Beluga (Delphinapterus) this increase has been secured by a longitudinal division of the fifth digit, and a splitting of the digits took place in Ichthyosaurus. Hyperphalangism, or the formation of additional phalanges, is clearly shown in aquatic reptiles (Plesiosaurus, Ichthyosaurus) and also in mammals which have become adapted to progression in water (Globicephalus and other Cetaceans). In the Manatee (*Manatus americanus*) Baur[1] has shown that an extra (fourth) phalanx develops adaptively on the third digit during the growth of the individual under mechanical influence, whilst the embryos possess only the usual three digits. The same tendency is exhibited by Chelonia, *e.g.* in the *Trionychoidea*, in which the fourth digit in each limb never has less than four phalanges; in addition the members of this group are losing their claws, which are unnecessary for fin-like structures, only the three inner digits being provided with them. In Carettochelys of New Guinea only two of

Goodrich, *Quart. Journ. Micr. Sci.* xlv. 1901) the pectoral and pelvic limbs clearly show a reduction of the basal radials to a single element, supporting two elements (one of which is regarded as a pre-axial radial), but at any rate this arrangement gives us a strong hint of the Tetrapodan type.

[1] *Biolog. Centralblatt*, viii. 1887, p. 493.

the elongated digits possess claws; in the adult Loggerhead (*Thalassochelys caretta*) only the claw of the first digit remains.

It is noteworthy that the paddle-finned fishes with Amphibian affinities, both the Crossopterygii and the Dipnoi, reached a very great development in the Middle and Upper Devonian, probably as a result of the severe desert-conditions of the Old Red Sandstone, when the fishes of the evaporating seas and lagoons were forced either to become adapted to an amphibian existence or to perish miserably from suffocation and starvation. The exigency of crawling on the mud by means of paddle-fins would soon result in the evolution of an elbow-joint, just as it has done at the present day in the case of the Hopping Gobies (Periophthalmus and Boleophthalmus) of the tropical mangrove swamps or the somewhat similar blenny Alticus, which is also adapted for progression on land.

One of the clearest instances of the principle set forth in this paper is exemplified by the sudden rise of the Hydrozoan class of Graptolites.[1] The oldest form is the branched, tree-like Dictyonema, occurring in fanlike or funnel-shaped colonies. It appeared first of all in the Cambrian of the Appalachian Mountains, and—as in many other characteristically Silurian groups—a general eastward migration gradually took place, so that Dictyonema is found at a somewhat later date in Northern Europe; in the Baltic Region it is found immediately overlying the Obolus-conglomerate of the uppermost Cambrian.

Other early forms of the *Dendrograptidi* (*Dendroidea*) or dendroid Graptolites are Dendrograptus (*e.g.* the dichotomous *D. Hallianus* of the Potsdam Sandstone), Callograptus and Ptilograptus; in all of these branched fixed forms, with uniserial hydrothecæ, no virgula occurs. Dictyonema, however, which lasted from the Upper Cambrian to the Middle Devonian, showed remarkable and unusual persistence for this class of organisms, perhaps owing to the fact that it may not have grown on the sea-bottom like its nearest relatives but was

[1] It is necessary, however, not to lose sight of the circumstance that the relation of the Graptolites to the Calyptoblastoid Hydrozoa is no longer universally accepted. The appearance of Diplograptus with its central plate (pneumatophore) partly covering a series of vesicles (gonothecæ) from the base of which the hydrorhabds radiate outwards has led to the suggestion that the Graptolite colony was a Medusa-like organism, and that the hydrorhabds were its tentacles.

probably a freely floating colony, since it terminates basally in a sicula attached to a long thread[1] (nema). The repetition-series of the numerous branches and the closely repeated hydrothecæ of the *Dendrograptidi* constituted the preliminary developments, which by themselves were sufficient to induce a great amount of variability. It is therefore not surprising that the true Graptolites arose so suddenly and developed so large a variety of forms, particularly since their ascendance into a numerous and successful class was associated with the change from a sessile, littoral habit (though perhaps attached to seaweeds) to an exclusively pelagic, freely floating or even swimming mode of life. This change was correlated with special adaptations such as air-bladders (pneumatophores) and possibly even swimming-membranes (Dichograptus).

The earlier of the true Graptolites such as *Bryograptus* of the Upper Cambrian (Tremadoc) among the *Dichograptidi* or *Axonolipa*, are repeatedly branched, and as their evolution proceeded we see the inevitable reduction and specialisation in different directions rapidly taking place. The *Dichograptidi* stand nearest to the dendroid Graptolites and resemble them in possessing no virgula and in the hydrothecæ being in contact back to back; the extent of branching has already become reduced in Tetragraptus and Loganograptus. Even in this group a still further reduction has taken place from diprionidian to monoprionidian forms; in the later and still more specialised order of the *Axograptidi* or *Axonophora* (which, however, possesses a virgula giving greater rigidity to the polypary) this reduction is still more pronounced. Sometimes it even becomes evident in a single individual colony, as in Dicranograptus, in which the polypary is at first diprionidian but soon divides distally into two monoprionidian branches.

It is in consonance with the principle discussed in this paper that the progressive simplicity exemplified by the reduced extent of branching, as well as by the reduction of the series of hydrothecæ from a double to a single row, goes hand in hand with an increasing specialisation and complexity of form of the hydrothecæ. The earlier hydrothecæ were straight and possessed straight apertures; as time pro-

[1] This is true for *D. flabelliforme*, Eichw.; in *D. cavernosum*, Wiman, however, an adhesive disc is present.

ceeded the apertures became curved and were often produced in a spine, whilst in the Silurian period they attained even greater complexity. Moreover, the specialisation of the latter forms with regard to the pelagic habit is exhibited not merely by the Upper Silurian Diplograptus, with its pneumatophores, but by Retiolites with its netted periderm. The latter structure is an adaptive device in the direction of lightness combined with strength, which is met with again, for instance, in the pelagically specialised swimming Crinoid Saccocoma of the Jurassic.

The continuous evolution of one generic type from another is particularly well shown by graptolites and is therefore of great value in its bearing upon my theory. It is clear also that the expression of a Graptolite genus differs widely and essentially from the ordinary conception of the term. Thus, since the same type of hydrotheca is found in the three successive forms *Bryograptus reflexus*, *Tetragraptus denticulatus* and *Didymograptus fasciculatus*, it is considered that they stand to each other in direct genetic succession and exemplify a progressive specialisation from the original much-branched form. A similar conclusion is reached in the case of the successive series *Bryograptus Callavei*, *Tetragraptus Hicksi* and *Didymograptus affinis*, which all agree in possessing in common another type of hydrotheca. Furthermore, since the species of the genus Monograptus, as at present constituted, exhibit a great variety of hydrothecæ, it is evident that this genus is polyphyletic and is merely a convenient conception for signifying that divergent branches in the genealogical tree of the graptolites reached a similar stage of evolution at the same period.

Among Echinoderms the general principle discussed in this paper can also be detected, although the origin of the class as a whole must have taken place in pre-Cambrian times and is consequently beyond our ken. Stalked Cystoidea belonging to the primitive bilateral Carpoidea (showing an absence of the characteristic pores of Cystids) are already known from Cambrian rocks, *e.g. Trochocystites bohemicus* Barr. (Mid-Cambrian of Bohemia); hence the sessile Aristocystis of the Ordovician of Bohemia, which is usually regarded as a very primitive form, can only dimly indicate to us the ancestral forms (the Ampho-

RIDEA) of the extremely heterogeneous and diverse assemblage of Cystids. Aristocystis has probably undergone some simplification and reduction, especially in the matter of the absence of arms and ambulacra. Yet it still shows primitive characteristics in the repetition of similar parts (which is capable of inducing exuberant variability), *e.g.* in the numerous plates without definite orientation which form the sac-like body, with an undifferentiated and indefinite porous structure of the stereom. Even the Cambrian Trochocystites among the CARPOIDEA shows some specialisation in the ring of large plates encircling the planoconvex calyx, which is otherwise composed of an indefinite number of small plates (larger, however, on one side than on the other). Here a primitive bilateralism has been retained and emphasised, and although the CARPOIDEA are stalked forms, yet radial symmetry has not resulted from the adoption of a stem for a sessile habit. Indeed, since the short, pointed stem is hollow and plated it is not improbable that it was adapted for a balancing or directive function rather than for the purpose of fixation.

As soon as a stalked habit had become general a tendency towards pentamerism must have rapidly made itself felt, for as I have already explained (p. 401) with regard to the prevalent pentamerism in flowers, it can be derived for geometrical reasons from the exigency of combining economy of material with the attainment of the greatest amount of enclosed space.

A progressive reduction and specialisation can be traced in the Cystids; among the DIPLOPORITA, Glyptosphærites shows a distinct advance in type upon Aristocystis in the pentamerism of its ambulacral grooves. Its polygonal plates, however, are still numerous and irregular and the diplopores are irregularly arranged. This is also the case in the earlier DICHOPORITA as exemplified by Echinosphærites. In this group the pores are definitely arranged in pore-rhombs, and it contains transitional forms leading up to the Blastoids and Crinoids, both of which afford clear instances of a rapid reduction from the indefinite number of plates of the early Cystids to a strict pentamerism of the calycal plates. The Blastoids have been shown by Jaekel[1] to be derived from the Dichoporite division of the Cystids by an ever-increasing regularity in the orientation of

[1] *Stammesgeschichte der Pelmatozoen*, pt. i. 1899.

the food-grooves, by their closer structural relations to the theca and in the more perfect development of basals and radials. It will be noticed that in the early ancestral forms, such as the Silurian Cystids Proteroblastus or Mesocystis, we meet once more with a pronounced multiplicity of parts, in this case consisting of an indefinite number of interradial plates and the irregularly scattered diplopores. From these forms the earlier Blastoids, such as the Ordovician Asteroblastus and Blastoidocrinus (sometimes classed separately as Parablastoidea), seem to stand in close genetic relation; among other transitional characteristics their radials are not forked and numerous small plates are intercalated between the radials on the one hand and the large deltoids and ambulacra on the other hand. The Blastoids, however, appear to have suffered from a rapid over-specialisation and excess of regularity and were consequently unable to survive the rigorous changes of conditions at the close of the Carboniferous period, which proved fatal to so many of the older types of animals.

Pentamerism had also appeared already in certain Cystids, *e.g.* Echinoencrinus among the Dichoporita, which are closely related to the ancestral Crinoids; and a distinct transitional series to the regular pentamerism of Crinoids has been traced in the *Caryocrinidæ*, from the Ordovician Hemicosmites to the Silurian Caryocrinus, which exhibits close affinities to monocyclic adunate Crinoids such as Hexacrinus of the Middle Devonian. At the same time there is a concomitant reduction of the numerous brachioles to the usual five arms of Crinoids.

Pentamerism was no less well marked in Crinoids than in Blastoids, but the plates of the calyx retained a more uniform and generalised character than the highly specialised deltoids and forked radials of Blastoids. In many cases, however, a still further reduction in number took place in Crinoids, especially in the dicyclic forms, in which the infrabasals may be reduced to four, or more usually to three, upon which the five basals are superposed. Even the basals in the dicyclic forms become also subject to a reduction to three, evidently for the purpose of giving greater strength to the calyx; but the radials of course always remain five in number, corresponding to the invariable and constant number of the arms. Infrabasals no longer occur so frequently nor are they so well developed in the Mesozoic as in the Palæozoic Crinoids; and they are obviously undergoing reduction in the Mesozoic forms.

Turning to the Echinoids, it is evident that the Palæozoic forms represent a plastic and fluctuating stage of transition in the evolution of the phylum. The inconstancy in the number of the rows of plates of the corona, and the flexibility (in most cases) and variety of form of the test, with its fragile, thin, and loosely articulating plates, which occur in the earliest Echinoids in comparison to the Mesozoic genera, are characteristics which present an analogy to the manifold variety of forms exhibited by most groups near their point of origin, just as we have already (p. 399) seen to be the case in the Mesozoic *Bennettiteæ*, just before a reduction to a working optimum had been arrived at by the early Angiosperms. The Palæozoic Echinoids displayed a tentative variability in various divergent directions, and the Cidaroids alone out of all these offshoots from the parent stock survived to perpetuate the race. From the Cidaroidea all the Mesozoic and modern Echinoids can be derived.

Although the Ordovician Bothriocidaris is at present the oldest known Echinoid, it is probable that its single row of plates in each interambulacrum and its rigid test represent an early experiment in reduction. On the other hand, in view of the multiple and inconstant number of rows in the interambulacral areas in other Palæozoic Echinoids such as Archæocidaris, Melonechinus (Melonites), Palæechinus, etc., the latter state of things should presumably be regarded as the more primitive feature. This is all the more probable, seeing that in post-Palæozoic forms the reduction to the optimum of two rows in each area has become a constant characteristic.[1]

Among the earliest forms a fairly near approach to the Cystids seems to be made by *Echinocystis pomum* of the Upper Silurian, in which the flexible test is composed of a mosaic of numerous (10 at the diameter) irregularly arranged and undifferentiated interradial plates with small spines, each ambulacral area containing four rows of pore-plates united at the upper pole without forming an apical disc; the anus is

[1] The Cretaceous *Tetracidaris* has indeed four rows of plates to each interambulacrum but they are reduced to two rows at the apical region.

situated excentrically and possesses the Cystidean characteristic of being closed by a valvular pyramid of plates. In Palæodiscus[1] (also from the Lower Ludlow Shales) a close approximation, on the other hand, is made to the Starfishes, for a series of ambulacral plates like those of the ASTEROIDEA occurs inside the plates of the corona, and the tube-feet in the oral portion of the radii apparently passed out between the outer ambulacral plates, but the chief radial water-vascular vessels seem to have run along inside the test; the radioles or movable spines are small and do not differ very greatly from those of some EDRIOASTEROIDEA and ASTEROIDEA.

In the Devonian Lepidocentrus—the earliest and most primitive of the Cidaroidea—the test is flexible and this flexibility is still retained to a less extent in the Lower Carboniferous Archæocidaris in which there are 4–7 rows of large overlapping plates in each interambulacral area and two rows of ambulacral plates. Oligoporus has four ambulacral and 4–9 interambulacral rows, whilst in the Carboniferous Melonechinus (Melonites) there may be as many as 4–11 columns of interambulacrals and 5–14 ambulacrals; in Lepidesthes there are 8–18 ambulacrals and 3–6 interambulacral rows; in Palæechinus (also Carboniferous) there are respectively 5–7 rows of interambulacrals, decreasing in number towards the poles and only two rows of ambulacrals. It is evident that (with the exception of the aberrant Bothriocidaris) the reduction to the optimum of two rows took place with greater rapidity in the ambulacral areas than in the intervening zones. The reduction in the number of plates went hand in hand with an increased rigidity and consolidation of the test as a whole, and an increase in the size of the spines.

If, as now seems likely, the Echinoids took their origin from an early offshoot of the Asteroids rather than directly from the Cystids, this would all the more point to the significance of a multiple repetition of similar parts in inducing the evolution of numerous divergent variations; Macbride[2] has suggestively remarked that "when we recollect that some of the oldest ASTEROIDEA known to us had very narrow arms and interradial areas edged by large square marginals, it does not require a

[1] W. J. Sollas, "On Silurian Echinoidea and Ophiuroidea," *Quart. Journ. Geol. Soc.* lv. 1899, p. 701.

[2] *Cambridge Nat. Hist.* i. p. 558.

very great effort to imagine how these marginals could be converted into the vertical rows of the interambulacra and the pointed narrow arms, becoming curved, could have formed the ambulacra."

In the organisation of the PELECYPODA we find a feature which has admitted of an almost inconceivable amount of variety, both of form and arrangement, viz. the manner of the hinge-attachment between the two valves of the shell. It is a matter of some significance for the theory of this paper that the most primitive of the PELECYPODA are those forms which exhibit the Taxodont type of hinge, in which the interlocking, comb-like teeth are numerous and similar in size and form. The primitive nature of the Taxodont class is clearly evidenced by the fact that the embryo shells of many of the higher forms (*Ostreidæ*, *Pteriidæ*, *Philobryidæ*, *Mytilidæ*, etc.) pass through the Taxodont stage of a more or less rectilinear or gently curved hinge-line with a considerable number of teeth; and that in still higher forms (*Condylocardia* and *Scioberetia*) this Taxodont stage, "present in the early embryo, is succeeded by the series of folds (characteristic of the young stages of the higher Pelecypods) that subsequently divide off into cardinal and lateral teeth, thus linking the Taxodont with the Heterodont and Desmodont types of hinge."[1]

Although considerable differentiation must have already occurred in pre-Cambrian times, yet the Taxodont shells (Ctenodonta=Tellinomya, Glyptarca, Redonia) are relatively numerous in the Cambrian period in comparison with the higher and more differentiated forms, *e.g.* Modiolopsis, and even in the Lower Ordovician a nearly similar disproportion is in evidence. It is therefore apparent that Pelecypods started with a Taxodont hinge; owing to this repetition-series of similar teeth a high degree of variability must have ensued, followed by a rapid reduction to an optimum, *i.e.* to the Heterodont form of a cardinal tooth and two lateral teeth. Increased specialisation has led to still further reduction or even complete suppression. The existing genus Nucula is one of those rare instances which have retained many really archaic characteristics, not only in the nature of its hinge-line, but in still possessing the primitive Aspidobranch type of gills, the primitive creeping foot and the

[1] Woodward, B. B, *Proc. Malacol. Soc.* vii. pt. 5, June 1907, p. 251.

nacreous type of shell. In the Palæozoic forms of the *Nuculidæ*, however, the ligament is mostly external, whilst in the recent forms it occurs internally, below the umbo. It is probable that *Tellinomya pectunculoides* of the Ordovician, with its equilateral, nearly circular form of shell, its curved hinge-line, equal adductor-impressions and semicircular, simple pallial line, stands very close to the ancestral form, and that the *Arcidæ*, with the straight hinge-line, formed an early specialisation.

The rise of the GASTROPODA, and still more of the highly organised CEPHALOPODA, must have been correlated with the acquisition of a jaw-ribbon or radula, which is such a typical characteristic of these classes that they are classed together as Glossophora. This structure forms an immense advance on the presumably Annelidan jaws (conodonts), which are known from the Cambrian. But, unlike the conodonts, the radula has never been definitely recognised in fossil form; hence its evolution cannot be tested by palæontological methods. However it has been conclusively shown by Martin F. Woodward [1] that the radula of Pleurotomaria represents the most archaic type amongst existing Gastropoda; and this genus is known to have persisted from the Upper Cambrian. Woodward comes to the conclusion that its radula, in which as many as three tracts on either side of the median are distinguishable, was derived from one in which all the teeth in a transverse row were similar.

An early reduction must have taken place to reach the optimum of three tracts on either side. Even in the case of Pleurotomaria the five tracts are not equally distinctly differentiated from each other, so that at a superficial glance only three tracts would be distinguished on each side of the median one. From this primitive Rhipidoglossate type all the remaining types can be derived by fusion and reduction. It will be sufficient here to indicate very briefly some of the main lines along which reduction has taken place. In the Rhipidoglossate Trochus only two lateral tracts are clearly distinguishable: x. 5. 1. 5. x. In the Docoglossate radula of Patella we find the series 3. 1. 2. 0. 2. 1. 3. in which it is assumed that the median tooth is either altogether aborted or sometimes reduced to a rudimentary plate. In the Tænio-

[1] *Quart. Journ. Micr. Sci.* N.S. xliv. p. 255.

glossate Cypræa all the teeth of each of the three tracts have been fused into one large tooth so as to give the formula 3. 1. 3. In the Rachiglossate Nassa fusion has reached the still further extreme of 1. 1. 1. Finally, the central tooth may be absent, as in the Toxoglossate Conus with the formula 1. 0. 1, or as in the Ptenoglossate Janthina and Scalaria with x. 0. x.

In spite of the high degree of specialisation in different directions which is so characteristic of Crustaceans at the present day, we find on closer examination that among the various classes there are still in existence some remarkably persistent and even generalised types. One of the most striking instances of such persistence is furnished by the discovery in 1893 [1] of the Tasmanian Mountain-shrimp (*Anaspides tasmaniæ*, G. M. Thomson), for which a new division—the SYNCARIDA—has had to be created,[2] equivalent in importance to the PHYLLOCARIDA containing the equally archaic form Nebalia. Anaspides can indeed hardly be distinguished from the Carboniferous Præanaspides discovered by Dr. Moysey [3] in the Coal Measures of the Nottingham coalfield in 1907.

Nebalia similarly represents a remarkable persistence of an extremely ancient and generalised type, with which Hymenocaris of the Cambrian, and the Ordovician Ceratiocaris [4] and Caryocaris are combined to form the separate class of the PHYLLOCARIDA or LEPTOSTRACA, intermediate between the ENTOMOSTRACA and the MALACOSTRACA. The even more generalised Apus is known from the Trias and seems to have been represented by the very similar Protocaris of the Lower Cambrian of North America. In point of fact, in the face of all these long survivals from the Palæozoic era down to the present day, it becomes really a matter for surprise that such successful classes as Trilobites and Eurypterids should have died out altogether when all the other and apparently more specialised classes of Crustaceans and Arachnids of Palæozoic times should have left descendants existing at the present day.

[1] Thomson, *Trans. Linn. Soc.* (2), vi. 1894–7, p. 285.

[2] Calman, *Trans. Roy. Soc. Edinburgh*, xxxviii. 1897, p. 787.

[3] *Geol. Mag.* 1908, p. 385.

[4] The Carboniferous *Ceratiocaris scorpioides* and *C. elongatus* have been, however, considered by some to be CUMACEA, but this view seems hardly tenable.

Several zoologists[1] have indeed within recent years treated the Trilobites as a generalised central group, from which not only Insects and Myriapods but even Pycnogonids (not yet known in a fossil state) have arisen in divergent directions.

On the assumption of the descent of Insects from a Trilobitan offshoot which had taken to a fresh-water habit, it is at any rate noteworthy that according to the theory of this paper the class of Trilobites can be held to comprise all the necessary characters which have already been seen to be preliminary to the origin of a new group: in particular the body-segments are extremely numerous and vary greatly in number. This circumstance is of course sufficient to initiate a great amount of variability. It is also a significant fact that the last marine survivors of the class occurring in the Carboniferous and Permian belong to the order *Proëtidæ*, which comprises trilobites with the largest number of thoracic segments, viz. up to 22. It is true that so large a number is not reached in the latest (Permian) forms, viz. Phillipsia (9) or in Proëtus (8-10), which ranges from the Ordovician to the Permian of the United States[2]—an unparalleled length of range for a Trilobite—but the *Proëtidæ* were evidently a somewhat generalised type of Trilobite, and it is only from a generalised stock that a new class can be evolved.[3]

It does not seem to be outside the range of probability that the severe desert-conditions of the Old Red Sandstone period, with its evaporating lagoons and shrinking rivers, may have been the means of causing in the (hypothetical) fresh-water branch of the Trilobites the evolution of greatly extended pleuræ, first of all for the purpose of offering a greater respiratory area in waters steadily becoming poorer in oxygen. For this theory it is necessary to assume that the Trilobitan ancestors of the insects and myriapods had abandoned the

[1] A. Handlirsch, *Die fossilen Insekten*, etc. Leipzig, 1906–8.

[2] G. G. Shumard, *Report on the Geology of Western Texas*. Austin, 1886.

[3] The Cambrian *Paradoxidæ*, which probably stand close to the ancestral stock, is the only other order of Trilobites in which the thoracic segments approach so large a figure, viz. 16–20. In the *Cheiruridæ*, which are nearly allied to the *Proëtidæ*, the thoracic segments vary from 9–18, and the pleuræ become partly separated from each other in *Cheirurus* and are totally separate in the phantom-like *Deiphon*; if these separated pleuræ became flattened and membranous they would almost foreshadow tracheal gills, which would (as discussed later) appear to be the forerunners of wings.

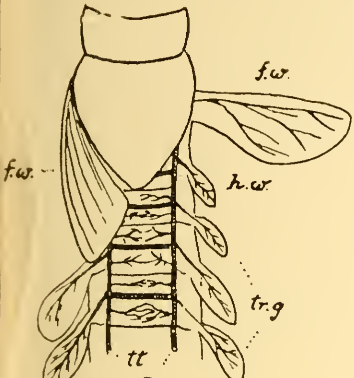
habit of secreting lime[1] in the exoskeleton (which would thus become purely chitinous) concomitantly with their migration from the sea into fresh water. The very fact of a creature with a calcareous exoskeleton taking to a fresh-water existence necessarily implies the diminution or total abandonment of the calcareous portion, owing to the solvent action of the relatively large amount of carbon-dioxide in river-water. The massive, stony carapace of most lobsters and crabs is not to be found among their fresh-water relatives such as the crayfish or the river-crab (Thelphusa). In the latter forms the carapace, though still in part calcareous, is so thin that it can be cut with a pair of scissors. The erosion of the umbos or apices of fresh-water molluscs, where the protecting epidermis has been worn away, is a familiar instance of the destructive power of carbon-dioxide on calcareous shells. According to Semper[2] the epidermis becomes destroyed in the first place in these prominent parts of the shell by boring fungi aided by the wearing action of fresh-water currents.

As a matter of fact, the young states of *Blattidæ* (*e.g.* of the Australian Oniscosoma among living insects and of some of the Carboniferous insects, *v. infra*) show that the wings are actually nothing but lateral expansions of the segments exactly as the pleuræ of Trilobites are the lateral expansions of the body-segments. It is only necessary to presume that the pleuræ of the hypothetical fresh-water Trilobite became thin and membranous and traversed by tracheæ in order to obtain an exact counterpart of a tracheal gill.

It is a matter of considerable importance that no undoubted insect-remains are known previously to the Carboniferous epoch, for the *Protocimex silurica* of the Upper Ordovician of Sweden and the *Palæoblattina Douvillei* of the Silurian of Calvados are evidently merely inorganic structures. Furthermore, the Little River group of New Brunswick, which has yielded so many remains of insects and was formerly considered to be of Upper Devonian age, has now been clearly proved to be not older than the lower part and not higher than the middle part of the Upper Carboniferous. Hence we

[1] It must be noted, however, that in the Myriapod DIPLOPODA (CHILOGNATHA) but not in other Myriapods the chitinous exoskeleton contains calcareous matter and is thus rendered much less flexible than in the other orders of MYRIAPODA.

[2] *Natural Conditions of Existence*, etc. p. 213. London, 1881.

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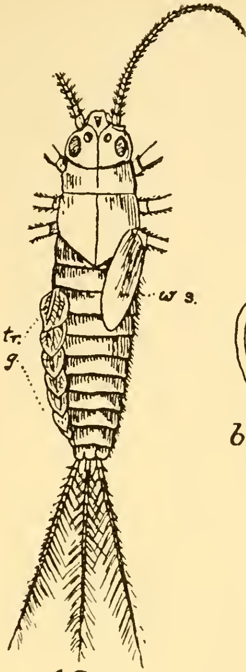


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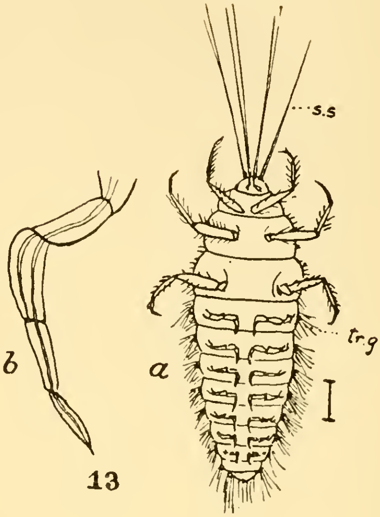


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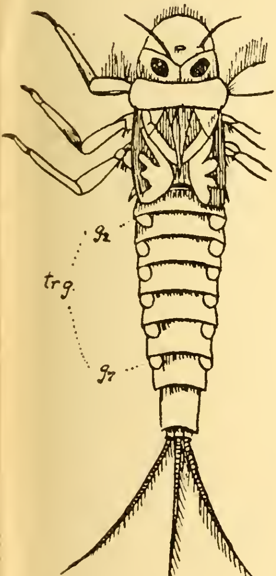


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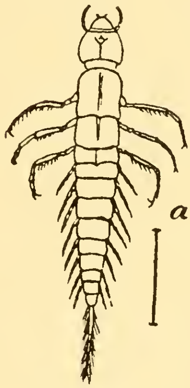


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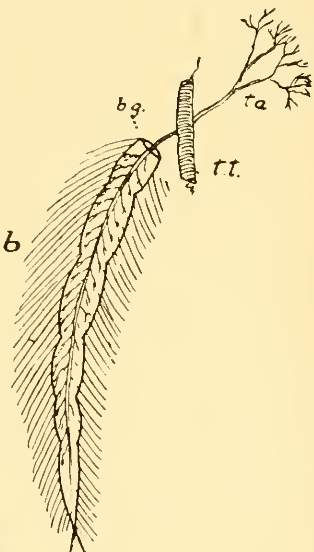


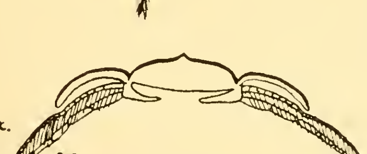


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FIG. 9.—*Chloëon dimidiatum.* Thorax and anterior segments. *f.w.*, forewing ; *h.w.*, hindwing ; *tr.g.*, tracheal gills ; *t.t.*, tracheal longitudinal trunks. (After Von Graber.)

FIG. 10.—*Chloëon dipterum.* Nymph, $\frac{7}{1}$ ; wing-sheath of left side and gills of right side removed. *w.s.*, wing-sheath ; *tr.g.*, tracheal gills. (After Vayssière.)

FIG. 11.—*Oligoneuria garumnica.* Nymph, $\frac{2}{1}$. *tr.g.*, dorsal tracheal gills. (After Vayssière.)

FIG. 12.—*Sialis lutraria.* *a*, larva ; *b*, tracheal gill ; *b.g.*, base of gill ; *t.t.*, tracheal trunk with which gill is connected ; *t.a.*, trachea given off to alimentary canal. (After Dufour.)

FIG. 13.—*Sisyra fuscata.* *a*, larva, ventral aspect ; *b*, abdominal appendage (gill) ; *s.s.*, sucking spears ; *tr.g.*, tracheal gill. (After Westwood.)

FIG. 14.—*Triarthrus becki.* Section through second thoracic segment. *ex*, exopodite ; *en*, endopodite. (After Beecher.)

find that insects exemplify a sudden outburst of a group in the Carboniferous, immediately subsequent to the unusually rigorous desert-conditions of the Old Red Sandstone, which were equally instrumental in inducing the evolution of air-breathing amphibians from paddle-finned fishes, or the air-breathing Scorpions and Anthracomarti from their purely aquatic allies. The pools of stagnant water would eventually dry up altogether and only those individuals which could reach another sheet of water would survive to perpetuate their race. During the process of stagnation [1] and evaporation it is not difficult to imagine that those forms in which the tracheal gills were more extensively developed, and sufficiently stiffened to prevent collapse, might be able to leap out of the water, and if this happened it seems obvious that the moist membranes would be able to absorb oxygen from the air. Such invigorating leaps would gradually become extended into short flights (analogous to those of flying-fish), by means of which the tracheal membranes would become stouter and stronger, and the necessary muscles would soon become developed in correlation with this new function, whilst at the same time the original respiratory function of the tracheal gills would still be actively exercised.

This picture is by no means so fanciful as it might appear to be at first sight, for it affords a plausible explanation of a possible gradual change from tracheal gills to gliding-planes and so to true wings, without implying any break in continuity of function. The transition from tracheal gills to wings can even be observed at the present day in Ephemerid larvæ, such as that of Chloëon, in which both the wings and the 6-7 pairs of tracheal gills entirely agree in their mode of origin; indeed some of the tracheal gills in this form are at one period of their development even larger than the hind wings (fig. 9), and the branching of the veins in the wing corresponds very closely to the branching of the tracheæ

[1] It is interesting to find that stagnant water seems to exercise a marked influence in increasing the extent of variability; this has recently been established for Cyclops by Dr. Esther Byrne (Fresh-water Species of Cyclops of Long Island, *Cold Spring Harbour Monographs* No. VII. 1909). In this monograph, which is based on several years' work, the author finds that "variation of a varietal type is strongly developed, but much more so in some species than in others; *it attains its maximum in the forms inhabiting stagnant waters*, which can only exist at all by the power of readily adapting themselves to environment."

within the tracheal gills. Of all modern insects the Ephemerids seem to stand nearest to the Carboniferous Palæodictyoptera, and are connected with them by a practically continuous palæontological succession of intermediate forms. Hence it is not unreasonable to presume that some of the modern forms would still possess some relics of archaic organisation to throw light upon the process of the transition from gills to wings, which must have taken place in pre-Carboniferous times. Now as a class the Ephemerids contain more forms with tracheal gills than any other class of insects. At this stage, however, it is necessary to point out that some confusion has arisen from not recognising that tracheal gills may have two distinct modes of origin, viz. (1) lamellar tracheal gills, as in Chloëon (fig. 10), Oligoneuria (fig. 11), Tricorythus (fig. 15), etc., which are clearly modified pleuræ, and it is only from these structures that wings could be derived; and (2) filamentous tracheal gills, which are modified abdominal limbs, as in Sialis (fig. 12) and even more clearly in the Hemerobiid Sisyra (fig. 13) inhabiting the cavities of the fresh-water sponge. Now if we consider a section through the Trilobite Triarthrus (fig. 14), it can be seen that there is no inherent difficulty in deriving the lamellar gills of Ephemerids from the pleuræ of a Trilobitan ancestor, whilst the fringed and jointed tracheal gills of Sialis can be derived with but little change from the setose exopodite of the Trilobite, and the more leg-like appendages of Sisyra from the endopodite. In the nymph of Tricorythus (fig. 15), a pair of filamentar gills are present at the same time with four pairs of lamellar gills, of which the anterior pair is greatly enlarged to act as a gill-cover to the three posterior pairs and is furthermore nearly equal in size to the immature wings.

It is unfortunate that the strata of the Old Red Sandstone are so ill-adapted for the preservation of delicate chitinous remains, as the Carboniferous insects already show a very considerable differentiation and reduction to an optimum, *e.g.* to three pairs of legs and to ten abdominal segments. Evidence is not wanting to show that the reduction had been of very recent occurrence, for the eleventh segment of the abdomen and the telson are not yet quite suppressed; the wings are equal in size and show similar neuration; a wing-like expansion with veins is present on the pre-thorax of several Protephemerid

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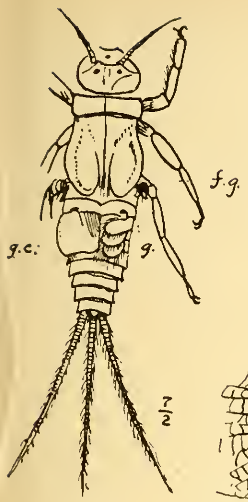
forms, *e.g.* *Lithomantis carbonarius* (fig. 16), *Stenodictya lobata*, *Homaloneurina Bonnieri*, *Homoioptera Woodwardi*, *Homœophlebia gigantea;* gill-like pleuræ occur in each abdominal segment in *Stenodictya lobata;* and *Corydaloides Scudderi* possessed tracheal gills (containing distinct tracheæ) in the imago, similar to the tracheal gills of certain Ephemerid larvæ of the present day. In the imago of the living Perlid Pteronarcys tracheal gill-tufts actually persist (fig. 17). It is also of interest to note that the larvæ of *Perlidæ* have rather large compound eyes, the ocelli being merely opaque spaces. The future wings are represented in these larvæ by lobe-like prolongations (varying in length according to age) of the meso- and meta-notum. Many of the *Perlidæ* also present the curious phenomenon of micropterism amony the males, *e.g.* Tæniopteryx, *Nemoura trifasciata*, *Perla maxima*, *Dictyopteryx microcephala*, *Isogenus nubecula.* It is not impossible that these cases may be instances of reversion in a primitive group of insects (all with feeble powers of flight), which has progressed only slightly in comparison with the majority of insects from the ancestral Carboniferous stock.

In the Protephemerid division of the Palæodictyoptera the wings could not be folded back over the abdomen, but remained horizontal when at rest and were only capable of motion in one plane, a characteristic which has been retained by the modern *Ephemera.* Some Carboniferous larval forms clearly show a gradual development of the wings, standing out horizontally at right angles to the body.

Representatives of the heterogeneous group of Myriapods [1] occur at an earlier date than insects, for the *Archidesmidæ* (Kampecaris and Archidesmus) have been found in the Old Red Sandstone of Scotland.[2] These forms, together with the numerous Carboniferous *Euphoberiidæ* (fig. 18) and *Archijulidæ* are placed in a special order, the Archipolypoda, differing from the Diplopoda in the dorsal scutes being more or less divided into two parts instead of being fused into one but agreeing with them in there being two pairs of legs to each ring,

[1] The Chilopoda, Symphyla and Diplopoda are now usually treated as independent classes.

[2] Peach, B.N., *Proc. Roy. Phys. Soc. Edin.* vii. (1882), p. 77; xiv. (1899), p. 113.

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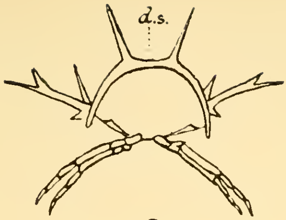


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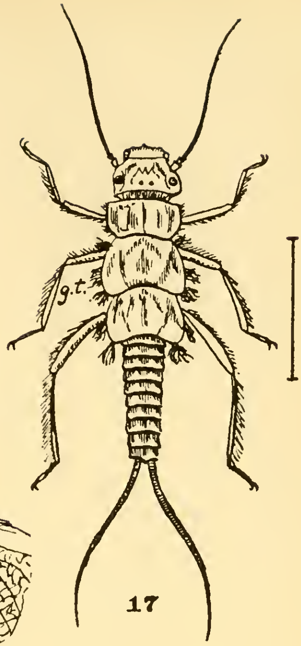


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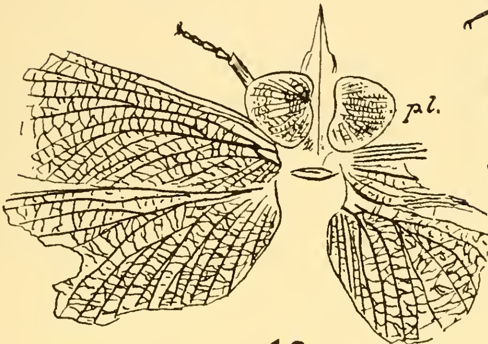


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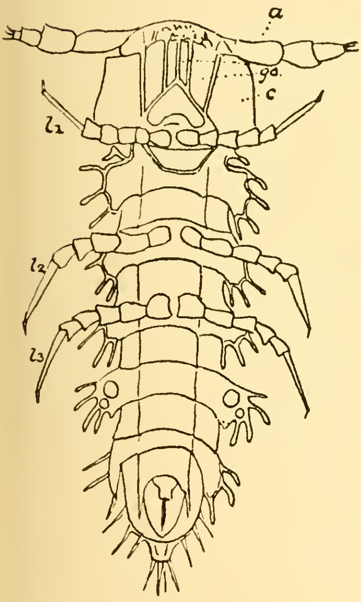


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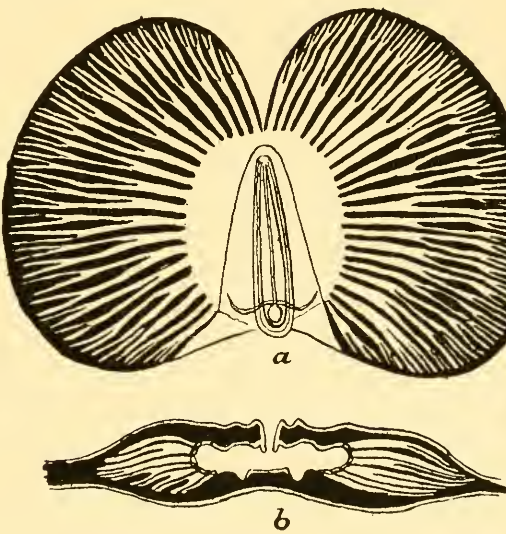


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FIG. 15.—*Tricorythus* sp. Nymph, $\frac{7}{2}$. Gill-cover (*g.c.*) of right side removed, showing gills (*g.*); *f.g.*, filamentar gill. (After Vayssière.)

FIG. 16.—*Lithomantis carbonarius*, $\frac{2}{3}$. *p.l.*, pro-thoracic lobe. (After H. Woodward.)

FIG. 17.—*Perla* sp. Nymph. *g.t.*, tracheal gill. (After Vayssière.)

FIG. 18.—*Euphoberia ferox*, Salter, $\frac{1}{1}$. Section of a segment. *d.s.*, dorsal shield with spines. (After H. Woodward.)

FIG. 19.—*Polydesmus complanatus*. Larva, just hatched. *a*, antenna; *g.c.*, gnathochilarium; *c*, cheek; $l_1$, $l_2$, $l_3$, the three pairs of legs. (After von Rath.)

FIG. 20.—*Scutigera coleoptera*. Tracheal mass of a dorsal plate. *a*, from above; *b*, transverse section. (After Haase.)

which thus corresponds to a double segment (two fused segments). Probably the Diplopoda have been derived from this order by a process of reduction but the earliest known type of this order—*Julopsis cretacea*—occurs only in the Cretaceous of Greenland.

There are several points in which the Archipolypoda can be held to approach the Trilobites, from which some zoologists consider them to be derived: the fusiform body is thickest in the anterior half or third; the cephalic appendages are borne on an apparently single segment. Stigmata occur on each segment, so that they were presumably air-breathers; but Scudder[1] considers that the lateral openings of Acantherpestes were branched in character, so that this type would help to bridge over the transition which must have occurred in the Devonian period between gills or gill-books and tracheæ.

In the Archipolypoda the pleuræ are well developed, and in the modern Polydesmus (fig. 19), to which the Devonian *Archidesmidæ* show much resemblance, we can even yet trace a distinct trilobation of the body comparable with that of Trilobites. The single pair of antennæ also is a characteristic which the Myriapods possess in common with Insects and Trilobites, in abrupt contrast to all Crustaceans (with two pairs) or to Arachnids (with only cheliceræ). In the larva of Polydesmus the lateral cheeks of the head present a striking analogy to the free cheeks of Trilobites; here too there are only three pairs of legs on the three anterior trunk-segments, presenting an interesting parallel to the permanent reduction to three pairs in insects. New segments gradually appear posteriorly and the number of legs increases, a state of things very analogous to the increase of vertebral segments in snakes or to the addition of the fifth pair of legs in some Pycnogons (Decolopoda and Pentanymphon), where it is a comparatively new development and not a primitive character; in fact the larval Pycnogon has only three pairs of appendages.

As a set-off to this increased number of similar segments, which is obviously a secondarily acquired characteristic (although it must have occurred early in the history of the group of Myriapods), the Symphyla show a marked reduction, possessing not more than twelve leg-bearing trunk-segments, with but one pair of legs to each segment and only a single pair of

[1] *Mem. Bost. Soc. Nat. Hist.* 1882 and 1884.

tracheæ respectively. The anal segment and the indication of a pre-anal segment would bring up the total number of segments to fourteen, which would closely correspond to the original thirteen or fourteen segments (thorax and abdomen) of insects. Probably the SYMPHYLA stands nearer to the Insectan stock, from which the THYSANURA branched off, than the other Myriapod classes and in several respects they show primitive characteristics. Possibly they are represented in the fossil state by Scudder's PROTOSYNGNATHA, which is constituted by the single Carboniferous form *Palæocampa anthrax*, with ten body-segments, from the Carboniferous of Illinois.

The essential characteristic of all Myriapods and Insects is their segmental tracheal system, which must have already appeared in their common ancestor; although at first sight no two structures could seem to differ more widely than tracheæ and lung-books (like those of Scorpions and Spiders), yet it seems probable that the former are in reality derived from the latter. It is among the Myriapods that we can even now dimly see how this was brought about, especially by examining the tracheal system of Scutigera[1] (fig. 20), among the CHILOPODA, although in their case there is obviously much specialisation. Here we find that each (unpaired and dorsal) stigma opens into an air-sac, from each side of which about 300 radial, branched tracheal tubes arise, closely packed together, forming a kind of lung. This arrangement is essentially similar to the tracheal lungs or book-leaf tracheæ of Arachnids, with the sole exception that in these the tracheæ are flattened out into lamellæ. Since ribbon-like tracheæ occur in Araneids, it is evident that book-leaf tracheæ could pass without any violent transition into ribbon-like tracheæ and finally into the typical tubular tracheæ, which are so eminently adapted for a purely terrestrial mode of life.

The multiple repetition of similar lamellæ would again be conducive to inducing an extensive amount of structural variations, capable of leading onwards either to the tubular, branching tracheæ of Insects or to the tufted tracheæ of PSEUDOSCORPIONIDA and many ARANEIDA. It is obvious that tracheæ must have arisen independently in Insects, Arachnids[2]

[1] It is placed in a separate order, the SCHIZOTARSIA, by F. G. Sinclair, *Camb. Nat. Hist.*, Myriapods, p. 46, 1895.

[2] In Araneids the main trunk of the trachea histologically resembles exactly the general chamber of the lung-sac and is quite different from the trachea of Insects.

and Peripatus.[1] It is a particularly suggestive circumstance that in the dipneumonic ARANEIDA tubular and book-leaf tracheæ occur simultaneously.

Since it has been shown by Brauer[2] that the lung-books of Scorpions are derived from gills borne on mesosomatic appendages,[3] there is clearly no difficulty in assuming that the gills of the hypothetical fresh-water Trilobite could similarly have been transformed into lung-books sunk in the body and communicating with the air by stigmata; and once this step was accomplished the further transformation into tracheæ could follow as indicated in the foregoing paragraph. A very primitive condition of tracheæ exists in Campodea, in which three pairs of spiracles are situated in the thoracic region, and the tracheæ of each stigma keep distinct, so that there are six separate small tracheal systems, three on each side of the body. In Machilis also the nine pairs of tracheal systems keep distinct from each other.

Other instances of the application of this theory to explain the sudden development of new groups in the organic world might be readily multiplied; but the foregoing examples may perhaps be considered sufficiently striking to obviate the undue expansion of this paper by entering into further details at the present juncture.

[1] *Peripatus* can hardly be regarded any longer as a member of the ancestral stock of Myriapods and Insects, but as a highly specialised side-branch of the Annelidan ancestor of Arthropods.

[2] *Zeitschr. Wiss. Zool.* lix. 1895, p. 351.

[3] Dr. F. W. Purcell (*Quart. Journ. Micr. Sci.* liv. pt. 1, 1909), in a memoir on the development and origin of the respiratory organs in *Araneæ* finds that in spiders also the lung-books are derived from gill-books similar to those of Limulus, for the first leaves of the lung-books appear on the free posterior side of the provisional abdominal appendages, quite outside of the pulmonary invaginations.

# THE GREAT STAR MAP

## III. STAR POSITIONS

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In the last article [1] it was shown that from the mere counting of the stars (taking note of their brightness) it is possible to infer some important facts about the nature of the universe in which we are placed: such as the existence of a cluster of stars to which our Sun belongs; and the existence of an extremely tenuous "fog" in the depths of space. But we can always learn much more from prolonged inspection than from a mere glance. A single photograph of a scene tells us only the actual situation at a given moment. It may suggest that changes are going on; but to be sure of these changes we must have another photograph taken later; and if we can get a whole series taken consecutively, as in a kinematograph, we may get a complete history of the changes. The Great Star Map in process of construction is only the first picture; others must follow it if we are to study the motions of the stars, and our knowledge will grow with each repetition. What revelations the future may have in store for us we cannot at present even guess, though it is not too soon to be learning something. The important point to be remembered—and its importance cannot be too strongly emphasised—is that the main purpose of the present project is to provide a basis for these future discoveries, by fixing the present places of the stars with such accuracy that movements can be detected readily. It is only by keeping this fact in mind that we can understand the reasons for the great labour which is being undertaken so cheerfully. A much less laborious project would tell us a great deal; thus nearly all the knowledge about the number of the stars of a given magnitude, which we considered in the last article, could be gathered from photographs taken on a very much smaller scale. There is for instance a very handy map of the complete sky published by the Harvard University Observatory, con-

[1] Science Progress, 1910, v. pp. 240–55.

sisting in all of only fifty-five glass plates, each 10 in. × 12 in., the whole weight of which is only about 30 lb., and the price $15 (rather under cost, owing to the liberality of the Observatory). When we compare with this the 22,154 plates of the Great Star Map, weighing 3 tons, or the 2 tons of paper which the chart reproductions will represent, it is clear that the discrepancy needs explanation. The explanation is simply that the "Harvard Sky," as it is called, though it will tell us many things, will not allow us to study the changes of the stars *in position*, because the scale is too small. Other changes can be studied with its aid: thus the magnitudes of the stars are shown by it rather better than on the Great Star Map, being more uniform in different parts of the plate; and we can study changes in magnitude by comparing two plates taken at different times. The Harvard Sky is actually being used in this way to discover new variable stars, and a great many discoveries have already been made. A positive copy of one plate is superposed on a negative of the same region taken on a different date, and the sharp eyes of three experienced ladies detect any want of correspondence between the pairs of images. At the end of the year 1909 the examination of twenty-one out of the fifty-five portions of the whole sky had yielded 211 new variables; and the efficiency of the search, and of the plates of the Harvard Sky as a means of conducting the research, is constantly attested by the rediscovery of known variables in the course of the examination. If changes in brightness of the stars were all that need concern us we need have no map larger in scale than the Harvard Sky, though it would be profitable to use a more powerful instrument so as to show fainter stars.

But it is important, it is indeed of the very greatest importance, to measure also changes in the positions of the stars; for this purpose the Harvard Sky is unsuitable because of its small scale. The places are correct so far as they go, but the residual uncertainty is more than ten times that of the Great Star Map. Consequently, to measure any given change of position we should have to wait at least ten times as long, and since the majority of the changes with which we are concerned may be expected to require a century or more for their complete identification on the plates of the Map as planned in 1887, those who made the plans cannot be accused of extravagance or hurry.

The scale of a star map depends essentially on the length of the telescope used in making it. The little instrument with which the Harvard Sky was made is only about a foot long, whilst those used for the Great Star Map are about 11 feet. The surface of the representation increases, of course, as the square of the linear dimensions, so that we might fairly expect the weight of the plates to be increased in the ratio 121 to 1; and since the plans for the Map involve covering the whole sky twice over, we must double this, getting 242. Now the ratio of 3 tons to 30 lb. is 224 to 1, so that the weight is adequately explained by the increase in scale. If we use the same factor (242 or thereabouts) to get an idea of the cost of the plates forming the large map, from the Harvard $15 (which however is less than the actual cost of the Harvard Sky, though it is generously offered for sale at this figure), multiplying 15 by 242 we get 3,600 dollars or £700. But the actual cost is greater than this because plate glass has been used and two edges of each plate have been specially ground; probably £1,500 or £2,000 is not too much to put down as the cost of the plates. But this after all is only a very small portion of the real cost of the Map, which arises chiefly from the work done on the plates after they have been taken. In order to expedite comparison with other plates to be taken in the future, the present positions of the star images on the plates are being carefully measured and the measures printed. How much shall we set down for this? It is impossible to make an accurate estimate since circumstances vary so much with nationality, but some rough idea may be gathered from the experience of Oxford, where the measures have been made and printed. Even here there are difficulties, as will appear from the following statement of a generously superior limit to the total cost:

Total Possible Cost of the Oxford Astrographic Catalogue.

| | £ |
|---|---|
| Telescope given by Dr. Warren De la Rue . . . . | 600 |
| Maintenance of University Observatory for twenty years, including Assistance . . . . . . . . . . | 13,000 |
| Salary of Professor for twenty years . . . . . . . | 18,000 |
| Government Grant (from the fund administered by Royal Society) in the years 1896–1910 . . . . . . . | 1,200 |
| Printing (shared between the Government and the University) | 1,200 |
| Total . . . . | 34,000 |

The chief difficulty is to determine how much of the two main items, the maintenance of the Observatory and the salary of the Director, are to be credited to this particular work. In the above statement the whole of the Professor's salary has been set down, not even deducting income tax; but some of it must be credited to teaching and other duties which fall on him as on his colleagues.

The same difficulty arises in a smaller degree about the second item in the list, as the assistants have by no means confined themselves entirely to work on the star map. As the best estimate possible under the circumstances we may take £20,000 perhaps as the Oxford contribution to this great work; and as there are seventeen other contributors, many of whom are not working under such favourable conditions, the total cost will be at least half a million sterling.

These estimates have not been made and quoted for sensational purposes but with the very definite object of showing the necessity for care in procedure. We are dealing with big figures—long periods of time and large sums of money. We must on the one hand spend money freely to save time: we must make a map on a large scale, so that we may determine the movements of the stars within a reasonable period. On the other hand, since every detail of the process will be repeated thousands or even millions of times, we must be extremely careful in settling the details so as to save expense. If one figure will suffice rather than two, the difference may seem trivial in a single instance, but when multiplied a million times will constitute a serious item of unnecessary expenditure. If one measure will give fair accuracy, then before yielding to the temptation to increase the accuracy a little by making a second measure, we must remember that we are multiplying the total cost of measuring by two and consider carefully whether the extra expense is justified; and that we are also multiplying the time required for completing the work by two, and consider carefully whether the completion can be so long delayed without serious disadvantage. When confronted by such problems, the different contributors to the scheme have naturally differed in their solutions. At Oxford we have throughout fixed our attention on getting the work done as quickly and economically as possible, consistently with certain rules laid down by the International Committee; nevertheless the work has taken twenty years. Of course with a larger staff the time might have been shortened; and at Greenwich, where the available resources are greater, a more elaborate programme has been completed in a time shorter by a year or so: but nowhere else is the work yet finished, the prospects of completion being in many cases very remote; seeing that at the outset five or ten years was mentioned as the proper time for the work, the present situation gives some cause for anxiety. The fact is that the necessity for strenuous economy in detail has not been sufficiently realised: some of the larger observatories strained at an accuracy scarcely possible even for them; and their weaker brethren, in attempting to copy their example, have been left far behind. Moderation and self-denial are just as necessary in astronomical work as in other walks of life.

Let us consider in detail the nature of the work to be undertaken. The process of measurement of the positions of the star-images on the photographic plates has been much facilitated, as already mentioned, by the impression of a *reseau* on the plates. Two series of equally spaced lines are ruled, one set at right angles to the other, so that the plate is divided up into a number of small squares of exactly the same size. It is necessary to specify what is meant by "exactly" in this connection. Nothing, of course, is really perfect or exact, but for practical purposes we may regard a ruling as exact if the errors are so small as to be negligible in comparison with the accidental errors of measurement. In this sense and for the purposes of the Map we may regard the little *reseau* squares as accurate and exactly equal. If we number them from left to right ($x$) and also from below to above ($y$), then two appropriate numbers ($x$ and $y$) will specify the square in which any star-image lies; and if in addition we measure the distances of the image from the sides of the square, we shall complete the specification of its exact position. The distances are expressed (in the decimal notation) as fractions of the side of a square and written down immediately after the whole numbers specifying the square. There is no difficulty about the whole numbers: the doubtful points all arise in connection with the fractions. It would take too long to consider them all; we take two important ones as illustrations.

Firstly, how often should we recur to the image of a single star? We have to measure two co-ordinates, $x$ and $y$; we may repeat the measures of each: we may do this for each of the two or three images which occur on the plate (according to the plan already explained) and take the mean; and we may then turn the plate round into another position and repeat the measures. (This last precaution will need no explanation to any one who has had experience of such measurement: it detects and eliminates well-known personal peculiarities in the measurer.) It would therefore be easy to adopt a plan of measurement which would involve recurring to the same star $2 \times 2 \times 3 \times 2 = 24$ times, without real superfluity. Indeed, such a process would be definitely advisable for a small piece of work wherein the utmost accuracy was desired. But what we have to settle with regard to the project before us is whether we can afford it. For comparison let us take the minimum instead of the maximum advisable programme: we can measure both co-ordinates of the star at a single setting on a single image, and this would be the actual minimum, but scarcely advisable—for there is nothing to check a mistake. To check mistakes we must have at least another measure; and if we turn the plate round through 180° to make this, we shall at the same time eliminate the personal errors referred to above. This, then, may be taken as the minimum advisable programme; it involves recurring to the same star twice and twice only. It was adopted at Oxford, and the work of measurement took even then a dozen years: it will be seen how easily this might have been turned into half a century or more.

The second important question of detail concerns the apparatus for measuring the fractions of a square. That which first occurred to the astronomer was the micrometer screw, with which he was already familiar in work at the telescope; at many observatories this type of instrument has been adopted for use. A spider-line in the microscope is set on the side of the *reseau* square and the reading of the micrometer screw noted: then the screw is turned until the spider-line falls on the star-image and the reading noted again: finally the screw is turned further until the spider-line falls on the opposite side of the *reseau* square and the reading noted once more. From these three readings and a little arithmetic the quantity required is deduced. This process can be made very accurate, though there are some difficulties, especially those resulting from gradual wear in the screw when it is used thousands of times. But though accurate, it is very slow. It is far quicker to abolish the screw and substitute a finely divided scale in the field of view of the microscope. It has been shown that the fractions can in this way be read off at sight without losing time in turning a screw. The rapidity of the process naturally excited the suspicion at first that it might be too rough; in order to combat this prejudice, one of the advocates of the new method took over his apparatus to Paris on the occasion of the assembly of 1896 and offered to give a demonstration. A committee was appointed to sit upon him: they shut him in a room with his machine and a plate he had never seen before; he was to produce as many measures as he could in half an hour. At the precise second completing the thirty minutes the door was opened and his measures impounded. It was found that the prisoner had measured twenty-five stars with satisfactory accuracy, and by many this demonstration of the qualities of the machine was accepted as sufficient. With experience a still greater pace can be acquired but we may take fifty stars per hour as a fair average rate for one person (though two people working together can do better). Now it is easy to spend two or three hours on the same fifty stars if we use a screw instead of a scale, so that here again we have a danger of unduly prolonging the work.

The view here expressed that it is stringently necessary to study economy of time and labour is frankly that of an advocate. On the other side there is considerable weight of tradition and opinion which remains unshaken even by such consequences as the great delay in completing what was originally intended to take ten years. Our scientific traditions have come down to us from times when workers were so few and scattered that almost anything they produced, however planned, was precious if not priceless; we see the consequences of this early practice in some huge editions of the correspondence of great scientific workers in which nothing is too commonplace to be included. It is quite possible that considerations of economy have been rightly disregarded in dealing with this sacred past; but does this justify a similar attitude with regard to the future which is better under our control? Scientific workers are no longer few and isolated; they are numerous and they are binding themselves into organisations among which that for making the Great Star Map has an early and an honourable place. It does not seem unreasonable that the changed conditions should leave their mark on the methods of work, and that the relation of cost to value of product should be considered as in other enterprises. In old days the value of the product was so high that any cost could be neglected; and there are still cases where this is the correct view—let us hope there always will be. But in the case of a great piece of straightforward measurement like the Star Map, the value can be expressed very definitely by the "probable error" of the result; and alternative plans for the work can be compared by setting down the probable error and the cost (in time and money) of each. So far as I know, however, this principle has not yet been applied except in a special case in geodesy. It is certainly one that may be applied in other sciences if judged sound; but at present it scarcely seems to have met with the approval, even the attention, of any large body of scientific workers. I trust no apology is needed for inviting attention to it in a scientific review of this kind, even at the expense of a slight digression.

Let us now consider what is to be the outcome of this immense piece of work. What are we likely to learn from these millions of measurements? As already stated, the interest will come when it is repeated—in the study of the movements of the stars; which are so minute that, as a rule, at least a century is required to discern them even by our improved modern methods. The movements are not really slow; we may take the velocity of our earth in its annual journey round the sun—about 20 miles a second—as a fair sample of the velocities of the stars. But our great journey from side to side of the sun (nearly 200,000,000 miles across) would seem a minute movement to the nearest star, and to the great majority would be imperceptible. This is however not the only movement of the earth; the sun himself is moving and we partake of that motion also. It is not a circling or oscillatory motion, but is in the same direction year after year, so far as we can at present ascertain, the distance traversed each year being about 400,000,000 miles. One year's journey is therefore scarcely more perceptible from the distant stars than the circling movement of the earth; but as year follows year the successive steps add together and the cumulative effect becomes ultimately perceptible even to very distant stars. Now all the stars are moving in this way—persistently in the same direction—year after year. Hence, though the movement in one year may be imperceptible, by waiting ten years or twenty or a century we ultimately perceive the movements of many of them. The more distant require even longer—how much longer we cannot yet say; this is one of the questions on which we hope to get some light by the work on the Great Star Map and its successors. Our knowledge will grow: we shall find that after ten years a certain percentage of the stars have moved; after twenty, new movements previously undetected or uncertain, will be added; after thirty, more still, and so on; and by watching the run of the sequence we may even be able to predict what will happen in longer periods not yet reached, though this extrapolation has its risks.

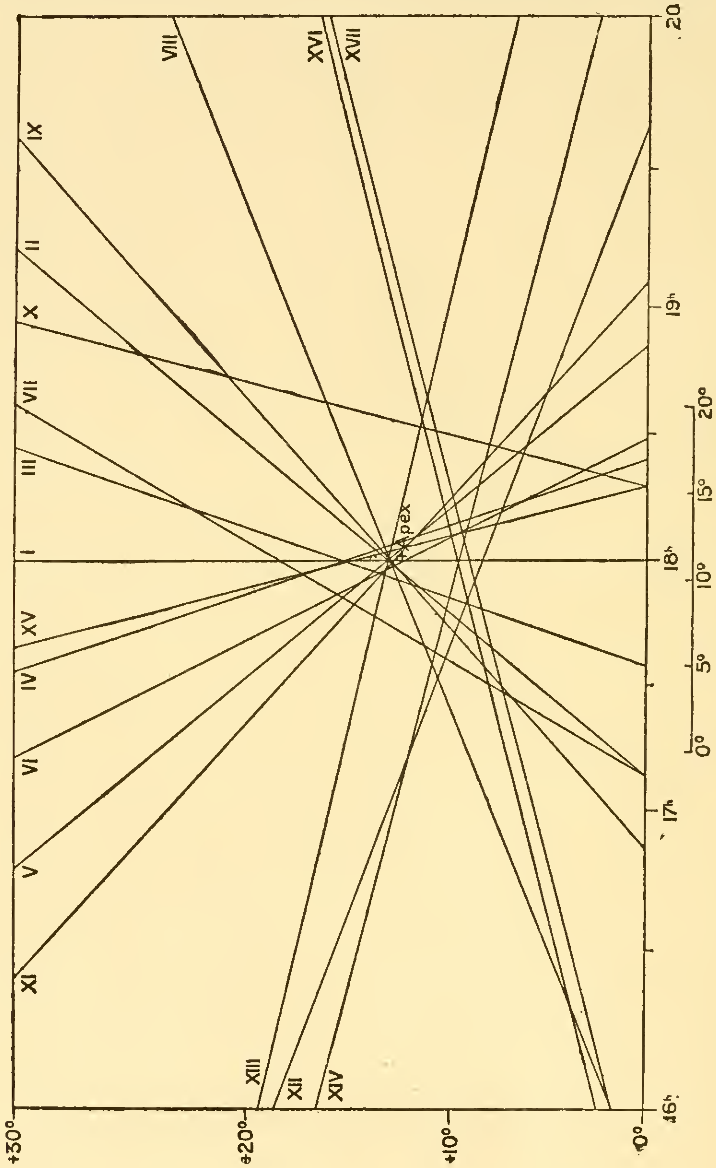
We cannot, of course, afford to repeat the whole map every ten years; we must be satisfied with samples made as representative as possible. We have already obtained some samples at Oxford and the following results will serve to illustrate our expectation. Plates have been repeated after intervals varying from ten years to seventeen and the measures compared. If the measures of any star in either co-ordinate differed by more than 1″·2 (the angle subtended by an inch at a distance of 2½ miles) they were carefully repeated. In a number of cases the discordance was found to be due to some mistake or a careless measure. (It must be remembered that many thousands of measures were made in all, and that occasional mistakes are inevitable.) But in the majority the difference was confirmed as due to a real movement of the star. Such movements were nevertheless very rare—on the average less than 2 per cent. of all the stars examined.

The percentage was higher for the longer intervals somewhat as follows:

| | | | | | | |
|---|---|---|---|---|---|---|
| After | 10 | years, | 1 | per cent. | had moved | appreciably. |
| „ | 12 | „ | 1½ | „ | „ | „ |
| „ | 14 | „ | 2 | „ | „ | „ |
| „ | 16 | „ | 2½ | „ | „ | „ |

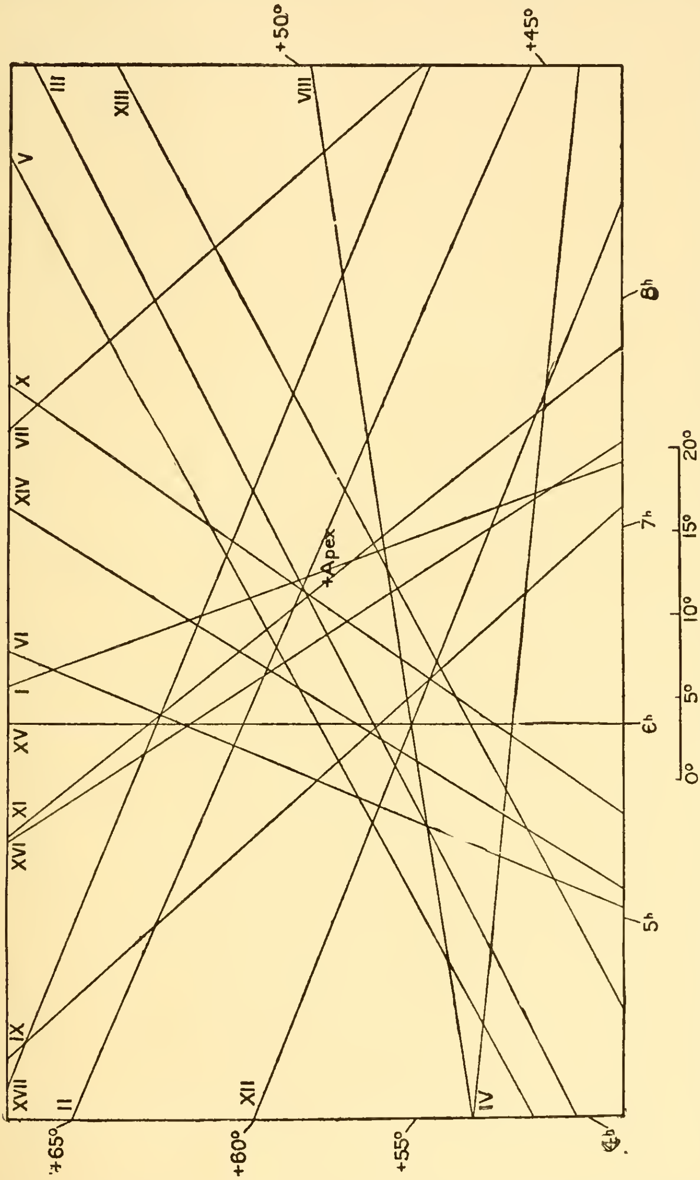
But this method of statement is defective by reason of a most significant fact brought to light by the results themselves.

Of the plates examined for these movements some were of regions in the Milky Way thickly populated with stars—say




500 or 600 on a plate; others were of regions remote from the Milky Way and contained only 50 or 60. [These numbers are smaller than they should be; that is why the plates were

repeated and the comparison rendered possible.] Now we might naturally expect to find more moving stars among the




Mr. Eddington's diagrams of the convergence of movements from seventeen pairs of regions covering the whole sky.
Note.—Each picture represents a portion of the celestial sphere corresponding nearly to that occupied by Europe on the terrestrial sphere.

500 than among the 50, but we do not; there are just as many per plate (that is to say on a given sky-area), in any part of the sky, whether it be densely or sparsely covered with stars. It is

as though a fall of snow had collected into huge irregular drifts and then a shower of rain fell: the number of raindrops falling on a given area would have nothing to do with the quantity of snow upon it already, and we should only make confusion by expressing the rain as a percentage of the snow on the area. Similarly we must give up the idea of finding so many sensible movements per cent. of the stars on a given area and think of the number per unit area, irrespective of the population by other stars. The moving stars are in fact distinct from the others in some way, and it is pretty clear that the distinction consists in their being considerably nearer to us. If this be the correct interpretation, we infer that the stars near us are scattered more or less uniformly and do not show the structure which is so striking a feature of those more distant. This is a fact of fundamental importance and suggests from a new point of view the idea of a solar cluster—a group of stars of which our sun is one standing somewhat apart from its distant surroundings—which was suggested by the counts of stars of different magnitudes as described in the last article.

And its importance is enhanced in consequence of a great discovery made within the last few years, that there are two great streams of stars meeting one another. It was assumed until about 1904 that the movements of the stars were at random in all directions; in that year Prof. Kapteyn of Groningen showed that this could not be the case,[1] but that there must be at least two main streams of stars passing one through the other. This suggestion has been confirmed by elaborate investigations of Eddington, Dyson and others: and in particular Prof. Dyson (now Astronomer Royal) showed that this bifurcation was specially characteristic of the largest proper motions: that is to say, the stars nearest to our sun are moving in this way in any case, whatever may be the real facts about the more distant stars of which our knowledge is still uncertain and incomplete. Is there any reason for thinking that the bifurcation characterises *only* the nearest stars and ceases beyond? One such reason has already been indicated: the nearest stars are apparently distinct from those more remote which cluster towards the Milky Way. Hence

[1] A few months later Mr. H. C. Plummer independently pointed out the same fact. (*Mon. Not. R.A.S.* vol. lxv. p. 568.)

the bifurcation must be proved independently for these remoter stars, since there is apparently a breach of continuity.

But another reason has been suggested also. In a most interesting lecture on the Milky Way, delivered to the British Association at its South African meeting,[1] Mr. A. R. Hinks of Cambridge developed the idea that the Milky Way was made up of a number of independent star-clouds or clusters. If these are in relative motion, as they presumably are, there will be occasions on which one cloud meets another. The stars in each being widely scattered, one will pass through the other freely, without much risk of collision between any of the members. This supposition would explain all the main facts as we know them at present; but we cannot say how far it will fit in with facts to be discovered in the future, when we have compared plates taken at greater intervals, and begin to learn something of the movements of the more distant stars.

By the kindness of Mr. Eddington (Chief Assistant at the Royal Observatory, Greenwich) I am enabled to reproduce two diagrams which show the very latest piece of evidence in favour of the existence of these two star-drifts. It should first be premised that since a series of parallel lines, such as the parallel edges of a box, appear to us to converge to a point (the "vanishing-point" of perspective), so a cluster of stars moving in parallel paths, like a flock of migrating birds, would seem to us to have movements converging to a definite point in the heavens. A beautiful instance of such convergence among some stars in the constellation Taurus was detected a couple of years ago by Prof. Boss of Albany, N.Y. He had suspected its existence for nearly twenty years, but the knowledge of the stellar motions was too inaccurate to convert his suspicion into certainty. This has only come with the completion of a vast research on the movements of the stars which he has conducted with infinite patience: removing one source of error after another by a laborious series of approximations until at last he was able to produce a catalogue of movements freed, as far as possible, from all discernible systematic errors. Incidentally he got values for the motions of the Taurus cluster sufficiently accurate to make it clear that they were apparently converging to a point. With this clue and the help of spectroscopic observations he was able

[1] See an abstract in *Proc. Camb. Phil. Soc.* vol. xiii. Pt. IV.

to determine the distance of the cluster to be 120 light-years away from us (that is, light from the cluster takes 120 years to reach us; the distance in miles, if that be preferred, is 800 million million); and it is receding in an oblique direction. It passed us closest about 8,000 centuries ago, at about half its present distance: and he gathered further particulars of its history and shape, which we can scarcely stop to notice here. One further point, however, is of importance. The individual stars seem to keep their places in the procession without internal rearrangement—they move in almost strictly parallel lines and at the same pace.

Now the streams of stars to which Kapteyn called attention are of a different kind; the internal movements are considerable; it is only the average movement which is steadily in one direction. But when we take such average movements in different parts of the sky they tend to converge to a point like the actual motions of the individual stars of the Taurus cluster. It is this convergence of average movements which Mr. Eddington has represented so beautifully in his diagrams. He divided the whole sky into thirty-four areas, and found (from the great catalogue of movements just published by Prof. Boss) the average movements in each area. It would take too long to explain how he identified the average movement for each of the two drifts: it must suffice that the process was ingenious and effective.[1] He was able to draw the two lines for each of the thirty-four regions, or rather for each of seventeen pairs which he preferred to use. The test of the validity of the hypothesis is that these lines should converge to two points in the sky representing the goals towards which the two clouds of stars are drifting. The reader can judge for himself. In one case the convergence is very striking. It is not, of course, perfect: we could scarcely expect perfection when dealing with averages of imperfect observations, but the approximation is clearly a very close one. In the other case the convergence is less marked, but the reality is brought home to us by an analogy. "If from seventeen points," writes Mr. Eddington, "distributed uniformly all over the earth, tracks (great circles) were drawn, every one of which passed across

[1] Those who care to read more will find Mr. Eddington's paper in the November number of the Monthly Notices of the Royal Astronomical Society. It gives references to previous work.

the Sahara, they might fairly be considered to show strong evidence of convergence: the distribution of the 'drift II.' directions is quite analogous."

We may note yet one more point in this very interesting paper. Eddington found that there was a whole class of stars which it was better for him to exclude. They seem to have a common motion of their own, like that of the Taurus cluster. Moreover, their spectra are all alike (of the Orion type), which is further evidence of relationship; and finally they present two indications of great distance—first that their apparent movements are very small, and next that the stars themselves cluster towards the Milky Way. (We have seen that the stars presumably near to us have large proper motions and are distributed indifferently.) The inference that the distant stars forming the Milky Way do not share in Kapteyn's two drifts seems to be plain. Recurring to Hinks's idea of star clouds, it seems probable that these "Orion" stars belong to a distant cloud, distinct from the two which have met and mingled in our neighbourhood. But before we can accept these rough suggestions as facts we must do much more work in the examination of stellar movements, such as it is the object of the promoters of the Great Star Map to initiate.

One feature of such work on the stars which impresses itself deeply on the consciousness of those who undertake it is worthy of more than passing notice, though it may not be easy to communicate the impression to others. In dealing with the comparison of the places of thousands of stars at two different epochs, a feeling of awe is evoked on finding so few cases of change. As one turns over page after page of records and sees at a glance that the differences are too small to be significant, the first feeling of mere satisfaction at the accuracy of the measurement gradually yields to this growing sense of the profundity of the depths of space which makes this awful stillness. It might not be suspected that pages of figures could serve to develop so sentimental an impression: the layman would be prepared to learn that the observer of distant stars in a huge telescope might feel emotion, but figures, especially in a cataract of thousands, seem far too prosaic. Nevertheless the interpretation of the figures becomes with practice a very rapid mental process, so that one sees behind them the realities they indicate.

A long piece of work of this kind is indeed effective in condensing a number of mental processes. Another illustration of a very different kind may be given. It has been already explained that to guard against mistakes each plate is measured twice over in reversed positions. The two measures of any star are represented by quite different figures, connected by the rule that their sum must represent the whole width of the plate, 26·000. Thus, if the first measure be 8·352, the second (in the reversed position) should be 17·648; since the sum of these two numbers makes 26·000. Now it will be seen that one of these numbers can be derived from the other by the following processes: subtract 8 from 25 and we get 17; subtract 3 and 5 each from 9 and we get 6 and 4; subtract 2 from 10 and we get 8. This is a straightforward but not very simple mental operation, which most of us would perform for the first time with some wariness. It fell to the lot of one of the computers at Oxford to perform it many thousands of times in reading proof-sheets. He presently became so adept that it was easier for him to read the derived figures than the direct ones! If set to read actual figures before him in the usual way, he would stumble; but allow him to transpose them as above and he proceeded with confidence and accuracy. We know that the picture of external objects which falls on our retinas is inverted, and that nevertheless there is no consciousness of inversion in our perception of them; and this result has been ascribed (though not without misconception) to long habit. It was, however, quite new to me to find that the mental process described above could be rendered automatic by the practice of a few months.

(*To be continued*)

# THE EVOLUTION OF THE FUNCTION AND STRUCTURE OF THE FINS IN FISHES

By H. H. SWINNERTON, D.Sc., F.Z.S., F.G.S.

Most people have watched a fish in its native element and have been fascinated by the grace and ease of its movements; very few have stopped to inquire about the relation of these to its form and structure. This is all the more surprising because the subject is not without a practical bearing. Investigators interested in naval architecture have made many experiments which throw much light upon this relationship.

The general resemblance between the hull of a boat and the body of a fish is familiar to all. This, of course, is traceable to the similarity of the purpose to be served, viz. easy movement through water. The advantages of the form adopted can be most easily realised by considering first the case of an unsuitable form such as a rectangle (fig. 1). Experiments on this subject have consisted either in drawing the body through the water, or in causing the water to flow past the object.[1] The results are the same in both cases. In front of the rectangle a mass of still water accumulates, which forms a natural bow. The rest of the water moves past this bow and along the sides of the rectangle without offering any more resistance to it than would be offered to a boat with a bow. At the stern, however, the water turns suddenly in and produces a strong swirl, which tends to suck the body back. The resistance to easy movement is thus felt mainly not at the bow but at the stern.

The case is very different when a spindle-shaped body is used (fig. 2). In place of the watery bow there is now a proper one. Towards the stern the water is let back gently into its place, thus preventing a swirl. The same conditions are fulfilled by a fish's body. This increases from the tip of the bluntly

[1] See for example Prof. H. S. Shaw, "Experiments on the Nature of the Surface Resistance in Pipes and on Ships," Inst. Nav. Archit. 1897.

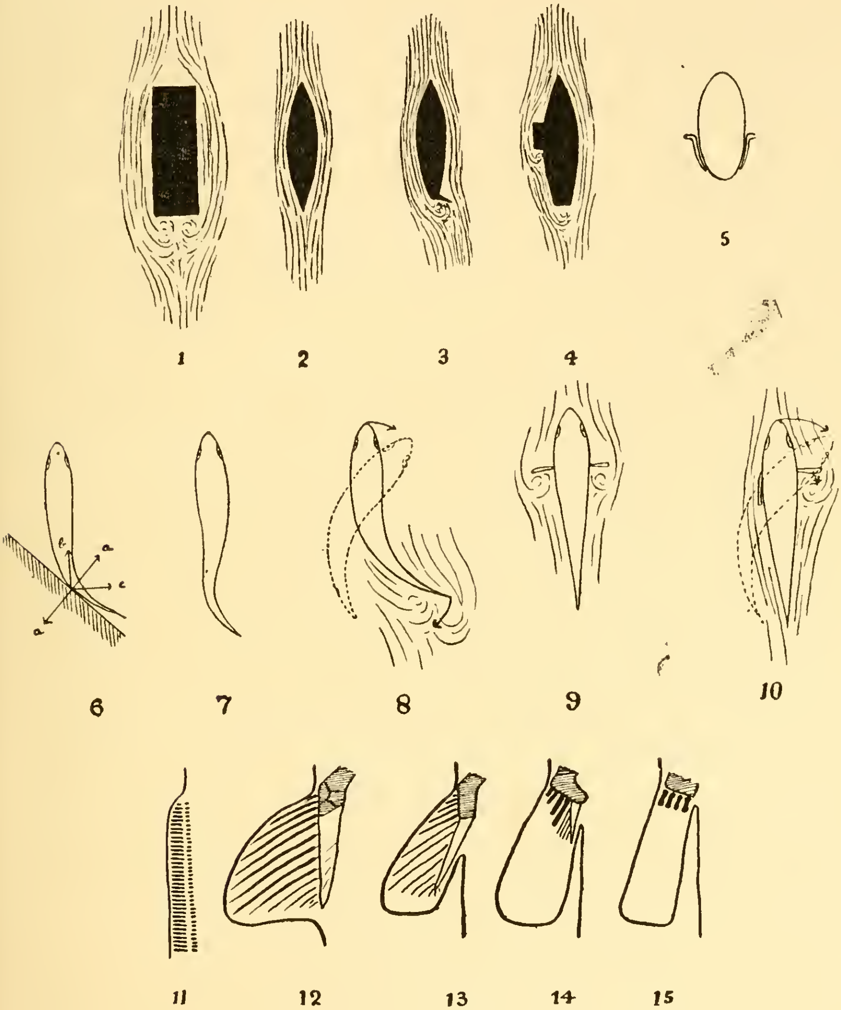
conical head to its widest and deepest part in front of the middle of the trunk. Thence it tapers off gradually, and finishes in a filmy tail fin.

The tail with its fin is the sole propelling organ of the fish. It usually occupies at least half the length of the body but in some, such as the dogfish, it is proportionally much longer. The mechanics of progression are briefly as follows: starting with the tail already bent to the right, it begins to make a stroke towards the left. The water against which it strikes offers a resistance such as would be offered by a frictionless inclined plane. In fig. 6 the pressure (*a*) of this plane against the fish has two components (*b*, *c*), one of which sends the fish forwards. As the fish glides on, the tail passes over to the left of the line of progress in such a manner (fig. 7) that by the time the stroke is finished it is already in position for the return stroke. These points can be detected more easily in long, lithe types such as the dogfish than in short, stiff ones like the goldfish.

Which way will a fish strike its tail when it is about to turn to the right? This is not a question which can be answered off-hand, but it can soon be settled by means of a simple experiment. Cut a thin strip of celluloid to the same shape and size as the fish from its head backwards. Weight this strip so that it will neither rise nor sink in water, and then tie it to one side of the body, say the right. The tail is now free to strike only in one direction, viz. towards the left, and it will now be found that the fish can turn only towards the left. This experiment repeated for the other side gives similar results. The stroke of the tail gives the body such a bend as is shown in fig. 8. The swirl produced behind this tends to suck the tail back. For the time being the whole body is kept rigid, with the result that the snout swings round like the bow of a boat. The fish thus makes a sharp turn by rotating on a vertical axis or fulcrum.

The above description applies more particularly to the bony fishes in which the front part of the body is stiff and practically rigid. In lithe fishes such as the dogfish and sturgeon rapid darts are not indulged in and the fish changes its course by simply gliding round a curve.

Now we may turn to the consideration of the fins, taking first those which are situated in the middle line. Of these there are usually two above—these are the dorsal fins; one below—the ventral fin; and one at the end—the tail fin. The function of the last has been already considered. The generally accepted ideas on the functions of the others are well expressed




in the following quotation: "To give stability to the body, and to steady its course when swimming, the fish has a dorsal and ventral keel, formed by the anal and dorsal fins, which like the sliding keel of a yacht can be raised or lowered as occasion requires. When these fins are removed the course of the fish becomes zigzag, and the animal wobbles."[1] Yes, and most of us would "wobble" if similarly important pieces of our anatomy were removed.

The anal or ventral fin and the hinder of the two dorsal fins are accessories of the tail fin, they are always situated in the tail region and give this a greater purchase on the water. The front dorsal fin has a quite different function, the importance of which is indicated by the fact that it is the largest median and often the only dorsal fin and that its front border is frequently made very strong by means of stout spiny rays. A cursory glance at a fish that is swimming rapidly is sufficient to show that this fin is not in use at such times but lies close along the back like the ears of a hare running at full speed. The moment, however, that the fish makes a sudden turn to the side it is erected to the fullest extent. Evidently its function is associated with the turning movement.

It will usually be found that the strongest and highest part of the front dorsal fin is situated at the point where the body is deepest, and that the vertical line which passes through the fin and the body here, is the one referred to above as a fulcrum. The great vertical depth makes this part of the body less liable to lateral movement than any other part; thus a suitable axis is provided around which the tail and snout may rotate when the fish attempts to turn rapidly even in a confined space. The power to execute such rapid turning in a small space is essential to the welfare of a fish that lives in streams and shore waters where plants, stones, rocks and other obstructions abound.

The current notions upon the function of the paired fins may be gathered from the following quotation: "The dorsal position of the centre of gravity renders the equilibrium of the body unstable, and were it not for the balancing action of the paired fins the fish would float belly upwards, as is always the case after death." Again: "The paired fins may act as lateral keels in steadying the course of the fish especially when the fins are extended and their plane horizontal. They certainly seem to act as balancers in keeping the fish on an even keel and in counteracting the tendency of the fish to turn belly upwards, a result which is attained by slight upward and downward movement of the fins, particularly of the pectoral

[1] *Cambridge Natural History*, vol. Fishes, p. 350.

fins. A fish deprived of its pectoral members sinks downward at the head and assumes an oblique position in the water. Removal of both the pectoral and pelvic fins of one side causes the fish to roll over to that side; and if the fins are removed from both sides the animal turns belly upwards like a dead fish."[1] The chief mistake made by the performer of these experiments was in assuming that the cutting off of the fins was a no more serious operation for the fish than having his hair cut is for a man. As a matter of fact the mere shock of the operation is sufficient to upset the delicate control which the fish has over the movements of all its muscles, including those which govern the walls of the air bladder.

Turn now to observations upon able-bodied fish, *e.g.* a goldfish. When it is advancing rapidly through the water the larger part of each paired fin lies flat against the body, whilst the upper border alone projects to form a shallow keel (fig. 5). If now a piece of silk or cotton be tied round the fish so as to hold the fins flat against the side and thus put them completely out of action it will be found that the fish does not "turn belly upwards like a dead fish" but that it swims about as merrily as ever. It suffers one inconvenience however, for it now bumps its snout frequently against the sides of the aquarium or other objects in its path. After about five minutes it learns to obviate this by bending its tail fin sharply to the right or left. This leads to the production of a swirl which slows down and eventually stops the movement of the fish (cp. fig. 3). At other times it turns sharply aside if space permits.

From this experiment it is evident that the fish's body is in a state of stable—and not unstable—equilibrium. It would indeed have been strange if after all these millions of years nature had not succeeded in producing a creature which was in a state of stable equilibrium in its own medium, especially when such stability would be a marked advantage. Nevertheless the centre of gravity of the fish is not very much below the centre of gravity of the water it displaces. This was demonstrated with a fish that had both pairs of fins tied up. A piece of lead was tied on to its back. The weight of this rolled it over immediately. The size of the lead was reduced bit by bit until it was able to keep upright as long as it was swimming rapidly. Even then if it slowed down it rolled over. The weight of the hapless victim

[1] *Cambridge Natural History*, vol. Fishes, p. 353.

of these experiments was 9·68 grammes, that of the lead at the end of the experiment was ·188 grammes, or less than one-fiftieth of the total weight of the fish. That would be to the fish what a three pound weight would be to a man weighing ten stone. Therefore, though the fish is normally in a state of stable equilibrium, very little is needed to upset it. The mere removal of the fins would do something towards this. It may safely be claimed then that whatever the function of the paired fins may be they are certainly not balancers.

There are two occasions when the paired fins are thrown vigorously into action. The one is when the fish is making a sharp turn; the other is when it is bringing itself to a standstill after a rapid dart. In the former case (fig. 10) it extends the fin on the side towards which it is turning to the full and keeps the other flat against the side. In the latter case (fig. 9) it extends both fins to the full.

The purpose served is well indicated by another of Dr. Hele Shaw's experiments. In this (fig. 4) a spindle-shaped body having a projection from one side was used. The presence of this led to the production of a swirl behind.

Compare this with the moving fish. Suddenly both fins are extended with their planes transverse to its axis and behind each a swirl is produced. This causes a retardation which brings it rapidly to a standstill. If only one fin is extended a more or less fixed point comes into existence around which the body swings by reason of its momentum. This is analogous to the case of a man who runs along the street and, suddenly catching hold of a lamppost, swings round it (fig. 10).

In both the cases just dealt with the function of the paired fins is that of a brake. Whilst this is the main purpose they serve, they are also used for guiding the fish to lower or higher levels in the water. In sticklebacks and their allies this function is modified for the purpose of tilting the body into a vertical attitude. This attitude is also assumed by other fishes. The sticklebacks also use their pectoral fins for fanning a stream of water through their nests. By a similar movement of these fins they can swim backwards quite rapidly. Other fish, *e.g.* trout and roach, can use them in the same way but not so effectively.

Types of fish such as the sturgeon cannot use their paired fins as brakes for the simple reason that they cannot place them transversely to the axis of the body. But the necessity for sudden stopping is not so great in their case as they do not indulge in rapid darting. On the contrary they glide with snake-like motion through the water. The front part of the body is not swung round like the head of a boat turning in a confined space, because they are so lithe that even this part of the body participates in that U-shaped bend with which they glide round a curve. Nevertheless these fins can be tilted upwards or downwards and thus the fish is able to ascend or descend. The execution of these movements is also helped by the vertical flexibility of the body which is not possessed by the shorter fishes. In the dogfish and skate the head can be inclined so as to serve the same purpose.

When turning sideways the various fish just mentioned often roll partly over to the side and they are then able to use their paired fins for horizontal as well as vertical curves.

In the primitive fossil shark Cladoselache the paired fins were very little more than horizontal lobes on the side of the body. They possessed the tilting power, referred to in dealing with the sturgeon, to only a very slight degree, if indeed they had it at all. They served mainly as lateral keels.

Looking at all these facts broadly it is possible to recognise a progressive extension of the functions of the paired fins. Their primary purpose was that of a keel, viz. to steady the fish as it swam through the water; to this was added that of a lateral rudder directing the fish either towards the surface or towards the bottom; finally there was added the brake function, enabling it to execute sharp turns or to come suddenly to a standstill.

It remains now merely to indicate the way in which the internal structure seems to have been modified in association with this extension of function.

According to many the hypothetical starting point for the paired fins was a continuous fin-fold running along either side of the body from just behind the head to the base of the tail. Here the two folds converged towards one another and united in the median ventral fold, which passed backwards, round the tip of the tail and then forwards along nearly the whole length of the back. All these folds were supported internally by a series of gristly or cartilaginous fin-rays (fig. 11).

Possibly all these folds helped in propelling the body by undulatory movements such as are now shown by the median fins of fish like the eel. But the main burden of propulsion was borne by the tail. In performing this function the tail and a greater part of the trunk would be constantly bending first to one side and then to the other. This would not inconvenience the median folds, on the contrary they would help to give the tail a greater purchase on the water. On the other hand the paired folds would be alternately stretched and crumpled, and the enclosed rays parted and crowded in a way that would be a distinct disadvantage. Only in two regions would this not take place: the one near the head, where of course the bending of the body was at a minimum or even non-existent; the other at the point where the folds converged and where, as a consequence, they would be hardly more affected by the bending than would the median fins. The disappearance of the folds in the region between these two would naturally follow. It may be explained as being due either to mechanical, selective or other influence according to the predilections of the reader. In some such way as this the paired lobes seen in Cladoselache may have originated.

At first the base of attachment of the lobe thus isolated must have been very broad as it is even now in the early stages of development of the sturgeon.[1] But the same influences which lead to the isolation of the two pairs of lobes would lead also to a gradual narrowing of the base. At the same time the outward extension of the lobe would add to its efficiency as a keel.

The skeleton of the fin in Cladoselache (fig. 12) consisted of a basal portion within the body wall and a rayed portion in the lobe itself. The front rays were stout and formed a firm keel. The hinder rays were thin and the corresponding margin of the fin extended into a membrane. It is quite possible that this margin could be turned up or down slightly and so could be used for guiding the fish to different levels. The attachment of this margin to the body wall would however limit its usefulness in this direction. It is evident that if this margin were released from the body wall a double advantage would be gained, since the base of the fin would become correspondingly narrower and at the same time the hinder portion of the fin would be freer to perform tilting movements. The

[1] S. Mollier, "Die Entwickelung der Paarigen Flossen des Stors," *Anatomische*, Hefte Bd., 1897, p. 16.

next figure (fig. 13) represents a condition approximating to that which is found in some primitive sharks and suggests a way in which this change might have been brought about, viz. by a rocking outwards of a portion of the basal skeleton.

The fin has now a considerable portion free for use in connection with the new or lateral rudder function. Meanwhile in order to provide this movable fin with a firm basis to work upon, the front part of the basal skeleton seems to have become modified into a limb girdle by extending upwards and downwards in the body wall. Some of the rays still worked upon this girdle, the remainder worked with the free portion of the basal skeleton which had rocked outwards. This latter could still be used as a keel but was concerned mainly with the performance of the new function. The former still served mainly as a keel but became available also for tilting by reason of another change exemplified in the sturgeon. Up to this point in our inquiries the line of attachment of the front rays to the shoulder girdle—which may be called the glenoid line—has been parallel to the length of the fish. In the sturgeon (fig. 14) the girdle has rotated so that this line is nearly at right angles to the axis of the fish. This rotation has made it possible, not only for the hinder margin or half, but for the whole fin to be tilted like a lateral rudder. In the sturgeon the joint along this glenoid line is so stiff that the fins can be no more than tilted. A less clumsy joint would have enabled it to carry the tilting to such an extreme that the plane of the fin would have been at right angles to the axis of the fish. The brake function would then have been attained.

The rotation of the girdle with the glenoid line naturally brought the front rays into conflict with the hinder rays and the latter became crowded between them and the free basal skeleton. The sequel to this crowding is their complete disappearance in the higher bony fishes in which only the rays which are attached to the girdle are present (fig. 15). The intermediate stages in this process are preserved for us in the fins of lepidosteus and amia.

The loss of these rays apparently under the stress of mechanical necessity has its parallel in the wing of the bird. The ancestor of the bird undoubtedly had five digits, now there are only three, the two outer ones having disappeared. This is associated with the way in which the wrist joint of the bird works. In our own case if the hand be held horizontally with the palm downwards the wrist works so that the hand rises and falls; in the bird and its ancestors it worked so that the hand moved sideways and outwards until the little finger lay flat against the fore-arm. The outer digits thus became crowded together in the angle between the arm and the hand. Under these circumstances the fourth and fifth digits disappeared.

To return to fishes. The rotation of the glenoid line indicated in the sturgeon does actually take place during the development of some bony fishes,[1] *e.g.* salmon. In others the rotation is upwards instead of inwards, so that this line runs down the side of the body instead of across its ventral surface. But in both cases the same end is attained, both allowing the fin to be extended so that its plane is at right angles to the body.

Thus it is possible to recognise a complete sequence from the broad-based fin acting as a keel, through the narrow-based fin with limited freedom acting as a lateral rudder, to the narrow-based fin with great freedom of movement which enables it to be used either as a keel, a lateral rudder or a brake.

[1] H. H. Swinnerton, "A Contribution to the Morphology and Development of the Pectoral Skeleton of Teleosteans," *Quart. Journ. Micro. Sci.* 1905.

# THE TRANSLOCATION OF CARBOHYDRATES IN PLANTS

## PART II

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In the first part of this article[1] an attempt was made to show that a detailed examination of the structure and relations of plant tissues, in particular those of the leaf, makes it appear highly probable that the whole of the products of photosynthesis are translocated in the sieve-tubes, in accordance with the theory put forward by Czapek in 1897. It is now necessary to see how far the theory suggested by a study of structure is supported by the results of physiological experiments.

The experimental investigations of the problem may be divided into two classes. In the one the paths of translocation of food materials were sought by observing the results of interrupting possible paths by removing a portion of the tissues. Such surgical operations, whilst yielding instructive results, scarcely enable sound conclusions to be drawn as to the rôle of individual cells, as from the nature of the case it is practically impossible to interrupt the paths that may be formed by some types of cells (*e.g.* sieve-tubes) without affecting others near them. Consequently the inferences drawn apply for the most part to whole tissues only and to a much less degree to their constituent elements.

More exact knowledge has been afforded by the other class of experiments in which the contents of individual cells have been examined by chemical and microchemical methods. In this way the amount and distribution of various substances in the plant have been made out with a fair degree of accuracy; by varying the experimental conditions it has been possible to follow the path of the sugars much more closely.

[1] Part I. appeared in SCIENCE PROGRESS, October 1910, pp. 256-85.

It will be convenient to deal with each type of experiment separately and to show how it has been applied to the various tissues under consideration.

## Experimental Interruptions of Conducting Channels

The earlier experiments of this kind took the form of removing a ring of tissue from a woody plant as far down as the cambium and then observing the amount and nature of the subsequent changes in the portions above and below the ringing.

Hanstein, in 1860, employed this method and obtained a number of interesting results. He found that a cut twig of a dicotyledonous tree if placed in water or moist air develops roots at the cut end and a few leaves or shoots above. But if a ring of tissue be removed quite near the base roots only develop above it, whilst if a vertical connecting strip of tissue be left the twig behaves as if no ringing had been made.

In the case of a shoot which has already produced roots at its cut end a ringing just above them prevents their further development and new roots form above the ringing. When the tissue is removed at some distance from the lower end a few small roots develop below and are the more numerous and well developed the greater the distance of the ringing from the end of the twig.

If, however, the operation be performed on monocotyledonous plants (which have scattered bundles) or on any plant having strands of phloem internal to the zone of wood, as a rule roots develop both above and below the ringing. The growth of those below shows a strict relation to the relative amounts of internal phloem not interfered with by the operation.

Hanstein therefore concluded that the unthickened tube-like cells of the phloem are the characteristic organs for the conduction of formative sap, whilst the cambium and the thickened bast cells are at least not essential, and the parenchyma is not sufficient, for such conduction.

However, his experiments were not considered by Sachs to be altogether conclusive. Sachs contended that if Hanstein's "conducting cells" really conducted all the assimilates they ought to contain starch as well, which was rarely the case. As the point will be dealt with again it will be sufficient to remark that this objection was removed by the work of later investigators who found starch in the sieve-tubes of numerous plants.

Of the many subsequent investigations carried out by the method of ringing those of Lecomte (1889) may next be dealt with. In the case of entire plants which were ringed in spring after the expansion of the leaves he found that the vegetative and floral growth above the ringing became much more active, that callus developed more vigorously at the upper than at the lower edge of the wound, and that the branches grew in diameter considerably above the wound but scarcely at all below it.

In order to study the last phenomenon more closely branches were ringed in June, and in September transverse sections were cut two centimetres above and below the ringing. The comparative development of tissues is shown in the table below, which is taken from Lecomte's paper. (The numbers represent divisions of an ocular micrometer.)

| Plant. | Centre to cambium. | | Cambium to fibres outside the phloem. | | Fibres and cortex. | |
|---|---|---|---|---|---|---|
| | Above. | Below. | Above. | Below. | Above. | Below. |
| *Sambucus nigra* | 330 | 240 | 100 | 65 | 45 | 45 |
| *Vitis vinifera* | 240 | 146 | 114 | 60 | 128 | 80 |
| *Prunus domestica* | 191 | 155 | 55 | 18 | 40 | 32 |

From these examples it is seen that above the wound there is a considerably greater development of tissues than below; this applies especially to the phloem, that of *Prunus domestica* having above the wound three times the radial extent of that below. In the cortex the difference is much less pronounced and so Lecomte concluded that "the phloem above the wound is richer than the cortex in nutritive substances capable of being employed in growth." But as Pfeffer remarks, in order for growth to occur the tendency to grow must exist as well as a supply of materials; such a tendency is not very marked in the cortex.

In some other experiments the tissues were removed only as far as the fibres outside the phloem. It was then found in *Sambucus nigra*, *Cissus quinquefolia* and the vine that the pads of corky tissue formed on the two edges of the wound were approximately equal and that there was no marked difference between the development of the tissues above and below the ring. Such a result indicates that the phloem plays an important part in the conduction of the substances required to form new tissues.

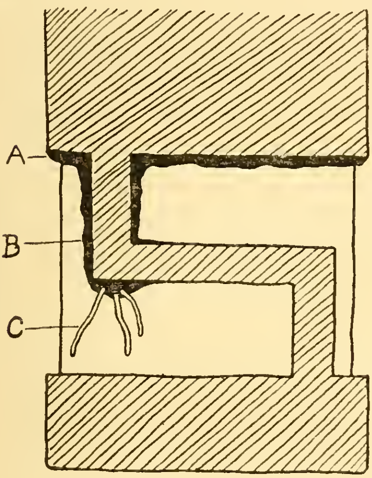
Lecomte paid particular attention to the effects of ringing twigs upon the accumulation of starch in the various tissues. Above the ringing it became very abundant in the cortex, the phloem parenchyma, the medullary rays and the external region of the pith. The corresponding tissues below the ringing proved to contain very much less starch or to be nearly starch-free (*Vitis vinifera*, *Cerasus Padus*, *Quercus Robur*). At the level of the interruption the medullary rays of the wood, and also the pith, showed very little starch. Furthermore in the vine, the sieve-tubes of which ordinarily contain starch, no accumulation of this could be observed, though large quantities of albuminous substances were demonstrable.

These experimental results led Lecomte to conclude that the sieve-tubes do not appear to conduct anything but albuminous substances, that carbohydrates are contained in the parenchymatous tissues, and that they travel from one cell to another in the various types of parenchyma occurring in the stem. Moreover, the very small amount of starch in the pith and the medullary rays of the wood at the level of the wound seems to show that in these tissues conduction only goes on radially.

Lecomte thought that if carbohydrates travel in the sieve-tubes they would accumulate and form starch under the above conditions. But Czapek, who repeated these experiments in a modified form and obtained the same general results, pointed out that the storage cells with which the sieve-tubes are connected do accumulate large quantities both of starch and of nitrogenous assimilates. He considered that the failure of starch to accumulate in the sieve-tubes is no argument against the theory that these elements serve to conduct soluble carbohydrates, since starch is only formed when the concentration of the sugar exceeds a certain value. The surplus sugar is then deposited as starch, which thus represents a reserve, rather than carbohydrate in course of translocation.

As shown by the researches of Meyer and of Schimper the point of formation of starch from sugar varies greatly in the leaves of different plants; accordingly they can be divided into "sugar" and "starch" leaves. Among the former come most of the Monocotyledons. According to Czapek the sieve-tubes do not serve to store assimilates but simply to convey them to the storage tissues in which they are then deposited. He himself was able to show that the assimilates are not conducted horizontally to any great extent through the parenchyma of the cortex, etc.

In March he took twigs of willows and of poplars, 30 cm. long and 1—2 cm. in diameter, and removed the tissues outside the cambium so as to leave near the lower end a

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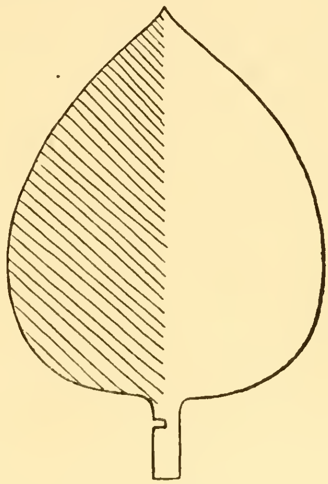

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FIG. 17.—Diagram to show formation of callus (black) in ringed twig.

FIG. 18.—Diagram to illustrate Czapek's experiments with petioles. The shading here indicates starch.

connecting strip consisting of three portions, an upper and a lower vertical, and a middle part longer than either running horizontally (fig. 17). The twigs were kept in a warm, saturated atmosphere and in diffuse light. After twenty-five days under these conditions it was found that in some cases callus had developed below the upper edges of the wound (A) and along the sides of the upper vertical connecting strip (B). Immediately beneath this upper vertical strip the development of callus was more vigorous, and in one or two cases rootlets also had appeared (C). Along the horizontal limb, however, the formation of callus was restricted to within a very few millimetres of the upper limb and elsewhere no callus had been produced.

The organic materials used up in the formation of the callus must have come from the tissues above the wound. Czapek therefore held that the distribution of the callus showed that conduction of organic material through the connecting bridge was quite possible in the longitudinal direction, but that such conduction could go on only to a very small extent horizontally. He concluded that the paths of translocation were rectilinear and longitudinal, and that the cells of greatest importance in this process must be those which are longitudinally extended and are connected in strands, *i.e.* the sieve-tubes and possibly also the cambiform cells—longitudinally elongated cells of the phloem parenchyma. Such horizontal conduction as was found to occur could, he thought, easily be accounted for by the occasional lateral connections of sieve-tubes with each other, and by the very oblique end walls of the cambiform cells which thus touch each other laterally to some extent.

Czapek adds that although he has so far been unable to obtain direct proof, he "imagines on many grounds" that the sieve-tubes play the most important part in the process of translocation. However, this experiment did not differentiate between carbohydrates and proteids, both of which are needed in the formation of new tissues, and so Czapek next endeavoured to determine the paths of the carbohydrates alone.

At the end of bright afternoons he made incisions half-way through the petioles of the leaves of various plants in such a way as to remove half a transverse section of the tissues (fig. 18). The leaves were suitably supported and darkened till next morning, when their starch content was examined by means of a solution of iodine.

The general results were as follows. In plants in which the vascular bundles run separately through the petiole and no cross-connecting strands occur between them, the half of the lamina corresponding to the cut side of the petiole was found to have been more or less prevented from losing its starch. In extreme cases it still contained a great deal of starch when the other half of the leaf had become quite starch-free.

But where the vascular bundles anastomose, as in ferns, *e.g.* the lady fern (*Athyrium filix femina*), or there are cross-connecting sieve-tubes, *e.g.* the gourd (Cucurbita), it was found that the operation did not prevent the removal of the carbohydrates; the two halves of the leaf apparently lost their starch equally quickly.

Czapek maintained that since the elements of the vascular bundles alone were completely interrupted by the transverse cuts, while the parenchymatous elements were still in connection laterally and diffusion could proceed in the latter equally well in all directions, translocation of the sugars must go on in the sieve-tubes.

Where cross-connecting sieve-tubes did not occur, the incision produced a complete interruption of the paths of translocation, whilst this was not so where such cross-connections were present. In the latter case the sugars could still pass into the sieve-tubes of the uncut portion of the petiole and travel through them.

However Haberlandt refuses to accept Czapek's assumption that in parenchymatous tissues horizontal and longitudinal conduction can proceed with equal ease. In fact he considers that the results of Czapek's incision experiments are "an interesting confirmation" of his own theory that the protoplasm of the transverse walls of parenchymatous cells is much more permeable than that of the lateral walls, and that elongation of cells indicates the direction in which the cell contents most readily travel.

Haberlandt himself attaches greater importance to some of the experiments made by Schimper in 1885. In one of these the veins were carefully removed from a leaf of *Plantago media*—an operation which can be performed without injuring the surrounding nerve parenchyma to any extent, since the bundles are provided with a sheath of thick-walled, unlignified cells which easily break away from the starch sheath. Plants with leaves treated in this way, together with normal control plants, were placed in darkness from May 31 to June 10. At the end of this period Schimper examined the starch and sugar content of the leaves and concluded that the operation did not markedly hinder the emptying, and that the vascular bundles were unnecessary for the normal translocation of the carbohydrates.

However the details given are insufficient to enable one to rely upon the experiment as being critical for determining the question at issue. The sugar content is the most important thing to be examined, and Schimper's account of this is here quite inadequate. The disappearance of starch from mesophyll is not by itself a satisfactory indication of the amount of sugar translocated from the leaf, for as Sachs showed in the case of cut leaves a considerable loss may be due merely to translocation into the larger veins. Moreover during ten warm days respiration must have caused a considerable diminution in the amount of starch and sugar present in the leaves, quite apart from translocation.

Czapek was of the opinion that while Schimper's experiment showed that the sugars could pass from the mesophyll into the "Leitscheide," it was quite another question whether they left the leaf by this path or by another in the normal course of events.

Numerous ringing experiments have also been carried out on plants possessing a laticiferous system with results of considerable interest.

As mentioned above, Hanstein found that ringed twigs of dicotyledonous plants having bicollateral bundles (phloem both external and internal to the wood) behave differently from those with collateral bundles (phloem only external to the wood), as far as the production of roots is concerned. He thought therefore that if laticiferous tubes could take the place of the phloem in conducting nutriment, ringed twigs with collateral bundles and laticiferous tubes in the pith should behave like ringed twigs having bicollateral bundles.

However, experiments with twigs of *Ficus Carica* (which has medullary laticiferous tubes) showed that only a very poor development of roots took place below the ringing, so that a sufficient supply of nutriment could not have been forwarded by the laticiferous tubes in the pith.

In 1905 Kniep obtained a similar result with *Ficus australis* which also has medullary tubes. Hanstein performed another experiment upon this species of Ficus. A ring of tissue was removed from a twig just beneath the growing end, and the leaves already formed above it were plucked. The upper part of the twig died, which he thought would scarcely have happened if it had been able to obtain nutriment from the assimilating part below the ringing for the unfolding of new leaves.

Two years later, in 1866, Faivre expressed his belief that latex is really a nutritive sap, an opinion partly based upon an experiment with a small plant bearing eight leaves of a species of Ficus. At a point a few centimetres above the insertion of the roots he cut away a ring of tissue in the usual manner. After two years instead of the original eight leaves above the ringing there were twenty-five. Below the ringing no leaves had developed, the cortex had shrivelled and the roots showed no increase. But as Kniep pointed out, this experiment does not show that the latex has a nutritive value. A similar result would have been given by a plant without latex, for the assimilates formed by the initial eight leaves could well be employed in the production of new tissues.

In order to do away with this objection Kniep worked with *Ficus Carica* in a somewhat different manner. In May, before the plant had developed leaves, he made ringings on several shoots at various distances from their terminal buds, and watched the subsequent development of the leaves and the increase in length of the newly-formed internodes. To minimise as much as possible the formation of assimilates above the ringing the young leaves were removed very shortly after they had unfolded; in a similar manner the young leaves of unringed twigs were removed. It was found that in equal times the leaves at the end of the unringed twigs grew to a much larger size than those on the ringed twigs. The formation of new leaves on the ringed twigs became continually weaker, until at last it quite ceased; this occurred so much the sooner the nearer the ringing had been made to the growing point.

From these experiments, and from others carried out with plants grown for a time in darkness, Kniep concluded that in a ringed twig development of the upper portion is only possible at the expense of the reserves already stored in its tissues and that the medullary laticiferous tubes contribute little if anything to the formation of new tissues.

It may be noted that the greater development of adventitious buds upon the lower portions of ringed shoots than upon the corresponding portions of unringed shoots, repeatedly observed in these experiments, suggests that an upward conduction of formative materials in the tissues removed by the ringing was prevented, the result being that more nutriment became available for the production of new buds, etc. Such a result is to be expected if the phloem serve to distribute the assimilates to places where they are required.

The chief inferences to be drawn from the results of experiments such as those described above are that the parenchyma by itself is insufficient to conduct the organic materials required for the growth of new tissues, that these substances probably travel in the phloem and that the value of laticiferous tubes either for conducting nutriment or for providing a reserve supply is somewhat uncertain. The further conclusion drawn by Czapek, that of the phloem elements the sieve-tubes play the most important part in the conduction of the assimilates as a whole, could hardly be regarded as beyond question if it were based only on the experiments described above. In none of these was it possible to study the conducting properties of any one type of cell in anything like a satisfactory manner, and therefore other methods of inquiry must be considered.

### Chemical and Micro-Chemical Investigations

In 1859 Sachs attacked the problem by means of micro-chemical examinations of the amount and distribution of the chief forms of plant food materials such as sugars, fat, starch and proteids present at various periods during the germination of seeds. The splendid work which he did in this manner gave a new impetus to the study of plant nutrition and led to a wide application of his methods to numerous other cases. His conclusion that carbohydrates travel for the most part through the innermost layer of the cortex has been mentioned in the first part of this article together with Heine's criticisms.

Later on, in his *Text Book of Botany* (English translation, 1882, p. 91), Sachs appears to have been somewhat influenced by the work of Briosi (1873) who, finding that starch grains could be made to pass through sieve-plates by artificial pressure, thought that this might also occur in the plant, and that in this way the sieve-tubes served to conduct carbohydrates—an opinion adversely criticised by Lecomte. In the passage referred to Sachs attributes to the sieve-tubes the function of serving secondarily for the transport of carbohydrates. Similarly in his *Lectures on Plant Physiology* (English translation, 1887,

p. 358) he considered that "in the case of a very vigorous transport of starch . . . even the phloem of the vascular bundles may take part in it."

With regard to the laticiferous tubes Sachs was inclined to think that the presence of carbohydrates in their contents could only indicate that the tubes served to convey these substances to the growing points.

A considerable amount of work has been done upon the function of latex by observing the changes in its constitution during different stages of the growth of the plant either under normal or artificial conditions.

For example Faivre (1866) found that the latex of *Morus alba* in vigorously growing parts of the plant in spring is very thin and apparently poor in contained substances; from this he concluded that it plays an important part in vegetable nutrition.

Schwendener, who repeated the observations in 1885, could come to no definite conclusion. He found, however, that "in those organs whose latex had become thin there occurred now and then plug-like masses which had evidently arisen from clotting." These were composed of numerous little spheres closely packed together. Schwendener felt he could not give any opinion upon the quantity of materials used in this way but at all events the fact is one to be kept in mind when dealing with changes in the fluidity of latex.

In referring to this point Kniep suggests that in plants whose tissues undergo changes in preparation for a winter rest, it would not be very surprising if changes also occur in the latex which result in its becoming thicker and more slimy. He also suggests that the thinning observed in the spring may be due in part to the water forced into the tissues at that time of the year. Moreover Faivre did not demonstrate an actual increase in the contained substances on the approach of winter, which Kniep thinks would be necessary in order to prove that the latex constitutes a reserve.

Faivre also grew seedlings of *Tragopogon porrifolius* in the dark and again found that the latex became more watery. Similar results were obtained when plants were grown in light but in an atmosphere devoid of carbon dioxide. He concluded from such results that latex is produced in the leaves and is translocated to places of consumption, as apparently it loses in substance under conditions which normally bring about a consumption of reserves, and besides the latex practically no other reserves exist in the seedlings.

Similar experiments were made in 1887 by Leblois who worked with the closely allied *Scorzonera hispanica* and was led to express the opinion that under the conditions of his experiments "the latex was a product of secretion and not a reserve."

Kniep also made a number of observations upon the latex of *Tragopogon floccosus*, *Campanula media*, *Vincetoxicum nigrum* and *Chelidonium majus*, seedlings of which he grew both in darkness and in light. The results obtained seemed to him to make the conclusiveness of Faivre's experiments still more questionable.

The changes in the constitution of the latex of *Euphorbia Lathyris* between germination and production of seeds were studied by Schullerus in 1882. In addition he cultivated seedlings under various artificial conditions. From the results of the whole of his work he drew the conclusion that latex is a formative sap which is conducted in the laticiferous tubes.

In the experiments hitherto described an apparent diminution in the materials contained in the latex was usually taken to mean that some of these had been transferred to the surrounding tissues to assist in their development. But as Kniep points out, several other causes might bring about a consumption of the constituents of the latex. For example the starch might be employed in forming or regenerating the other organic materials present, or in the construction of new protoplasm and membranes for the tubes themselves during their own growth, or finally it might be directly used up in respiration.

In the light of these considerations the conclusions drawn from experiments in which plants had been darkened for many days appear rather unreliable. As a case in point, Treub's experiments (1883) upon seedlings of *Euphorbia trigona* may be given. Portions of the young stem and of the leaves were darkened with tin-foil during periods varying from *three to six weeks*. In very few cases did the starch completely disappear from the laticiferous system, though the end branches of the tubes became starch-free, as well as the other tissues of the darkened stem. Apart from the pathological conditions which must have prevailed, it is extremely difficult to tell from an experiment of this kind whether the starch has been used up in nourishing the surrounding tissues or has been employed in the laticiferous tubes themselves. Treub, however, did not consider the latter contingency as at all probable. In addition the exact significance to be attached to the disappearance of starch from particular portions of a non-septate tube is not altogether clear.

The starch grains in the latex of some Euphorbias have a remarkable power of persisting long after darkening has caused all traces of starch in the parenchymatous tissues to disappear. For instance, in 1885 Schimper, whose results were subsequently confirmed by Kniep, found that after being in darkness during twelve days the starch grains in the latex of *Euphorbia Peplus* showed no diminution in size, though they appeared to be rather less numerous. In repeating and extending these experiments Kniep kept one plant of *Euphorbia Lathyris* in darkness during twenty-two days, and even at the end of that period found "no difference in the starch content of the latex."

Such a persistence of starch grains also occurs among most plants in the guard-cells of stomata and in the starch sheath. To account for it either the plastids associated with the formation of the persistent grains must be supposed to differ somewhat from those producing the starch in the assimilating cells or else the other cell contents must be different in the several cases referred to. In this connection it is extremely interesting to note Kniep's observation that corrosion of the grains only occurs to a very slight extent and only after a long time—a fact which rather suggests some difference in the production of enzymes effecting hydrolysis of the starch.

Diastase itself does not appear to have been demonstrated as yet in the latex of Euphorbias; indeed Kniep states that the appearance of the starch grains in such latex is never like that of grains artificially exposed to the action of this enzyme. On the other hand, malic acid was found to produce very much the same "melted off" appearance as is actually shown by the starch grains under consideration, and it is well known that calcium malate is present in the tubes.

Kniep agrees entirely with Schimper that "the laticiferous tubes of the Euphorbiaceæ are not concerned to any marked extent with the conduction of carbohydrates." With regard to the starch grains themselves he does not think they form a typical reserve available for the nourishment of other tissues in the plant, and admits that their true significance "must still be considered an open question."

As Haberlandt points out in the fourth edition of his *Physiologische Pflanzenanatomie* (1909), one cannot generalise from the starch grains of the Euphorbiaceæ. It is not inconceivable that the latex of different families of plants may function in various ways; that sometimes its possibilities as a food supply, and at other times its protective properties, may occupy the most prominent place. In the case of *Brosimum Galactodendron*, the cow-tree or milk-tree of Venezuela, large quantities of latex flow out when a notch is made in the stem, and this latex is sufficiently palatable and nutritious to be used by men like ordinary milk. Here at least the latex is rather of the nature of a food than a protection against the animal world. In many other cases, however, it is by no means palatable, and sometimes it is poisonous—*e.g.* the Upas-tree (*Antiaris toxicaria*) of Java—while its property of coagulating on exposure to air provides a ready means of closing small wounds.

In the investigations which have been so far dealt with considerable attention has been paid to variations in the amount of *starch* under diverse conditions of experiment. Many valuable results have been obtained in this way, but it is more important to make similar observations with regard to the *sugars* present, since the carbohydrates actually travel either in these or other soluble forms. As a rule starch is only deposited when the concentration of these soluble carbohydrates exceeds a value varying greatly with different plants.

The work of Schimper (1885) upon translocation is of particular interest, as he examined in detail the changes in amount undergone both by starch and sugar when leaves were allowed to empty in darkness. After being decolourised in alcohol the leaves were placed in a solution of chloral hydrate in water to which iodine was also added. In this way the tissues were made fairly transparent, and as the agent caused the starch grains to swell, Schimper was able to examine their distribution without cutting the tissues. The sugar distribution was examined by means of Fehling's solution.

It was found that the disappearance of starch proceeded in different ways according to the plant chosen. In the case of *Impatiens parviflora*, it first left the bundle sheaths, next the adjacent cells and finally the more distant cells of the mesophyll. In *Hydrocharis morsus-ranæ*, on the other hand, it left the more distant mesophyll cells first and apparently accumulated in the bundle sheaths. Later these became starch-free, the small sheaths first, then the larger ones, until finally no starch remained even in the largest sheaths.

Leaves of *Impatiens parviflora* left several days in darkness became sugar-free; in this case it was found that the "glucose" left the mesophyll first, next the finest sheaths and later still it could only be detected at the bases of the stronger veins. Ultimately the sugar left the lamina altogether and could only be demonstrated in the petiole. A number of experiments of this kind led Schimper to conclude that sugars travel almost exclusively in the bundle sheaths. But he only followed their passage as far as the bundle sheaths; his method was not well adapted for observing the contents of delicate cells like sieve-tubes, situated in the middle of the tissues of the veins, indeed he appears to have neglected these elements almost entirely. His observations were therefore incomplete, though accurate as far as they went.

It will be seen that similar changes in the distribution of sugars would occur if they passed from the mesophyll into the small sheaths and were then withdrawn by the sieve-tubes which the sheaths enclose. Continuous diffusion from the sheaths into the sieve-tubes would eventually remove the whole of the sugar from the leaf; naturally the smaller sheaths would first become empty.

I have frequently observed stages in the process of emptying in which the sheaths of the small veins contained no sugar, while their sieve-tubes contained a considerable amount. In the light of the anatomical facts previously considered it seems not unfair to infer from such a distribution of the sugar that it had passed from the sheath into the sieve-tubes and was in course of translocation in these elements. This is made all the more probable by the fact that in still later stages sugar could only be detected in the sieve-tubes nearer the bases of the veins. (Fig. 19.)

As previously pointed out a certain amount of sugar passes directly from the mesophyll into the nerve parenchyma of the stronger veins; this, slowly diffusing through the cells, gives Schimper's results in the larger veins.

Schimper's work on laticiferous tubes has already been referred to, but it will here be well to indicate his general line of argument. He considered that if the laticiferous tubes serve to remove carbohydrates from leaves in the same way

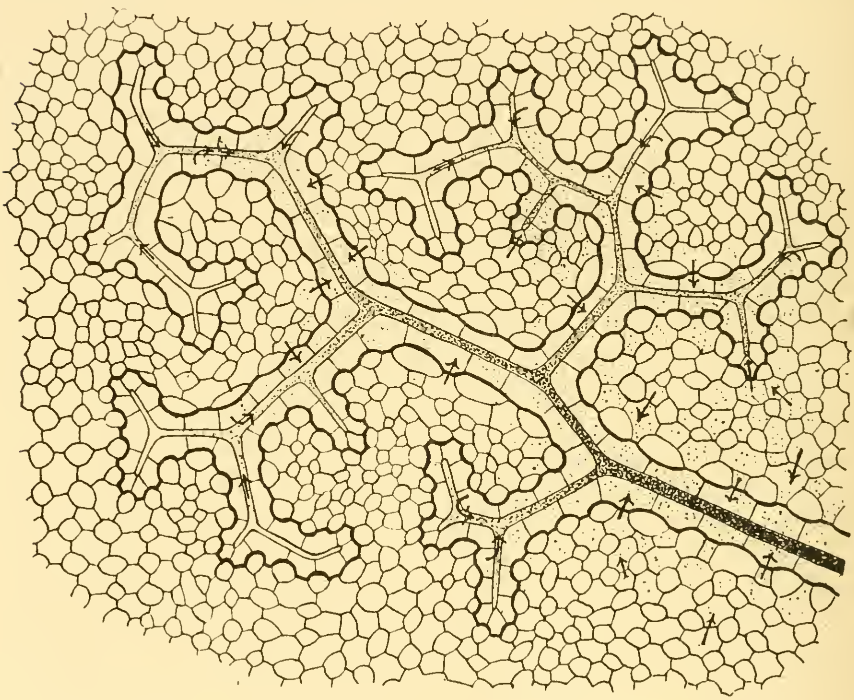

FIG. 19.—Diagrammatic representation of the distribution of sugar in a leaf which has been in darkness for some time.

The concentration of the sugars is indicated by dots, while arrows show the direction of motion. Only the spongy mesophyll, bundle sheaths and the phloem of the vascular bundles are represented.

as he supposed the bundle sheaths to do, then certain things should hold good. In the first place the carbohydrates present in the latex under ordinary conditions must *rapidly* disappear on darkening the plant; secondly there must be a definite relation of the assimilatory cells to the laticiferous tubes, such as had been described by Haberlandt; and thirdly the carbohydrates formed in the mesophyll must travel towards the laticiferous tubes just as they do towards the bundle sheaths.

The first point has been dealt with above, whilst Schimper's observations upon the second were given in the first part of this article. It may be added, however, that Prof. Biffen in 1897 made further examinations of such anatomical relations and concluded that "taking all observed cases into consideration . . . we may safely say that the blind endings of the laticiferous system are generally connected with the palisade cells." On the other hand Kniep's studies led him to believe that the anatomical relations do not lend any stronger support to the conducting than to the protective function of the laticiferous tubes.

With regard to the third point Schimper found that only in the case of *Euphorbia myrsinites* was there any reason to suppose that the sugars passed into the laticiferous tubes; in the other species of Euphorbias with which he worked the emptying proceeded just as in plants without laticiferous tubes. He considered that on the whole his experimental results were opposed to the theory of conduction by means of the tubes.

The method employed in 1897 by Biffen in his work upon the function of latex enabled him to obtain quantitative estimations of the amount of *sugar* present under various conditions, and thus afforded data of considerable significance. Unfortunately it was not possible to carry the research beyond a certain point and only a preliminary account is available.

Latex was drawn from various species of Euphorbias and after precipitation of the proteids by excess of alcohol and evaporation of the filtered solution to dryness, the sugars were extracted with warm water and estimated by Fehling's solution. Some results, expressed in terms of the sugar equivalent in grams of cupric oxide per cubic centimetre of latex are given below. *Euphorbia pulcherrima* before darkening gave the value ·021, while after one day in darkness this fell to ·002—a tenth of the initial value. No trace of sugar could be found after another day in the dark.

In another series of experiments the latex was examined at different times on the same day, and an increase in the sugar was found at the end of the afternoon thus:

| 10.0 a.m. | 4.0 p.m. |
| --- | --- |
| ·032 | ·049 |
| ·030 | ·046 |
| ·033 | ·046 |

From these and other considerations the conclusion was drawn that "one function of latex is to carry reserve materials in the plant."

Although the method used is not altogether free from objections (cf. Parkin, 1900), one that is has yet to be discovered and in its absence no very definite statements can safely be made with regard to the extent to which conduction takes place in laticiferous tubes.

It has already been stated that Czapek showed the existence of sugars in the sieve-tubes of numerous plants, but in addition he made an experiment with the object of proving by microchemical means that these sugars *travel* in the phloem. A gourd plant was darkened during nearly a week to allow the leaves to empty of their carbohydrates. Then one leaf was exposed to bright sunlight during eight hours, the rest of the plant being kept in darkness. At the end of this time the exposed leaf was cut off and the iodine test showed that starch had been formed in it. Transverse sections were then cut from the petiole of the illuminated leaf and from those of the darkened ones, and these were tested for sugar by means of Fehling's solution. The result was that the part of the bundles containing sieve-tubes in the petiole of the leaf which had been assimilating became markedly distinguished from the other tissues by the presence of a considerable quantity of red copper oxide, formed presumably by the action of reducing sugars. No such distinction between the tissues could be made out in the case of the petioles of the darkened leaves. The results were therefore taken to indicate that the sugars are translocated in the phloem, though Czapek did not in this place explicitly state that the sieve-tubes themselves are the conducting elements.

The difficulty in an experiment of this nature is to secure proper controls; it is indeed practically impossible, as the same tissue cannot be examined both at the beginning and at the end of the experiment. Individual leaves empty at different rates: even after ten days in darkness I have found sugar present in the sieve-tubes at the base of one of the larger veins of a Cucurbita leaf. But the results of Czapek's experiment certainly do in a measure justify his conclusion.

Although Schimper and Czapek were led to support different theories, their results are really complementary, since the former only followed the sugars over part of their journey, whilst the latter attempted to trace their further path in the sieve-tubes. But as Czapek's evidence is considered by Haberlandt to be insufficient to justify his conclusions it remained to be seen if a more satisfactory proof of the translocation of sugar in sieve-tubes could be obtained. This is not very easy by means of Fehling's solution, its use as a sugar test having several disadvantages. A good deal of diffusion of cell contents occurs in the hot aqueous solution ; delicate tissues are liable to injury on heating owing to the action of caustic potash ; the granules of copper oxide precipitated are very small and not always easy to observe ; crystallisation sets in on the slide and entails a rapid remounting of the tissues tested, making comparative experiments less convenient.

Moreover Fehling's solution is not reduced by cane sugar and is reduced by other organic compounds which may be present—*e.g.* tannin. Hence it affords by no means an ideal method of examining the distribution of sugar in plant tissues. A very much better method was introduced by Senft in 1904, by means of which osazones of the sugars can be formed with comparatively little diffusion. These are easily visible and permanent in glycerine. Using this method I have been able to obtain photomicrographs showing the distribution of sugars in the tissues after exposing plants to definite conditions of experiment. Some account of the work was given at the Sheffield meeting of the British Association last year, but the research is still in progress and details must await future publication.

The depletion of leaves in darkness was studied by examining the sugar distribution in long longitudinal sections of the midribs, larger veins and petioles of various leaves. When the right stage in the emptying was hit upon it was found that while the distal ends of the veins were quite free from osazones, the sieve-tubes nearer the base contained them. The amount present in the sieve-tubes *increased towards the base of the veins* (cf. fig. 19), and osazones were found frequently in the nerve parenchyma.

Where the vascular bundles of a lateral vein entered the tissue of the midrib the sieve-tubes of these smaller veins were often very conspicuous on account of the bright yellow osazones contained in them—*the surrounding parenchyma frequently being entirely without osazones.*

Taken in conjunction with all the foregoing considerations such results seem to furnish strong evidence in favour of the theory that sugars are translocated in the sieve-tubes.

The work is being extended to the lower plants with the object of finding out how far sieve-tubes in general serve to conduct carbohydrates. Some results have been obtained which indicate that the sieve-tubes of Gymnosperms and the "leptoids" of Polytrichum function in this way.

The sieve-tubes of such Algæ as Laminaria are also under investigation, and some attention has been paid to other problems of translocation.

Among the latter are the forms of sugar translocated, the possibility of a periodicity in such translocation of uncombined sugars, and the upward conduction of sugars in the vessels in spring.

It is hoped that by the study of translocation in the lower plants more light will be thrown upon the conduction of food in general and that it will be possible to arrive at a clearer understanding of the functions of the various tissues of such lower plants. In this connection Lepidodendron is peculiarly interesting. Prof. Seward has suggested that in the living stem of this fossil plant the physiological division of labour might have been different from that which we find in the nearest living representatives of the genus, as in such stems "there is no indication of any tissue which can be identified anatomically with true phloem." This at once suggests that in such plants the conduction of sugars was probably carried out in a manner unlike that in modern plants which possess true sieve-tubes, and provides a further problem in the physiological anatomy of fossil plants.

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ERRATUM.—Fig. 1 on p. 260 of Part I. of this article is after Haberlandt, not Schubert.

# THE PROBLEM OF THREE BODIES

By F. W. HENKEL, B.A., F.R.A.S.

The complete solution of the "problem of two spherical masses moving under their mutual gravitation, their relative positions and motions being known, at any one instant to determine their motions at any other time," was obtained by Newton more than two hundred years ago and given to the world in the first book of his immortal *Principia*. He showed that the bodies would move in similar paths round their common centre of gravity (or mass) and these paths would be one or other of the curves known as the conic sections, ellipse (circle as special case), parabola or hyperbola, the speed at any point determining which of these curves would be described. When one body is greatly more massive than the other it is often found convenient to imagine it reduced to rest and refer the motion of the other body to it, rather than to the common centre of gravity, by applying to both bodies a motion equal and opposite to the actual motion of the heavier one.

If now instead of two bodies only we have three or more whose positions and masses are known, the *general* problem of determining their future relative positions far transcends the present power of our mathematical methods and seems likely to long remain insoluble. However, for all the cases we have to deal with in *our* system, where we have two bodies moving round their common centre of gravity and the third body either very much smaller than the central one or very distant, approximate methods are available whereby a solution to any required degree of accuracy may be obtained. We have in the Planetary theory a predominant central body, the Sun (whose mass exceeds that of all the planets together, more than seven hundred times), round which revolve much smaller bodies, the planets. The motion of each of these latter will be mainly determined by the action between itself and the sun, so that to a first approximation we may consider the planet as moving in an ellipse round its central body. Then by the principle of the "superposition of small motions" we may separately allow for the difference of attraction which each other planet exerts upon itself and the sun, which differences are known as "perturbations," till we obtain as close an agreement between theory and observation as we desire. In the case of the moon, though the sun (the main disturbing body) attracts our satellite more than twice as much as the earth does, yet since the deviations from elliptic motion are due not to the whole action but only to its difference, which is but a small fraction of the whole amount, similar methods apply here also.

It is, however, the opinion of high authorities that the lunar motions cannot be *completely* accounted for by gravitation alone and an important paper on the "Fluctuations of the Moon's Motion" was one of the last contributions of the late Prof. Newcomb to astronomical science (*Monthly Notices R.A.S.*, January 1909).

These methods, depending on a series of successive approximations for cases where the attraction of one body greatly exceeds that of the second, on the motion of the third, cannot be expected to give solutions of the general problem.

The advance made by G. W. Hill consisted in substituting the "variational curve" for the ellipse as the moon's intermediate orbit; thus was introduced the idea of *periodic solutions* into the problem of three bodies. Previous work had always begun by taking the undisturbed ellipse which the moon would describe under the attraction between itself and the earth alone, and supposing the various elements of this orbit to be continually changing under the sun's disturbing action. The moon was supposed to be always moving in an ellipse but the shape, size and plane of the ellipse itself was in a continual state of flux. At any instant, however, there will be only one ellipse, which will pass exactly through the moon's position at that moment and in which the velocity will be the same as the actual velocity. If at this moment we suppose the disturbing force to cease the attraction of the central body will cause the moon to continue moving in this curve, which is known by the name of the "instantaneous ellipse"; it is this curve which was taken as the standard.

Hill's "variational" curve may be described as the moon's circular orbit supposed in the plane of the ecliptic, affected solely by the inequality known as the "variation" (which, as far as terms of the second order, is independent of the eccentricity and inclination, *Principia*, Prop. 66). The orbit is a periodic one when referred to a plane rotating uniformly with the sun's apparent rate of motion, the period being a synodic month (interval from one new moon to the next). The "elliptic" and "inclinational" inequalities are "free" oscillations about this periodic orbit, the annual equation is a "forced" oscillation having as period of the disturbance an anomalistic year.

M. H. Poincaré, in his *Methodes Nouvelles de la Mécanique Céleste* has dealt with some other special cases. He takes two of the three bodies as having infinitesimal masses and obtains a class of solutions when these bodies move in circles about the third body. From this he proceeds to the consideration of cases when the masses of the two "planets" are small but no longer infinitesimal. Periodic solutions are arranged in three classes and though it would appear that the initial circumstance of the motion may often not give rise to exactly periodic forms yet it will frequently happen that these forms are approximations of which the actual motion may be regarded as a "perturbation." He finds that four classes of periodic solution exist, depending respectively on the four arbitrary constants: (1) the period of the infinitesimal body; (2) the constant of energy; (3) the time of conjunction; (4) the longitude of conjunction.

In the work referred to Poincaré also discusses general questions, such as the cases where the existence of periodic orbits may be inferred, their method of appearance and disappearance and general laws as to their periods of oscillation, etc.

Sir George Darwin has taken the constant of relative energy of the motion of the infinitesimal body as his single arbitrary quantity. No general method being known for the determination of periodic orbits, he has made a numerical determination of as many cases as possible and has traced the changes of form that correspond to different values of the relative energy.

The three bodies he calls the Sun ($S$), Jove ($J$) and the planet or satellite, respectively, and takes Jove of unit mass moving in a circle of radius unity round the sun of mass 10, all three bodies lying in the same plane. The third body is taken of infinitesimal mass (a material particle), so that no question of its action on the others can arise. Thus when referred to moving axes, both the Sun and Jove can be represented as fixed points in diagrams. The ratio 10:1 for the Sun in terms of Jove is taken, so that thereby all the phenomena of perturbation are greatly exaggerated as compared with those of nature, and are thus more clearly shown.

He first obtains the Jacobian integral:

$$V^2 = \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 = 2\,\Omega - C$$

($C$ is a constant, $V$ denotes the velocity of the particle relatively to axes moving with angular velocity the same as that with which Jove and the Sun move). The function $\Omega$ is the potential of the system and so the equation $V^2 = 2\Omega - C$ is called the equation of relative energy. Since in all real cases $V^2$ must be positive, $2\Omega$ can never be less than $C$ and accordingly the particle can never cross the curve represented by $2\Omega = C$; if this curve has a closed branch inside of which the particle is it must remain inside, if outside it must remain outside. This agrees with Hill's result in assigning superior and inferior limits for the position of the moon.

Since $2\Omega$ also $= \nu\left(r^2 + \frac{2}{r}\right) + \left(\rho^2 + \frac{2}{\rho}\right)$ where $\nu$ is the sun's mass in terms of Jove (taken here as 10) and $r$ and $\rho$ are the distances of the particle from the Sun and Jove respectively, we see that when $r$ and $\rho$ are small the equation obtained by putting $V = O$; $2\Omega = C = \frac{2\nu}{r} + \frac{2}{\rho}$ nearly, the curves of "zero velocity" are like the equipotential curves round two attracting centres of masses $2\nu$ and 2 and by putting various values for the constant $C$ they may be drawn. Thus when $C$ is large they are closed ovals round Sun and Jove, the one round the former body being the larger. As $C$ diminishes the ovals swell and unite into a figure of eight.

When $r$ and $\rho$ are large the equation becomes approximately

$$\nu r^2 + \rho^2 = C \text{ (a Cartesian oval).}$$

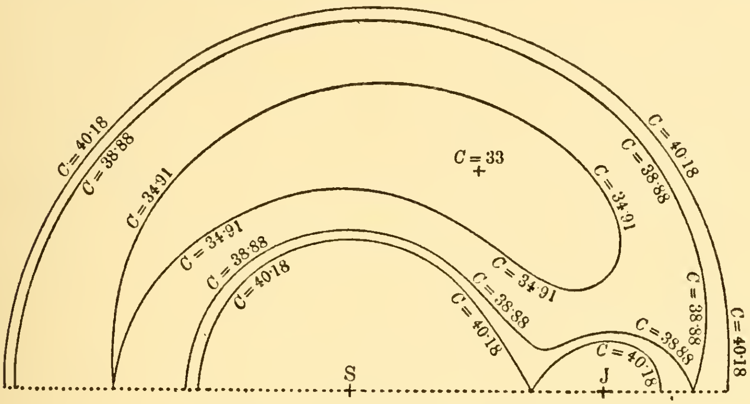
For large values of $C$ the curve of zero velocity consists of two closed branches round Sun and Jove respectively and a third closed branch round both bodies. As $C$ decreases the larger oval shrinks and the inner ovals swell and unite into a figure of eight which gradually takes a form like that of an hour-glass with continually thickening neck. The outer and inner curves meet at length, $C$ continually diminishing, uniting by the smaller bulb of the hour-glass figure (that around Jove) touching the other. The curve is then something of the shape of a horse-shoe, which breaks up, for a still smaller value of $C$, into two elongated pieces. These elongated pieces shrink quickly and contract into two points, when $C$ attains the minimum value (33).

Thus there are four critical stages in the development of the curves when $C$ has the values between 40·18 and 33. The first stage is when the internal ovals unite into a figure of eight. This evidently must occur between $S$ and $J$ and the point is one of zero effective force where the planet may revolve without motion relatively to the two other bodies (fig. 1). It is found that for the values assumed for $\nu$ (10), here $r = 1 - \rho$ and turns out ·717, $\rho = $ ·282 and $C = 40·18$. Thus this point is the beginning of a family of orbits.

The second position, where the smaller end of the "hour-glass" figure unites with the outer curve, occurs at a point in $SJ$ produced beyond $J$, and here $r = 1 + \rho$. For this case the values of $\rho = $ ·347, $r = 1·347$, $C = 38·88$ (fig. 1). This point lies beyond Jove, on the line $SJ$ produced, and is similarly a position at which the planet may revolve without relative motion, but, as before, the motion is unstable.

The third critical position, where the "horse-shoe" breaks, is similarly in the line $SJ$, produced beyond $S$, and here $\rho = r + 1$. The values for the quantities $r$, $\rho$ and $C$ are approximately $r = 0·95$, $\rho = 1·95$ and $C = 34·91$. This is also a case of unstable motion. The fourth position occurs when $C$ is a minimum, and the elongated pieces shrink to points. Here $\frac{\delta C}{\rho r} = 0$ $\frac{\delta C}{\delta \rho} = 0$ giving $r = 1$; $\rho = 1$; $C$ becomes $3\nu + 3 = 33$. If we draw an equilateral triangle on $SJ$ as base, its vertex will be at this fourth critical point, and since two such triangles may be drawn, one on each side of the base $SJ$, there are two points corresponding to this position.

This is a solution of the problem of three bodies, known to Lagrange, the three bodies, Sun, Jove and planet standing at each corner of an equilateral triangle, the latter revolving with uniform angular velocity. It is approximately realised in the actual solar system by the Sun, Jupiter and a minor planet recently discovered, which revolves at a distance from the Sun nearly the same as that of Jupiter. Though for the case of $\nu = 10$ the motion is unstable, yet for values greater than twenty-five it is stable. The five critical positions thus correspond with particular exact solutions, three of which are unstable and two are in general stable. The critical curves of the family $2\Omega = C$ are given in our figure, being symmetrical on both sides of the line $SJ$, after Sir George Darwin's paper. It may be seen from this figure that for values of $C$ greater than 40·18 the third body must be either a superior planet moving outside the larger oval, or an inferior planet inside the larger internal oval, or a satellite within the smaller oval, but cannot exchange one of these parts for either of the others. When $C$ lies between 40·18 and 38·88 the body may be a superior planet, or an inferior planet, or a satellite, or a body moving in an orbit partaking of the two latter charac-




Curves of Zero Velocity (Darwin).

Critical values: $C = 40{\cdot}18$, $C = 38{\cdot}88$, $C = 34{\cdot}91$, $C = 33$.

$$10\left(r^2+\frac{2}{r}\right)+\left(\rho^2+\frac{2}{\rho}\right)=C.$$

teristics, but it cannot pass from the first condition to either of the two latter. If $C$ is less than 38·88 and greater than 34·91, the body may move anywhere except within the horse-shoe shaped region (shown in the figure). Here it is possible for a body once started as a superior or inferior planet or satellite to change the characteristics of its motion from one of these forms into either of the others. When $C$ is less than 34·91 and greater than 33, the "forbidden regions" consist of two strangely shaped spaces on each side of the line joining Sun and Jove. For values of $C$ less than 33 the body may move anywhere. Since it is found that for values of $C$ greater than 40·18 the third body must be either a superior planet or an inferior planet, or a satellite, and these cases are treated of in the Lunar and Planetary theories, the only values dealt with by Sir George Darwin are those less than about 40·5, and in the main simply periodic and direct orbits are considered (*i.e.* those which are re-entrant after a single revolution, though some of them may contain loops). A few non-periodic orbits are however dealt with in his later work, and some real and apparent retrograde paths also. (Superior planets whose motion is direct *in space* will of course appear retrograde when referred to the rotating axes since these latter revolve more quickly than outer planets.) For the case when $C = 39$, Sir George Darwin gives a figure showing how a planet at first moving round the sun in an elliptic orbit of moderate excentricity, gradually experiences increasing disturbance from the action of Jove, and the elliptic form will be altogether lost, the body being either drawn off when near aphelion and in conjunction with Jove, to circuit round the latter, or it may revert once more to the sun, but its path will be of a totally different kind. In general a body after being drawn off would make several circuits round Jove until a concurrence of apojove with conjunction produced a severance of this connection. This may sometimes happen after one revolution only, but if the "neck of the hour-glass" giving the curve of zero velocity be narrow the body may move many times round one of the centres before its removal to the other. "It seems likely that a body of this kind would in course of time find itself in every part of space where its motion is possible." Thus sooner or later it would pass close to the Sun or to Jove, and colliding with one or other, be absorbed. In this way stray bodies may be gradually swept up by the sun and planets, a process which Dr. See supposes to be the cause of the equatorial acceleration observed in the rotation of the Sun, Jupiter, and Saturn, *i.e.* the fact that regions nearer the equators of these bodies perform their rotation in a shorter time than parts farther north or south. Some non-periodic orbits round Jove and round the Sun, others passing from Jove to the Sun, together with their conditions of stability and instability, are determined by various criteria, and regions within which stability is impossible are marked out. In some such way as this it is hoped that an explanation of Bode's empirical law as to the relative distances of the planets from the Sun may be found. In his later paper (*Monthly Notices R.A.S.*, December 1909), besides giving a fresh investigation of the stability of periodic orbits founded on the suggestions of Hough, he discusses cases of retrograde motions and the manner in which certain direct orbits pass into the retrograde form. The important "orbits of ejection" when a body is ejected from the Sun towards Jove, or from Jove towards the Sun, are discussed, and the forms of the paths given for different values of the energy constant $C = 38$, 34, 30, 26 and 20. For the value of $C = 20$ the body ejected from the Sun starts in a straight line towards Jove, and is gradually slightly deflected by the latter. By the time it has reached and passed the place where Jove was, the latter has passed away, and the body falls back again on the Sun nearly in a straight line. This orbit is the limiting form of four coalescent ones, the motion in which may be either direct or retrograde. The ejectional stage affords one method of transition whereby direct paths may become retrograde or *vice versa*, another way being passage through the apex of the equilateral triangle on $S\ J$ as base, this point being one of zero force, as already mentioned. A body may reach this apex with an indefinitely small velocity, either direct or retrograde, and the direction of motion would then change. M. Burrau has also treated the subject of orbits of ejection, and in addition he has considered cases where the "Sun" and "Jove" are of equal masses, which though not applicable to our own system may have valuable bearings on the conditions of "other stars than ours." Charlier has also discussed the problem of oscillating satellites, and arrived at similar results to those given by Darwin, whilst Professor Whitaker has given a criterion for the discovery of periodic orbits, and important papers on this subject have also been contributed by H. C. Plummer. There can be little doubt that the further progress of theoretical astronomy will be largely on these lines for some time to come, and it is to be expected that much additional light will be thrown upon many of the more obscure portions of the science.

In connection with the application of these results to Cosmogonic theory, one or two of which we have already briefly outlined, Dr. See has suggested that an additional term might be added to the Jacobian integral to represent the rate of increase of the constant of relative energy as the particle revolves against resistance and steadily drops nearer to the centre of attraction (*Astr. Nach.* 4341–42). The action of a resisting medium diminishing the velocity at every point and thus causing the resisted particle to draw nearer the attracting centre or centres, it may, if circulating within the "hour-glass" region enclosing both bodies, finally come to one or other of the closed folds and acquire a constant of relative energy such that it cannot again escape therefrom. There seems reason to believe that light will be thrown upon the problem of the origin of the Zodiacal Light, and possibly of the "Gegenschein" also. Though the investigations of Sir George Darwin are confined to cases of motion in one plane, and indeed the circumstances of our own system are such that this is very nearly true, yet it is very easy to extend these results to space in general by regarding the curves as contour lines of a surface, whereby a notion of the surface may be obtained. Lord Kelvin was of opinion that all possible orbits for any value of the constant of energy are essentially unstable, but Darwin has given reasons for supposing that this instability being a very slow process "the whole history of a planetary system may be comprised in the interval required for the instability to become manifest."

# THE PREVENTION OF MALARIA[1]

PROF. RONALD ROSS, poet, mathematician, and apostle of tropical sanitation, gives us in the first half of this book the fruit of twenty years' research and experience with regard to the causation and prevention of a disease which, on his estimate, causes "between a quarter and a half the total sickness in many tropical countries." The remainder of the book consists of reports from those who have put Ross's principles and methods into practice in every part of the globe; their results afford a brilliant testimony to the immense value to humanity of the modern discoveries regarding the etiology of malaria, a subject in which Ross himself led the way and broke entirely new ground.

The aims and objects of this book are essentially practical and utilitarian; it is intended to set forth concisely and in a form popularly intelligible the relevant facts concerning the natural history and causation of malaria, in order to show clearly how the disease may best be prevented and eradicated. It is, in short, a manual of instruction for medical men, sanitary officers and administrators upon whom may fall the task of combating malaria in some part of the globe. For this reason the more purely scientific aspects of the malarial problem, such as the marvellous and complicated life-history of the parasite in its two hosts, vertebrate and insect, are dealt with as briefly and simply as possible.

Whilst avoiding, however, zoological technicalities in the body of the book, the author suggests in an appendix a new terminology for "The Phenomena of Cytogenesis," by which is meant the developmental cycle of unicellular organisms such as the parasite causing malaria. If, in this age of rapid advance in scientific knowledge, there be any who remember the period, not much more than ten years ago, when in consequence of Ross's startling discoveries with regard to the transmission

[1] *The Prevention of Malaria.* By Ronald Ross, C.B., F.R.S., Nobel Laureate, with Contributions by Prof. L. O. Howard, Col. W. C. Gorgas, and others. [Pp. xx + 669.] (London: John Murray, 1910. Price 21*s.* net.)

of malaria by mosquitoes the life-cycle of the parasite was being studied eagerly in all parts of the world, they will recollect that almost every writer on the subject at that time invented a new terminology of his own for the various stages of the parasite. It is somewhat late in the day to add another system of nomenclature to the many already consigned to oblivion and that put forward by Ross labours under the further disadvantage of attempting to found a general terminology upon the intricacies of a special case, instead of employing terms long in use and founded on a wider outlook, in order to adapt them to the special requirements of those occupied with a limited range of objects.

The malarial theorem, as Ross aptly terms it, may be stated briefly as follows. Malaria in man or animals is a disease caused by a minute parasite which lives and multiplies in the blood, passing its trophic phase within the red blood-corpuscles. The parasite is propagated from one vertebrate host to another by the agency of certain mosquitoes, which, on sucking the blood of an infected vertebrate, take various stages of the parasite into their stomachs with the blood; one stage of the parasite resists the action of the digestive juices of the mosquito and passes through a sexual process of generation and multiplication within the body of the mosquito, the cycle ending after some days with the production of many thousands of minute germs ("protospores," Ross) lodged in the salivary glands. If the mosquito then succeeds in biting a vertebrate of the right kind, that is to say, of the kind which is a specific host of the parasite in question, the germs pass down its proboscis and are inoculated into the blood, thus bringing about a fresh infection of the disease. Every statement made in the foregoing sentences is based on rigorous experiment or accurate microscopical investigation. It only remains to add that the various species of malarial parasites are specific to certain hosts, vertebrate or insect; those of birds, for instance, can only be transmitted by mosquitoes of the subfamily Culicinæ; those of men only by the subfamily Anophelinæ.

The principles of malarial prevention depend on the facts stated above; their aim is to interrupt the complex chain of circumstances upon which the existence of the parasite depends. If the parasite cannot pass from man into the mosquito, or from the mosquito into man, it becomes extinct with the death of its host, if not before. Hence there are three general principles of prevention: (1) *mosquito-reduction*, that is to say, measures undertaken with the object of destroying mosquitoes or their aquatic larvæ, or of abolishing or reducing the pieces of water suitable for them to breed in; (2) *treatment* of human beings with quinine, either with the object of reducing the cases of sickness, or in order to enable the healthy to resist infection, should they chance to be bitten by an infected mosquito; (3) *protection* of human beings against mosquitoes by various methods, especially by rendering houses proof against these insects. Any of these three methods are sufficient to extirpate malaria completely, if carried out thoroughly. If, in a given locality, the Anopheline mosquitoes are exterminated, or if there are no infected persons for them to bite, or if they are prevented from biting human beings, the disease cannot continue. But in practice it is always impossible to carry out any of these methods completely and it is necessary to rely upon a combination of the three. The best results are always obtained by mosquito-reduction, since it can be effected by administrative regulation, whilst treatment and protection depend mainly on individual effort—always difficult to control or enforce.

Such are, very briefly, the main aspects of the problem of preventing or extirpating malaria; but many pages are required to set forth the intricacies and complications of the question. Ross is the first to attempt to deal with a problem of this kind mathematically. The following is a sample of his method.

"Let $p$ denote the human population of the locality; $mp$ the number of malaria-infected persons; and $imp$ the number of these with gametids in their blood [capable of infecting the mosquitoes that bite them]. Here $m$ and $i$ are fractions. . . . Again let $a$ denote the number of Anophelines (of some malaria-bearing species) to each human being—so that $ap$ denotes the total number of Anophelines in the locality, and $aimp$ the number of Anophelines compared with the number of persons with gametids. Let $b$ be the proportion of these (say $\frac{1}{4}$) which succeed in biting; $s$ the proportion (say $\frac{1}{3}$) which succeed in maturing the parasites; and $b$ the proportion which succeed in biting another person. Then $bsbaimp$ gives the number of Anophelines which succeed in infecting persons. . . . For example, in a village containing 1,250 people, 750 infected people and 3,000 Anophelines, $p = 1{,}250$, $m = 0{\cdot}6$, $a = 2{\cdot}4$; and we calculate roughly that the number of infecting Anophelines and also of inoculations will be about 9·4." (It is not clear, however, to what length of time this applies, whether week, month or year.)

It is pointed out further, however, that a mosquito which is infective may bite a person who is already infected, so that the expression $b^2saimp$ does not denote the new infections. If $mp$ denotes the number of persons already infected, $p - mp$ denotes the number of healthy persons. Then the number of mosquitoes which succeed in biting healthy persons is shown to be $b^2sai(1 - m)mp$; and if each of these mosquitoes bites a different person, the expression will denote the addition to the number of cases of malaria in any locality during a given period.

It is extremely doubtful, however, if the biting factor can be expressed so simply as is suggested by Ross. It is known that the malarial parasite requires a certain length of time to develop in the mosquito; on p. 52 this period is stated to be "more than a week." During this period the mosquito probably feeds one or more times without being in a position to produce an infection; on p. 55 we are told that Anophelines feed "every few days." If, for example, we put the developmental period of the parasite at eight days, and suppose that the mosquito feeds every three days, it is clear that it will not be able to produce an infection before its fourth feed, counting as the first that at which it took up the parasite. It is not necessary to suppose, however, that all these four meals must consist of human blood; it is only essential, in all probability, that the first and fourth feeds of the mosquito should be at the expense of human beings, in order to produce an infection of malaria.

Following Ross's method, let $b$, as before, express the proportion of Anophelines, say $\frac{1}{4}$, which succeed in biting human beings, and $b_1$ the proportion, say $\frac{5}{6}$, which succeed in biting *any* vertebrate during a given time; then the biting factor must be expressed by $b^2b_1^2$, $= \frac{25}{576}$, or $\frac{1}{23}$ approximately; in any case it is less than $\frac{1}{16}$ $(b^2)$, since $b_1$, whatever value be given to it, must be less than unity. But this again is far from expressing accurately the complexity of the actual state of affairs, since it is highly probable, from what is known of the transmission of other Protozoal infections, that the infectivity of the mosquito persists a long time, when once acquired ; that is to say, that a mosquito which can infect at its fourth meal can do so also at its fifth, sixth, . . . or *n*th feed, probably for some months. It seems clear, therefore, that the biting factor requires a much more complex mode of expression than that given to it by Ross.

The method of mathematical exposition is developed by the author in an interesting manner through many formulæ and equations. For instance, if $m$ be the proportion of infected persons at the beginning of an anti-malarial campaign, and $m_1$ the proportion at the end of a given period, then

$$m_1 = m + b^2sai(1 - m)m - rm,$$

where $r$ denotes the average proportion of infected persons who recover during a given period. That is to say, the number of persons infected with malaria at the end of a given period will equal those at the beginning, *plus* the number of new infections, and *minus* the number of recoveries. This is a very important consideration, since it shows that if, as the result of anti-malarial measures, the new infections are less in number than the recoveries, the disease must disappear inevitably, in course of time, if the preventive measures are continued persistently. Ross suggests that the malaria formerly prevalent in some parts of England has died out gradually in this manner.

We doubt if this method of treatment will make malarial problems any clearer to the ordinary person who is unaccustomed to express his thoughts in mathematical formulæ. The most that can be said for it, from the purely practical point of view, is that the symbols $b$, $s$, $a$, $i$, $r$, form a useful *memoria technica*, by means of which to keep in mind the various factors upon which the prevalence of malaria depends in a given locality. The formulæ themselves tend rather to produce perplexity, especially when printed inaccurately, as on p. 254, lines 11 and 12, where it is clear either that $m$ and $m_1$ have been interchanged, or that "less" should be "greater" and "reduced" should be "increased." Whilst on the subject of misprints, it is to be regretted that the book does not show evidence of very careful proof-reading, especially as regards proper names. The honoured name of Leuckart is spelt wrongly on every one of the numerous occasions on which it is cited, and we note Giolgi for Golgi (p. 67) and Ostler for Osler (p. 646).

Turning to the second half of the book, it is impossible to give in a brief space an account of the many anti-malarial campaigns in all parts of the world that are described and reported upon by twenty-one different writers. It must suffice to state briefly the results obtained in Ismailia, that most striking example of the practical importance of Ross's researches and of the efficacy of his methods, as set forth by Mr. H. C. Ross.

The town of Ismailia was founded by M. de Lesseps, who intended it to be a model city, a thriving port and the head-quarters of the Suez Canal Company. It progressed rapidly, rising to a population of 10,000, until in 1877 malaria made its appearance, 300 cases being notified in that year. From 1885 to 1902, the number of cases annually was seldom much below 2,000, and in 1891 it rose to over 2,500, more than one quarter of the whole population. In 1902 an anti-malarial campaign was started under the auspices of Prof. Ronald Ross, directed chiefly towards the extermination of the mosquitoes. A marsh near the town was drained; pools were filled up and a mosquito-brigade was organised consisting of a European foreman and two natives. The duty of the brigade was to visit every house once a week, to treat the cesspools with petroleum, in order to kill the mosquito-larvæ; to empty all standing vessels or tubs containing water; and to clear all irrigation-channels of reeds, so that the water could flow swiftly. Penalties were imposed on the inhabitants if they did not report to the authorities the existence of untreated collections of water. The work cost £2,000 the first year and about £1,000 a year subsequently. For this relatively trifling outlay the most remarkable results were obtained. The number of cases of malaria fell at once, in 1903, to 300; in 1904 there were 90 cases; in 1905, 37; since then no fresh cases of malaria contracted in Ismailia have occurred: the disease is stamped out. It is necessary, however, to continue the preventive measures, since if the mosquito-brigade stops work for a week, the mosquitoes return; and there is always the danger, in that case, of a recurrence of the epidemic through infected persons from without coming into the town.

Nothing shows more clearly than the case of Ismailia the possibility of combating malaria effectually in any locality if anti-malarial measures, especially that of mosquito-reduction, be carried out rigorously. The failure to obtain similar results by similar methods in other places must be ascribed mainly to the obstacles which stand in the way, of which the chief are ignorance and blind resistance to administrative efforts on the part of the people, or official apathy and parsimony on the part of the governing classes. Professor Ross speaks out strongly on these subjects, as he has done frequently before. It is to be hoped that this book will be widely circulated amongst medical men and administrators in the tropics, and that it will lead to a proper understanding of the measures to be taken against one of the greatest obstacles, in many lands, to civilisation and progress.

E. A. MINCHIN.

# RECENT ADVANCES IN HIGH TEMPERATURE MEASUREMENT

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Such rapid strides in the science of pyrometry have been made during the past decade that it may be of interest to review recent progress, particularly of the more practical applications. In order adequately to place the subject of recent advances before the non-specialist reader some elementary explanations as to the principles involved are necessary. The most familiar type of temperature measurer is the well-known ordinary thermometer, in which the general property of the expansion of a body by heat is utilised to obtain a scale of temperature. A bulb forming the containing reservoir is blown upon the end of a fine glass capillary tube of uniform cross-section. A liquid such as mercury or alcohol, having a much greater expansion than that of glass, is filled into the bulb, and the changes in volume of the liquid with temperature are indicated by its rise and fall in the capillary tube. If it be desired to compare the indications of the thermoscope thus obtained with those of other observers, it is necessary, in order to convert it into a thermometer, to adopt some definite "scale," preferably as near as possible the same for instruments of all kinds. For this purpose therefore at least two fixed points must be selected. The two universally chosen are the temperature of melting ice and the boiling-point of water under one atmosphere pressure.

On the Fahrenheit Scale of temperature in common use in this country, the ice-point is arbitrarily called 32° and the steam-point 212°, the interval between them being divided into 180 equal parts called degrees Fahrenheit. Similarly on the Centigrade Scale which is almost universally used for scientific and now very largely for technical work, the ice-point is called 0° and the steam-point 100°, the Centigrade degree being the $\frac{1}{100}$th part of this interval. Over this small range all substances, both solid and liquid, used in dilatation pyrometers expand fairly uniformly as the temperature rises, and between the same fixed points all give practically the same scale. When, however, observations are made at much higher temperatures it is found that different kinds of glass expand differently, hence two thermometers made of different glasses and both correct at 0° and 100° might differ very considerably at 300° C.

For this reason, therefore, it is desirable to refer all temperature measurements to some scale independent of the properties of the substance used to indicate it. The methods for the practical realisation of such an "absolute" scale have been shown by Lord Kelvin, but it is not necessary to enter upon them here. Many years ago it was found that nearly all gases, when sufficiently far from their liquefying points, expand nearly equally for equal rises of temperature. Thus for the mercury in the mercury thermometer we may substitute air, hydrogen or nitrogen, and measure temperature by measuring the changes of either pressure or volume of the gas thus confined. Owing to the larger coefficient of expansion of the gas as compared with the mercury, much greater sensitiveness may be obtained if required, and what is of more importance the expansion of the containing envelope, if its material be suitably chosen, may be made very small in comparison with that of the measuring gas. Hence uncertainties in the knowledge of this expansion become of much less account. A gas-thermometer has also the advantage of being applicable over a very wide range both in the downward and upward direction.

In 1887 the International Committee of Weights and Measures, meeting at Sèvres, fixed provisionally the Fundamental International Scale of Temperature, as defined in the following resolution :

> "That the International Committee of Weights and Measures adopt as the Normal Thermometric Scale . . . the Centigrade Scale of the Hydrogen Thermometer, having as fixed points the temperature of melting ice (0°) and that of the vapour of distilled water in ebullition (100°) under the normal atmospheric pressure, the hydrogen being taken under the manometric initial pressure of one metre of mercury. . . ."

This International Scale was founded on the classic work of Chappuis done at the Sèvres laboratory with the large gas-thermometer having a bulb of platinum-iridium of over a metre in length and a litre capacity. Chappuis found that the differences between the temperature scales given by hydrogen, air and nitrogen under various initial pressures could be considered as identical for all but the highest class work and that with hydrogen, at all events, the scale thus obtained was probably very close to Lord Kelvin's "absolute" scale. He made elaborate comparisons of the relation subsisting between the readings of a number of specially prepared mercury thermometers made of French hard-glass and the gas-scale. The behaviour of this kind of mercury thermometer had been the subject of long study, and the type was a great advance on any previously used. Over the range 0° to 100° the greatest difference between the French hard-glass mercury and the hydrogen scales occurred at 40°, at which point the mercury thermometer read higher than the hydrogen by ·112°.

Thermometers calibrated at the Sèvres laboratory under the direction of Drs. Benoit and Guillaume, provided with a system of correction tables and capable of being read to an accuracy of about ·002° C. were sent out with all the national metric standard measures of length and mass, and a number were also issued to science laboratories and kindred institutions. Thus the introduction of the International Scale was rendered possible in practice.

The large Sèvres gas-thermometer was not well adapted for work much above 100° C. and investigation showed that at temperatures considerably below 200° C. some action occurred between the bulb and the hydrogen it contained. A second gas-thermometer suitable for much higher temperatures was therefore erected. This was fitted with bulbs of either hard glass or porcelain. With this instrument it was also found impossible to use hydrogen for accurate observations at the higher temperatures, and therefore nitrogen or air was substituted for this gas; at high ranges these give a temperature scale only differing from that of hydrogen by an amount probably within the limits of experimental error. Observations with this gas-thermometer were made up to about 600° C.

Among the most important of the more modern investigations using the gas-thermometer are those of Callendar and of Callendar and Griffiths, who worked with a form of apparatus differing from those of Chappuis in several important points. From the time of Regnault onwards the practice seems to have become general to employ the constant-volume method of working. Callendar, however, introduced a form of constant-pressure thermometer, in which several of the most serious sources of difficulty in gas-thermometer measurements were notably diminished. In his instrument the pressure in the working bulb is adjusted by means of a delicate oil-gauge to be equal to that of a quantity of gas at 0° C. confined in a second bulb surrounded by melting ice. A system of capillary tubes similar in all respects to those connected with the measuring bulb, and exposed to the same changes of temperature serves to compensate exactly for the uncertain "dead-space" correction. An additional advantage is that the bulb in this method of working is not subjected to any pressure strains internal or external.

Callendar and Griffiths, using this type of thermometer and employing air as their measuring gas, made a determination of the boiling-point of sulphur and obtained the value 444·5° C. under normal pressure, 760 mm. of mercury.

This point is of special importance in high-temperature measurement, since it has been adopted as the upper fixed temperature of standardisation for the platinum resistance thermometer to be described later and on its accuracy the whole scale of the resistance-thermometer depends. This value 444·5° was exactly confirmed by the later investigations of Chappuis and Harker, who found for the same temperature on the constant volume nitrogen scale the value 444·7°, the difference between the two determinations being almost exactly that demanded by theory.

Before proceeding to discuss work at higher ranges a few further words regarding the mercury thermometer are necessary. The construction of mercury thermometers for high temperatures has undergone considerable modification of recent years. Investigations had shown that with almost all thermometers exposure to temperatures even as low as 100° C. causes zero changes and with some of the glasses in use these were of relatively large and very uncertain amount. In general the change produced was found to be made up of two parts, the one, a rise of zero, being permanent in its character, the other being a temporary depression followed by gradual recovery after a more or less prolonged period at ordinary temperature

The first of the two effects may be largely overcome by annealing the instrument for some time at a much higher temperature than that at which it is intended to be used, following this by a process of slow cooling to ordinary temperature. The internal strains set up in the glass during manufacture are thus relieved. Formerly it was not uncommon to find an ordinary "chemical" thermometer graduated to 360° C. show a permanent rise of 10° or even 20° after a brief exposure to 300° or 350°. Owing to the introduction of better glasses and improved methods of manufacture these large permanent changes are now much rarer and the temporary depressions much smaller than used formerly to be the case. Another important change in the modern method of constructing a mercury thermometer for use above 100° C. is that, instead of exhausting the space above the mercury, for work up to 350° C. this is now almost always filled with nitrogen at approximately atmospheric pressure. The pressure of the gas prevents the splitting up of the mercury column, which often occurs above 250° C., if the thermometer is vacuous. By the use of some of the newer refractory glasses composed of borosilicates thermometers are now constructed which will stand brief exposure to 550° C. or even 575° C. To raise the boiling-point of the mercury sufficiently, the filling of nitrogen or carbon dioxide gas is introduced under 16 atmospheres pressure. This type of thermometer, graduated usually into intervals of 2° or 5° C., is generally divided from about 180° C. to 550° C. and for checking purposes is provided in addition with either the freezing or boiling-point. The divisions instead of being approximately equal volumes throughout are made gradually shorter at the higher ranges to bring the readings into accord with the gas-scale. The use of these thermometers above about 480° C. is however not to be recommended, as any prolonged exposure (say an hour or two) even to 500° C. only, generally leads to the gradual softening of the bulb, which blows out under the considerable internal pressure.

In the use of high-range thermometers the fact is often lost sight of that if the whole of the mercury up to the reading be not subjected to the same temperature as the bulb very considerable errors may arise. In technical work very many disputes have been caused by lack of attention to this point. The amount by which a correctly divided thermometer would read low under these conditions may be roughly estimated from a knowledge of the number of degrees in the thermometer stem, which are not at bulb temperature, the average temperature of the exposed portion, and the apparent coefficient of expansion of mercury in the particular kind of glass used over the range in question. An approximate value for the latter at moderate temperatures is ·00016. The determination of the "stem correction" as it is called is, however, at best very uncertain, and if the extent of exposed stem be great and the temperature high, the correction may reach 30° C. Thus, for example, if the bulb temperature be 300° and the amount of stem exposed be 200° at an average temperature of 50° C., the correction would amount to 9° C. It will thus be seen that if a mercury thermometer be intended to be used only partly immersed, either it should be graduated to read correctly with some definite amount of exposed stem or a correction must be applied.

During the past few months experiments have been commenced with a view to the manufacture in England on a large scale of mercury thermometers in which fused silica is substituted for glass, the difficulties in the way of the construction of capillary tubes of reasonably uniform cross-section having been largely overcome. Owing to the well-known excellent properties of silica-glass at moderate temperatures these thermometers, if produced sufficiently cheaply, should present notable advantages.

Our knowledge of high temperatures has been greatly extended during the past ten years by a number of researches, among which may be mentioned those of Holborn and his associates working at the Reichsanstalt at Charlottenburg, of Day and his co-workers at the Geophysical Laboratory at Washington, also the work of the American Bureau of Standards at Washington and the National Physical Laboratory at Teddington. These and other workers have carried gas-thermometer measurements to much higher temperatures and by perfecting various forms of electric furnace to obtain the necessary uniformity of heating, have succeeded in reaching comparative agreement at temperatures up to 1,000°C., the upper limit of the experiments thus far made being 1,500°C.

Although the gas-thermometer, as representing as nearly as possible the absolute scale, has been accepted by general consent as the ultimate standard of temperature measurement, to use it successfully even at moderate temperatures is extremely difficult. Much labour has therefore been spent on the evolution of suitable appliances to serve as "working" rather than "primary" standards. Of these the most successful and easily applicable for moderate ranges are the electrical methods of temperature measurement, the first of these being the method of the electrical resistance thermometer. This depends for its application on the fact that the resistance of most conductors made of pure metals changes rapidly with the change of temperature. For the higher temperatures the refractory character, ductility and general properties of platinum first used by Siemens have caused it to be specially selected for this purpose and in most resistance methods this metal is now used.

The platinum thermometer has the great advantage that, provided the wire used for the "bulb" be comparatively pure, standardisation at three points only is required to establish a temperature scale serving for measurements up to 1,000° C. If the symbol *pt* be employed to denote any temperature on the Centigrade platinum scale, and $R$, $R_{100}$ and $R_0$ are the observed resistances of the thermometer at the temperatures *pt*, 100° and 0° respectively,

$$pt = \frac{R - R_0}{R_{100} - R_0} \times 100.$$

To reduce temperatures on the platinum-scale to the gas-scale the relation between T and *pt* must be known. Callendar showed that up to 600° C. their difference could be expressed by the equation

$$T - pt = \delta\left[\left(\frac{T}{100}\right)^2 - \frac{T}{100}\right]$$

the value of $\delta$ for most pure wires being approximately 1·5. Following his suggestion platinum thermometers are now almost universally standardised by determination of their resistance in ice, steam and sulphur vapour.

Platinum thermometers having a protector tube of glazed porcelain may be employed up to 1,100° or 1,200° C. for intermittent, and to 800° or 900° C. for continuous work without serious changes taking place in the wire, provided the instrument has before use been subjected to a thorough annealing process. Care should also be taken that the protecting sheath be quite gas-tight, to preserve the platinum from the reducing action of furnace gases. Vapours of metals, such as iron, which in an oxidising atmosphere has an appreciable tension at 1,000° C., rapidly destroy the platinum metals and should be particularly guarded against.

Recent improvements in the apparatus used for resistance measurements, and in particular the introduction for use in resistance coils of metals having practically no temperature coefficient, have rendered possible the design of portable commercial forms of platinum pyrometer outfit and capable of giving high accuracy. One of these forms is made direct reading on the gas-scale, if required; when arranged for use up to a maximum temperature of 1,200° it can be set to $\frac{1}{4}$° C. Such pyrometers are eminently suitable for the heat treatment of steel over the range 700° to 900° C., where, in some kinds of commercial practice, a difference of 5° C. in the hardening temperature makes a quite perceptible difference in the nature of the product obtained.

Recently a new form of platinum thermometer has been introduced, in which the wire, instead of being wound on to a mica cross, is wrapped upon a tube of clear, fused silica, a second silica tube being shrunk over this for mechanical protection. Thermometers of this type may be constructed so that their lag is very small—a matter of importance in some cases. It has been found, however, that at high temperatures fused silica devitrifies slowly, reverting to its crystalline form of tridymite, and that it is therefore not desirable to submit any pyrometers in which this material is employed to temperatures above about 900° C. except for short periods.

Platinum thermometers have the advantage that they can easily be arranged to give a continuous record, and for purposes where automatic temperature registration is desirable over long periods they are extensively used. For records of the temperature of the hot blast for iron furnaces, and for flue and steam temperatures, they are probably the most suitable instruments to employ. For use in scientific work as in practical standards, they present the great advantage over thermocouples that the same instrument may be used, if required, for a range of from 0° to 1,100° C. with approximately constant sensitiveness; also that it is easy to design instruments, whose size and shape, resistance and sensitiveness are best adapted to the particular purpose in view.

The second electric method of temperature measurement is founded upon the fact that, in a circuit composed of two different metals, if one of the junctions is heated, a thermo-electric current is produced dependent on the temperature difference set up. The chief advantages of the method over that of the resistance thermometer are that if suitable metals are used it is applicable up to considerably higher temperatures; also that it is possible by its means to measure the temperature practically at a point, instead of the average temperature over the space occupied by the "bulb" of a resistance thermometer, which may be any length up to 4 or 5 inches. The metals generally employed for work at high ranges are those of the platinum group, two of the most useful couples being formed of a wire of pure platinum on one side against an alloy of platinum with 10 per cent. of iridium or rhodium. The electromotive force given by these couples is roughly fifteen microvolts[1] per degree C. for the iridium and ten for the rhodium. Wires from ·3 to ·6 mm. diameter are usually employed.

The rhodium couple may be used with success in technical work up to about 1,300° C., if well protected from the action of furnace gases and the iridium couple to about 1,000° C. The glaze of the porcelain outer tubes is, however, rapidly destroyed at temperatures beyond 1200° C., and most kinds of glaze are viscous at about 1,150° C. Objections have been urged that, owing to the volatility of iridium, its use in a thermocouple is not advisable. Experience in the use of many kinds of thermocouples has shown, however, that over the range of temperatures employed for the heat treatment of iron and steel round 800° C., there is little difference in the durability of the two alloys, though the rhodium alloy should always be used, if continuous work above 1,000° C. is intended.

For ordinary purposes the readings of the electromotive force of couples are made by connecting the terminals of the couple directly to some form of galvanometer and in nearly all cases the type of instrument employed is the moving-coil galvanometer in which a small suspended rectangular coil, carrying the whole current, moves in the field of a strong permanent magnet. This type is chosen on account of its freedom from external magnetic disturbance and its constancy of sensitiveness. The readings are taken either by means of

[1] One microvolt = one-millionth of a volt.

a pointer moving over a graduated scale or by the familiar "light-spot" method of the physical laboratory invented by Lord Kelvin. Owing to the fact that the electromotive force given by these couples is comparatively small it was not found practicable till the last few years to make a good portable galvanometer on which the temperature readings could be taken. Now, however, there are on the market several types of commercial instrument which are amply sensitive without the use of the light-spot and are free from zero-drift. It is possible with care with such an instrument of the better class to measure moderate temperatures to about 1° C., if the temperature of the "cold junctions"—that is, the point where the two wires of the couple itself are joined to the copper connecting-leads—be known to a sufficient degree of accuracy. Unless the temperature coefficient of the galvanometer employed be sufficiently small and its resistance sufficiently high, resistance changes in the circuit also affect the result, generally in a somewhat uncertain manner; hence for the highest class of work the directly deflected galvanometer is replaced by a suitable potentiometer, in which part or the whole of the electromotive force generated by the couple is balanced. On such an instrument, if correctly designed, it is possible with ease to follow temperature changes to ·05° C. during the freezing of a pure silver crucible in an electric furnace or to measure the melting-point of platinum on the thermo-electric scale to within 1° C.

One of the most important recent improvements in thermoelectric pyrometry has been due to the fact that two firms have succeeded in producing large homogeneous ingots of platinum and of the 10-per-cent. alloys used for thermocouple wires in a state of high commercial purity, and that thus it is possible to substitute for any couples others giving the same constants. This saves much labour in the recalibration of instruments.

Base metal couples of various kinds have been introduced for work at lower temperatures. Thus for temperatures to 800° C. the alloy of copper and nickel, known as eureka or constantan, may be used against a wire of soft copper or iron. The sensitiveness of the latter combination is roughly 50 microvolts per degree C., increasing slightly with rise of temperature.

During the past decade very great progress has been made in the development of some entirely new methods of temperature measurement, and pyrometers for which considerable accuracy is claimed are now available for the determination of the highest obtainable terrestrial temperatures, such as that of the crater of the electric arc. These depend on measurements of the radiation given out by the hot body, from which its temperature can be estimated. A convention has arisen by which these pyrometers using visible light only are spoken of as "optical" pyrometers, and those in which the whole heat spectrum is concerned as "radiation" pyrometers. Both types have the advantage over the resistance pyrometer and thermocouple, that no part of the instrument itself is subjected to the destructive action of the high temperature.

The intensity of the radiation emitted by a hot body increases enormously with rise of temperature, the amount of the increase varying with the wave-length. An incandescent body at 2,000° C. emits more than 2,000 times as much red light per unit area as it does at 1,000° C.: hence it would appear that a photometric measurement of the light evolved from any very hot substance should be a sensitive and easy way to measure its temperature. If at a given temperature all substances emitted the same amount of light, this would certainly be the case; but it has been found that, under ordinary conditions, the nature of the radiating body, especially of its surface, greatly affects the amount of radiation sent out. Carbon or iron emit per unit area considerably more light and heat than incandescent platinum or molten copper at the same temperature. The work of Stewart, Kirchoff, Wien and others has shown, however, that if substances of varying emissivity are placed inside a uniformly heated enclosure and looked at through a small aperture in its wall, the amount of radiation sent out under these conditions is independent of the substance and is a function only of the temperature. Such an enclosure is called by Kirchoff a "black body."

The relation between the temperature and radiation from a black body has been accurately studied, and it has been found that the total energy radiated per unit area is proportional to the fourth power of the absolute temperature.[1] Thus if $Q$ be

[1] The "absolute" temperature of a body is its temperature as measured not from that of melting ice, but from "absolute" zero. On the Centigrade Scale this point is − 273° C.; hence absolute temperatures are obtained by addition of 273° to the Centigrade temperature.

the quantity of heat radiation, $T$ and $T_0$ the absolute temperatures of emitter and receiver, and $k$ a constant,

$$Q = k(T^4 - T_0^4).$$

When $T_0$ is atmospheric temperature and $(T - T_0)$ exceeds say 500° C., $T_0^4$ becomes practically negligible in comparison to $T^4$, and $T$ becomes $\sqrt[4]{\frac{Q}{k}}$.

In order to make any radiation or optical pyrometer give correct readings, it is necessary that the substance whose temperature is to be measured should either be one by nature approximately "black," like carbon, or that it should be placed inside an enclosure in which it radiates under "black-body" conditions. In practice the approximation to "blackness" of many furnaces whose temperatures are to be measured is very close. If it is undesirable to have an opening in the furnace, a long tube closed at one end and made of iron, fireclay, or other refractory material may be built into the furnace wall, and readings are then taken by sighting on the bottom of this tube. This arrangement, while giving a good approach to "black-body" radiation, also serves to keep any flames which might be present from exerting disturbing effects on the readings obtained.

One of the most important of the new forms of instrument is the Total Radiation Pyrometer of Féry, which depends for its action on the law just enunciated. This pyrometer is none other than the instrument used by the late Lord Rosse in his researches on lunar radiation, modified to a smaller form adapted for the purposes of temperature measurement. It consists of a reflecting telescope of short focal length, which is sighted upon the hot object. The radiation received is concentrated by a gilt concave mirror upon the junction of a very minute and sensitive thermocouple placed at the focus. The terminals of the couple are connected to a millivoltmeter, which may be graduated to read directly the temperature of the body on which the telescope is pointed. The usual types of the instrument are graduated from 500° C. upwards and the sensitiveness becomes greater the higher the temperature, differences of about 2° being measurable at the higher ranges. To obtain this sensitiveness, however, the range of any one pattern is usually limited to 700 or 800° from the first graduation. For the millivoltmeter, if desired, a recording arrangement can be substituted by which a continuous trace can be obtained. This may, if required, be at some distance from the furnace.

A simpler form of the Féry instrument is now made, in which for the thermocouple and registering galvanometer is substituted a very small bimetallic spiral actuating a long aluminium pointer, moving over a scale graduated directly in temperature degrees. Both of these instruments are arranged to give readings independently of their distance from the emitting source, provided the latter is large enough to give in the instrument an image of sufficient size to cover the receiving disc. The readings are independent of the personal element introduced by the observer, no question of individual judgment as to equality of brightness or colour-matching being involved. On the other hand the disadvantages of the total radiation instrument are that it requires a larger area of uniform temperature upon which to focus than the other types of optical pyrometer, and that no glass, mica or other absorbing screen may be interposed in the path of the rays, unless special arrangement be made to determine for each temperature the considerable absorption thus caused.

Other total radiation pyrometers are those of Thwing and of Foster. Though the details in these are quite different, they are both constructed on the same principle as the electrical type of Féry instrument just described, but are somewhat simpler and cheaper, no sighting or focussing device being considered necessary.

Another and older type of optical pyrometer is the Le Chatelier in its original form, and as modified by Féry, the latter being known as the Féry Absorption Pyrometer. In these instruments and also in that of Wanner only light of one wave-length is used for pyrometric measurements. All three are photometers, in which comparisons are made of the intensity of the red light coming from the hot body, whose temperature is to be measured, with that from a standard lamp of some form or other. A system of lenses forming a telescope of low magnifying-power is used and the measurement consists in adjusting to equality two patches of light, which appear simultaneously in the field. In the Le Chatelier instrument, this adjustment is made by altering the size of an iris diaphragm ; in the Féry by sliding past one another two graduated absorbing wedges. In both these instruments a real image of the object whose temperature is to be measured is formed by the telescope; the comparison source is a standard lamp, in which amylic acetate is burnt in a flame of stated dimensions. In the newer forms of the latter instrument three overlapping temperature scales are usually provided, the lowest temperature on the first scale being 800 to 900° C. and the highest measurable about 4,000° C. In the Wanner instrument the comparisons are made by adjusting to equal brightness the two similar halves of a circular disc of light one part of which is illuminated from the object sighted upon, and the other from a small electric lamp attached to the instrument. The brightness of the lamp, which is maintained incandescent from a portable 4-volt accumulator, is previously adjusted to the desired amount by comparison with an amylic acetate standard flame. The Wanner instrument differs from the other two in that it is not an ordinary telescope, but a straight-vision spectrophotometer and that no real image of the object on which it is sighted is formed by the optical system. The chief advantages of these instruments over the total radiation pyrometer are that they can be used on smaller objects; if required readings can be taken through an interposed window, with very little alteration in the results obtained. The Féry absorption type can be employed for the determination of the temperature of the electric arc or even of an incandescent lamp-filament.

Another type of optical pyrometer much used for certain purposes is that known in America as the Morse and in Germany as the Holborn-Kurlbaum instrument. In the latter of these a small incandescent lamp with a plain horseshoe filament is placed at the focal-point of a short-focus telescope, sighted upon the object whose temperature is to be measured. The current through the lamp is supplied from a small portable accumulator and is adjusted by a rheostat so that the bend of the filament appears equally bright with the hot background, and at this moment becomes indistinguishable from it. The magnitude of the current is then read off on a suitable deflection-ammeter, and reference to a table gives the required temperature. In the commercial forms of instrument a direct-reading temperature scale may be provided in addition to the readings of current on the ammeter, if desired. This type of instrument may be used from about 600° C. upwards and gives good results. The better forms are provided with several of the celebrated incandescent lamps, some of which can be reserved as reference standards, and used to check constancy of the relation between temperature and current in the working lamps. For measurement of temperatures above 1,400° C. a system of mirrors forming a weakening-device is placed in the path of the light in front of the object-glass of the instrument, and by this means a second scale extending from about 1,200° C. to 2,200° C. is obtained, without risk of damage of the working lamps by over-running. This type of instrument is perhaps a little easier to adjust than the others, and can be used on a small object. It and the Wanner however require accumulators, which are unnecessary with the other patterns. None of these types of "optical" as distinct from the "total radiation" pyrometer can be made to record nor can their indications conveniently be transmitted to a distance. Moreover they all involve a personal element in the setting, which is not present with the "total radiation" pyrometer.

Of the remaining available methods of measuring temperature only two need be mentioned, the calorimetric method of Siemens and that depending on the use of fusible materials as indicators, such as the well-known "cones" of Seger and the Watkin "recorders."

The calorimetric method involves the use of a cylinder of iron, copper, nickel or other metal, which is heated in the furnace whose temperature is to be measured, and then dropped quickly into a calorimeter containing a definite amount of water. The rise of temperature thus produced in the water is indicated by a thermometer; by a simple device involving a sliding-scale, the temperature to which the specimen of metal was heated is obtained without calculation. This method of measurement, introduced by Sir William Siemens about forty years ago, is still used successfully in many industrial processes. Its practical upper working limit is however not much above 1,000° C.

Seger cones and other similar heat-recorders are used largely in pottery works, where the temperature distribution over wide areas in a large furnace needs controlling at a number of points. Usually three to six different samples of refractory mixtures of definite melting-points are simultaneously exposed; from a subsequent examination of these after withdrawal from the furnace the temperature is deduced. The method is capable of giving results sufficiently accurate for many purposes, but variations in the time of exposure to the high temperature and also in the rate of heating make considerable differences in the results.

For measurements in the region above the range of the gas-thermometer (say 1,200° C.) some extrapolation scale of temperature is at present the only available provisional standard. Two chief methods have up to now been employed in the establishment of such a scale. The first—the thermo-electric method—depends on the hypothesis that the formula, which represents the relation between electromotive force and temperature in a thermo-couple up to 1,200° C., holds at higher ranges. The work of the National Physical Laboratory has shown that a formula of the usual parabolic type, which represents this relation very closely for almost any thermo-couple formed of the platinum metals, gives a consistent extrapolation scale up to the melting-point of platinum. On this "thermo-electric scale" the melting-point of nickel is 1,427° C., that of pure iron 1,502° C., and that of platinum 1,710° C. It is to temperatures on this scale that the readings of couples are almost always referred.

The second extrapolation scale is based on the assumption of a definite value for the so-called constant in the Wien-Planck equation, expressing the relation between intensity of radiation and temperature. The usually accepted value for this constant is about 14,500, but it is still appreciably uncertain. On this optical scale, which should agree with the thermo-electric and gas-scales at 1,200°, the melting-point of nickel would become about 1,450° C. and that of platinum 1,750° C., whilst the "black-body" temperature of the crater of the electric arc, which is nearly independent of the current and voltage employed, is about 3,500° C.

A word in conclusion may be devoted to the standardisation of the various types of instruments which have been mentioned. This can be accomplished in this country at the National Physical Laboratory, which was opened at Teddington in 1902. Into the methods by which these standardisations are carried out, it is beyond the province of this article to enter. During the past eight years it has been found possible at that institution gradually to extend the scope and range of such standardisations so that methods and apparatus are now available for tests on practically all types of instruments. Mercury thermometers may be 2 inches or 6 feet long, 0·2 to 2 inches in diameter, their scale may embrace only ½° C. in all, or 700° C., may be divided so that 1 mm. = ·01° C. or 10° C., may be intended for an immersion of two inches or five feet; hence apparatus of the greatest variety is necessary for these alone, and in almost all forms of instruments similar wide variations of pattern have to be arranged for. For thermometer standardisation a series of baths is provided, in which are stirred liquids heated electrically, available for temperatures up to 700° C. For thermo-couple, resistance, radiation and optical pyrometers electric furnaces of very varied types have been designed, those for moderate temperatures up to 1,400° C. being wound with nickel or platinum. Above that point special forms of electric furnace are used, in which the resistor is made of carbon or graphite. In these furnaces controllable steady temperatures up to about 3,000° C., uniform over considerable areas, may be obtained, while for attainment of the highest terrestrial temperatures arc-furnaces are employed.

The following is a table of the values of a number of fixed points employed in pyrometric work:

| | Deg. Cent. | |
|---|---|---|
| Freezing-point of tin | 231·9 | |
| „ „ cadmium | 321·0 | |
| „ „ lead | 327·4 | |
| „ „ zinc | 419·0 | |
| „ „ antimony | 631·0 | |
| „ „ aluminium | 657·0 | |
| „ „ silver | 961·0 | (in reducing atmosphere) |
| „ „ copper | 1083·0 | „ „ „ |
| „ „ gold | 1082·0 | |
| „ „ nickel | 1417·0 | (thermo-electric) 1450·0 (opt.) |
| „ „ platinum | 1710·0 | ( „ ) 1750·0 (opt.) |

# REVIEWS

**The Fats.** By J. B. LEATHES, M.A., M.B., F.R.C.S. Monographs on Biochemistry. Edited by R. ADERS PLIMMER and F. G. HOPKINS. (London: Longmans, Green & Co., 1910. Price 4*s*.)

IN somewhat doubtful English we are told, in the opening sentence of the General Preface to this series of Monographs on Biochemistry, that the subject "is enlarging its borders" to such an extent that no single text-book "can adequately deal with it as a whole." This undoubtedly is true: monographs such as have been projected should therefore be of utmost use to earnest students; unfortunately the editors have not yet been very successful in accomplishing the object they have in view—the books issued are not all readable and interesting to the extent that is desirable. The latest addition to the series appears to us to be one of the least successful—to speak plainly, it is a careless production, disappointing particularly because the author gives us far too little of himself. No one is more at home with the fats and yet he brings us into close quarters with them and tells us things of real consequence from a biochemical standpoint only in the last brief chapter of the book.

In his few lines of preface, Dr. Leathes says that the field of biochemical work to which the book refers needs workers who may be either physiologists who have "trained themselves chemically or chemists who are alive to the legitimate aspirations of biology." We fear the former will gather scant chemical comfort from the book; as to the latter, they will have considerable difficulty in arriving at any clear understanding of what may be "legitimate aspirations of biology," owing to the cryptic character of the language in which much of the information is conveyed. On reading the last chapter, it is impossible not to be struck by the extent to which the descriptions given are rendered obscure by loose expressions and the too frequent use of the "jargonese" in which physiologists perhaps more than any other class of scientific worker are prone to indulge. Neither chemist nor biologist will gain much from the slim account of the composition of the fats given in chapter i.; moreover, it is full of unessential things and unsystematic; not the slightest reference is made to Haller's all-important work on the etherisation of fatty acids by alcoholysis of fats. Chapters ii. and iii., in which the extraction and estimation of fat and the character and properties of fats are considered, although all too short, are of more value, as they contain many practical hints derived from the author's wide experience: they are marred, however, by slovenly writing. The following is a choice example taken from a page full of verbiage: "The flask is heated on an asbestos board 12 cm. in diameter, with an opening in its centre 5 cm. in diameter, with a small flame, till the insoluble acids are melted, and then more strongly." If an author, two editors and a printer's reader cannot do better than this, they should give up book-making. The effect on students of such construction, of such punctuation, is disastrous—how are they to form their style with examples of this kind before them?

Chapter iv. on the physiology of fats is the really important section of the volume and all Oliver Twist's feelings of desire for more come over us when we read it. Had this last chapter been expanded into a complete essay, so as to make clear the many issues of biological importance connected with fats, a really useful monograph would have been produced.

Chemists as well as biologists, at the present time, need a thorough book on the chemistry of fats—one in which the fats are fully and methodically treated, with feeling, from the chemical standpoint but with due reference also to their biological significance. The accounts given in the current text-books are meagre and of little practical value, whilst the pretentious special works fail to afford the aid that is required on most essential points, being written apparently to show the author's learning and so that a very safe margin is always left to be dispensed in consultation. There is no book in which the vast mass of information on fats in the possession of botanists and physiologists is properly codified and discussed. It is time that fatty substances were once more brought into fashion among chemists: of the three great groups of compounds of primary physiological importance—the albuminoids, the carbohydrates and (*pace* the author) the lipoids—the last is by no means less interesting than the others; innumerable problems in connection with their occurrence, their genesis, their utilisation, call for prolonged and intimate study and workers are much needed in this field. The medical schools should furnish them; Dr. Leathes, in the past, has set a splendid example which his colleagues might well follow. One of the tasks of the University Commission now sitting should be to galvanise chemistry into being in the Medical Schools and so secure for the science the recognition due to it as *the* vital science: it is in no way surprising that students of medicine have but scant belief in the usefulness of the subject when they see so little use being made of the golden opportunities the hospitals afford. At present the poor student is left to pick up knowledge of method as best he may—logic comes nowhere in his scientific studies; as a rule he cannot even sniff the atmosphere of research: if more were done to make him familiar with the arguments on which conclusions have been based, he would at least know that dogma counts for little in practice. The work of Lawes and Gilbert and the other pugilists who have contended over fat might well have been put before students in considerable detail from this point of view in the book under notice—if such subjects had been discussed properly, we should have had a monograph of value to students at large on the biological significance of fats.

**Sketch of a Course of Chemical Philosophy.** By STANISLAO CANNIZZARO (1858). Alembic Club Reprints, No. 18. [Pp. 55.] (London Agents: Simpkin, Marshall, Hamilton, Kent & Co., Ltd. 1910. Price 1*s.* 6*d.*)

IN concluding his notice of Cannizzaro in a previous number of this Journal (July 1910, No. 17, pp. 147–60), Mr. Muir expressed the opinion that it was high time that an English translation were made of the great Italian's course of chemical philosophy. The want to which he called attention has long been felt; it is now met by the publication of the famous letter by the Alembic Club, as No. 18 of their valuable series of reprints. Permission to publish the translation was received from the venerable chemist only a few days before his death. The Club is to be congratulated on the special excellence of the translation; it is also to be thanked most warmly on behalf of chemists and physicists generally for having placed so valuable a document at public disposal. As the translator remarks in his brief preface: "The facts are marshalled and their bearing explained with absolute mastery of pedagogic method and one is compelled to the conclusion that Cannizzaro's students of 1858 must have had clearer conceptions of chemical theory than most of his colleagues of a much later date"—even than a majority of students of physical science of to-day, it may be surmised. So fundamental a matter as the determination of molecular weight finds no place in our text-books on physics and those on chemistry usually deal with it in a perfunctory manner. Cooke's *New Chemistry*, in the International Science Series—now a book of some antiquity—is almost the only work that does justice to the subject. Now there can be no excuse for the neglect of Avagadro's theorem and every self-respecting student of molecular theory should take pleasure in mastering Cannizzaro's essay, on account of the subject-matter and also because it is so brilliant an example of pedagogic method.

The letter is full of noteworthy paragraphs to which attention might well be specially directed : none is more important than the following :—

" In order to lead my students to the conviction which I have reached myself, I wish to place them on the same path as that by which I have arrived at it—the path, that is, of the historical examination of chemical theories."

And it may be added, of chemical discoveries ; indeed, if students of chemistry regularly followed the path indicated by Cannizzaro, they would have some chance of acquiring feeling as chemists ; a careful study of his memoir should lead many to appreciate the value of logical statement and to regret the frequency of its absence from modern writings.

**Die Pflanzenproteine.** Von THOMAS B. OSBORNE, New Haven, Conn., U.S.A., Deutsche Übertragung von FRAU HELENE SCHLESINGER, Wien. (Sonderabdruck aus : Ergebnisse der Physiologie, herausgegeben von L. ASHER und K. SPIRO, X. Jahrgang, S. 47-215.) [Pp. 169.] (Wiesbaden : Verlag von J. F. Bergmann, 1910.)

TO all who know what vegetable proteins are, it is needless to say that the name of Osborne stands pre-eminent among workers with this enigmatical class of substances : during the nineties and the years of the present century, a regular flow of communications has issued from his modest laboratory in New Haven, Connecticut, which have gained for him a world-wide reputation as the authority on such matters. At first he was engaged in isolating and characterising proteins ; of late years he has applied himself to the wearisome task of studying as fully and exactly as possible the products of their hydrolysis and his results now rank in advance of those of all other workers in the field. The present account is to be read in conjunction with the author's contribution under the same title to Plimmer and Hopkins' series of Biochemical Monographs (Longmans, Green & Co.) and with the chapter he has recently contributed to Abderhalden's *Biochemische Arbeitsmethoden*. Between the present time and the appearance of Ritthausen's classic work on Albuminous Substances in 1872, no attempt has been made to give a comprehensive account of our knowledge of vegetable albuminoids : the appearance of the memoir is therefore more than welcome and most opportune. It is impossible to criticise such a book—those who know the writer's work and who read through its pages cannot but feel that they are in the hands of the surest guide of the time in a region of indescribable complexity. The advance of knowledge recorded is very satisfactory. In Liebig's day only four distinct vegetable proteins were recognised ; the number was increased to ten in Ritthausen's time ; now it is not possible to set a limit to the number obtainable from seeds. Latterly Osborne has much improved the method of dissection by etherifying the mixture of amino-acids in presence of zinc chloride at 110° ; the result is a considerable increase in the proportion separated of some of the constituents. It is remarkable how large a percentage of the constituents of many proteins has now been accounted for ; in the case of maize Zein, for example, amino-compounds amount ing to as much as 80 per cent. of the weight of the protein hydrolysed have been isolated. The results obtained in the case of wheat Gliadin, maize Zein, pea Legumin and hemp Edestin may be quoted as illustrations of the remarkable differences in composition that proteins present :—

| | Gliadin. | Zein. | Legumin. | Edestin. |
|---|---|---|---|---|
| Glycin . . . . . . | 0·00 | 0·00 | 0·38 | 3·80 |
| Alanin . . . . . . | 2·00 | 8·98 | 2·08 | 3·60 |
| Valin . . . . . . . | 0·21 | | ? | 6·20 |
| Leucin . . . . . . | 5·61 | 17·95 | 8·00 | 14·50 |
| Prolin . . . . . . | 7·06 | 9·01 | 3·22 | 1·70 |
| Aspartic acid . . . . | 0·58 | 1·73 | 5·30 | 4·50 |
| Glutamic acid . . . . | 37·33 | 26·17 | 16·97 | 14·50 |
| Serin . . . . . . . | 0·13 | 1·00 | 0·53 | 0·33 |
| Tyrosin . . . . . . | 1·20 | 3·55 | 1·55 | 2·13 |
| Cystin . . . . . . | 0·45 | — | — | 1·00 |
| Lysin . . . . . . . | 0·00 | 0·00 | 4·98 | 1·65 |
| Histidin . . . . . . | 0·61 | 0·82 | 1·69 | 2·19 |
| Arginin . . . . . . | 3·16 | 1·35 | 11·71 | 14·17 |
| Ammonia . . . . . . | 5·11 | 3·64 | 2·05 | 2·28 |
| Tryptophane . . . . | 2·35 | 0·00 | — | — |
| Phenylalanin . . . . | — | 6·23 | 3·75 | 2·40 |
| Oxyprolin . . . . . | — | — | — | 2·00 |

Such are the stuffs of which men in large measure are made—and cobwebs too—and most things that are the outcome of vital agency; and it is probably because of the infinity of permutations and combinations that can be effected with such building materials that an infinity of forms can arise, although the type cannot be greatly varied.

The one complaint to be made against the book is that it is far too short—and English readers will regret that it is not issued in English : it is to be hoped that it will be shortly.

**Qualitative Chemical Analysis: Organic and Inorganic.** Third edition. By F. MOLLWO PERKIN. [Pp. xii + 337.] (London : Longmans, Green & Co., 1910. Price 4*s.* 6*d.*)

So long as the Board of Education frame their syllabus to include chemical analysis, so long teachers in Schools and Polytechnics will recommend books which are considered to be suitable as a preparation for the Board's examination. Therein lies the secret of the success of most of the books of this class. The excuse for adding one more to the already large array of text-books on Qualitative Chemical Analysis appears to be justified when the third edition is reached. Dr. Perkin has apparently produced a book well adapted to the requirements of the Board of Education, and evidently he is keeping his eyes open for changes in the syllabus.

Admirable as this alertness is, it should not be forgotten that the issue of a new edition is the opportunity for modifying any views previously advanced, or any theoretical conceptions which have subsequently been shown to be faulty.

Since the publication of the first edition in 1901, much has been done to show that the statement on p. 15—"hydrolysis is a secondary change due to ionisation," is not true.

It seems unnecessary to make particular mention that when compiling the book, the author consulted the works of Ostwald ; this fact is so extraordinarily patent.

After almost avoiding the old story in explanation of the colour changes of indicators—charging the ions with it—we find on p. 20 the student is advised to consult headquarters for further information. Surely the work of the past ten years is also worthy of the student's attention.

On this page is also to be found the following delightful explanatory statement: " Many colour changes are due to the conversion of one ion into another. Thus the monovalent permanganate anion is a deep purple, while that of the divalent $Mn''$ cation is very light pink. Hence, when the permanganate $MnO'_4$ is reduced to the divalent $Mn''$ cation by the action of reducing agents, a striking loss of colour follows."

From the student's point of view great exception must always be taken to such dogmatic statements as this. Apart from its being illogical to make use of circumgyratory argument, to fling such paragraphs at any student can only lead to disastrous results. No matter what the subject dealt with may be, the educational value of this kind of teaching is nil.

Apart from these aspects, bearing in mind the modest size of the volume, a surprising store of very useful information is to be found amongst its pages; both as regards the behaviour of simple substances and of mixtures towards chemical agents.

It would be unusual, but nevertheless refreshing, to find in books of this description some statement to the effect that although the tables of separation they contain are those generally employed, other schemes of separation are not only possible but are sometimes used. There seems to be a growing idea that tables of separation are akin to the laws of the Medes and Persians: that there is no alternative scheme of procedure: that magnesium, for example, is never precipitated in the wrong group, and so forth. The time devoted to this branch of chemical training is generally so short that a scheme of separation cannot be worked out individually, but a short clear statement of the facts of the case would serve a very useful purpose.

The chapter describing the properties of some of the rarer elements forms quite a good addition to the book.

As a bench companion, Dr. Perkins' book cannot fail to be of great assistance to those who engage in qualitative analysis. J. V. EYRE.

**Physical Chemistry, its bearing on Biology and Medicine.** By J. C. PHILIP, M.A., PH.D., D.SC. [Pp. ii + 312.] (London: Edward Arnold, 1910. Price 7*s*. 6*d*. net.)

THIS work, as its name implies, is intended more particularly as an introduction to Physical Chemistry in its application to the chemistry of vital processes. Whilst a great part of the book, of necessity, is pure physical chemistry, those parts which deal with the biochemical aspect of the subject are by no means valueless to the student of general chemistry: in fact, they might be read with advantage as an introduction to a broader view of the subject that would temper the purely physical attitude so commonly adopted, which does not appeal to the average student of chemistry. It is questionable whether the ordinary medical student would derive much benefit from such a book or is intended to; but it should be useful to those who wish to attain to more than the smattering of biochemistry required for examination purposes, to whom a knowledge of the main principles of physical chemistry is invaluable if not essential. The elements of the subject are treated of at some length and most of the phenomena are gone into sufficiently to enable the student to become further acquainted with them comparatively easily. Two chapters are devoted to colloidal solutions and a third to absorption in relation to colloids. A considerable number of references, including practically all the papers from which examples have been taken, are given in footnotes.

The book possesses an advantage over the majority of elementary books in that no attempt is made to put forward a complex subject in a manner which would suggest to a student that a simple explanation of all the phenomena can be given; nor does the author insist so strongly on particular theories as is usual. But in expounding a subject in this way, very great care must be exercised. Thus, in dealing with osmosis, after leading up to osmotic pressure from the analogy between diffusion in gases and solutions, the author passes to the conception of osmosis as the result of attraction between solvent and solute in a way which might easily confuse a student unacquainted with the subject. It would perhaps have been advisable to insert a paragraph, such as the second on page 39, at an earlier part of the chapter.

In the interest of students, protest should be made at the cost of the book; it is essentially a book for students, not for the library shelf: therefore it should be sold at a price to suit the student's pocket. D. C.

**Fossil Plants.** A Textbook for Students of Botany and Geology, Vol. II. By A. C. SEWARD, M.A., F.R.S. [Pp. xxi + 623, with 265 illustrations.] (Cambridge: University Press, 1910. Price 15*s*. net.)

DURING the last few years the shelves of students of Palæobotany have been enriched by three notable books by British botanists, which are so diverse in their aim and in the method of treatment, though covering much of the same ground, that they will all three be indispensable to serious students of the subject and complementary one of the other. Beside Bower's *Origin of a Land Flora*, and Scott's *Studies in Fossil Botany*, Seward's second volume of *Fossil Plants* will now take its place, and without doubt will appeal to and be appreciated alike by students of botany and of geology.

Though twelve years have elapsed since the appearance of the first volume of *Fossil Plants*, the unavoidable delay which has occurred in publishing the second volume cannot be said to have been of any disadvantage, as it has enabled the author to embody in his later volume many fundamental changes in our conception of the relationship of fossil plants, changes which have been occasioned by the rapid progress of the study of Palæobotany during the last decade. Luckily comparatively few additions of importance have been made to the subject-matter of vol. i.; on the other hand the advances in our knowledge of the structure of the extinct members of the Lycopodiales, the Filicales, and the Gymnosperms have been so considerable that Prof. Seward has found himself compelled to expand his projected second volume into two volumes, of which the one under review contains an account of the Sphenophyllales, the Lycopodiales and the Filicales, together with some plants of doubtful affinity and some members of the Pteridospermæ, that new group of plants the recognition of which is due to the careful investigations of Oliver and Scott into the seed-bearing ferns of the Carboniferous period.

Sphenophyllum, of which the vegetative organs were described in vol. i., is taken up again in vol. ii.; this commences with an account of the structure of the cone of this plant, of which we know a good deal more by the discovery in 1905 of *Sphenophyllostachys fertilis*. Prof. Seward is therefore able to rediscuss the affinities of the Sphenophyllales in the light of the most recent investigations. The interesting fossil, Cheirostrobus, combining some features of the Equisetales with those of the Lycopodiales and obviously closely related to Sphenophyllum, is critically examined and placed in a suborder of Cheirostrobæ, which with the Sphenophylleæ are placed into the group Sphenophyllales. These on careful consideration of all the available evidence Prof. Seward does not unite with the Psilotaceæ, as has been done recently by Thomas and Bower, though undoubtedly there are some curious resemblances between Psilotum, and still more between Tmesipteris, and the Sphenophyllales. On the other hand the differences between Psilotaceæ and the bulk of the Lycopodiales warrant in the author's mind their separation from the Lycopodiales.

The account of the Psilotales and of the recent Lycopodiales is altogether excellent, and the full discussion of morphological and anatomical features of the recent members of each group is of the utmost value, particularly to students of geology desiring to take up seriously the study of fossil plants. But botanical students could not desire a more concise and at the same time critical description of the leading members of these groups.

The Lepidodendraceæ are dealt with in the masterly manner natural to one who has contributed so largely to our knowledge of this group of plants.

We note that Prof. Seward retains Binney's designation of *Lepidodendron vasculare* for the more usual *Lepidodendron selaginoides* of Sternberg. While accepting his arguments in favour of the adoption of this specific name as valid, one cannot help expressing a hope that after the decisions arrived at by the recent International Congress at Brussels, some uniformity of practice in Palæobotanical nomenclature will be established. The difficulties with which Palæobotanists are faced in deciding which name to adopt in the case of plants of which the various organs are discovered separately and only subsequently correlated are often very trying.

In dealing with the histological features of the Lepidodendraceæ we note with interest the suggestion that the fine bars which connect the horizontal bands of the scalariform thickening in the tracheæ are suggested as possibly representing a partial absorption of the pit-closing membrane which in many recent and fossil forms has entirely disappeared according to Gwynne Vaughan.

The very vexed question as to the nature of the Ulodendroid scars is fully discussed, and the author inclines to the adoption of the branch-scar hypothesis and makes the useful comparison with the phenomenon of cladoptosis as observed in certain Conifers and even Dicotyledonous trees.

It will be noticed that the genus Bothrodendron, our knowledge of which is now more complete than of any of the fossil Lycopodiales, is placed by Prof. Seward in a special subdivision of Bothrodendreæ; for though both in its external appearance and internal structure it bears considerable resemblance to the genus Lepidodendron and was often included in that genus, yet certain substantial differences of structure, and more particularly the knowledge we have now gained of its strobilus, warrant the separation of this genus from the Lepidodendreæ. The ultimate position of Lepidocarpon and Miadesmia in the group of Lycopodiales is cautiously left for the moment in suspense.

In the group of Filicales our knowledge of recent as well as of fossil forms has been considerably advanced of late years, and Prof. Seward gives us a clear and concise account particularly of those features which are of importance in interpreting the structure and relationship of fossil forms. A much-needed word of caution is given in describing the variation in the leaf forms of recent ferns many of which, if met with in isolated specimens as fossils, would undoubtedly have given rise to a multiplication of species. The evolution of the more complex types of the stele met with in ferns is dealt with in a clear and concise manner, and is of course of importance in appreciating the relationship and sequence of forms met with in fossil ferns, such as the Osmundaceæ for example, which have been so successfully investigated by Kidston and Gwynne Vaughan.

The interesting group of fossil ferns to which Renault gave the name of Botryopterideæ, containing "altogether extinct and generalised" types, has been shown to contain plants of considerable diversity of structure and Prof. Seward has now substituted the term Cœnopterideæ (κοινός = Latin *communis*, common or general) for the Botryopterideæ in the wider sense and subdivides it into the Botryoptereæ and Zygoptereæ. The new term has certainly advantages over the descriptive but somewhat clumsy name of Inversicatenales suggested by Paul Bertrand and the Primofilices of Arber, which latter prejudges somewhat their evolutionary position.

The last chapter contains a number of genera belonging probably to the Pteridosperms, with other ferns of more doubtful affinity. The better-known Pteridospermæ and their seeds will be dealt with in vol. iii., the appearance of which will be awaited with eagerness. For in spite of the substantial intellectual fare provided in vol. ii., the food for thought is of so stimulating a nature that the reader looks forward with a whetted appetite to the appearance of vol. iii. and has not the feeling of satiety which a less enthusiastic or a less skilful author would have produced in his readers with a book containing so much detailed information. But the engaging way in which Prof. Seward introduces descriptions and illustrations of recent plants in explanation of fossil forms and of their structure, makes the subject one of living interest and carries the reader along, be he geologist or botanist. The copious references to the extensive literature on recent and fossil plants will be of the utmost value to the serious student of palæobotany, and in every other respect, in typography, illustrations and index, vol. ii. of Prof. Seward's *Fossil Plants* enhances the reputation alike of the author and of the Cambridge Biological Series. F. E. WEISS.

**A Textbook of Experimental Physiology for Students of Medicine.** By N. H. ALCOCK, M.D., D.Sc., and F. O'B. ELLISON, M.D., with a Preface by E. H. STARLING, M.D., F.R.C.P., F.R.S. [Pp. xii + 139.] (London: Churchill, 1909. Price 5*s*. net.)

THIS work is to be welcomed not only for its own sake, but also for the indication which it affords of the extension of the view that medical students would be better trained for their future work if they did more physiological experiments upon man. In the Preface Prof. Starling rightly insists upon the importance of the change which is taking place in the practical teaching of physiology in this country. More attention is paid to experiments upon the higher animals, and this change has received recognition in the revised regulations for the examinations in Physiology in the University of London.

The practical course laid down in this text-book begins with a chapter upon the physiological anatomy of the rabbit; this will serve as a useful revision of the knowledge obtained by the student during his study of biology. There then follows a course of special exercises which deal with muscle, circulation, digestion, respiration, blood, secretion of urine, temperature, nervous system and special senses. There is a good selection of experiments anddemonstrations; the diagrams are clear but there are no graphic records.

It is very difficult to devise a satisfactory course of experimental physiology for large classes of students. It will probably be found that the reaction against experiments on "muscle and nerve" is proceeding too rapidly; time and experience in practical classes will be required for the selection and teaching of experiments upon man. There is evidence of this in the present work, for if allowance be made for the demonstrations upon anæsthetised mammals which must be performed by a teacher who holds a licence to make experiments upon animals, there does not appear to be sufficient practical work to occupy the student during two terms, the time generally given to experimental physiology. The teaching of practical physiology for medical students is more thorough in this than in any other country, and it would be a step backwards if the course were shortened. It is probable that the authors will find it necessary to extend the exercises.

Another suggestion which one would wish to make for future editions of the book is that typical graphic records should be included; in the present edition the student will miss the guidance afforded by such tracings, and an extra tax will be thrown upon the teacher. In many exercises a more detailed description of the results to be expected would be an advantage.

The book is a useful addition to the practical works upon experimental physiology and will be welcomed by all teachers of medical students.

M. S. PEMBREY.

**Researches on Fungi.** An Account of the Production, Liberation and Dispersion of the Spores of Hymenomycetes treated Botanically and Physically, also some Observations upon the Discharge and Dispersion of the Spores of Ascomycetes and of Pilobolus. By A. H. REGINALD BULLER, B.SC. (Lond.), D.SC. (Birm.), PH.D. (Leip.)., Professor of Botany at the University of Manitoba. [Pp. xi + 287 + Pl. V + figs. 81.] (London: Longmans, Green & Co., 1909. Price 12*s.* 6*d.*)

THE massive fruit-bodies of the Hymenomycetes, such as those of *Agaricus* and *Polyporus*, are clearly complicated structures with a very high degree of adaptation to the production and discharge of spores. The detailed mechanism of these reproductive organs, however, has been almost completely neglected, and until lately botanists were ignorant of the way in which the spores separated from the basidia and escaped from the pores or from between the gills, and little was known of the number of spores produced or of how they were dispersed.

As a result of numerous observations and ingenious experimentation Prof. Buller has enabled us for the first time to form a clear picture of the hymenomycetous fruit-body as a working mechanism. By a series of careful measurements and calculations the author first gives body to the generally accepted view that the development of gills, spores, spines, etc., is for the purpose of increasing the spore-bearing surface. He shows that the common mushroom has 20 times as much hymenial surface as it would have in the absence of gills; in *Polyporus squamosus* the proportion is 11, and in a specimen of *Fomes igniarius* with twenty layers of tubes it is 1,000. The large area of the hymenium would lead us to expect a large spore-production, but one is hardly prepared for the enormous profusion in which these structures are produced. By collecting the spores and distributing them in water and then counting the number in a small sample it was shown that a single pileus of the ordinary field-mushroom may produce 1,800,000,000, and *Coprinus comatus* 1,000,000,000; the number of spores produced by a single *Polyporus squamosus* with many pilei may exceed 50,000,000,000, and that of a Giant Puff-ball may reach seven billion.

The author was also able to show that the fall of spores from the fruit-body could be easily observed with the naked eye by throwing a strong beam of light in their path. The spores can then be seen falling in the form of curls and wreaths of bright points. By this means the path of the spores could be followed and the effect upon them of convection currents in the air could be studied. By this method also the surprising discovery was made that forms such as the corky and leathery *Polypori* and the xerophytic *Marasmius* and *Schizophyllum* will retain their vitality for months or years in the dried state. They only require to be moistened for the fall of spores, which is stopped by desiccation, to start again.

The question of the manner in which the spores leave the basidia and escape from the pores or from between the gills was closely investigated, and by dint of careful observation and experiment the actual path of the spore was clearly shown. The spores were found to be forcibly shot away from the basidia (not simultaneously as stated by Brefeld, but successively) to a distance 0·1 to 0·2 mm. The mechanism of spore-discharge is probably that of a jerk due to the sudden separation of the two parts of a double wall in the sterigma. Such a wall was not observed, but since the spore and basidium both remain turgid after separation a double wall must be hypothesised. The specific gravity, size, and rate of fall of the spores in still air were all carefully determined by ingenious experiments. From these data it was calculated that the spore of *Amanitopsis vaginata* leaves the basidium with a horizontal velocity of about 40 cm. per second, and completes its horizontal path in $\frac{1}{100}$ of a second. Later it falls vertically between the gills at a rate of about 0·5 cm. per second, which for some unexplained reason is about 46 per cent. more than would be expected from Stokes' Law.

As the spores of all the Hymenomycetes are very adhesive, a violent discharge to a distance less than that separating opposite pairs of gills (or opposite walls of a pore) is the only way by which spores can be provided with a means of free escape from the hymenium of the ordinary fruit-body. The importance of the vertical position of tubes or gills is thus made clear; it can be calculated that in the ordinary mushroom a tilt of about 5° will prevent the escape of half the spores.

The function of the stipe is, as Falck suggested, the provision of a space beneath the pileus within which the spores may be caught by air-currents.

The author was unable to confirm Falck's theory of the importance of the respiration of the fruit-body in producing heat and so giving rise to ascending convection currents round the pileus. In the case of a fruit-body of *Polyporus squamosus* examined *in the open* the spores were seen to drift away sideways and not to be caught up by vertical currents. It is possible, however, that such currents may play a small part in other cases.

The interesting problem of the advantage of the "deliquescence" of the gills of the *Coprini* was attacked by the author. It is pointed out that the process is probably one of autodigestion and that its purpose is apparently the removal of parts of the gills which have already shed their spores; by this means space is provided for the shedding of the next set of spores. Thus all the spores in succession find a free path of fall, in spite of the fact that the frailty of the flesh of the pileus prevents the gills as a whole being held horizontally as in the stouter fruit-body of *Agaricus*. In the smaller, second part of his work Prof. Buller deals briefly with the ejection and distribution of the spores in some Ascomycetes and in *Pilobolus*.

In criticism it may be pointed out that although the author has succeeded in giving for the first time a detailed working picture of the fruit-body of such well-known forms as *Agaricus*, *Polyporus* and *Coprinus*, and so has earned the gratitude of all botanists, yet he too readily assumes the absence of upwardly directed hymenia in the group. Owing also to the slightly popular nature of the presentation of the work the distinction between old and new material is not always clear. It may be suggested further that the apparent capacity of a fruit-body to withstand the effect of ether-vapour for such a lengthened period as a week requires further investigation, as do also the cytological changes occurring in the hymenium and basidia of fruit-bodies when revived after desiccation for many months. It is to be regretted also that the author was unable to publish the chief of his new results in some scientific journal, as he originally intended. The publication of his work in book-form alone and at a high price will certainly delay the dissemination of his results among working botanists, especially abroad.

In conclusion, it is evident that Prof. Buller is to be congratulated on a fine piece of work in which he breaks new ground not only in the biology of the hymenomycetous fruit-body but in the border-land of botany and physics.

V. H. BLACKMAN.

**A History of Birds.** By W. P. PYCRAFT, with an Introduction by Sir RAY LANKESTER. [Pp. 458+xxx.] (London: Methuen & Co., 1910. Price 10*s*. 6*d*. *net*.)

MR. PYCRAFT apparently wrote his book and then encountered the familiar difficulty, unusually great in a subject on which so many books have been written, of choosing a title; in any case the title selected does less than justice to the intention of the volume. Mr. Pycraft's own preface and the delightful introduction by Sir Ray Lankester explain that the purpose of the writer was to set out the interplay of inherited constitution and moulding environment in producing the characters of living birds. The scheme is attractive, the author is an expert anatomist and a keen observer, the result is admirable. A notable body of exact information upon structure, classification, habits and instincts is ingeniously combined to form a coherent series of studies; the illustrations, both technical text-figures and pictorial plates, are excellent in themselves and pertinent to the argument. Those learned persons to whom the facts may be already familiar will take pleasure and profit from seeing them acquire a new significance from their new setting, whilst to the beginner in ornithology Mr. Pycraft's book will serve as a stimulating introduction and a compendious text-book. Most zoologists will learn much from Mr. Pycraft, will accept many of his inferences and dispute not a few of them. And what better can one say of a book?

Mr. Pycraft begins with an admirably clear account of the distinctive structural characters of birds, and then deals in broad outline with their ancestral history and the possibility of arranging them in a phylogenetic tree. He shows that the origin of birds from reptiles is an almost inevitable inference from the reptilian characters found throughout the structure of modern birds, characters still more strongly reptilian in the Jurassic fossil *Archæopteryx*, which none the less was essentially a bird. He maintains that the evidence points to the descent of birds from arboreal reptiles, and supplies a clever and plausible account of how the gap may have been bridged. His account of the avian palate is naturally somewhat elaborate, partly because of the intrinsic importance of that structure in the case of birds, an importance upon which his own contributions to knowledge have led him to lay special stress. It is well known that Huxley, in preparing his famous lectures on birds, treated them as extinct animals, paying special attention to the structure of the bony palate. He distinguished and named the salient types of palatal structure and founded his classification on them. Later workers succeeded in blurring the edges of Huxley's distinctions, showing the existence of transitional forms and of misleading convergent resemblances. Mr. Pycraft himself put back the subject on a firm basis by showing that there were two fundamental types, one which he called "palæognathine," characteristic of the struthious birds, the other that he called "neognathine," characteristic of higher birds; moreover he showed that the neognathine bird passed through a palæognathine stage. Thereupon Mr. Pycraft plunged into a familiar pit, in which, it is true, he is in the company of perhaps a majority of systematic zoologists. His major dichotomy of living birds is into *Palæognathi* and *Neognathi*, whereas his own discovery that all birds have once been palæognathine makes the presence of that character useless as an indication of affinity.

In three interesting chapters (iv, v, vi) Mr. Pycraft supplies an able sketch of geographical distribution, habitats and migrations of birds, showing very clearly the interdependence of these phenomena. He wisely refrains from any categorical theory of migration, but plainly leans to the view that it arose from seasonal variations in food supply, and that the continued incidence of such economic pressure has gradually increased the range of movement. He believes that the routes of migration are a family tradition, slowly altered by geographical changes but so definite that when, for instance, English migrants become extinct, their places will not be taken by other birds of the same species, as the latter have a different traditional summer or winter home. Chapters vii to ix sketch the relations of birds to animate nature, to plants, to various kinds of mammals and to one another. The instances selected are chosen from a wide range, including such varied subjects as the agency of birds in the dispersal of plants, the parasitic habits of the cuckoos and various cases of social instincts. In chapter x Mr. Pycraft gives a careful and very interesting summary of the peculiar features of the relations of the sexes in birds; in another chapter (xxi), perhaps designedly far removed from his description of the facts, he gives a brief discussion of sexual selection. He deals with the problem in a fashion that is suggestive rather than conclusive, and is plainly not satisfied that the theory of sexual selection is a sufficient explanation of sexual coloration and display, antics and song. Nothing could be better than his series of chapters (xi to xvi) on nests and eggs, the care of offspring, the plumage of chicks, and so forth. It is a part of the subject to which he has given great attention, and his presentment of the facts and selection of suitable illustrations are alike admirable. It is a pity however that Mr. Pycraft did not refer to the passage in *Notes by a Naturalist on H.M. "Challenger"* before regretting, in a superior way, that Moseley did not examine the alleged pouch of King Penguins and compare it with the marsupial pouch. He would then have found that Moseley did not use the word "pouch" in a structural sense, but described the fold of skin and the mode in which the egg is supported by the feet of the penguin.

Mr. Pycraft's interpretations of the very varied conditions of nestlings is novel and convincing, and is a marked advance on the conclusions of other writers. He believes that the Hoatzin represents a surviving primitive condition. There is enough food-yolk to allow development to proceed to such a stage that the newly hatched bird can creep about actively in the branches of the tree which contained the nest. By a series of adaptations, birds which lay their eggs on the ground or in exposed places have come to produce still more precocious nestlings fully fledged and capable of great activity as soon as they are hatched; in other cases, chiefly where nestlings if too active would fall out of the nest and be killed, the food-yolk has been reduced and the young are hatched in a helpless state. In yet other cases, birds which must be supposed to have had precocious nestlings comparatively recently have changed their habitat, and the rate of development of the young has been retarded secondarily.

It is impossible, in the short space of a review, to discuss the later chapters of this book, in which the author deals with such subjects as variation, inheritance, natural selection, isolation, and adaptation, although these are really valuable contributions to many disputed problems. Mr. Pycraft has an exceedingly happy way of dealing simultaneously with arguments and instances, and presents to the general biologist a novel range of material for discussion, whilst the ornithologist is led gently from familiar facts to the most difficult and generalised problems of evolutionary science. P. CHALMERS MITCHELL.

**Science, Matter and Immortality.** By R. C. MACFIE. [Pp. x + 300.] (London: Williams & Norgate, 1909. Price 5*s*. net.)

"SCIENCE," says Robert Louis Stevenson, "writes of the world with the finger of a starfish"—Mr. Macfie devotes his book to showing that she writes with the "radiant finger of a star." He brings much knowledge, the fruits of wide reading of philosophy, poetry and science to his task. There is much natural eloquence in the book, but in too many places the dignity of the theme is marred by over-emotion. "The grip of the atoms is the grip of the great Hand of God," or "Evolution and dissolution are merely the systole and diastole of the Heart of God," are phrases which, with due reverence, we feel may fittingly compete with the saying of the greatest oracle of obscurantism: "Whereby why not? If so what odds? Can any man say otherwise? No. Awast, then!"

The book begins with the atomic theory and ends in a Pantheism of the vaguest character. "All ideas give place to the final integrating emotional idea—God. Nor is the God an unknown God." If He be indeed known to the author the latter manages the introduction but lamely, for God seems to be in turn force, matter, beauty, or the universe at large! It does not help us much to be told that to science "there is one God, the greatest among gods and men, unlike mortals both in mind and body." Or that "the God of Science speaks in the thunder and smiles in the sunshine. He is so great that the stars eddy round His feet not ankle-high, yet so loving that He makes roses and sunsets for the human heart."

Apart from these unfortunate rhetorical weeds, the book gives an interesting though very brief account of the growth and nature of modern theories of matter and ether, of the matter of life and evolution. The growth is traced from the Greek atomists to the electrical theory of the atom, and, as is fitting, the historical survey includes a chapter devoted wholly to Lucretius. It is well to be reminded, as the author reminds us, of the prescience of great minds; for instance, of Faraday's conception of "Radiant Matter," as he called it in 1816, half a century before Crookes' experiments on electric discharges in high vacua gave the first experimental demonstration of this fourth state of matter which Becquerel's discovery of material radiations in 1896 has helped to elucidate. The author's historical sketch suffers, however, from the defect that it is too one-sided. The backwaters and eddies might at least have been indicated as well as the main stream of advance in knowledge. For example, the memoir by Prof. Puluj on the inherent absurdity and dynamical impossibility of the fourth state of matter might well have been mentioned. It was considered to be of such importance at the time as to deserve translation and republication by the Physical Society, along with Helmholtz's wonderful study of contact potential! Crookes' amazingly skilful experiments received scant recognition, and his insight provoked something akin to contempt until the study of radio-activity enlarged the current physical concepts.

The best of the book lies, to our mind, in the chapters on evolution—the best and the worst, for the rhetorical blemishes to which we have already drawn attention are most crowded in the section on the origin of man; while the chapter on serial evolution is an excellent statement of the weak points in the evolutionary hypothesis which are too apt to be forgotten. "Granted infinite germinal variations, we have yet to find machinery sufficient to select the right ones, and such machinery we have not found. The selective agencies that have been suggested might prune an oak certainly, but would never shape a single leaf of it." The problem is still in some ways just as Empedocles left it. We still want to know whether things are or are not moved by a certain principle contained within themselves to arrive at certain ends. Ray Lankester admits that "we have no reason to suppose that the offspring of the beetle could in any number of generations present variations on which selection could operate so as eventually to produce a mammalian vertebrate." "Good!" says our author, "but why begin at beetles? Why not go a step further back and say that we have no reason to suppose that the offspring of the *amœba* could in the course of any number of generations present variations on which selection could operates o as eventually to produce a mammalian vertebrate?" "Here is an amœba! Here is a man! Can any sane thinker affirm that one amœba has been stationary for millions of years, while the other has become fish, monkey, man?" No! the hypothesis of evolution is amply justified because it correlates so many facts and has made possible so many advances, but the mechanism of the unfolding of types of dead and living matter is, perhaps must be for the most part, a mystery unless, as Larmor suggests, we find the solution in the structure of the atom.

W. B. Hardy.

**Diseases of the Skin, including Radiotherapy and Radiumtherapy.** By Ernest Gaucher, translated and edited by C. F. Marshall. [Pp. xii + 460.] (London: John Murray, 1910. Price 15*s.* net.)

Dr. Marshall has already done good service to his English-reading colleagues by bringing to their notice the opinions of the French school of medicine, especially on the subjects of cutaneous and of syphilitic diseases. He has added notably to these services by the preparation of the volume under consideration.

This book consists mainly of a translation of the volume on diseases of the skin in the well-known *Nouveau Traité de Médecine* (Brouardel, Gilbert and Thoinot) written mainly by Prof. Gaucher, with additions from other works of the same author. Chapters have been added on special subjects (*e.g.* radium therapy and X-ray treatment) by such French authorities as Wickham, Degrais, Domenici and Gastou. The work of Gaucher as Professor of Cutaneous and Syphilitic Diseases in the Faculty of Medicine in Paris is probably not so widely known in this country as that of certain of his colleagues in this branch of medicine, but to those who have had the opportunity of seeing Dr. Gaucher at the Hôpital St. Louis and of listening to his teaching, the impression conveyed is that Gaucher is first of all a well-trained and sagacious physician. He has constantly in mind the close relationship of the subjects on which he gives special instruction with the general discipline of medical study. We find, as is to be expected, that the opinions expressed in this volume are well balanced, and have always in view the general condition of the patient as well as the special disease. The pronounced opinions held by Gaucher as to the close connection between internal diseases and disorders of metabolism and their external manifestations, handing on the traditions of the French school, are expressed throughout his teaching, and may be specially appreciated whilst reading the chapters on auto-intoxication and diathesis. The quotation of one sentence will be sufficient to indicate his point of view :

"The cutaneous secretions, therefore, in arthritic subjects play the part of vicarious secretions ; hence the frequency of cutaneous eruptions in the gouty, and in all those in whom nutrition is impaired."

The classification of the volume is a simple one. After the preliminary chapters dealing with elementary lesions and the general etiology of the diseases of the skin, the various affections are classified on an etiological basis, such as the conditions due to external non-parasitic origin, and those due to parasitic influences. Then follow two groups showing especially the influence of French medicine—namely toxic, medicamentous, and alimentary diseases ; and diathetic disorders. Under the latter heading are considered some of the most important skin diseases, such as eczema, psoriasis, lichen and acne. The other groups, namely those of nervous and of congenital origin, complete the classification.

It may be said that the chief characteristics of Prof. Gaucher's teaching are first of all a certain dogmatism in statement appropriate to elementary instruction, and an impatience of the fantastic opinions and methods of treatment which are so apt to absorb the attention of specialists with limited general knowledge.

The attention devoted to treatment is, indeed, somewhat meagre, but the concentration on well-tried methods is specially to be commended at the present time. The chapters on treatment by rays of light, X-rays and radium are short, but give a sufficiently intelligible account of these modern methods for general purposes.

The illustrations in the book are numerous, and consist of reproductions of photographs and of the well-known models in the Museum of the St. Louis Hospital ; many of these illustrations are useful, but such blocks as Figs. 15 and 70, showing practically nothing, are only disfigurements in a printed page.

The book can be safely recommended as a text-book to the medical student in his years of final study, and for post-graduate work.

**The Cambridge County Geographies**: Cambridgeshire. By T. McKenny Hughes and Mary Caroline Hughes. [Pp. xiii + 271].—Cornwall. By S. Baring-Gould. [Pp. ix + 164].—Derbyshire. By H. H. Arnold-Bemrose. [Pp. x + 174].—Fifeshire. By Easton S. Valentine. [Pp. ix + 187].—Kent. By George F. Bosworth. [Pp. viii + 146].—Lanarkshire. By Frederick Mort. [Pp. viii + 168].—Westmorland. By J. E. Marr. [Pp. ix + 151]. (Cambridge University Press, 1910. Price 1*s*. 6*d*. each.)

The Cambridge County Geographies are intended primarily for schools ; but they will serve equally well as handbooks for general use. They are not guide-books, but in many ways even the traveller will find them more useful and certainly more interesting, than the ordinary guide-book.

The scheme of arrangement is the same in all. From one-third to one-half of the volume is devoted to the physical geography of the county, including the topography, geology, natural history and climate. The remainder deals with the inhabitants, industries, history, antiquities, etc. A short account of the principal towns is added. Each volume is provided with two maps, one orographical and the other geological ; these are pasted directly to the covers of the book, forming the inside of each cover and also the opposite fly-leaf, either map can therefore be referred to instantly. The size is necessarily limited but for most purposes the convenience of the arrangement outweighs the disadvantage of a rather small scale.

The general plan of the series could hardly be improved and the execution of the plan leaves little to be desired. No single author can be equally interested in all the matters dealt with; accordingly there is considerable variation in the space allotted to different sections. Thus in the volumes above mentioned the number of pages devoted to geology varies from about one-tenth to about one-twentieth of the whole. Such differences are to be expected and provided that the author be selected with due regard to the special character of the county the variety of treatment may be a positive advantage.

But a much more serious difficulty has evidently made itself felt. It cannot be assumed that the readers will have even an elementary knowledge of all the subjects touched upon. Several of the sections therefore begin with a short general introduction. In the case of architecture, antiquities, etc., a very few words of preliminary explanation are sufficient to make the rest of the section intelligible. But climate and geology are too complex and too far removed from ordinary observation to admit of such treatment, and the introductions to these subjects are not always very satisfactory. Moreover, the same introduction is made to serve in several volumes, without regard to the special description that follows; consequently terms are often explained that are never used. In the volume on Cornwall there is a full list of the geological systems and their subdivisions not one of which is mentioned in the account of Cornish geology. In the volume on Kent the list of systems does not give the subdivisions, whilst in the description of the county only the names of the subdivisions are used and there is no reference to the systems. In both these volumes the introductory remarks occupy about one-half of the section.

In the sections on climate the preliminary observations are even more decidedly out of proportion to the rest. In most of the volumes the greater part of the chapter is devoted to a rather vague and not very intelligible account of the climate of England as a whole, followed by a few statistics referring to the county concerned. Only in the volume on Lanarkshire is there any serious attempt to deal with the climate of the county itself.

Since it is not possible to explain the principles of a science in three or four pages, it would probably be better to omit these introductions. A map of the rainfall of the county would certainly be more useful than a map of the rainfall of England, especially as the latter appears in many of the text-books now in common use. A geological section of the county would be more in place than a geological section across England and Wales.

In other respects there is but little to criticise. Within the limits imposed it would be difficult to do more than has been done; and in general it may be said that the object of the series has been attained. The volumes will certainly be very useful in schools where modern methods of teaching geography are used; they will also be found very convenient for general use.

It should be added that all the volumes are well illustrated and that the printing is of the high standard which is usually associated with the Cambridge University Press.

PHILIP LAKE.

# FRANCIS GALTON

A GREAT personality has passed from us in the death of Francis Galton. To few has it been given to live so full and so valuable a life. The Goddess Fortune, it is true, offered him opportunity with a generous hand; rarely however has an offer been more amply justified than that she made to Francis Galton, for the treasure has been invested by its trustee in safe things, which will never cease to pay. Galton never flew too high nor attempted to probe too deep; he was conscious both of the extent and of the limitation of his powers; his work is of lasting value to mankind because he possessed in a high degree that sense which tells its owner which tasks lie within and which beyond his power.

But we should be disloyal to Galton if we regarded him as an isolated personality detached from his natural setting—his ancestry. Indeed we are not truly loyal to what he believed unless we regard the individual as a product (in the strictly literal sense of a continuation without cessation of individuality) of his ancestors and intelligible only in the light of a knowledge of those ancestors. Turgenev and Samuel Butler owe their supreme position as novelists to the fact that they perceived this.

Perhaps the most remarkable instance of a monopoly possessed by a single family over a particular business is the limitation to the Darwin family of the business of bringing home the truth of evolution to the understanding of mankind. We make no apology for the term business. It is no longer necessary to point out that Charles Darwin's achievement was not to discover evolution but the much heavier task of forcing mankind to believe in it. To satisfy oneself of the truth of evolution is the work of a philosopher; to convince other people of this truth is a labour of Hercules.

Erasmus Darwin married twice. By his first wife he was grandfather to Charles Darwin, by his second to Francis Galton. "His hereditary influence," says Galton (*Memories of My Life*, p. 7), "seems to have been very strong. His son Charles, who died at the early age of twenty of a dissection wound, was a medical student of extraordinary promise." Erasmus may not have been—probably was not—the first to perceive the fact of evolution; there can be little doubt that Buffon was before him. But with Erasmus Darwin the business of making evolution credible passed over once and for all to the "nation of shopkeepers" and the Darwin family. Not only was Erasmus Darwin's enunciation of the doctrine of evolution less equivocal than Buffon's (through no fault of the latter however) but the sudden conversion of Lamarck, late in life, to a belief in evolution followed so closely after the appearance of a French translation of *The Loves of the Plants* by E. Darwin, as to leave little room for doubt that the two events were causally connected. Of Charles Darwin's participation in the task of making evolution credible no more need be said.

We must now turn to the part played by his cousin. Galton, by laying the foundation of an exact science of heredity, not only took steps to fill in the most serious gap in the evolutionary hypothesis, but sowed the seed of a tree which will furnish the best, if not the only, antidote to that outbreak of *a priori* speculation which followed the publication of the *Origin*. Galton's vivid perception of the necessity of having things surely and certainly described before any attempt was made to explain them found its expression in the application of statistical methods to the study of biological problems. It was not only by that part of his work which laid the foundations of Biometry that Galton supplemented the work of Darwin; he also drove home the applicability of our knowledge (such as it is) of evolution and heredity to the furtherance of human welfare and thus laid the foundations of the science of Eugenics.

Galton's greatest work in the purely scientific sphere must be regarded as the foundation of the Biometric method and philosophy rather than in the promulgation of the law of heredity which is associated with his name. This law, which was deduced from a study of Basset-hound pedigrees, will in the future be remembered, not so much as a true summary of a vital process but as the expression of a first valiant attempt to detect some order in the chaos of hereditary phenomena as they appeared at the time. Its purely provisional nature is foreshadowed by the fact that its author did not commit himself to a dogmatic statement on the question whether it was applicable to the individual or to the mass, and demonstrated by the fact that only in rare, probably accidental, cases does it apply to masses and that in no case does it apply to individuals. Galton's work on Basset-hounds will always be remembered for the same reason as will De Vries's work on *The Evening Primrose*, estimating both of these at their lowest possible valuation. Both were the first attempts to break the ground in a new field of evolutionary inquiry. The pioneer nature of Galton's work in heredity is gracefully and fittingly acknowledged by Johannsen, whose *Erblichkeit in Populationen und in reinen Linien*, which represents the most recent and ingenious attempt to interpret a certain class of hereditary phenomena, is dedicated to him.

No account of Galton would be complete without reference to his work on Finger Prints. The source of his interest in this subject was delight in the observation and systematisation of the phenomena themselves ; their application was a subsequent matter. His interest in them arose through a request to deliver a Friday evening lecture on the system " devised by M. Alphonse Bertillon for identifying persons by the measurements of their bodily dimensions." Galton tried to persuade M Bertillon to incorporate finger prints into his system of identification, but without success. He found however that they had already been employed by Sir William Herschel in his district in India, who succeeded in introducing them into Bengal and subsequently throughout the whole of India. At the present day there are few civilised countries in which they are not employed for the purposes of the identification of criminals. Galton's hopes that finger prints would prove to be of high anthropological significance were not fulfilled. He was unable to find that any particular type is characteristic of members of widely divergent human races; or of such widely different types within a single race as " students of science, students of art, Quakers, notabilities of various kinds and a considerable number of idiots at Earlswood Asylum." Only one result of positive theoretical significance emerged from this study : this was the demonstration that the variation of the pattern was of the discontinuous kind, and the conclusion that the various types had been evolved without the aid of natural selection.

It is a curious coincidence, if indeed it be a coincidence, that the respective founders of two great schools of heredity, the Biometric and the Mendelian, were born in the same year. Galton and Mendel were both born in 1822. Galton (*Memories of My Life*, p. 308), with characteristic courtesy, refers to Mendel's work immediately before he refers to his own law. His estimate of the significance of the work done and inspired by Mendel seems to us to be so true and concise that we make no apology for quoting it together with the rest of the paragraph in which it occurs. "I must stop for a moment to pay a tribute to the memory of Mendel, with whom I sentimentally feel myself connected, owing to our having been born the same year 1822. His careful and long-continued experiments show how much can be performed by those who, like him and Charles Darwin, never or hardly ever leave their homes, and again how much might be done in a fixed laboratory after a uniform tradition of work has been established. Mendel clearly showed that there were such things as alternative atomic characters of equal potency in descent. How far characters generally may be due to simple, or to molecular characters more or less correlated together, has yet to be discovered." The grace and simplicity of this, the delicate manner in which Mendel is associated with Charles Darwin and the soundness of the critical estimation are very characteristic of Galton. It is often said that the course of Charles Darwin's work and thought would have been very different if Mendel's work had come under his notice. It is curious to speculate as to what might have been the course of hereditary inquiry if Galton himself had made the experiments actually carried out by Mendel.

Galton's claim to fame, however, does not rest only on his pioneer work in the investigation of heredity. He will be perhaps longer known, and he is at present more widely known, as the champion of the application of such knowledge of heredity as we possess to the improvement of the human race; in other words, as the founder of Eugenics.

His interest in this question first found expression in two articles on "Hereditary Talent and Character" published in *Macmillan's Magazine* in 1865, which, curiously enough, is the year in which Mendel published the results of his own classical researches. The width of Galton's interests at this time may be gathered from the fact that the publication which preceded this was one on "Spectacles for Divers, and the Vision of Amphibious Animals," the one which succeeded it being that on the "Conversion of Wind-charts into Passage-charts." But the actual investigation of the problem of race improvement was then laid aside for many years because he felt that "popular feeling was not then ripe to accept even the elementary truths of hereditary talent and character upon which the possibility of Race Improvement depends." Moreover he himself was "too much disposed to think of marriage under some regulation and not enough of the effects of self-interest and of social and religious sentiment." The term Eugenics was first applied by Galton to the scientific attempt to ameliorate the human race in his *Human Faculty*, which appeared in 1883; and his interest and inquiries up to date were gathered up into his "Huxley Lecture" before the Anthropological Institute in 1901 on the "Possible Improvement of the Human Breed under the existing conditions of Law and Sentiment."

The active prosecution of eugenic inquiry has been handed over to the professed representatives of the Biometric school. How this has been done may best be told in Galton's own simple words. After referring to the foundation by Professor Karl Pearson of a Biometric laboratory in University College and the institution of *Biometrika* and his connection with it as Consulting Editor, he says (*Memories of My Life*, p. 320), "The ground had thus become more or less prepared for further advance; so after talking over matters with the authorities of the University of London and obtaining their ready concurrence, I supplied sufficient funds to allow of a small establishment for the furtherance of Eugenics. The University provided rooms and gave the sanction of their name and various facilities and I provided for a Research Fellow and a Research Scholar. The Eugenics laboratory of the University of London is now situated in University College, in connection with Professor Karl Pearson's Biometric laboratory, and I am glad to say he has consented to take it for the present at least under his very able superintendence; as I am too old and infirm to look properly after it." Eugenics, it may be well to remind the reader at this point, is officially defined in the minutes of the University of London as "The study of agencies under social control that may improve or impair the racial qualities of future generations either physically or morally."

The actual investigation of eugenic problems is thus cared for. But there has also sprung into existence the Eugenics Education Society, which acts the part of a middle man whose function is to exhibit the results of those investigations so that they shall be intelligible and palatable to the lay mind.

The institution of the eugenic movement was evidently regarded by Galton as his life-work, for he concludes his autobiography with a restatement of his views concerning it. "I take Eugenics very seriously, feeling that its principles ought to become one of the dominant motives in a civilised nation, much as if they were one of its religious tenets." Here is a life-work of which any man might well be proud.

Galton practised the principles which he preached in his marriage with the daughter of Dr. Butler, for many years Headmaster of Harrow and then Dean of Peterborough. Dr. Butler was not merely an able classicist and mathematician himself but transmitted his qualities in full measure to his children and grandchildren. The remarks which follow the reference to his marriage and to his wife's family are perhaps the most interesting, because the most intimate, of his eugenic pronouncements.

". . . The Butler family well deserve study as an instance of hereditary gifts, but this is hardly the place for it.

"Neither can I enlarge as I could have done on the far greater importance of being married into a family that is good in character, in health and in ability, than into one that is either very wealthy or very noble but lacks these primary qualifications. . . .

"I protest against the opinions of those sentimental people who think that marriage concerns only the two principals; it has in reality the wider effect of an alliance between each of them and a new family."

It is a happy circumstance that Galton lived long enough to complete that vivid and detailed picture of himself, *Memories of My Life.* This is by no means a piece of elaborate self-analysis. The picture presented to us of the author is conveyed by his own simple unaffected style and seen in the mirror of his varied environment. The dominant note in Galton's personality was his simplicity; to the last he preserved a childlike delight in trivialities which is an attribute of only very great men. His delight at having a *genus* of plants related to the Hyacinths named after him was unbounded and we cannot but be deeply moved by the little drawing of *Galtonia candicans* which concludes his autobiography.

Perhaps the most beneficent symptom of Galton's perennial youth was the sympathy which he felt and delighted to express with those who were beginning. There are, to our own personal knowledge, many men who have not yet attained to half the age at which Galton died and who have done and will do work of the highest scientific value (in the most literal sense of the word scientific), whose greatest and in some cases only encouragement has been a kind word or line from the great man whose death has deprived them of a true friend and science of a noble servant.

A. D. D.

# THE ETHICS OF FOOD

## III. BREAD

THE word "bread" which is common to several languages appears to have been applied originally to pieces of bread obtained by breaking up the loaf. It is strictly applied to the mass of dough made by moistening and kneading the flour of grain and baking it. Custom has led to the use of the term also in the far wider significance of "daily bread" or food in general and as a consequence there is some tendency on the part of the public at large to look upon bread as a complete food, sufficient in itself to support life and maintain the body in health. This idea is entirely erroneous; though bread is the food of the people in the widest sense, it is far from being the only food.

The recent journalistic campaign in favour of the so-called "Standard Bread," which appears to have been promoted mainly for the purpose of securing advertisements, contains so many statements which are essentially false and misleading that it is desirable to consider on a broad and impartial chemical and physiological basis what are really the facts known to the scientific worker. The problem is a very intricate one; moreover we have learnt only quite recently in the least to understand what the composition of flour is and what the changes are that it undergoes during digestion. Like many another attributed to the medical profession, it is difficult to discover that the statement that white bread is objectionable on the score of health and deficient in nutritive value has any justification in fact, nor is the medical profession particularly qualified to pronounce an opinion on such a question.

Wheat is the cereal used in largest quantity, though there is quite as much if not more nutritive matter in oats and some of the other cereals, wheat being now preferred to other cereals wherever it is brought into competition with them—rice having been largely displaced by it in the East and maize in America. This pre-eminence of wheat is due to the fact that it affords the only flour from which a light vesiculated loaf can be made by fermentation and a means provided of giving starch to the body in a pre-eminently digestible form.

In the eyes of the expert, good quality in bread is the outcome of excellence in several points which have been concisely defined to be *strength*, *colour* and *flavour*; these are regarded as of importance somewhat in the order stated. The term "*strength*" is one which is used continually in relation to flour: it has been defined as "the capacity for making large, shapely and well-aerated loaves" and therefore includes the three separate qualities, stability of dough, yield of bread and size and shape of loaf.

From the point of view of the public, flavour is regarded as of minor importance; until recently weight has been attached chiefly to colour or rather to absence of colour in the loaf and there has been a demand, especially on the part of the operative classes and in the mining and manufacturing districts for a very white loaf.

Those who know the British workman are aware that he is gifted with far more acumen than is generally supposed; his selection of the whitest possible bread and perhaps the popularity of the "black bread" cry at a recent general election afford no exception to this statement. In the past, when wheat was dear, bread was often made with dirty flour or with flour produced from sprouted, badly harvested wheat or from wheat full of foreign seeds. All sorts of other cereals, in particular barley, sometimes to the extent of 25 per cent., were mixed with it. This is well shown by the evidence given before the Parliamentary Committee on adulteration in 1855. Dr. Hassal, in his historic work on Food and its Adulteration, published in 1876, devotes several pages to these adulterations. The effect has been to make the working man shun dark-coloured bread; those who have lived sufficiently long on the continent of Europe to eat the dark-coloured bread during months at a time will agree that our white bread is in every way a more pleasant and palatable article, although there is no proof that the one is more nutritious than the other.

As a matter of history, it must be remembered that threshing by machinery is also a modern innovation, its introduction dating from just before the invention of the roller mill. Rational threshing has involved an immense improvement in the purity of flour.

### Colour

It is of interest at the present juncture to discuss this question of colour more in detail. As a matter of fact the colour of a loaf is largely a question of optics and depends even more on the character of the flour used than on its whiteness. It is agreed that perfection of colour implies brightness of appearance in both crumb and crust and it is found that a weak flour, though very white in colour, makes loaves of poor, dingy appearance. Questions of the refraction and reflection of light enter far more into the colour of the loaf than is supposed.

It is well known to the baker that the better aeration of a loaf produced by the admixture of a stronger and possibly darker-coloured flour with an originally weak flour causes a distinct increase in the apparent whiteness of the bread.

The invention of the roller mill and the vast improvement in the technique of milling, which has been so marked in the last few decades, has involved the almost entire elimination of fragments of dirt and husk from flour: for this reason bread has tended to become whiter than formerly, though the nutritive qualities have been in no way impaired.

There is of course no relation between the colour of bread and its nutritive value and the standard of colour is from this point of view a false one.

The fetish of colour-worship being established, means were sought by the trade of improving the colour of flours which lacked whiteness and were principally for that reason classed as lower grade, again without any reference to their nutritive value. The spurious addition of alum—originally added so as to make it possible for the baker to deal with flour milled from wheat which had begun to germinate—which has the effect of making the bread look very white, is happily a thing of the past in this country.

A modern development, however, is the application of methods of bleaching flour artificially in milling practice. Most of these methods involve the agitation of the flour, during a few seconds only, with an atmosphere containing very small quantities of the oxides of nitrogen or ozone, whereby a process of nitration or oxidation or both of the colouring matter takes place and the flour is bleached more or less according to the time of exposure. Opinions of scientific men are divided as to whether the treatment is harmful or the reverse to the flour and to the digestibility of the bread made from it. The United States authorities have gone so far as to forbid the use of bleached flour within their dominions. In Britain the reverse opinion is probably that generally held: it is at least certain that there is no positive evidence that the bleached flour affords a bread which is in any way harmful to digestion. In any case it is a question between baker and miller; the public in getting the white bread it desires is not getting bread of less nutritive value.

In brief, the colour of the loaf bears no relation to the nutritive value except in so far as the whiter colour brought about by improved aeration of the loaf is due to the presence of a stronger flour. It becomes necessary therefore to examine the connection between strength and nutritive power.

### Strength

It has long been sought to correlate the somewhat elusive quality defined as strength of a flour with its chemical composition and although much yet remains to be done in this direction, sufficient has been established to indicate that, as a rule, the strongest flours are those which contain most gluten—that is, the most nitrogen. Strictly speaking, strength is dependent on the quality rather than on the quantity of the gluten but flours in which quality does not accompany quantity are the exception rather than the rule and need not be considered here.

The "strongest" flours are obtained from Canada and the United States and the baker cannot do otherwise than use a proportion of these if he wish to make the class of bread now in vogue. Bread made from weak English flour is quite different in character and will contain a slightly smaller proportion of protein. An average English flour contains from 9 to 10 per cent. of protein; a Canadian patent flour will contain upwards of 14 per cent. And this is in general still true when the English flour is enriched by the presence of the germ and the outer layers of the grain. The experiments of the Home Grown Wheat Committee have proved that it is possible to grow strong wheat in England but unfortunately the diminished yields of grain and straw obtained with such varieties are against their general adoption by farmers.

## Flavour

The flavour of a loaf depends not merely on the ingredients of which it is made but also on the skill displayed in the milling of the flour and the manufacture of the loaf. The presence of intermediate products of fermentation of a dextrinous nature are of advantage. Added sugar has no effect on the flavour but a very small addition of diastase causes a perceptible improvement. Probably flavour is largely dependent on enzymic action and since the germ is that part of the wheat berry which contains most enzymes, its presence has a distinct effect on the flavour of the loaf. Unfortunately bread is judged chiefly by appearance and its flavour is of little importance in determining the selling price.

Flavour is very much influenced by variations in the operations involved in the manufacture of the loaf, more particularly by the extent to which fermentation is prolonged. Judging also from the experience gained in other fermentation industries, *e.g.* brewing and wine-making, the race of yeast used will have a direct bearing on the flavour.

Dr. F. Ehrlich of Berlin, in a series of most important investigations, has shown how some of the fusel oil constituents and fruity ethers of spirits are products of the action of the yeast on the protein constituents of the mash. There is evidence tending to prove that similar changes go on in dough when fermentation is sufficiently prolonged.

There is a tendency in big bakeries in the large towns to shorten the time of fermentation and this may possibly be one explanation of the complaint that modern bread is deficient in flavour. It is well known that the minutest traces of fruity ethers have an extraordinary effect in stimulating the flow of the digestive juices and exciting sensations of taste. Much is made of the statement that bread containing the germ will retain moisture better than white bread. This is true in so far as the former is of a more sodden texture: it contains hygroscopic products produced by the decomposing action of the germ. But surely this additional moisture is a disadvantage to the public and more particularly in the eyes of those reformers who now abuse the baker for using strong patents flour possessing a high capacity of absorbing water.

Another matter of controversy is the method of baking—

whether it should be in a slow relatively cool oven or in a hotter quicker oven. The latter method is obviously the more rational, as the object of baking is to distend the gluten cells as far as possible by the rapid expansion of the enclosed gas before they are fixed in size by heat. There is no doubt the baker might with advantage produce loaves with a little more crust than appears to be his practice at the moment.

In order to appreciate the controversies connected with the bread question it is necessary to pay some attention to the process of milling.

Wheat consists of the embryo or germ amounting to about 1·5 per cent. of the whole, the starchy kernel which makes up 85 per cent. and the outer envelope or bran equivalent to 13·5 per cent. In the milling process it is very difficult to pulverise the bran completely, consequently it has been the miller's first object to remove it. The bran consists of several layers which yield in turn various mill-products—bran, pollards, sharps and middlings—representing different fragments from without inwards.

Formerly, in the old method of stone grinding, the germ was left in the flour but it is now removed as offal. The reason for this is twofold: the oil in the germ is very liable to become rancid and so spoil the keeping properties of the flour—a matter of great importance—whilst the soluble enzymes of the germ act on the starch and gluten and cause the loaf to darken in colour and become yellow in the oven. The flour is derived from the endosperm and before grading is termed "straight run"—it amounts to about 70 per cent. of the weight of the original grain. Usually it is further graded into "patents" and "households or seconds flour" and other subdivisions, the milling process being so controlled that the lowest grade of flour is derived from the outermost layers nearest the bran. As a consequence the lowest grade is slightly richer in protein than the patents flour. Certain food reformers have persistently advocated the use of bread made from the ground entire wheat berry, though without success, for the simple reason that the human digestive apparatus very soon rebels at the bread, though for the moment it may be pleased with it on account of the fullness of the flavour and the variation from white bread.

The term semolina has been used in the recent crusade as if it represented a particular part of the wheat berry. In reality it is applied to the granular products obtained when the endosperm is first broken. The breaks are separated into coarse semolina and coarse middlings and the semolinas further separated by wind or gravity purifiers and subsequently reduced to flour which is included in the straight grade flour. All flour therefore includes the semolina: this is not a special characteristic of the 80-per-cent. flour.

The crusade has been abandoned in favour of the cry for a flour containing 80 per cent. of the grain, including the semolina and the germ. Such flour, it is claimed, is richer in protein, oil and phosphates and is said to make a more wholesome, cream-coloured loaf. From motives of patriotism and popularity, the cry has also been for the exclusive use of English-grown wheat to make such bread. It is true that the 80-per-cent. flour will contain slightly more protein and proportionately a good deal more mineral matter and oil than 70-per-cent. flour milled from the same wheat but as the subsequent examination will show, the percentage of chemical composition is not a criterion of the nutritive value.

From the point of view of common sense a food must be judged on (1) the nutritive values of its constituents and on (2) the proportion of these which are digested. Moreover, although it is customary to deduce the nutritive value directly from the proximate chemical analysis, it must not be forgotten that this is a very uncertain way of arriving at the truth. The proteins, for example, are built up of a number of separate units which the chemist describes as amino-acids, etc.; there is much evidence to show that the presence of each of these units in minimum quantity is necessary for the proper maintenance of health. Deficiency in one unit cannot be compensated by an excess of another. Proteins in foodstuffs are measured at present merely by an estimation of nitrogen, which gives no information whatever as to the quality of the protein. It is therefore possible that in any particular food there is an excess of a particular unit and a deficiency of another essential unit and it is not correct to maintain that an increase in the amount of protein in a particular food, as for example bread, necessarily means that the body derives full benefit from it. It will be obvious that the scientific valuation of food is far from being a simple matter and the subject urgently requires fuller investigation.

What is quite clear is that it is not the high percentage of nutriment in a food but the ease with which it can be digested and assimilated and then made use of by the individual that is of importance. Extra protein is of little use if the body simply burn it up in the same way that it burns the inexpensive carbohydrate starch.

Gluten is an excessively complex material consisting in the main of two proteins named gliadin and glutenin, both of which are characterised by the fact that they are built up of a large proportion of a particular amino-acid named glutamic acid which is characteristic of other cereal proteins. These cereal proteins as a class are very different in their composition from animal proteins. The germ of wheat contains another protein named leucosine which is intermediate in character between the cereal and animal proteins and it is possible that the addition of the 1·5 per cent. of germ to flour does add to bread a minute quantity of constituents which it otherwise lacks and so renders it a more complete food. As a matter of fact less than 30 per cent. of the germ is protein, so that the addition of the whole of the germ to flour does not increase the amount of protein by 0·5 per cent. But at present no one can say that these fragments of protein digestion are of such special value as to make their presence of consequence and it is a fallacy to claim that their presence would compensate for other proved disadvantages of wholemeal bread.

Numerous careful studies have been made on the digestibility and nutritive value of bread, particularly those of Rubner (1879), Pappenheim (1890), Constantinidi (1887), Raudnitz (1892) and Moeller (1897).

The most recent and systematic investigation is that of Professor H. Snyder carried on at the University of Minnesota in 1890–2, under the immediate supervision of the late Professor W. O. Atwater, whose reputation as an authority on the nutritive value of food is beyond question. These investigations were published by the United States Department of Agriculture in 1903 (Bulletin 126).

The flours compared were 72 per cent. patents; entire wheat flour obtained by removing about one-half of the bran before grinding, *i.e.* equivalent to about 85 per cent.; and 100-per-cent. flour, representing the entire wheat kernel or much the same as standard flour.

It was conclusively established that the *patents flour* ground either from hard or soft wheats which had a somewhat lower amount of protein than the other two samples contained a *higher proportion* of *digestible protein* and *more available energy* than the coarser grades.

Owing to the fact that they were finely ground the digestibility of all these flours was found to be high. When less perfectly ground the bran was still less absorbed and the experiments may therefore be regarded as most conclusive. The bran is not acted upon by the digestive juices owing to the large amount of cellulose present which prevents their access.

As A. E. Humphries has repeatedly pointed out, wheat is a particularly hardy plant and the husk is the cover provided by nature to protect the food of the baby plant under all sorts of unfavourable conditions. In consequence this husk consists largely of woody fibre able to resist disintegration and obviously indigestible.

There is also evidence that the presence of bran prevents the assimilation of a certain percentage of the food which would otherwise be digestible.

It is sometimes contended, generally under the guise of that mystic phrase "the authority of the medical profession," that the presence of bran in bread is valuable on account of the slightly aperient effect that it has. Those who have realised how delicate are the intestinal membranes will hardly accept this policy of "scratching"; even if bran does not harm, indeed even if it help the healthy grown-up individual, a plea must be raised on behalf of children and invalids. Sufferers from constipation can always turn to wholemeal bread without requiring that the bread supplied to the general public should be spoilt for their selfish ends.

Bread is in part digested in the mouth, its starch being converted into dextrins by the saliva. For the saliva to be efficient it must be brought into contact with as large a surface of the bread as possible. This takes place most easily with toast and biscuits, which are easily pulverised and saturated with saliva. Obviously the lightest, most vesiculated and driest bread presents a larger surface and is more readily attacked than a closer, moister bread which forms doughy masses more difficult to chew that do not soak up the saliva.

This explains the well-known indigestibility of new bread and is also applicable to the bread from 80-per-cent. flour; the presence in the latter of the germ and of the inner coatings of the husk, to say nothing of the fact that it is supposedly made from weaker flour, renders it impossible for the baker to obtain a satisfactory result with it.

Standard flour will not give the light vesiculated loaf which the baker can make from his ordinary mixture of foreign and English patents flour and in well-known words the eater of it

> Gets him to rest, cramm'd with distressful bread.
>
> *Henry V.*

Even in the case of white bread the absorption in the body is not complete, the loss amounting to about 4·5 per cent. of the total solids. A closer study of this loss shows that the greatest share falls on the proteins, of which 20 per cent. are unabsorbed: this fact is in agreement with the statement already made that the body does not require an excess of a particular protein, although it has been also attributed to the large amount of starch present. The carbohydrates of bread are practically completely absorbed but only three-quarters of the mineral matter is made use of. In the case of wholemeal bread, the total loss is as much as 14 per cent. and one half of the mineral matter is unabsorbed, so that the supposed value of the additional mineral constituents is considerably discounted. Rubner, Goodfellow and others have made this fact quite certain and it is difficult to see how any rational body can advocate the advantages of a bread containing increased mineral matter in view of these facts. It is true the amount of ash in flour has steadily decreased of late years, indeed the low percentage of ash is an important criterion of purity of the best patents. This, however, is due to improved methods of cleaning the wheat from adherent dirt and to the fact that the flour no longer contains the detritus of the milling stones, not to the removal of the intrinsic mineral constituents of the flour.

Weight for weight bread is one of the most nutritious of the ordinary foods and it is also amongst the cheapest, yet it is far from being a perfect food for the reason that it contains rather less than half the proportion of protein to carbohydrate required for an ideal food and the proportion of fat is very small. In an ordinary mixed diet, these deficiencies are supplied by the other foods that are taken, many of which are of a highly concentrated nature: thus fat and protein are taken separately from other constituents. It is permissible therefore to regard bread as being to some extent a bulky diluent of our other food and its main function is heat-forming rather than muscle-building.

Modern evolution has not carried civilised man sufficiently far from his more savage ancestors to enable him to limit the choice of his meals to the oft-talked of "tablet" and he still requires bulk to give the feeling of satisfaction. The case of the diabetic to whom bread is forbidden and who complains of the feeling of emptiness after what would seem to be a large fat and protein meal is a sufficient illustration of the fact.

If the point of view here developed be the correct one, it would suffice to leave the nation's bread as it is, trusting to the millers and bakers to give us a clean, palatable and digestible loaf and looking to our other food as hitherto to supplement whatever may be the deficiencies in nutritive power.

If we ate bread alone, everything would be of consequence; as we do not, it cannot be asserted that this or that constituent is of such consequence that the sacrifice of no portion can be permitted. Bread is not like mother's milk, a natural food: nothing can be omitted from the latter without injury to the child; but as wheaten flour is only one of the foods which man, in course of time, has learnt to use and its quality bears no genetic relationship to his needs, there is no reason to suppose that any slight variation we make in the course of its preparation is likely to be of serious consequence.

The problem when bread constitutes the chief or only source of nutriment is of another kind and at the best the "improved" bread offers but a poor solution of the economic question.

Such questions as the handling of bread in a hygienic manner in its passage from the oven to the consumer and the ever-prevalent scandal of short weight are far more worthy of public attention than any attempt to produce a dirty, indigestible loaf, thereby encouraging the use of inferior, badly cleaned flour.

In dealing with the problems presented by food, it is obviously important that discussion should not proceed on sentimental lines, and that it should be kept out of the hands of the faddists as far as possible. The practice so popular with the American press of writing up a subject in order to force advertisement is certainly not one that should be favoured in such a connexion; the coincidence of advertisement with advocacy in our press of late, to say the least, is not a healthy sign. The result of the recent agitation must have been that a large amount of bread has been sold full of muck, to give it a dirty appearance indicative of "wholeness." Various opinions have been quoted; it would be interesting if we could see these opinions in their original form before they were worked up by the reporter—the specimen given by *Punch* in its issue of March 15 is probably very much to the point. We have reason to know that the opinions attributed to Dr. Gowland Hopkins were never given by him *ad hoc* and that he was unaware that he would be quoted as an authority. He is undoubtedly one of the few men in the country whose opinion on such a matter is worth considering—probably there are not more than half a dozen others who can speak with any degree of authority on the question. There can be no medical opinion on the subject worth consideration, as there are no data on which a valid opinion can be formed.

The difficulty and complexity of the problems afforded by food cannot be overestimated; it is not likely that inquiry can help us much in a case such as that afforded by bread—our methods of analysis are not sufficiently developed, even if it were possible to formulate an issue definite enough to be made the subject of inquiry. It is pretty clear, however, that there can be no Standard Bread, if we take into account the great differences met with in the wheats of the world and the variations which are undoubtedly determined by season and soil. The ratio of starch to nitrogenous constituents (gluten) varies greatly—we cannot fix any standard ratio. Nor is it in the least clear that we need take composition into account. Bread is of use to us for the most part as fuel—it is burnt up for the most part as fuel, just as coal is in a fire grate; it is used probably to quite a minor extent for constructive purposes, in tissue formation. We cannot say that it contributes this or that indispensable element; we are not in the least aware to what extent, if at all, different foods may be supposed to overlap as sources of supply of this or that ingredient. What we do seem to know is that food should be pure and fresh and that a varied diet is most desirable. The public should have the common sense to demand that they shall not be misled by sensation-mongers on such a subject: that they have been most seriously misled is highly probable.

# THE GREAT STAR MAP

## IV. SOME INCIDENTS OF THE WORK

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The general history of this enterprise as exemplified more particularly in the portion of the work undertaken at Oxford has now been given and it remains to notice several incidental investigations of different kinds which have branched from the main project. In a piece of work already extending over about twenty years, in a new department of science such as the application of photography to astronomy, it is only natural that the consequences of the departure should not have been foreseen in their entirety at the outset.

The first novelty which attracted our attention at Oxford was what is called the "magnitude equation" of the Cambridge meridian observations. In order to determine completely the places of the stars on any one of our photographic plates, it was necessary to know the places of a few of them in the sky, so that we might virtually peg down the plate in its proper place on the sky and refer all the new and previously unmeasured stars to their proper positions. For this purpose a large number of meridian observations made at the Cambridge Observatory some years before were ready to hand. Having selected the stars required and used them without difficulty for the purpose described, we found what are called the "constants of the plates." For each plate two stars would have sufficed, had everything been theoretically perfect; but in order to compensate for the small errors of various kinds unavoidable in scientific work, it was desirable to make use of many more stars than the theoretical minimum. From the general average of all the stars considered we ascertained the relative errors of individual stars; it was soon seen that a peculiarity was manifest in the individual errors depending upon the brightness of the star, and from independent information it was known what was the reason of the discrepancy.

The Cambridge observations had been made by watching the transit of a star across spider webs, recording the time of transit according to the clock. It has long been known that different observers have a persistent personal characteristic which has been called their "personal equation," in virtue of which they are systematically a little early or a little late in their records. More recently it has been found that even the same observer will vary in his habit according to the brightness of the star, the general tendency being to be late for the faint stars. The tendency is more marked in some individuals than others. The Cambridge observer (the late Mr. A. Graham) apparently had a strongly marked tendency of this kind.

Various methods have been suggested for the valuation of this habit, especially the method which depends on using gauze screens to reduce the light of a star and thus to substitute for it a virtually fainter star occupying exactly the same space as the brighter one. If the observer were free from the magnitude equation error, he would make a record of the transit precisely the same in the two cases; but if he be subject to the malady, his records will differ by an amount which affords a measure of his predisposition. There are, however, some difficulties of a practical kind in using this method, and it was pleasant to realise that in the photographic plate we had found a simple and effective means of determining the magnitude equation without the necessity for any special observations on the part of the observer.

A few details may be given which bring out some interesting points. The first attempt at detecting the magnitude equation from the Oxford measures was made by Mr. Hinks in 1897, when only seven plates had been measured (*Mon. Not. R.A.S.* lvii. p. 473). The material available was only sufficient to demonstrate the value of the method, Mr. Hinks recording his opinion that "when the reductions for the Astrographic Catalogue are completed, it will be possible to discuss very accurately the personal equations depending on magnitude." The reductions are now completed and the discussion is being undertaken; but we did not wait until now for confirmation of the forecast. In 1899, when 600 plates had been measured, an examination was made of the accumulated measures (*Mon. Not.* lx. p. 3), the stars being grouped in half-magnitudes; and it was found that the fainter stars had been "observed late" by Mr. Graham as follows, taking as standard those of magnitude 6·0:

DETERMINATION OF MR. GRAHAM'S MAGNITUDE EQUATION IN 1899.

| | | | | |
|---|---|---|---|---|
| | | | s. | |
| Magnitudes | 6·5 to 6·9, | 147 stars, | 0·016 | late. |
| " | 7·0 to 7·4, | 320 " | 0·025 | " |
| " | 7·5 to 7·9, | 504 " | 0·038 | " |
| " | 8·0 to 8·4, | 572 " | 0·059 | " |
| " | 8·5 to 8·9, | 1226 " | 0·086 | " |
| " | 9·0 to 9·4, | 2001 " | 0·146 | " |

It will be noticed that there are roughly twice as many stars in the second group as in the first, and in the third twice as many again, this being the natural increase of stars in the sky as we go to fainter magnitudes. But at the fourth group there is a discontinuity, owing to the fact that only half the available material was discussed beyond this point. The labour was considerable and it was thought that the examination, which was in any case only preliminary, need not be carried further at that time. In the fifth and sixth groups the increase is resumed. The discontinuity should not, of course, affect the averages in the last column, and we see that there is an increase of "lateness" which seems to be rapidly growing, since not only the quantities themselves, but their differences, get larger and larger. There is no suggestion of a sudden jump, such as we shall notice in a moment: the smoothness of the growth was considered to be satisfactorily established and the matter was left there until the present time. The measures are now printed and a final examination can be made, using the whole material and classifying the stars in smaller subdivisions, especially where they are numerous, as is the case for the fainter magnitudes. The work is only in its early stages but already an important new fact has come to light, as will be seen from the following figures, from which the brighter stars have been omitted, because hitherto only a few observations of them have been collected definitively:

PROVISIONAL RESULTS FROM THE EXAMINATION OF 1911.

| | | | | |
|---|---|---|---|---|
| | | | | s. |
| Magnitudes | 7·9, 8·0 and 8·1, | 84 | stars, | 0·049 late. |
| „ | 8·2, 8·3 and 8·4, | 102 | „ | 0·052 „ |
| „ | 8·5, 8·6 and 8·7, | 197 | „ | 0·073 „ |
| „ | 8·8 and 8·9, | 143 | „ | 0·085 „ |
| „ | 9·0, | 259 | „ | 0·098 „ |
| „ | 9·1, | 138 | „ | 0·155 „ |
| „ | 9·2, | 124 | „ | 0·170 „ |
| „ | 9·3, | 130 | „ | 0·176 „ |
| „ | 9·4, | 81 | „ | 0·178 „ |
| „ | 9·5, | 154 | „ | 0·191 „ |

The remarkable thing here is the sudden jump from magnitude 9·0 to 9·1, after which the further change is small. The actual difference in brightness of the star is so small that it is hard to believe that this discontinuity can have arisen naturally. It is possible that the observer had some special rule of procedure when he set out to observe a star catalogued as fainter than 9th magnitude—for instance, he may have arranged the illumination differently. The further investigation of this matter must be left until more stars have been examined ; but enough has been said probably to show the value of the photographic measures as a check on intricacies of personal equation.

A new enterprise of a more important and unforeseen kind arose from the discovery of the little planet Eros in 1898. Our solar system, which at the time when the days of the week were named contained only five planets in addition to the sun and moon, is now known to consist of many hundreds, and new members are being discovered almost weekly. Most of them are tiny rocks, probably not more than one or two hundred miles across, with no perceptible influence on the movement of their more important brothers and sisters ; in fact, of no particular interest, as far as we can see at present. Astronomers were beginning to get rather tired of the continual discoveries of new small planets, which brought increased responsibility for keeping watch on them and increased labour in calculating their movements, without any obvious advantage from the increase in our knowledge.

It was therefore a distinctly sensational incident when one of these discoveries proved to have a considerable importance, owing to the fact that the tiny object moved in an orbit which, in one part, was exceedingly close to the orbit of the earth.

The orbits of these small planets lie in general between those of Mars and Jupiter and up to 1898 none of them had been suspected of approaching the earth nearer than the planet Mars. But it was seen that the orbit of Eros lay within that of Mars and that only a few years previous to its discovery (namely, in 1894) the earth and Eros had been simultaneously in the adjacent portions of their orbits and had therefore been very close together.

Now such a close approach affords an opportunity of a special kind for determining accurately the distance of the little planet from the earth. Usually the planets are so far away that their distances are many hundreds of million miles, exceeding the diameter of our tiny earth so vastly that it is difficult to institute an exact comparison between the two, as we must if we wish to express the former in units familiar to us. The difficulty is precisely the same as that which we find in realising the distances of remote objects by the use of our eyes alone. There is no similar difficulty in perceiving the distances of objects close to us—say those within an ordinary room : they present different aspects to our two eyes and from these differences in aspect we are able to judge of the distances. But the change of aspect is smaller for objects more remote and we know that it is entirely insensible for an object so remote as the moon. Indeed, our power of perceiving distance by means of the difference in aspect for our two eyes breaks down long before we reach the moon, although we do not always realise the breakdown, because other methods based on general experience frequently come to our aid.

In the same way, astronomers pointing telescopes from opposite sides of the earth to the same object can perceive its distance by a method similar to that we use unconsciously when we look at anything with our two eyes. But the observations become difficult when the object is too far away and are only satisfactory for a comparatively close object.

The heavenly body closest to ourselves is of course the moon; and we know its distance within twenty miles. Up till 1898 the next closest known were Mars and Venus, on favourable occasions; and accordingly much time and trouble have been spent in determining the distances of these two planets when there has been a Transit of Venus or a favourable Opposition of Mars.

It should perhaps be remarked here that the *relative* distances of all the planets from the sun and from one another are known with great precision from the times which they take to describe their revolutions round the sun; so that when any one of them has been determined, we can obtain any other we please by a simple rule-of-three sum. Or, to put the matter in another way, we can make an accurate map of our solar system, being in doubt only about the scale of miles which usually accompanies such a map. Any single distance on the map being known, we could construct this scale and so find all the others. Hence it did not much matter whether we determined the distance of Mars or of Venus or of any other planet which might offer greater advantages than they; the new discovery of Eros offered just such greatly increased advantages. The opportunity is however not open always but only at certain times and seasons. One particularly tempting opportunity had been unfortunately lost in 1894 owing to our ignorance of the planet's existence. But it was seen that another opportunity was coming in 1901, not so favourable as that of 1894 but still well worthy our attention. It may be added that the next good chance will not come till 1931, so that it is easy to understand the anxiety of astronomers to take advantage of the occasion of January 1901. They were, however, taken at a disadvantage by the comparatively short notice. There was no time to think of preparing special instruments; prudence suggested utilising such instruments as were already in good working order, and especially the battery of photographic telescopes engaged in making the Great Star Map. It needed a good reason to justify this diversion of their activities from the great work, which was alone sufficient to occupy their undivided attention, but the reason which had presented itself so suddenly was felt to be good enough. At the meeting at Paris, in 1900, of the Committee charged with the work on the map, the President (M. Loewy) proposed that this digression should be made; and the proposal was unanimously adopted.

Accordingly for some months during the winter 1900-1, most of the telescopes were withdrawn from the work on the map and were turned on the little planet Eros. The chief aim of the programme was to take photographs as soon as possible after sunset and as late as possible before sunrise; for on these occasions the telescope would be as nearly as possible on opposite sides of the earth. A few sentences ago we compared a pair of eyes to a pair of telescopes pointed at the same object from opposite sides of the earth; but a single telescope may be made to serve the purpose of a pair, since the rotation of the earth carries it round during the night from one side to the other; and this will explain the sunset and sunrise exposures to the planet Eros. In this way many hundreds of such photographs were obtained.

The next thing to do was to measure all these photographs accurately, in order to determine the place of the tiny planet among the stars. This place was of course changing continually, owing to the movement of the planet round the sun and indeed owing to a similar movement of the earth also. But it was possible to devise methods of allowing for this movement and correcting the measures for it. There would remain the displacement due to "parallax," that is to say, to the finite distance of the planet which it was required to measure. The greatest amount of such displacement was about 23″—about one-hundredth part of the moon's apparent diameter; which it was desired to measure if possible to the thousandth part of itself. Hence the measurement required a new order of accuracy; the apparatus already in use for the Great Star Map needed modification in essential details for this new enterprise. Moreover it is a familiar fact in scientific work that, when we proceed to the next decimal place, we always encounter a number of unforeseen difficulties of all kinds; and the measurement of the Eros plates was no exception to this rule. Some of these difficulties arose in the course of the measurement at the separate observatories and were vanquished as they arose. Particularly was this the case at our national observatory at Greenwich, where a complete determination of the distance of the planet was made from the Greenwich plates alone, without help from those of any other observatory and with very satisfactory results. But to get the full advantage from all the many photographs taken it was necessary to co-ordinate all the measures made at the different observatories, which brought to light a new crop of difficulties. It is to the lasting credit of Mr. A. R. Hinks, of the Cambridge University Observatory, that he undertook, as a volunteer but with the full approval of the President of the Committee charged with the work, to collect and co-ordinate all the results. The labour was very heavy and has occupied a large part of his working time during ten years. The difficulties which cropped up were new at every turn and great ingenuity was called for in overcoming them.

One such difficulty may be mentioned in illustration. It has been remarked above that, when stars are observed visually, there is apt to be a "magnitude equation," *i.e* a difference between the records for bright and faint stars but that the introduction of the photograph seemed to offer a check on these errors, being itself free from them. On this assumption Mr. Hinks proceeded to treat the measures of the Eros plates; but on comparing the measures at different observatories, he found between them just such differences as affect old visual observations under the head of magnitude equation. The differences were not so large perhaps but were nevertheless sensible. It may occur to the reader to inquire whether the differences arose in the measurements of the plates, which were of course made visually; but such a possibility was eliminated by the method of measuring each plate twice over, as has been explained in a previous article, the plate being turned completely round for the second set of measures; any magnitude equation would affect the two sets of measures in reverse directions and could thus be both detected and eliminated. This alternative being ruled out, it followed that the error must be in the plates themselves and it was a very disturbing discovery to find that we had not, as had been hoped, freed ourselves from such a kind of error by the introduction of photography.

Some comfort was forthcoming from the further discovery that many of the plates *were* sensibly free from this error, but these only increased the puzzle. What could be influencing those which showed unmistakable traces of it? Ultimately the cause was found in a faulty lens or rather in the faulty arrangement of the pair of lenses which go to make up the object-glass of a telescope. We have now realised that this arrangement must be carefully made and that faults render us liable to this old trouble; but it also seems probable that with care on the part of the instrument maker, the trouble can be avoided or, at any rate, rendered conveniently small. It is easy to sum up in a few words in this way the net result of the investigation; but the investigation itself was a long and tedious one and is perhaps even yet scarcely complete. It was faced, along with many others, with great courage and patience, with the ultimate result that, in the spring of 1909, Mr. Hinks was able to announce to the Paris Academy of Sciences a most satisfactory result for the distance of the planet and, by implication, for that of the sun and of the other members of the solar system.

He was also able to add a value for the mass of the moon. It may seem strange that this altogether different measurement of mass is to be deduced from the figures which give us a measure of length; but the fact is that we measure the mass of the moon by noting a certain length, namely, the distance by which it pulls the earth from side to side as it waltzes round with it. The earth and the moon may be compared to a pair of partners dancing round a ballroom; if they were of equal size, they would swing equally to right and left of their average path; but the moon is much the smaller and only pulls the earth a very little way from side to side. Nevertheless the oscillation is perceptible and it alters the aspect of the planet Eros in the same kind of way as the oscillation of a telescope from one side of the earth to the other. Indeed the two oscillations are combined together in the measures and we only separate them by the fortunate circumstances that one takes place in a day and the other in a month.

Before leaving the conclusion of this great problem of the planetary distances, which has come down to us through the ages, a word or two may be devoted to its history. The Greeks made attempts to determine the sun's distance but they were very crude: for example Aristarchus of Samos made it only nineteen times the distance of the moon or about 4½ million miles; and it was long before anything like the true value (about 93 millions) was arrived at. In the middle of the nineteenth century the margin of doubt was some millions of miles, but it was expected that the transits of Venus in 1874 and 1882 would reduce this margin within narrow limits. The observations made at these famous transits were however very disappointing, and even before the second of them was due, some astronomers had already turned to other methods for finding the sun's distance, especially the observation of the planet Mars and later of one or other of the small planets. The best determination of this kind, previous to the Eros determination, was that by Sir David Gill at the Cape of Good Hope about 1889, who obtained a result closely like that at which Mr. Hinks arrived twenty years later. But there are other measurements which bear an interesting relation to this direct measure of distance, especially those which compare the velocity of the earth in its revolution round the sun with the velocity of light. This comparison can be made in two entirely independent ways which take account of two entirely independent movements of a star, one directly in the line of sight and the other at right angles to it. Until the invention of the spectroscope, the latter was the only movement of a star, real or apparent, of which we could take account. It was discovered by Bradley early in the eighteenth century that by noting the apparent changes in direction of any star, we could find the ratio of the earth's velocity to that of light; for the aberration, as he called it, was due to the relation of these two velocities. Hence if we can find the velocity of light by independent means, we can deduce the velocity of the earth and from this the length of its path during one year; from this the radius of its orbit can be found, which is the quantity we seek. Now the velocity of light has been successfully measured by terrestrial experiments with sufficient accuracy, so that the distance of the sun can be deduced from measures of aberration. But curiously enough the value so found does not quite accord with that given by Sir David Gill and confirmed by Mr. Hinks. The discrepancy has been rendered more remarkable within the last year or two by the successful measurement of aberration by the other method, using the spectroscope, which enables us to measure the velocity of a star in the line of sight. The velocity thus found is partly that of the star and partly that of our earth; in many cases (though not in all) we may consider that of the star as steady but that of the earth varies during the year, being sometimes towards a particular star and sometimes directly away from it. By comparing the observations on these two occasions, we can eliminate the steady velocity of the star and deduce the velocity of the earth alone, from which we get, as before, the distance of the sun.

Now measures made recently on this plan have given a result in satisfactory accordance with that of Gill and Hinks, and have thus rendered the isolation of the other result from aberration the more remarkable. There are some who think that the discrepancy will ultimately lead us to the discovery of some new phenomenon about which we are at present entirely in the dark.

To illustrate what is in their minds we may recall that Lord Rayleigh was led to the discovery of Argon by paying attention to minute discrepancies in the values he obtained for the density of nitrogen from different sources; and not only was the discovery of Argon important in itself but it has led to others of vast importance. So that all these may be said to have originated in the study of a minute discrepancy between two measures of what purported to be the same quantity. Is it possible that the future may have in store for us similar weighty consequences, traceable to the study of this discrepancy in the measure of the sun's distance?

But astronomers know only too well how easily such discrepancies may turn out to be due to some source of error that has been overlooked. Their science is concerned, perhaps more than any other science, with minute measurements which a minute error will nullify or disturb; and they must be continually ready to see the edifices which they have spent some labour in building tumble down like a house of cards owing to some tiny flaw in the foundations. An instance of this occurred as a by-product of the Oxford measures and will serve as an illustration. In the year 1902 Sir David Gill made the suggestion that the brighter stars were apparently rotating as a whole with respect to the fainter stars as a whole, basing it upon many thousands of observations made at two epochs about half a century apart. If the whole universe was rotating together we might not be able to perceive it. Many familiar tests would fail just as they failed to reveal the rotation of the earth to our ancestors. Should we have yet learnt this great fact if our sky had been permanently cloudy so that we never saw the stars? We might have suspected it from the recurrence of daylight; and we might have actually inferred it if we could have surveyed the earth in some way and found its equatorial bulge, which we might have rightly ascribed to the effects of rotation. Similarly we might be able to infer the rotation of the whole universe of stars if we can be sure of its equatorial bulge of which the Milky Way is a possible manifestation. That brilliant thinker Henri Poincaré has made a rough estimate of a superior limit to such a rotation in his book *Science et Méthode* (p. 285), finding a second of arc in 3,000 years, or a complete rotation in four thousand million years, which is really not very long considering that geologists would like to take it all for the life of our earth itself.

But Sir David Gill was not dealing with this general rotation of all the stars together: he thought he had detected a *relative* rotation of the bright stars and it seemed possible that some evidence might be gathered from the photographic measures in the following way. Of the stars whose places had been determined at Cambridge about 1880, some had been photographed at Oxford in 1892, others (say) in 1902. Assuming that there was in reality a relative drift of the bright stars as suggested, the plates of 1892 ought to show ten years of it when compared with the Cambridge observations of 1882, whilst those of 1902 would show twenty years of it. By simple subtraction we could get ten years of the drift. The subtraction is rendered necessary by the existence of the "magnitude equation" already noticed at the beginning of this article, which would affect both determinations of drift and prevent the drift from being identified from either source by itself, though it could be found from their difference if it could be rightly assumed that the effect of magnitude equation was the same in both cases. The experiment was accordingly tried, though scarcely under such favourable conditions as sketched above: with the result that a drift of the bright stars seemed to emerge of about the magnitude assigned by Sir David Gill *but in the contrary direction* (*Mon. Not. R.A.S.* lxiii. p. 56). Attempts were made to find a reason for the discrepancy but on extending the enquiry to movements in declination (*Mon. Not.* lxiv. p. 3), proper confirmation was not forthcoming, and it was suspected that there was some unknown source of error (*loc. cit.* p. 18). At that time it had not been suspected that magnitude equation could occur in photographic measures; but when subsequently Mr. Hinks came across a gross case of it in the work on the Eros photographs, as above mentioned, it was seen that the unknown source of error had probably been detected and the significance of the measures was thus destroyed (see *Mon. Not.* lxv. p. 55).

But if in this instance we failed to obtain what was searched for with much labour, on another occasion we made a considerable find without looking for it at all. The history of our Oxford portion of the map was made remarkable by the quite unexpected discovery of a New Star. It would be possible to institute a regular search for new objects by the use of star maps, comparing one plate with another taken on a different date; again, if the spectra of the stars are photographed as at the Harvard Observatory, then a new star might reveal itself on inspection of a single plate by the peculiarity of its spectrum. But neither of these methods was in the least degree in our minds in the course of the Oxford work on the map and the discovery was entirely accidental, as will be seen from the following account.

At the beginning of the year 1903 we were within sight of the completion of the measures and hoped to reach it before the end of the year. For several reasons the actual completion was ultimately delayed beyond this date but that is a point which does not concern us just now. In the hope of completing the measures before the end of 1903, we were making great efforts to secure all the plates which had not yet been taken.

If the favourable season for taking a particular plate before it "runs into daylight" is lost, we may have to wait nearly a year before another opportunity recurs; so that it was important to obtain all the January plates in January 1903, not leaving any gaps for January 1904. To expedite matters, when there came a specially fine night or two, a large number of plates were taken, which were set aside for development until the good weather was gone. In England we have learnt to prize these exceptional nights; and it may be remarked in passing that we occasionally get nights as good as anywhere in the world, though the occasions are not so frequent as in California, for instance. All too soon the indifferent weather came, the plates were developed and, to our great disappointment, it was found that they were not satisfactory. There had been an unfortunate failure in sensitiveness of the films, which is apparently liable to happen in the manufacture of extremely rapid plates; when straining at the limit of sensitiveness some very slight cause may produce a notable failure to reach that limit. The disappointment was the greater because it was practically the first of the kind; throughout the whole work the plates had been uniformly satisfactory, in spite of the risks just mentioned, otherwise we might perhaps have been on our guard against the shortcoming. There was, however, nothing for it but to take the photographs again; and if there had been need for special exertion before, this need was now much greater in consequence of the diminished time at disposal.

No very great surprise therefore was felt when one or two of the new plates were found to be faulty from a different reason. There was no further failure in sensitiveness, for the plate-makers were most sympathetic about our disappointment and immediately furnished an excellent batch of new plates. The fault was now that the telescope had not been accurately pointed to the right region of the sky—a kind of mistake which might reasonably be ascribed to the strain of working against time. But it is a good rule in astronomical work (probably also in other walks of life) to get to the bottom of any mistake if possible and so it proved in this instance. On comparing one of the wrongly set plates with another of the same region, it was seen that it contained a strange object which ultimately proved to be a New Star. The mistake had arisen because it is customary to select as guiding star the brightest in the neighbourhood (as being most easily identified) and the new star had blazed up so as to be brighter than any other near it; so that Mr. Bellamy had accepted it without question as the one to which he was to point his guiding telescope during the taking of the photograph.

We could not be sure for some little time of the nature of this object. It might be a planet or a variable star. The first alternative was soon disposed of, because it is easy to look up the places of the planets which could be bright enough; and, moreover, a planet would probably have betrayed itself by a slight movement between the three exposures given to each plate in the making of the map. The second alternative occupied attention rather longer. There are many stars scattered over the sky whose brightness varies considerably, so that they might at one time show an emphatic image on one plate and at another time be too faint to affect the plate at all. Many of these are well known and can be found in catalogues already published; others are being discovered year by year and no doubt we are still unaware of many to be discovered in the future. During the afternoon the lists were searched without finding any mention of a variable in that particular neighbourhood; and when in the evening the star was found to be still shining in the exact place of the photograph, telegrams were sent to other observers inviting their attention to it as probably a New Star. Any remaining doubts were dispelled by the spectroscopic observations and Nova Geminorum took its place as No. 18 in the list of Novæ which had been discovered in the history of astronomy.

Readers of the daily press will probably have seen recently an announcement of a similar discovery by the Rev. T. E. Espin, of Darlington, which is No. 22 in this list; for during the past autumn no less than three special objects were discovered at the Harvard Observatory from the examination of photographic plates. The total number is, however, still not large, though from the facts that up to the year 1884 only eight had been recorded and that the other sixteen have all been found in the last quarter of a century, we may infer that the rarity is partly due to our own lack of vigilance, and that the few discoveries recorded would probably have been supplemented by many others had a more systematic watch been kept.

We are at present not very well informed as to the nature of the celestial event which is represented by the appearance of a new star. A few things about it we know. In the first place the event is a sudden one, the light of the star increasing enormously within a day or two by something like twelve magnitudes—that is to say, in a ratio of about 1 to 80,000—then the light slowly diminishes—slowly but not quite steadily; there are fluctuations in the course of the diminution and these fluctuations were specially noticeable in the case of the new star of 1901—in Perseus. Sir Robert Ball gave us, at the Royal Astronomical Society, an amusing account of his experiences at the time when the fluctuations were such as would cause the star first to disappear to the naked eye and then to reappear again. He had taken a party of visitors into the open to show them the new star, only to find that it had disappeared; on the next night he took out another party to show them the disappearance and, as though to spite him, it had reappeared again. But these were only temporary vagaries, as the star was soon permanently lost to our sight and then even to telescopes of moderate power. It still remains, however, as a very faint object visible in large telescopes.

Another incident in the history of this particular new star may be noticed, for it seems to tell us something about the origin of such objects. When the light had become very faint, so that photographs of the region were necessarily taken with long exposures, there was found to be a faint nebulous light surrounding the star and successive photographs showed that this nebulous appearance was expanding in all directions, just as though there had been an explosion and the fragments were still flying outwards. The phenomenon aroused the greatest possible interest, for a rapid change—that is to say, any change which is perceptible in a few weeks—is almost unprecedented in the case of the stars and could have only one of two explanations: either the star is specially close to us so that the changes *appear* larger than usual or, if the star be at a distance similar to those of other stars, the changes themselves must be on a gigantic scale. It was soon seen that the latter was the right alternative and it was inferred that the velocities of the flying fragments must be comparable with the velocity of light (nearly 200,000 miles a second).

Now there is an interesting physical question, whether it is possible for gross matter to move through the ether with a velocity greater than or even as great as that of light. At first the hope was entertained that we were going to get some information on this interesting question but a more practical alternative was suggested, viz. that the velocity exhibited was not that of matter but actually that of light itself. The observed facts would be explained if the nebula had been in existence previously but had been without illumination, so that we were unaware of it; just as we are unaware of an object in a dark room until a flash of lightning illuminates the room. In such a case the illumination appears to be instantaneous, but since light does actually take time to travel, it cannot be quite instantaneous, which we should realise were the room billions of miles in size. The room taken up by a nebula is of this size and the flare-up of the new star therefore illuminated it gradually, beginning with the nearer portions and spreading to those more distant as time went on. This explanation of the facts was confirmed by a remarkable experiment. The light of the nebula was analysed by means of the spectroscope and found to correspond with that of the original flare. A spectrum is, after all, only a glorified name for a colour; we may represent the facts in simple language by substituting names of colours. The events would then be as follows: the star rose to its greatest brightness with a blue light, which afterwards turned to red and remained red as the light died away. Now the light of the nebula was not found to be red, as it would have been if it belonged to the star in its later stages, but was found to be blue, and must therefore owe its existence to the blue flare some months previous, which had taken that time to traverse the huge distances separating the outlying portions from the centre. If this be so, we may further suspect the nebula of having been concerned in some way in the original outburst. It seems plausible that some kind of encounter between the previously faint star and the previously faint nebula should have resulted in a great development of heat and light which sent the news to us.

It would be interesting to get confirmation of this possibility in other cases, but unfortunately the conditions are not always so favourable. Nova Persei blazed up brighter than the first magnitude stars, and though there have been New Stars even brighter than these (such as that of 1572 which was even visible in the daytime), most of those we now find are much less bright; so that if they are accompanied by the illumination of nebulæ our resources are not able to photograph them.

Here we must conclude this brief review of a quarter of a century's work on the Great Star Map and other matters related to it. The work is far from concluded as a whole, though two portions of it have been so far finished as to enable us to form some idea of the completed whole. But the attainment of any particular stage is after all only an incident in a journey, for in a very real sense the map will never be finished. Our real concern is not with the state of the heavens at any particular moment but with the changes which may be discerned by comparing one epoch with another; accordingly when we have mapped out any region satisfactorily we are not at the end but at the beginning. Our real work consists in watching the development of change, which may be slow to declare itself to our brief lives but will persist relentlessly during eternity.

# GROUSE DISEASE[1]

By ARTHUR E. SHIPLEY, F.R.S.
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"The longer I live, the more I am convinced that the apothecary is of more importance than Seneca; and that half the unhappiness in the world proceeds from little stoppages, from a duct choked up, from food pressing in the wrong place, from a vext duodenum, or an agitated pylorus."

Thus, that incorrigible amateur-physician, Sydney Smith wrote of our poor suffering humanity, and thus we can as truly write of the grouse. Little stoppages, food pressing in the wrong place, a vext duodenum, and an agitated blind-gut and there you have "Grouse disease"!

At the onset I must, however, protest against that fallacious and all-embracing expression. It will be difficult to get rid of, for the average keeper and sportsman is seldom clinically inclined and if he see his birds diseased or dead or dying, and they are grouse, he is content to put it all down to "grouse disease" and to leave it at that. But grouse suffer and die from many diseases. In a few dozen birds examined chiefly in Cambridge the following disorders were seen:—Pleuro-pneumonia in a bird which had lived long in captivity; pericarditis; necrotic changes in the liver; chronic diseases of the peritoneum; and a septic infection due to gangrene supervening upon a broken wing.

Sick and dying animals are apt to creep away into corners and hide themselves; thus it comes about that when these animals die patently and in large numbers, the public is apt to regard this mortality as due to some disorder peculiar to the animal in question, and the disease receives the name of the species which is afflicted. Hence we hear of such illnesses as "horse-sickness," "silk-worm disease" and "grouse disease."

The disorder which is usually associated with the too com-

[1] A Lecture delivered at the Royal Institution, February 3, 1911. The Lecture includes certain paragraphs reprinted from *The Times* and *The Encyclopædia of Sport*, by the kind permission of the respective proprietors and editors.

prehensive expression "grouse disease" was investigated by Klein some eighteen years ago, and in this lecture it will be called Klein's grouse disease. Klein found in the tissues of the bodies of birds that had been dead for some time a certain bacterium, whose nature and life-history he investigated. This bacterium is now recognised as one of the *Bacillus coli* group, a widely spread group of bacteria which are found universally in the alimentary tract and which rapidly invade the tissues of the body after death. At the time Klein was working bacteriology was comparatively a new subject, and this invasion by bacteria of the tissues at the time of and after death was not appreciated.

Klein's grouse disease was associated also with some congestion of the lungs; the windpipe was described as dark in colour, the air-sacs contained blood, in the cavities of the lungs blood or some granular exudation occurred, the liver and kidneys were congested and soft, and there was an exudation on the peritoneum. We now, however, know that many, if not all, of these appearances in the chief organs of the body are but normal post-mortem changes and occur sooner or later after death in birds which were perfectly healthy when killed. Another feature attributed to the Klein's grouse disease was that its onset was comparatively sudden, its course rapid, and according to all observers it attacked healthy and plump birds. The present Inquiry has not yet succeeded in coming across any sick or dead birds which are plump or in good condition. All the grouse, and they amount in number to nearly two thousand, which have been investigated, have been weighed, and in every case where there has been any disease there has invariably been wasting; the sick birds are always thin, have lost flesh, and are in a poor condition. One final feature of Klein's grouse disease is its seasonal incidence; usually it is said to occur with greatest virulence in the spring, to die down during the summer, and to recur in a less virulent form in the autumn. To this seasonal variation I shall return.

Klein's grouse disease is still a matter of inquiry. During the last five years, whilst the Commission has been prosecuting its inquiries, this "disease" has frequently been reported, but on investigation the characteristics enumerated above have not been present; still, the sportsman and the gamekeeper, who do not weigh their grouse and seldom their evidence, and who are but imperfectly acquainted with post-mortem changes, firmly believe in the existence of this "disease," and it may be that it really exists and that it is the misfortune of the Inquiry that in their researches the investigators have not come across it.

If we now turn from what must seem rather negative criticism to the more positive results attained by the investigation of the last four years, we may begin by pointing out some of the difficulties which confronted the Inquiry.

In considering exceptions it is so immensely important to know the rule. In studying disease our starting-point should be the normal, the healthy; yet until lately no one has studied the healthy grouse, and indeed it is almost impossible to find a normal grouse, *i.e.* one free from parasites. A grouse cannot express to us its feelings; the state of its tongue, the rate of its pulse, even its temperature tell us nothing because we have no norm and no means of estimating the extent to which a diseased bird has departed from the standards of a healthy grouse. The nature of the numerous kinds of blood corpuscles, which alter in proportion so markedly in animals when they become parasitised, was but a few months ago quite unknown, the "blood count" uninvestigated; in fact, the Inquiry started, as regards the cause and symptoms of the diseases which affect grouse, practically at "scratch." It was, of course, known that the suffering birds lose their activity and are more easily caught than healthy grouse; their flight is slow and limited in length; they are said to seek water; the "call" becomes feeble and hoarse; the feathers of the back and throat lose their lustre and become ruffled; the eye is dimmed. But these external symptoms may be associated with several diseases and diagnostic of none. Nearly all of them occur in the two diseases Coccidiosis and Strongylosis which, according to the Inquiry, are responsible for a very large percentage of deaths among grouse.

Each of these diseases is caused by an animal parasite, and the investigation of the parasites attracted the attention of the scientific advisers of the Inquiry from an early date. I am afraid that in describing these organisms I shall have to use some rather long words; in extenuation I can only say, slightly altering Captain Kedgick's retort to Martin Chuzzlewit, "Well! I didn't fix the zoological language, and I can't unfix it, else I'd make it pleasant."

Five years ago we knew two internal parasites of the grouse (endoparasites) and two or three parasites which live outside the skin (ectoparasites). At the present time we know that grouse, like other animals, have a considerable fauna living both in and on them. They are in fact not only birds, but in a small way aviating Zoological Gardens. The scientific members of the Inquiry have recorded eight different species of insect or mite living either amongst the feathers or on the skin of the bird or in other ways associated closely with the grouse, and no fewer than fifteen animal parasites living in the blood, the alimentary canal, the lungs, or other organs. Some of these are negligible. They either exist in too small numbers or infest but a very small percentage of the birds; others, however, are found in about 95 per cent. of the cases investigated and two at least are associated with grave disorders which often terminate in death.

The interest of the insects and mites which live on the skin of the bird is that these very likely form the second host of the tape-worms, which undoubtedly do a certain amount of harm to the lining of the alimentary canal. There are, for instance, a couple of species of bird-lice, lively little creatures, which take cover amongst the small feathers—which, by the way, form their arid diet—like startled deer in the undergrowth of a forest. Few grouse are free from these bird-lice, perhaps hardly 10 per cent., and the number on each bird is to some extent a measure of its ill-health. On a healthy grouse perhaps but two or three are found. They are animals with stout and powerful jaws, which they use to bite off the barbules of the feather or the finer plumules which form their sole nutriment. What fluid they obtain to moisten this somewhat dry nutriment is not apparent, but the animals are active and by no means so easy to catch as one at first thinks. They lay very beautiful eggs attached in small groups to the base of the after-plume of the feather or between it and the main shaft. The young hatch out as miniatures of the parents and there is no metamorphosis. The same species occurs on the Black Grouse and upon the Willow, or Hazel Grouse. On a piner these bird-lice increase enormously in number, and their numbers to some extent serve as a measure of the gravity of the disease. Both of these bird-lice cast their skin several times; the exact number of times is, however, not known but cast skins are frequently met with. The young birds are probably infected with these ectoparasites whilst in the nests, the bird-lice falling from one bird to another when they are contiguous. They have also been known to cling to the grouse-fly and in this manner may be transported to a new host. In no case was any specimen of either of these two species found in the crop of the grouse.

Two fleas are found on grouse, one rare but the other is a well-known bird-flea which has also been found in the nest of the hawfinch, the dipper, the blackbird, the moorhen and others. Since it is known that a certain dog-flea is the second host of one of the cestodes of the dog and a rat-flea of a tape-worm of the rat, it seems possible that one of these fleas may be the intermediate host of one of the chief worms which infest the alimentary canal of the grouse. We have, however, not succeeded in finding the cysts, neither have we found specimens of the flea in the crop of the bird.

Then there is a tick, the common rice- or dog-tick, usually attached below the jaw of the bird or to the eyelid or to some other position where the beak cannot reach it. Ticks are responsible for the transference of a very fatal epizootic termed *Spirillosis* in fowls in the Sudan and for numerous other diseases which afflict man and cattle throughout the world, but ticks are not common on the grouse and the Inquiry has as yet traced no disease to them. In parts of Ross-shire, however, especially in certain woods, these ticks are said to be extremely numerous and the keepers aver that they frequently kill off large numbers of black-game. They are commoner during the spring and early summer but usually disappear at the beginning of July. Curiously enough a common cheese- or flour-mite was from time to time found in considerable numbers on the skin of the grouse and apparently these mites sucked the blood of their host for their alimentary canal contained red food.

Finally, there are a couple of true flies, the well-known grouse-fly which is apt to crawl up the sleeves of those who handle grouse in the early autumn. The grouse-fly belongs to the same group as the horse-fly and the sheep-tick. The latter, however, has lost its wings and burrows in the wool of the fleece. Most members of the family to which this grouse-fly belongs live upon birds; it particularly frequents swallows and other allied species. Recently Dr. Sharp has pointed out that the grouse-fly (*Onithomyia lagopodis*) is distinct from the ordinary bird-fly (*O. avicularia*). The habits of the grouse-fly are difficult to investigate. It is believed to suck the blood of the grouse, and very probably inoculates the bird with some of the protozoa which infests its blood. The adult or imago burrows amongst the feathers of the bird, and any one handling grouse during the late summer is apt to disturb a fly or two. Their feet, although large, are very beautiful. Each is provided with a pair of most powerful hooks. Altogether, these insects have a sinister aspect and they are very repellent to people who do not like flies. The grouse-fly occurs very frequently in larders where freshly killed grouse have been placed and after a short time they readily leave their dead hosts and accumulate on the windows. Like the fatal tsetse-fly of Africa, which conveys sleeping-sickness, they lay no eggs but produce one larva or maggot at a time and this immediately turns into a pupa. The pupæ of the grouse-fly, usually found in the nest of the grouse during August and September, are black, shiny, seed-like-looking objects. Probably each pupa takes some three-quarters of a year to develop into the adult fly and the latter disappears from October until June. There is thus a certain tragedy in the life of these insects. No parent ever sees its offspring, no offspring has ever known parental care. We have never found one of these flies in the crop of the grouse, nor have we succeeded in finding cysts in the bodies of flies which were broken up, or teased up, or cut into sections.

Finally there is another fly whose larva lives in grouse droppings. All these creatures have been carefully searched for the larva of the grouse tape-worms but so far with no definite success.

Of the fifteen endoparasites but two or three demand attention; the others are comparatively rare or innocuous, and some, such as the gape—or forked—worm so fatal to pheasants, are not normally parasites of the grouse. Occasionally by some accident they get into the wrong Paradise.

Some of these endoparasites, which live inside the body of the grouse, are responsible for the illnesses from which grouse suffer. Any attempt to control their number and their activity must depend on our knowledge of their life-history, hence the stress which has been laid on the external parasites which may function as the second or larval host of some of them.

At the time the present Inquiry commenced to inquire there were but two worms described as being in the alimentary canal of the grouse—the large tape-worm which lives in the small intestine all the year round, known to every sportsman, and a slender thread-worm which inhabits the paired cæca or blind-guts, which are unusually large in the grouse and play a very important part in its digestion. The latter worm under certain conditions, and when present in considerable numbers, is associated with one of the two diseases which have especially attracted the attention of the Inquiry.

*Davainea urogalli* (Modeer).—Of the three tape-worms that are found in the grouse, this species is by far the largest and by far the most common. It exhibits little seasonal variation and is found in considerable numbers all the year round. The birds become infected at an early age.

*D. urogalli* is normally found in the small intestine, though sometimes parts of it are found in other portions of the alimentary canal. As a rule, three or four individuals are met with. At other times, especially in weakly birds, there are dozens and these fill up the lumen of the intestine to such an extent that it is difficult to see how food can pass along.

*D. urogalli*, like most cestodes, produces a very large number of eggs at any one time. It may be, at a rough estimate, at least 100,000 but this figure is no measure of the reproductivity of the cestode, because as fast as new segments break off at one end new ones are formed just behind the head and the animal goes on producing new segments very much in the same way as a recurring decimal reproduces ciphers. Hence the eggs of this cestode must be scattered in countless millions all over the grouse moors. They are probably eaten by some insect or land mollusc and in the body of these invertebrates change into the cysticercus or larval stage.

The popular notion that grouse do not eat animal food is entirely wrong. For the first three weeks of the bird's life the greater part of its diet consists of insects or arachnids and from the crop of the first grouse I ever dissected I took six saw-fly larvæ, eight caterpillars of a Geometrid moth, one caterpillar of a smaller moth, two small Tineid moth and a number of Hemipterous insects resembling the frog- or cuckoo-spit, a fly, two specimens of plant-lice, one small spider, and the remains of four slugs. The gizzard of the same animal contained, in a more broken-up condition, two or three dozen larva of saw-flies and moths, some young Hemipterous insects and the pupa of two true flies.

In searching for the larval or cysticercus stage of these and the other cestodes, we have examined a considerable number of insects which occur commonly on grouse moors. We have also carefully searched the bodies of many fresh-water crustacea which abound in the pools and tarns from which grouse drink but hitherto our searches have met with no success. One specimen of cestode which infests the common fowl is said to have its second host in several species of the slug *Limax* but we have not succeeded in finding cysts of either form of tape-worm in this slug.

But besides the large tape-worm (*Davainea urogalli*), which was described by Baird fifty-seven years ago and the thread- or round-worm (*Trichostrongylus pergracilis*), described by Cobbold thirty-seven years ago, we have two other species of tape-worm and four other species of round-worm. One of the former is negligible, the other, the transparent tape-worm (*Hymenolepis microps*), is however to some extent associated with disease. These worms, like the larger species, may exist in incredible numbers in the duodenum or that part of the alimentary canal which comes just after the gizzard: yet they are quite invisible whilst alive. The contents of the alimentary canal in this region resemble a thick *purée*, which, on the addition of some fixing reagent, resolves itself into an inextricable tangle of fine threads, each representing a tape-worm. The head of these worms is hidden away in the folds of the lining mucous membrane of the alimentary canal and undoubtedly they do something to interfere with its continuity. A certain amount of inflammation is set up. We have no sure information as to the second host of this cestode but as a general rule the cysts of the genus *Hymenolepis* live in some insect or centipede, as is shown by the fact that the adults exist in bats, insectivores and insectivorous birds. Tape-worm cysts have recently been found in a flea by Professor Minchin and these cysts have been shown by Mr. Nicoll to grow into *Hymenolepis diminuta* in the intestine of the rat. Hence the suggestion, first made by Dr. Leiper, that the fleas of the grouse may be the second host of *H. microps* is well worth following up.

It is curious to note that this tape-worm disappears during the winter months, a fact which may afford some hint as to its second host. The large tape-worms, on the other hand, remain all the year round and must be of quick growth, for they are found 35 cm. in length in a young grouse but three weeks old.

Three other round-worms have also been shown to exist in the grouse. One of these in the duodenum may prove of importance. This species (*Trichosoma longicolle*) is allied to a form which lives in the human appendix and at times is the cause of appendicitis.

### Coccidiosis

Besides the worms we have in the grouse seven distinct unicellular or protozoan parasites which live in the intestines or in the blood of the grouse. Most of these are uncommon and comparatively harmless, but one, a *Coccidium* (there is no more popular word for it), is the cause of disease in the grouse chicks.

Since this disease was first found in the young grouse much has been written about it in the newspapers and in nearly every case the writer has taken the *Coccidium* to be a *Coccus*. Now a *Coccus* is no more like a *Coccidium* than a crocus is like a crocodile. The *Coccus* is a bacterium, a vegetable and it has a simple life-history, the *Coccidium* is a protozoan, an animal with as we shall see a very complicated life-history.

Dealing first with the Coccidiosis:

One of the aims of grouse-preservers is to have numbers of healthy young grouse chicks in order to produce stocks of strong birds. Bad seasons for grouse are partly due to epizootics among the young broods in the spring and the chief cause of mortality among grouse chicks is a small, one-celled microscopic animal parasite, *Eimeria* (*Coccidium*) *avium*. This parasite penetrates the lining membrane of the gut of the bird and gradually destroys it, thereby setting up digestive troubles in the form of intestinal inflammation (enteritis), accompanied by acute diarrhœa, which usually terminates fatally. Grouse chicks are most susceptible to Coccidiosis during the first six weeks of their life and if they can survive this period unattacked they usually reach adult life.

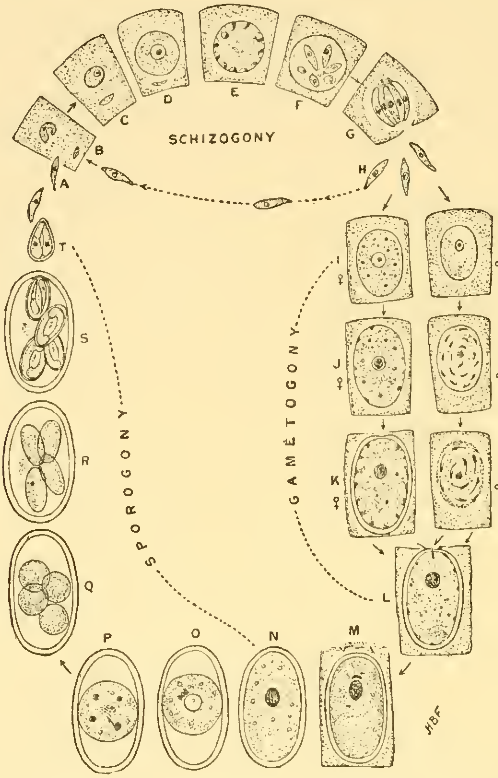


FIG. 1.—Diagram of the life-cycle of *Eimeria* (*Coccidium*) *avium*.

B–H illustrate the asexual reproduction (schizogony) of *Eimeria* (*Coccidium*) *avium*; I–L illustrate the formation of sexual forms (gametogony) of *E. avium*; N–T show the formation of the oöcysts (cysts) and sporocysts (spores) of *E. avium* (sporogony); A, sporozoite or primary infecting germ which is penetrating an epithelial cell of the duodenum of the host; B, sporozoite curving on itself before becoming rounded within the host-cell; C, young, growing parasite; D, fully grown parasite; E, parasite (schizont) with its nucleus divided (seen in transverse section); F, parasite forming merozoites, *i.e.* daughter forms of the dividing parasite; G, group of merozoites, arranged like the segments of an orange; H, free merozoites. I ♀, young female mother cell with coarse granules; I ♂, young male mother cell with fine granules; J ♀, growing female mother cell; J ♂, young males beginning to form in mother cell; K ♀, female gamete that has formed a cyst-wall for itself, but left a thin spot for the entry of the male; K ♂, fully formed males attached to their mother cell; L, union of ♂ and ♀: one male only unites with the female, the others are excluded; M, further stage in fertilisation; N, fertilised oöcyst (cyst) with its contents filling it; O, oöcyst (cyst) with contents forming a central mass: many such cysts are seen in the droppings of infected grouse; P, oöcyst (cyst) showing first stage in division into four; Q, oöcyst with four daughter spores forming; R, oöcyst with four fully formed sporocysts (spores); S, cyst with four sporocysts (spores), in each of which two sporozoites are formed; T, free sporocyst in which the sporozoites have taken up the most suitable position for emergence.

The disease—we may call it Coccidiosis for short—caused by this species of Coccidium (*Eimeria avium*) is brought about in this way. The grouse moor is simply peppered over with millions of oval cysts, or capsules, which represent the free-living stage in the life-history of the Coccidium. Each cyst is very resistant to changes of temperature and moisture, and can live for a long time. The cysts pass with the food or the water or the grit into the alimentary canal of the bird, and in the duodenum the thick cyst wall is dissolved and four spores emerge. Now, when one reflects on the thousands of cysts which are at times taken up by the grouse, one can readily understand that the presence of these numerous spores boring into the epithelial cells ultimately destroys the lining

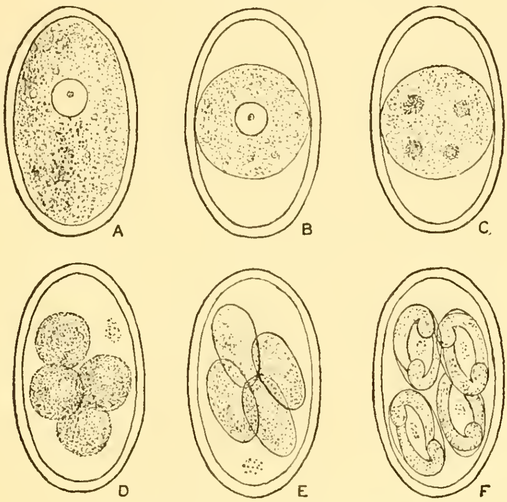


FIG. 2.—Oöcysts (cysts) of *Eimeria* (*Coccidium*) *avium.*

A, oöcyst (cyst) with contents completely filling it; B, oöcyst with contents concentrated to form a central mass; C, commencement of division of the oöcyst contents; D, oöcyst containing four rounded masses that will become spores; E, oöcyst containing four sporocysts (spores); F, oöcyst containing four sporocysts, in each of which two sporozoites have formed.

of the duodenum, where in fact the most active digestive processes are carried on in the normal bird. Furthermore, the Coccidia multiply in the intestine and the resulting progeny attack new regions of the alimentary tract, especially the cæca, which become swollen and inflamed. After a time some of them produce small forms (males) and others change into large forms (females); these two forms fuse and the resulting stage is the cysts mentioned above, which, passing from the body, infects the whole moor. There are many details omitted in this short abstract of the complicated life-history of this parasite, which have been worked out in the grouse by

Dr. Fantham at Cambridge; but enough has been said to show the nature of the disease, which is well known to occur in several other animals. There is a Coccidiosis of the rabbit and a very fatal form attacks pigeons, fowls and pheasants, and the grouse Coccidium, if administered to chickens, will set up the disease in fowls.

To repeat a little, for the life-history of this parasite is very complicated:

The chief source of contamination on the moors is the droppings of other diseased grouse. The droppings contain thousands of cysts (oöcysts) or spores of the parasite and these spores, with their hard coats, are extremely resistant and can endure for very long periods without the death of their contents, which gradually divide to form four smaller spores inside. The spores are scattered over the moors by the action of the wind and rain and, alighting on the heather or in the tarns of the moors, are taken up by the grouse in their food or drink. When the cysts are swallowed, they enter the gizzard of the bird and pass unchanged into the first part of the intestine, called the duodenum. Here the pancreatic juice is poured into the intestine, to aid in digestion, and under its influence the cyst-wall is softened and dissolved, and the four small spores (contained within the ripened spore or oöcyst) are set at liberty. Each small spore contains two active motile germs or sporozoites, which emerge from the softened spore-case and proceed to penetrate the epithelium of the duodenum. The young parasites ultimately cause the destruction of the lining of the first part of the small intestine —the region where, normally, the most active digestive processes occur. The *Coccidium* parasites multiply in the duodenal epithelium and then invade the cæca or "blind-guts," with disastrous results.

Sooner or later a limit is reached, on the one hand, to the power of the grouse chick to provide nourishment for the parasites, and on the other hand to the multiplicative capacity of the parasites themselves. The *Coccidium* then begins to reproduce sexually. Many small male parasites are produced, together with larger food-containing female *Coccidia*. The male and female parasites conjugate and then encyst, bursting through into the cavity of the gut and giving rise to the spores found in the cæcal droppings on the moors.

So far as the grouse chick is concerned, the formation of Coccidian cysts means either recovery or death. If the infection of the parasites has been a heavy one and multiplication of the parasite has proceeded apace, then the destruction of the intestinal epithelium has been so great that death of the grouse chick results. If, on the other hand, the epithelium of the intestinal wall has not been too much destroyed—fewer Coccidian parasites having been present—then the gut-epithelium may slowly regenerate and the young bird gradually

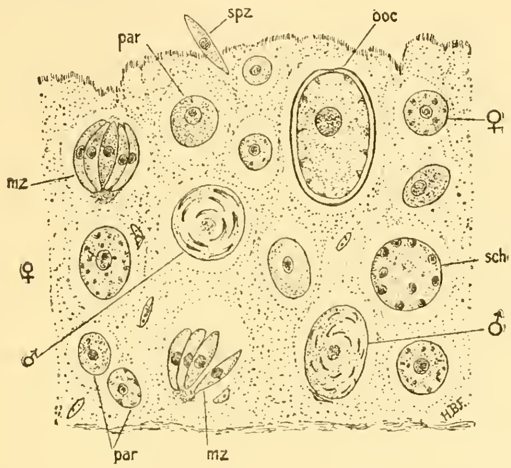


FIG. 3.—Small piece of the epithelial lining of the gut of an infected grouse chick, showing various stages in the life-history of *Eimeria* (*Coccidium*) *avium*, parasites of different ages being present.

*par*, parasite; *spz*, sporozoite or primary infecting germ; *sch*, schizont or dividing form; *mz*, merozoite or daughter germ; ♀, female Coccidium; ♂, male parasite; *oöc*, mature female, ready for fertilisation, with oöcyst wall formed round it.

recover and improve in condition after the Coccidian spores have been passed out with the cæcal droppings.

As a rule grouse are most susceptible to Coccidiosis during the first six weeks of their life and if they can survive the dangers of this period they usually grow up. The chief external symptom of the disease is diarrhœa, the legs show weakness, and the feathers, especially around the legs, are in poor condition, flight is feeble and the bird loses weight. Internally the alimentary canal is inflamed and digestion greatly impaired; perityphlitis is set up around the cæca, which become greatly enlarged. The blood corpuscles also undergo marked alteration and an anæmic condition prevails. Further, the destruction of the lining wall of the alimentary tract allows the escape of bacteria which are all very well in their place —*i.e.* the cavity of the intestine—but which are apt to set up trouble when they make their way into other tissues. This is however but a subsidiary matter; the real injury caused by the *Coccidium* is the destruction of the lining membrane of the alimentary canal.

Coccidiosis may be spread from moor to moor by the agency of flies. The maggots of certain flies readily eat the cysts, and it has been shown both experimentally and on the moor that the cysts pass through the bodies of both maggot and fly undigested and unharmed.

Several other one-celled microscopic organisms or protozoa, besides *Eimeria* (*Coccidium*) *avium*, as is mentioned above, have been found in the intestines of grouse and also in their blood, but in no case, so far, has any of these parasites been shown to have a markedly dangerous effect on the bird harbouring it. Some of these parasites, in fact, occur in perfectly healthy grouse and apparently are almost innocuous.

## Strongylosis

The second disease which the Inquiry has found responsible for grouse epizootics observed between 1905 and 1910 is one to which Cobbold drew attention in 1873, though he attempted little in the way of pathological investigation. According to him it is caused by the presence of a round-worm, now known as *Trichostrongylus pergracilis*, in the cæca. We may call the disease "Strongylosis of the grouse." The worms are minute, transparent, very slender, a little less than half an inch in length and they may exist in enormous numbers, 10,000 occurring in the two cæca of one bird. They are about equally divided between the two blind-guts. We may recall the fact that in the grouse the cæca are of unusual size and that in these birds the digested food is absorbed in this region of the alimentary canal alone.

The worms seem to be most numerous at the proximal end of the cæca, and when they exist in large numbers the contents of these most important diverticula of the alimentary canal become hard and very adherent to the mucous membrane, forming whitish patches when seen from the outside. After washing away the flocculent matter, which can only be effected with time, the mucosa frequently appears reddened. There was no reason to believe this inflammatory state is due to post-mortem changes. The ridges which run along the walls of the cæca become thickened, the villi, as a rule, greatly hypertrophied (in other cases atrophied) and both villi and ridges were embedded in the above-mentioned cementing material and seem to be composed of a mixture of mucous and granular débris. Numerous Strongyles were seen adhering to the mucous membrane and seem in places frequently to penetrate the villi. In some of the most serious cases the ridges were found to resemble masses of coral with cave-like depressions between the individual villi which frequently ceased to be distinguishable; from these depressions one or more Strongyli protruded. In section the epithelium was seen to be hypertrophied. The muscular wall suffered from an infiltration of fibrous tissue. The fat which normally lies at the bases of the ridges in a healthy grouse had disappeared and the blood-vessels showed thickenings of their walls. The connective-tissue base of the ridges is greatly increased and the vessels and the villi were dilated and probably increased in number. In most places the connective tissue contained a large number of cells probably inflammatory in origin, and in some cases fibroid change was taking place. The epithelium was proliferated and thrown into folds. The lymphoid follicles were indistinguishable.

The general condition of a grouse suffering from Strongylosis shows evidences of a chronic inflammation of the alimentary canal leading to fibrosis. The *Trichostrongylus* in some cases penetrates to the deepest portions of the crypt. The epithelium lining these depressions when the round-worm is adjacent to the walls of the intestine has greatly altered and there is a marked increase in the fibrous tissue at its base.

In fact we have, as Dr. Cobbett and Dr. Graham Smith, who have so ably worked out the pathology of Strongylosis and whose results have appeared in the *Journal of Hygiene*, say, "a chronic inflammation leading to fibrosis." This portion of the alimentary canal is, in Sydney Smith's words, both "vext" and "agitated."

It thus appears that the presence of these Strongyli leads to a chronic inflammation of the cæca and to fibrosis. The normal functions of these very important organs, the cæca, are interfered with and the tissue lining them is destroyed. The bird wastes away and ultimately dies. But there is another factor in the case. When the degree of infection surpasses 1,000 Strongyli in each cæcum, there is no doubt a larger increase in the *Bacillus coli* in the liver and in the lungs and other organs. These bacilli do no harm whilst free in the alimentary canal but when they pass the mucosa and reach the other tissues of the body they undoubtedly serve to set up various disorders.

But the presence of these worms and also, I believe, to a minor extent the presence of the transparent tape-worm in the duodenum, has a further and indirect ill-effect on the grouse. When hand-reared birds which were free from *T. pergracilis* were investigated, practically no bacilli were found in the liver or other tissues of the grouse. When the birds harboured from 100—1,000 round-worms, bacilli occurred in the tissues of about 50 per cent. of the birds—bacilli let out from the cavity of the intestine "by a worm's pin-prick," to quote Browning's "Lovers' Quarrel." When over 1,000 or more worms were present, the bacilli, with one exception, were found in the liver and in other organs of the body in 100 per cent. of the birds investigated. The exact relation of these bacilli to the sickness of the bird is still a matter of inquiry. It seems as if they are soon absorbed and that no specific disease is traceable to them, but if they exist in numbers their products must exert a harmful influence.

The existence of disease caused by the passage of these bacilli through the walls of the alimentary canal through lesions caused by tape-worms is less well established than through the disorganisation of the lining membrane of the cæca caused by *T. pergracilis*. On the other hand, one must not overlook the fact that *Hymenolepis* is especially numerous in the spring and autumn months, during which the greatest mortality takes place, and is absent during the winter when the birds are comparatively healthy.

It is seen that the small number of colonies of *Bacillus coli*, which can be cultivated from the tissues of a diseased grouse, points to the fact that these bacteria do not multiply to any extent in the tissues. Hence it would seem that no specific disease is caused by the infection of these bacteria. The toxic products of the bacilli, however, may be harmful, and should the bacilli exist in great numbers, there is little doubt that toxins would be produced which would have a very deleterious effect on the health of the bird.

But let us leave the bacteria and get back to the round-worms. How do they get into the grouse? Between 95 and 100 per cent. of birds on different moors contain these worms. There may be as many as 10,000 in round numbers in one grouse, about equally divided between the two cæca. Each female worm lays hundreds of eggs, which are constantly passing out of the bird's body and lie scattered all over Scotland. These eggs give rise to larvæ in about two days, the larvæ surround themselves about the eighth day with a capsule or cyst and undergo "a rest cure." After a period of quiescence they quickly change into a second and active larval form, which in wet weather—a not unusual atmospheric condition in Northern Britain—writhe and wriggle and creep and crawl until they attain the stem and the leaves and the flowers of the heather. Here these larvæ wait patiently until a grouse consumes them with the heather tips and then, once inside the alimentary canal, they become adult, make their way to the cæca and in four days ripe eggs are again infesting the moors.

It is recorded that Prince Bismarck once said to Lady Randolph Churchill, "Have you ever sat on the grass and examined it closely? There is enough life in one square yard to appal you."

It has always seemed to me a strange thing for the Prince to have said. To begin with, throughout his long life he had shown but an imperfect sympathy with the lower Invertebrata, and then, again, he was a man not easily appalled: but the saying is perfectly true. It is difficult for the layman to grasp what is going on in and on the soil and on the plants which it supports. Suppose we could by means of a gigantic lens magnify a square yard of a grouse moor one hundred times. The heather plants would be as tall as lofty elms, their flowers as big as cabbages, the grouse would be about six or seven times the size of "Chantecler" at the Porte St. Martin.

Creeping and wriggling up the stem and over the leaves and gradually yet surely making their way towards the flowers would be seen hundreds and thousands of silvery-white worms about the size of young earth-worms. Lying on the leaves and on the plant generally would be seen thousands of spherical bodies the size of grains of wheat, the cysts of the *Coccidium*, and on the ground and on the plants as large as split-peas would be seen the tape-worm eggs patiently awaiting the advent of their second host. It is perhaps a picture which will not appeal to all but yet it represents what unseen and unsuspected is always going on on a grouse moor.

Two other points remain, the seasonal character of the disease and whether any means can be suggested to check either Coccidiosis or Strongylosis or both.

"Grouse disease" is always said to be at its worst in the spring months, to decline during the summer and to recrudesce in a milder form in the autumn. Coccidiosis undoubtedly is a spring disease; it attacks the chicks and if they survive the first six or seven weeks of their life they usually live to grow up. This disease certainly abates during the summer but it does not recrudesce during the autumn. Strongylosis also occurs most virulently in the spring, when the birds are exhausted by a winter of semi-starvation and the female especially by the demands made on her by egg-laying; it is also prevalent in autumn but the worst cases have by this time presumably been killed off and those not so heavily afflicted are still struggling to survive. It is not as a rule reported during June and July but very few know what happens on the moor during these months. The grouse are almost unseen, their state of health is unknown. This again is a matter for further inquiry but at present the view that "disease" dies down during the summer has little but negative evidence to support it. It probably lingers on, gradually lessening in intensity until the near approach of August 12 again attracts the attention of the moor owner and the sportsman to his birds.

With regard to the prevention of the disease, a hopeful view can be taken. Intelligent management has already diminished and in certain cases almost, if not quite, eliminated the danger of disease, and this without resort to outside aid or scientific advice. There is reason to hope, with a clearly defined objective and a more general realisation on the part of moor owners not only of the immediate cause of grouse disease but also of the contributory conditions leading thereto, that the best methods which obtain at present in moor management will be more widely adopted and that the suggestions of new lines of experiment which will be put forward in the final Report will be followed up.

As to the stay of the disease when fully established in the bird no practical remedial measures can at present be suggested. The apothecary is to the sick bird of no more importance than Seneca. Nothing can be done "pour soulager les entrailles" of the patient.

One word of conclusion to recall the fact that the Grouse Inquiry has not issued its final Report, and this article cannot represent its final findings; research is still going on. All I have attempted to do is to summarise the existing knowledge of the diseases of the grouse, which have never before been so specifically defined. When the final Report is published remedial measures will be suggested. This Report will also contain chapters on moor management, on the economics of grouse-shooting, on the life-histories of the many parasites associated with the grouse and on many other matters connected with the well-being of the bird.

# THE RÔLE OF REFLEX INHIBITION

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It has been said, "The end of man is an action, not a thought." The aphorism though striking is but half-true, for surely thought *is* action. Yet that *obiter dictum* of Thomas Carlyle's states with curious fidelity the line which Physiology must follow in its study of nervous reactions. The nervous system, driven itself by the external world, drives and controls the organs of the body, and through these alone do its inner workings find expression. These expressions constitute the whole practical purpose or "end" of the nervous system. In the outcome of nervous reactions it is with the material expressions and with no other that Physiology can properly be said to deal.

## 1. Reflex Inhibition

Among the organs through which the inner workings of the nervous system find expression are glands and muscles, especially the latter. And pre-eminent among muscles are those clothing and actuating the bony levers of the bodily frame and therefore called skeletal. So completely are these muscles subjected to the nervous system that under natural conditions they enter into action only when the nervous system calls on them. A skeletal muscle after severance of its nerve, separating it from all commerce with nervous centres, lapses into paralytic quietude and its functional inactivity becomes so profound that in many cases its structure becomes in due course hardly recognisably muscular at all.

The contraction of skeletal muscle has two main forms: the mild, steady, more or less continuous form called *tonic*, which executes postures; the transient, vigorous form (*phasic*) sometimes called *alterative* (von Tschermak) which executes movements such as those of the chest in breathing and those of the limbs in all their varied exercise. Both these forms of contraction are excited and maintained by impulses issuing from the nervous centres and descending to the skeletal muscle *via* its motor nerve. So long as the motor nerve is not active the skeletal muscle does not contract and the one function of these efferent nerves is to activate their muscles.

But in the case of muscles other than the skeletal this does not hold good. The contraction of these other muscles is not so dependent on the activity of the central nervous system. The activity of the heart muscle for instance is largely independent of the central nervous system. The beat continues when the heart is completely separated from all connection with the central nervous system, *e.g.* when altogether removed from the body. Similarly, the intestinal and other visceral muscles continue their rhythmic contractions after complete severance from the central nervous system. The heart and these visceral muscles are it is true supplied with nerve-trunks from the central nervous system, and through those trunks the central nervous system can and does influence them. It can augment their contractions, and in so far its influence upon them resembles its influence upon the skeletal muscles. But to the cardiac and visceral muscles—and this is a significant point of difference from skeletal muscles—the central nervous system supplies efferent nerves not merely of one kind, but of two. To these muscles with autochthonous and quasi-autonomous power of contraction the central nervous system supplies not only driving, *i.e.* excitatory, nerves, but checking, *i.e.* inhibitory, nerves. It was on one of these latter, namely the check-nerve of the heart, the *vagus*, that the exercise of direct inhibitory power by nerve was first discovered. The vagus has power to slacken or even temporarily suppress the heart's beat. Knowledge of inhibition as a physiological phenomenon dates substantially from the discovery of this fact by E. H. and W. Weber, in the year 1846.

Search for inhibitory nerves similarly controlling the contraction of the skeletal muscles has naturally been prosecuted often. Invertebrate instances of such nerves have been found in the claw-muscles of Arthropods. But in Vertebrates none such have been discovered. Yet it were strange did that marvel of physiological mechanism, the Vertebrate nervous system, have no means of restraining the contractions of its skeletal muscles. The system of the skeletal musculature is complex and the confusion and wasteful expenditure of energy which would ensue were its opponent parts to obstruct each others' efforts would seem foreign to Nature's usual harmonious económy. Inhibitory as well as excitatory control of the separate muscles would *a priori* appear a necessity. In recent years evidence has been forthcoming that such inhibitory control does exist and is habitually exercised. Of this control and its employment this article has briefly to speak.

The contractions *tonic* and *alterative* of the skeletal muscles are, as said above, always directly incited by nervous discharge from the central nervous system. If that discharge ceases the contraction of the skeletal muscle subsides. To curb the contraction of a skeletal muscle all that is necessary therefore is to quell the discharge of the motor centre, which, lying in the central nervous system, emits impulses to the muscle. Each muscle has a motor centre of its own which presides directly over that muscle and is the only channel by which motor impulses can reach it. But upon the motor centre, on the other hand, many nerve-paths converge, transmitting to it nervous impulses from various other centres and regions. Of these nerve-paths some possess the power of exciting and activating the motor centre in question; others of inhibiting it and throwing it out of action. These latter curb the contraction of the muscle by quelling the discharge of motor impulses from the motor centre. The motor centre lies as it were an instrument passive in the hands of these opposing forces of excitation and inhibition exerted by the nerve-channels which reach it. Sometimes the one influence and sometimes the other influence is dominant; often the two are simultaneously in action, and then their opposed influences partially cancel in proportion to the relative intensities of their respective stimulations.

Just therefore as the mechanism intrinsic in the heart and driving its beat is controlled by a double set of nerves passing from the central nervous system to it—one set quickening and increasing the beats, the other slowing and lessening them—so the motor centre of each skeletal muscle is the field of meeting of two sets of opposed nerve-channels which influence it in opposed directions. But in the latter case the motor centre, the field of collision of the two opposed forces, lies *within the central* nervous system; the nerves which reach it, instead of being centrifugal and passing out of the central nervous system, are centripetal and pass in. They are afferent and conduct their impulses into the central nervous system, passing either fairly directly to the motor centre they influence or continued by secondary and relay paths which ultimately reach it, running sometimes long distances within the central nervous system from one part of it to another. The central nervous system itself may be likened to a great telephone exchange. Through it every entrant line can influence practically any of all the outgoing lines of the system, establishing temporary communication by sets of relay junctions often very complicated. All the channels which play upon the motor centre of the skeletal muscle excite or inhibit it through this central exchange. They therefore obtain their effect over the muscle *reflexly*. Hence the study of their influence on the muscle falls wholly under the head of reflex action. Some of these reflex actions are comparatively simple, as in cases where the afferent nerve impinges fairly directly upon the motor centre itself; others are very complex, as where the afferent channel exerts its influence on the motor centre indirectly through some other centre or through a whole chain of centres. The solidarity of the nervous system is so great that the condition of higher nervous centres commonly affects the condition of the lower centres and among these latter especially the motor centres of the skeletal muscles.

A question of importance and one to which we have at present no complete answer is whether the motor centre of the skeletal muscle when no afferent channel is actually at play upon it, lies at functional rest or whether it then exhibits any *spontaneous* discharge of motor impulses. Has it an autochthonous activity like the heart or is it, when the centripetal channels that influence it are inactive, itself quiescent, emitting no discharge? The view generally taken is that it exhibits no spontaneous discharge of motor impulses. Certainly the so-called tonus of skeletal muscles is due in the great majority of cases to a *reflex* discharge from the motor centre, not to *spontaneous* discharge. Thus the tonus which maintains steady contraction of the anti-gravity muscles in the posture of standing (dog, cat) is demonstrably reflex. On the other hand careful experiments have failed to detect the reflex source of the tonus which in the bird keeps the wings folded when not in flight (W. Trendelenburg).

On the motor centre of each muscle every influence which is to affect the muscle must impinge. The motor nerve issuing from that centre to the muscle is the sole channel by which the condition of the motor centre can exert influence on the muscle. The motor nerve consists of nerve-fibres, each of which is a centrifugal thread from one of the motor nerve-cells composing the discharging apparatus of the centre. Whatever the seat and source of the various reflex actions which excite or inhibit the muscle, each and all such reflexes take effect upon it solely by affecting in the last instance the motor cell and motor fibre, and by initiating or increasing or by lessening or stopping a flow of nervous impulses down the structures to the muscle. Hence in the chain of nervous conductors which every reflex employs the motor cell and its fibre constitute the last link and every reflex reaching the muscle from whatever source must use them. For that reason the motor cell and its centrifugal fibre is called the *final common path*, and the entrance or mouth to this final common path lies in the motor centre. Indeed, the entrance or mouth of the final common path is nothing else than the place of linkage of the motor cell with the various afferent conductors which impinge upon it.

As to this place of linkage, the microscope shows that the afferent conductors there impinging upon the motor cell impinge upon it in two portions of its surface. The afferent conductors themselves are delicate nerve-fibres, end-twigs of stem-fibres from nerve-cells more or less distant. These end-twigs terminate as seen in stained specimens specially treated for the microscope in tiny bulbous swellings set close to the surface of the motor cell. They end both upon that part of the motor cell which is the so-called cell-body and since it contains the cell-nucleus is termed the *perikaryon*. They end also upon the surface of the short, branched, tapering extensions of the perikaryon, the so-called *dendrites*. With one part of the motor cell the afferent terminals have no direct relation, namely, with that long, thread-like extension from the perikaryon which constitutes the motor nerve-fibre and runs to the muscle itself. The meeting place of afferent conductor with motor neurone is called the *synapse* and there the two though not structurally continuous stand in functional connection. There states of excitation induced in the afferent conductor on reaching its terminals are transmitted across the synapse to the motor neurone. The term *synaptic conduction* is reserved for this process, by which the excitatory state is propagated from neurone to neurone; the word *cell-conduction* is restricted to the process of propagation of the excited state (nerve-impulse) along the length of the neurone (nerve-cell) itself, *e.g.* from its one end to its other. Synaptic conduction is intercellular, cell-conduction is intracellular. The propagation of a nervous impulse along a reflex arc involves both these processes, since a reflex arc is always a chain of separate neurones. As to synaptic conduction and cell-conduction, both are electrical processes involving movement of ions in the dilute solutions which lie within the neurones (Macdonald) and also bathe the neurone surfaces outside. Of the essential nature of inhibition and whether its action-point lies at the synapse or in the perikaryon, is intercellular or intracellular, in spite of ingenious researches (Verworn, F. Fröhlich) we remain still largely ignorant.

But while much is still obscure in regard to nervous inhibition, certain types of its action have in recent years become familiar to us and enable us to decipher certain of the biological purposes which it serves.

## 2. Grading of Muscular Contraction by Inhibition

Experiment shows that by appropriate grading of the reflex stimulus exciting its motor centre a muscle can be made to contract weakly or strongly according as the stimulus applied is weak or strong. Other conditions remaining unchanged, the intensity of the motor response follows the grading of the stimulus with exquisite fidelity. This gradation of reflex response is of fundamental importance in securing that the degree of muscular action shall be suited to the circumstance evoking it. It is an obvious element in the co-ordination of the motor reactions of the animal. Much of the varied intensity of contraction exhibited in natural actions of the musculature is doubtless a simple consequence of the degree of intensity, mild or strong, of the stimulus that evokes a reaction.

But there is another way in which the grading of muscular response is obtained, and this latter is probably the more common under the complexity of natural conditions. A weak discharge of impulses from the motor centre does not invariably mean weak stimulation of the excitatory afferents. The motor discharge may be weak, although the excitatory afferents are being excited strongly. This is because their influence on the centre may be counteracted by concurrent operation of inhibitory afferents. Under natural circumstances it is usual not for one stimulus, but for groups of stimuli, to play concurrently on the organism through its afferent paths. Their opposed influences collide. Now, the depressor influence of an inhibitory afferent is just as capable of graded intensity as is the pressor influence of its excitatory antagonist. If while a pressor afferent is acting on the motor centre a depressor afferent is stimulated (fig. 1) the motor discharge, as judged by the muscles' contraction, can be curbed to any desired amount by suitably grading the intensity of stimulation of the inhibitory afferent. Representing the influence of the pressor afferent by + and that of the inhibitory by −, the contraction resulting from concurrent stimulation of the two afferents appears as the algebraic sum of + and − quantities. The muscle excited through a purely pressor afferent exhibits for each intensity of stimulus theoretically but one particular grade of intensity of contraction. But under the less simple though more natural condition where several afferent channels of opposed effect are concurrently at work on the centre any particular grade of contraction may represent any one of many different combinations of intensity of opposed stimuli. Even where only two different channels are competing it is impossible to know what state of interaction the state of the muscle represents except by quantitative observation of one at least of the stimuli.

The two processes of reflex excitation and reflex inhibition are to be regarded as co-equal in their importance for co-ordination. They are commonly combined, in the sense that the accuracy of a muscular contraction delicately adjusted to the extent and force of the movement which is required is usually a result of the graded combination of both inhibitory and excitatory influences coalescing upon the motor centres involved. Each therefore is not only itself capable of finely graded adjustment of intensity, but each forms also a means of finely grading the intensity of the other (fig. 2). Reflex inhibition with its gradation constitutes therefore a main means for co-ordinating to the momentary requirements of the organism the intensity of activity of its chief motor machinery, the skeletal musculature.

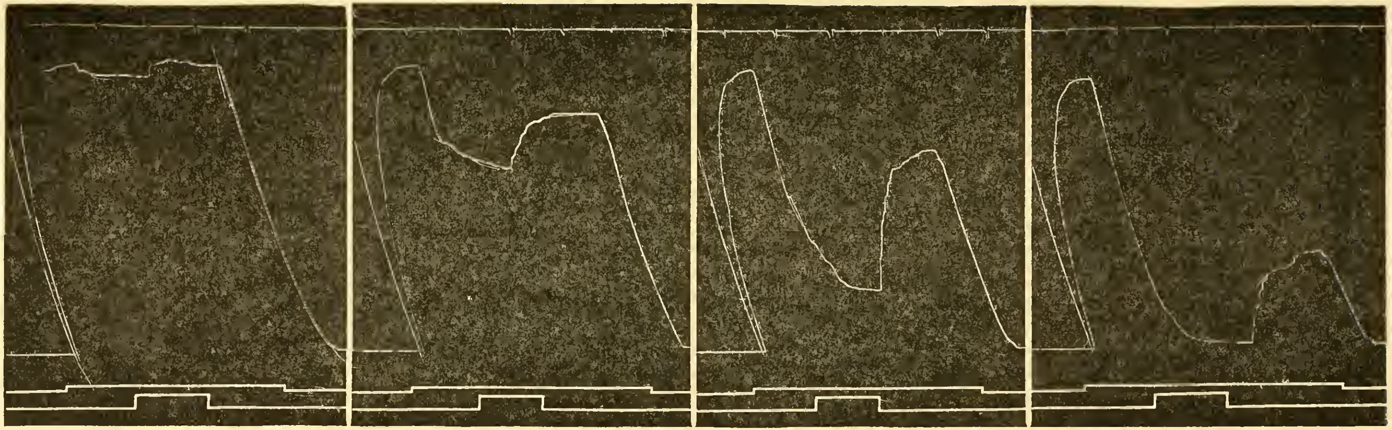

FIG. I.—Reflex contractions of semitendinosus, a flexor of knee, evoked by stimulation of ipsilateral peroneal upper signal line nerve.

The contraction is inhibited in each observation by an intercurrent stimulation of the contralateral popliteal nerve, lower signal line. The intensity of the contralateral stimulus—the inhibitory—is progressively increased from observation I to observation IV, while the intensity of the excitatory stimulus is kept the same for all the observations. Cat, decerebrate preparation. Time in seconds above.

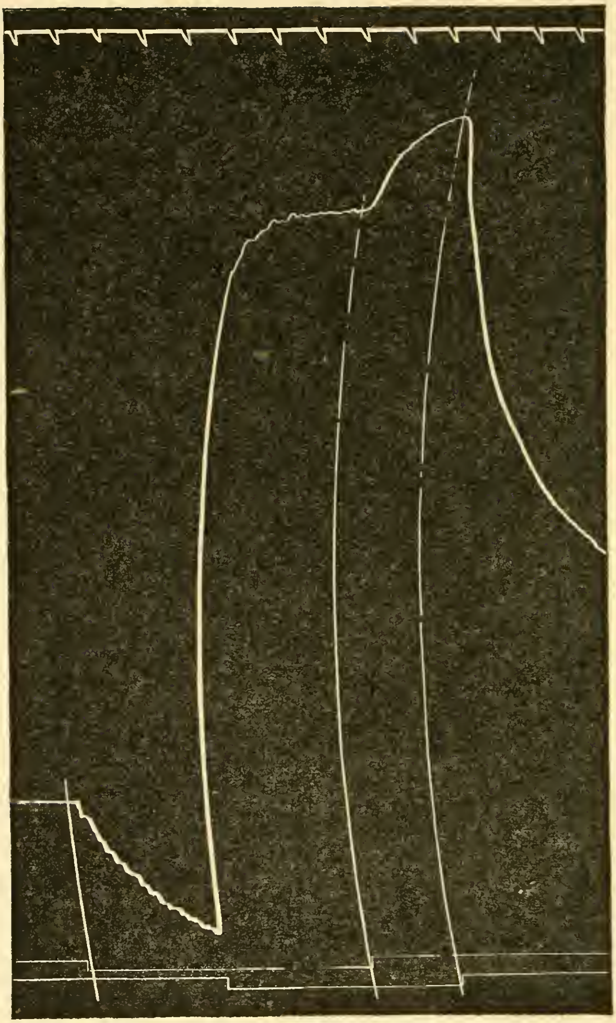

FIG. 2.—Reflex contraction of extensor muscle of knee.

Contraction of the muscle is indicated by rise of the myograph line, relaxation by fall. Upper signal line shows period of stimulation of ipsilateral peroneal nerve; lower signal shows stimulation of contralateral popliteal nerve. The observation opens with stimulation of the former with weak faradisation; the tonus of the muscle is reflexly inhibited and the muscle relaxes. The contralateral nerve is then stimulated, the stimulation of the ipsilateral (inhibitory) nerve still continuing; the muscle contracts reflexly and it might appear as if the inhibitory reflex was having no effect; the stimulation of the inhibitory nerve (ipsilateral) is then discontinued while that of the excitatory (contralateral) still continues. The height of the reflex contraction then assumes a higher level. The degree of reflex contraction under the opposed influences of the two nerves is thus seen to represent an algebraic summation of their *plus* and *minus* effects. Time above in seconds.

### 3. "Identical Innervation" and "Reciprocal Innervation"

A second office which reflex inhibition performs in the interest of co-ordination regards antagonistic muscles. In the anatomical arrangement of the musculature individual muscles are frequently so placed as to exert their pull in directly opposed directions. A striking example of this occurs at hinge-joints such as the elbow and the knee, where extensor and flexor muscles act on the same bony lever in exactly opposite senses. Experiment shows that in simpler reflex actions which produce flexion of the joint the stimulation of the afferent nerve evokes at one and the same time a reflex contraction of the flexor muscle and a reflex relaxation of the extensor (fig. 3). Conversely a simple reflex contraction of the extensor is accompanied by a reflex relaxation of the flexor. In this case, as in all others we are discussing here, the inhibition is not peripheral but central, that is, it has its seat not in the muscle but in the nervous centre about the starting-point of the final common path. The muscle relaxes because the motor discharge from that centre is abated.

The utility of this arrangement seems obvious. The same cause which increases the contraction of the protagonist muscle simultaneously diminishes the contraction of its antagonist. And the two are dealt with in proportionate degree; the intense stimulus which evokes a strong contraction of protagonist produces likewise a full relaxation of its antagonist; the weak stimulus evokes weak contraction and correspondingly weak inhibition. Expenditure of energy to merely overcome the contraction of one muscle by the contraction of another, or the motor activity of one nerve-centre by the motor activity of another is thus avoided as waste. This "reciprocal innervation" of the antagonistic centres and through them of their muscles ensures co-ordination by harmony.

Instances of reciprocal innervation of antagonistic muscles are easily obtained for study in the laboratory. For example, in an appropriate reflex preparation it is easily seen during reflex flexion at the elbow that while the flexor muscle is thrown into contraction the extensor muscle becomes relaxed and if before the stimulus any contraction were present in the latter, that contraction disappears as the reflex takes place. But such an instance does not fully exemplify reciprocal

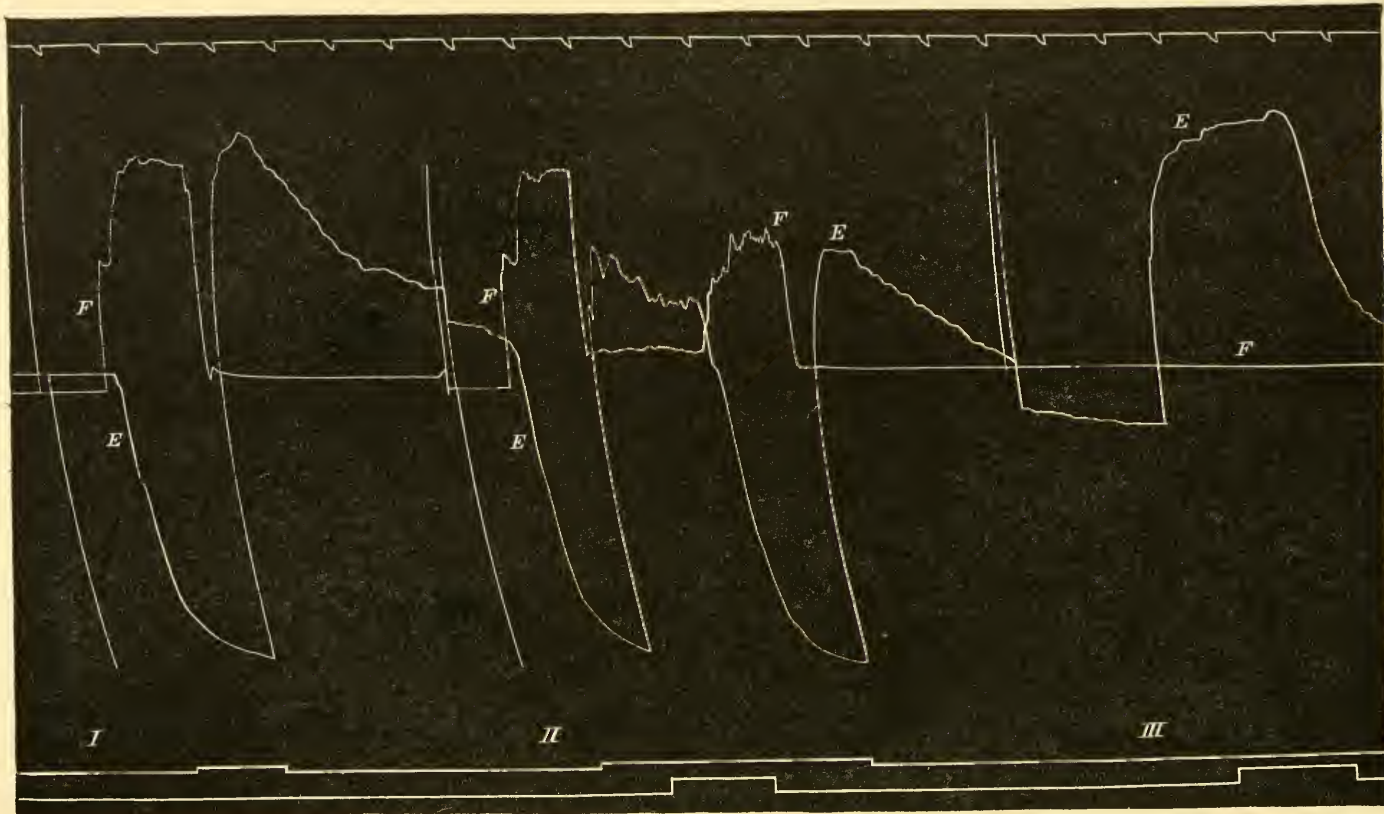


FIG. 3.—Record of the extensor and flexor muscles of the knee. $E$ = extensor, $F$ = flexor.

Contraction is shown by rise, relaxation by fall. Upper signal line shows time of stimulation of the ipsilateral peroneal nerve. When this nerve is stimulated the flexor muscle contracts and the extensor muscle, previously in tonic contraction, relaxes under central inhibition. The lower signal line shows time of stimulation of contralateral afferent nerve (popliteal). When this nerve is stimulated the extensor muscle contracts and the flexor muscle is inhibited, but its inhibition can only be seen if the muscle is contracting at the time when the inhibition occurs. In observation II both nerves are for a period under stimulation concurrently: the myograph then shows that both muscles then contract together but neither of them so strongly as would be the case were its excitatory nerve not partially defeated by the concurrent stimulation of its inhibitory, *i.e.* double reciprocal innervation is in play. Time above in seconds.

innervation, nor does it exemplify fully what usually happens in the elbow muscles when the act of flexion of that joint is executed naturally. If we voluntarily hold the arm semi-flexed nothing is easier than to make both flexor and extensor contract together; by simply feeling the hardness of the muscles through the skin we can assure ourselves of that fact. This concurrent contraction of both antagonists seems impossible to obtain under normal circumstances as a simple reflex by stimulation of any single point or any single nerve. But it can be obtained when we produce pure reflex reflexion of the joint in another way. While some afferent channels regularly produce reflex flexion of the joint, others just as regularly produce reflex extension of it. If two afferents, one producing flexion of the joint, the other extension, be stimulated simultaneously it is not difficult to find a grade of conjoint stimulation producing the flexion of the joint. In this flexion there is, if suitable strengths of stimuli are chosen, some contraction of the extensor muscle as well as of the flexor muscle (fig. 4); but the contraction of the flexor predominates and the joint is therefore flexed. What happens is instructively studied by the myograph. If the reactions of the muscles in response to either of the single stimuli are compared with their reaction under the double stimulus, the motor centre for the flexor is shown in the latter case to be under a two-fold influence; similarly the extensor centre also under a two-fold influence. Of the two afferents concurrently stimulated, that one which if stimulated alone causes *flexion* of the joint *excites* the motor centre of the flexor and *inhibits* that of the extensor; and the other afferent, which if stimulated alone causes *extension* of the joint, *excites* the motor centre of the extensor and *inhibits* that of the flexor. When both are stimulated simultaneously with appropriate intensity the discharge from the flexor motor centre represents the algebraic sum of the opposed excitation and inhibition which the two afferents exert on it, and the discharge from the extensor motor centre similarly represents the algebraic sum of its opposed excitation and inhibition (fig. 3). If the intensity of the stimuli be suitably chosen, both flexor and extensor motor-centres discharge, but the discharge of neither is as great as it would be were the antagonistic influences not present.

An interest attaching to this double reciprocal innervation is that it produces an effect on the antagonistic muscles resembling one that, as said above, is often observable in voluntary actions. The inference is that in these willed actions the

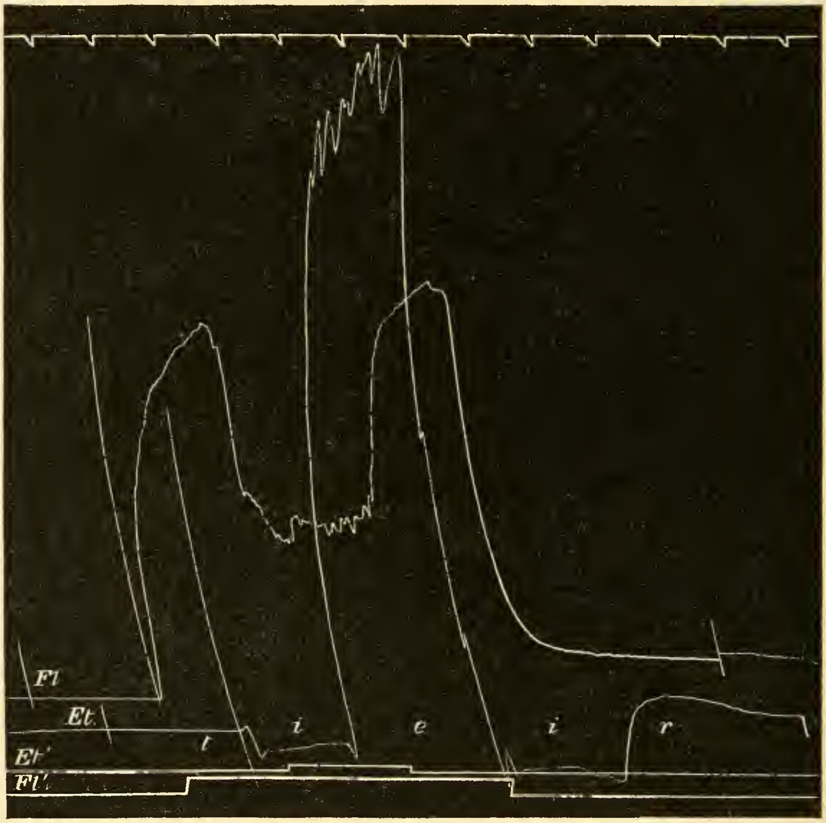


FIG. 4.—Reflex contraction of Semitendinosus, *Fl*, and Vastocrureus, *Et*, recorded with double-myograph.

The lever attached to vastocrureus (*Et*), writes not directly below but about a centimetre to the right of that attached to semitendinosus (*Fl*); control arcs, cut when the recording surface was not moving, indicate the amount of commencement of stimulus on each myogram line. The ipselateral peroneal nerve (afferent end) is stimulated as shown by the lower signal line *Fl'*, and causes contraction of semitendinosus and relaxation of vastocrureus. Then follows stimulation of contralateral peroneal nerve as shown by the upper signal *Et'*. This combined stimulation lessens, but not to extinction, the contraction of semitendinosus and at the same time brings into contraction the antagonistic muscle, vastocrureus. On reverting to the single stimulation again, the contraction of semitendinosus increases once more and that of vastocrureus disappears. Finally, on cessation of the single stimulation *Fl'*, a rebound, *r*, from inhibitory relaxation to increased tonic contraction occurs in vastocrureus synchronously with the relaxation of the flexor. Time above in seconds. Decerebrate cat.

cerebral centres have the power of employing simultaneously the play of two or more opposed influences upon the motor centres. In other words, in these willed actions the grading of intensity of muscular action appears to be obtained by pitting inhibition against excitation. In short, here again inhibition is made to grade the intensity of a concomitantly excited motor centre's discharge.

Certain reflex actions of natural occurrence similarly exhibit in some cases a contemporaneous contraction of antagonists. Thus, stepping consists essentially of alternate extensions and flexions of the limb and its reflex execution appears due to the interaction of two sets of stimuli, one set causing extension of the limb and other flexion of the limb. Two antagonistic stimulations influencing the extensor and flexor centres in opposite directions alternately predominate in competition one with the other. In some forms of stepping—*e.g.* when the step is full and free—one stimulus for a moment completely suppresses the other. Complete rest and unimpeded discharge thus alternate in each centre in turn. But in some forms of stepping, as when the step is not free but crouched, etc., the one group of stimuli only partially counteracts the other. Each centre then comes into action alternately but in one of them at least the discharge is at no time wholly quelled. That centre therefore never obtains a complete rest. It is probable that faulty habits of walking, running, etc., frequently have this latter character. The gracefully balanced stepping which proper systems of drill and exercise cultivate owe their excellence to physiological avoidance of this incomplete balance. The proper execution of the act ensures a moment of complete rest to each of the opposed motor centres engaged. Fatigue is in this way minimised.

It is sometimes assumed that under reciprocal innervation two antagonist muscles necessarily can never be in contraction at the same time, and that the two antagonistic motor centres never exhibit discharge of motor impulses concurrently. That this need not be the case is clear from the above examples. Not unfrequently the antagonists do concurrently contract and discharge. What reciprocal innervation does provide is that, in the execution of a muscular act, *augmentation* of contraction or motor discharge shall never occur concurrently in protagonist and antagonist, nor conversely *decrease* of contraction or discharge occur concurrently in the two. Reciprocal innervation secures that any increase in the contraction or discharge of the protagonist shall be accompanied by corresponding diminution of the contraction or discharge of its antagonist, even to complete suppression in the latter.

While thus laying stress on the reflex co-ordination of antagonistic muscles by reciprocal innervation, it must be remembered that not all muscles and pairs having antagonistic mechanical effects are dealt with in this way. The mechanical antagonism of muscles exhibits two main types. Muscles, according as they act directly on one joint or on more than one, are distinguishable into a single-joint class and a double-joint class, and so on. Single-joint muscles exert their pull from origin to insertion across one joint only; they *directly* affect that one joint alone. The most direct and simplest case of antagonism is between single-joint flexors and extensors acting on one and the same hinge, such as *brachialis* flexing elbow and humeral head of *triceps* extending elbow, or the lateral and medial muscles of the eyeball rotating the ball in opposite directions about the same vertical axis. In all cases of this direct antagonism the co-ordination of the antagonistic muscle pair is by *reciprocal innervation*.

A less simple kind of antagonism occurs where double-joint muscles are involved—muscles which act directly on two joints. At the knee the *vasto-crureus* (fig. 5) muscle which passes across that joint only is the great extensor of the joint, while a double-joint muscle, *semitendinosus*, is the main flexor of the joint; but this latter, besides flexing the knee, extends the hip. If the hip be prevented from extending and, still more, if it be actually flexed, the action of *semitendinosus* as a knee-flexor is enhanced. Experimental examination of the act of reflex flexion of the knee shows that when *semitendinosus* contracts to produce flexion, its antagonist at knee is relaxed by inhibition; but that at the hip, where *semitendinosus* is an extensor, it and the hip flexors are contracted together. The analysis of the reflex shows that *semitendinosus*, although potentially from its position an extensor of hip as well as a flexor of knee, is employed in fact by the nerve centres as a knee-flexor and not as a hip-extensor; nor is there evidence that it is ever used in any other than the former way. In using it the nervous system favours its effect at knee by simultaneously inhibiting its direct antagonist there; but it throws into contraction its antagonists at hip, thus immobilising the point from which the muscle as a knee-flexor takes its pull. A muscle which by fixating a joint enhances the effect oi another muscle crossing that joint to act on a more distant one is termed the pseud-antagonist

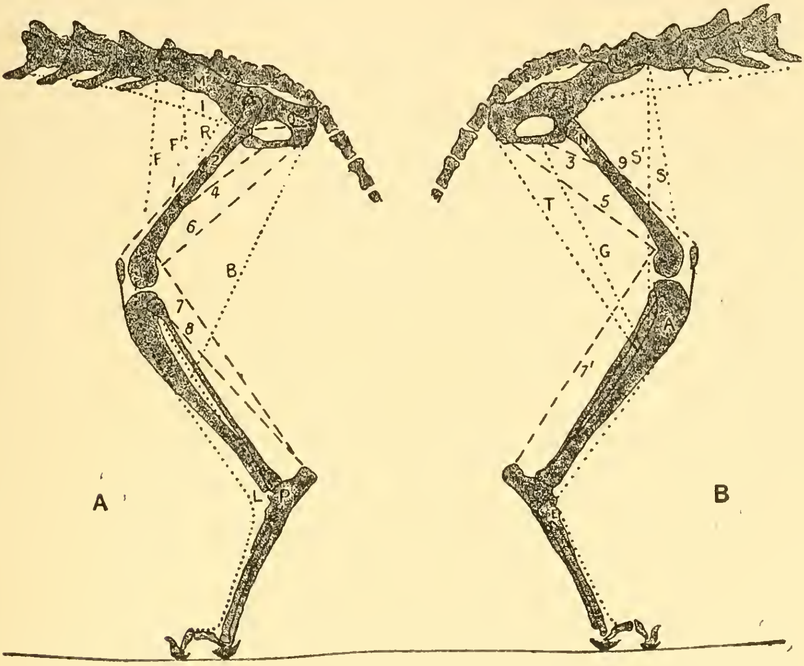


FIG. 5.—Muscles ascertained by direct analysis to be engaged in the flexion-reflex of hind-limb (cat).

A. Lateral aspect of limb ; B. Medial aspect.

Muscles engaged in construction (dotted lines).

A. Tibialis anticus.
B. Biceps femoris posterior.
E. Extensor brevis digitorum.
F. Tensor fasciæ femoris longus, $F'$ brevis.
G. Gracilis.
I. Psoas magnus.
L. Extensor longus digitorum.
M. Gluteus minimus (a second dotted line indicates its posterior part).
N. Pectineus.
P. Peroneus longus.
R. Rectus femoris.
S. Sartorius, lateral band, $S'$ medial band.
T. Semitendinosus.
Y. Psoas parvus.

Muscles relaxed by reflexed inhibition (interrupted lines).

1. Crureus.
2. Vastus lateralis.
3. Adductor minor.
4. Adductor major (in part).
5. Semimembranosus.
6. Biceps femoris anterior.
7. Gastrocnemius ext. $7'$ int.
8. Soleus.
9. Vastus medialis.
0. Quadratus femoris.

(This figure serves also to illustrate the crossed extension-reflex. In that reflex the muscles marked with numerals contract, and *A*, *B*, *S*, *T* of the alphabet-group are relaxed by inhibition—and none of the alphabet-group contract.)

of the latter. Pseud-antagonists, unlike true antagonists, are dealt with by "identical innervation"—that is, contract together and relax together. Thus, in reflex flexion at hip, when the hip-flexors contract *semitendinosus*, although a potential extensor of hip, contracts. It is used as a knee-flexor and knee-flexion often accompanies hip-flexion. But *semimembranosus*, which lies alongside of *semitendinosus*, never contracts with the hip-flexion; it relaxes: it is a hip-extensor and a true antagonist of the hip-flexors and is always used as such; it is used as if it had no influence at knee. Pseud-antagonists are a numerous class; they fixate a joint in order that double-joint muscles crossing it may act better at another joint. True antagonism is dealt with by "reciprocal innervation"; pseud-antagonism by "identical innervation."

### 4. Reflex Inhibition and Transition from One Muscular Act to Another

Not only does inhibition play a part in the co-ordination of muscles in simultaneous co-operation at one and the same moment of time; it also plays its part in the co-ordination of successive steps in a reflex series. One muscular act follows another and the same musculature is commonly employed for different effects in succession. The execution of the change from the old muscular act to the new is ushered in by an inhibitory suppression of the discharge of motor centres prevailing previously (figs. 6, 7) as well as by a discharge of motor centres previously at rest. Take the case where a reflex (decerebrate) preparation is exhibiting the steady postural reflex of standing and then draws up one foot on that foot being pinched. The extensors of the leg are in contraction as the animal stands; if then one foot be squeezed the foot is lifted and drawn up out of harm's way. In this action not only are the flexors of knee, hip and ankle thrown into contraction by the new reflex, but the extensors of knee and hip and ankle which were engaged in contraction by the previous reflex of standing are thrown out of action and their motor centres brought to rest by inhibition. In the transition from one reflex to another a muscle's activity is inhibited if it would offer obstruction to the new reflex. Inhibition ushers in the new reflex by wiping out any antagonistic persistence of the old. In default of this inhibition one of two things must happen: either, the stimulus which is exciting the old reflex must cease exactly as the stimulus exciting the new one begins

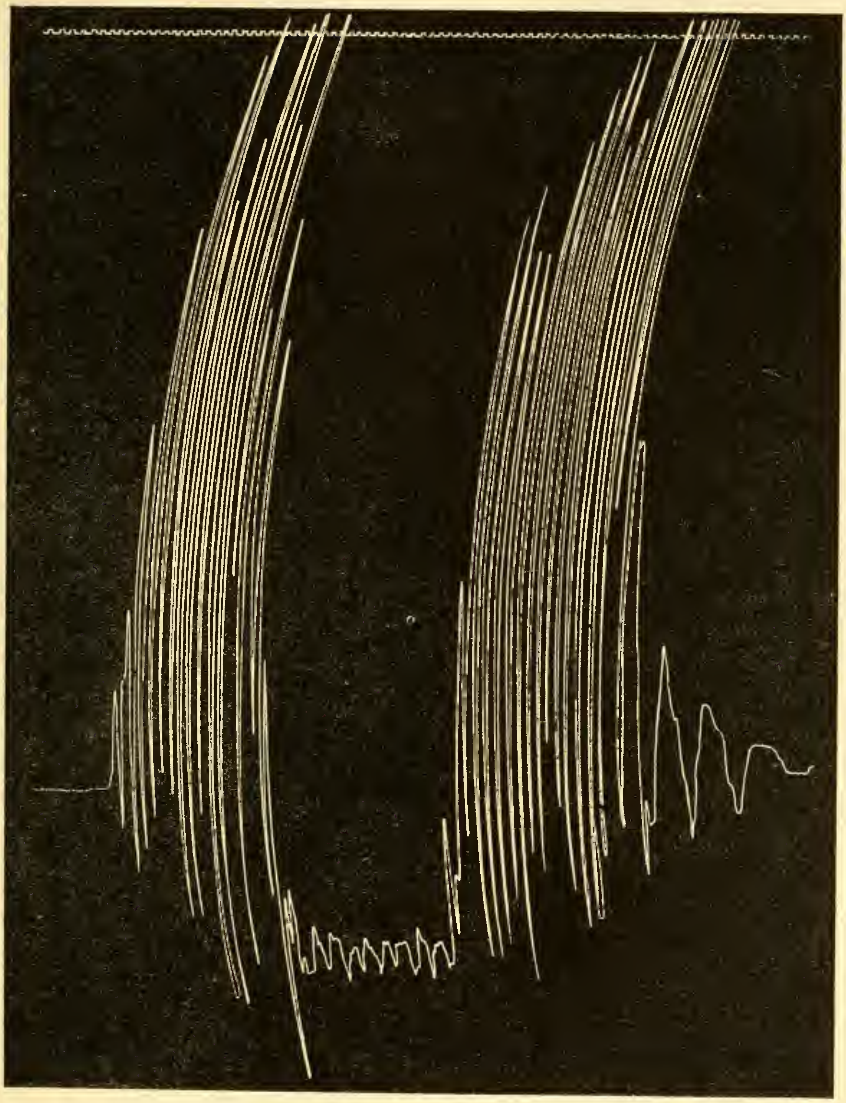

FIG. 6.—The "scratching" reflex of the hind-foot of the dog, elicited by electrically stimulating a skin-point at the shoulder.

During the course of this rhythmic reflex an electric stimulus was applied to the skin of the opposite hind-foot; this stimulus causes stepping movements in the foot which was previously executing the scratching reflex. The record shows the inhibition of the reflex scratching to make way for the new reflex, of much slower rhythm, of stepping. Time above in fifths of seconds.

—a state of things of obviously exceptional occurrence under natural conditions—or, there will persist during the new reflex activities belonging to the old with, in result, confusion of the two. For orderly sequence of reflex actions—also of willed actions—inhibition is a necessary part of co-ordination.

### 5. Reflex Inhibition and the Production of Rhythmic Alternating Movements

A yet further way whereby reflex inhibition contributes to co-ordination is by assisting to convert actions which are continuous into actions of rhythmic alternation. A stimulus continuously applied to certain skin regions in the decapitate mammal, or indeed to the transverse face of the cut spinal cord itself, provokes from the limbs the rhythmic movement of stepping. The manner in which this rhythmic alternating movement of extension and flexion of the limb results from the continued stimulus appears to be as follows. The stimulus excites one phase of the movement, *e.g.* extension; this movement generates in the musculo-articular structures of the limb a stimulus which in its turn reflexly influences the nervous centres of the limb; this secondary stimulus inhibits the extensor centres and excites the flexor, thus opposing the continuous stimulus $S$ and temporarily setting it aside. This secondary stimulus $\Sigma$ lasts until, by its own reflex movement, the limb is relieved from the posture which generated $\Sigma$. On $\Sigma$'s subsidence the original continuously applied stimulus $S$ again becomes effective. Thus reflex inhibition of the centres reacting to the continuous stimulus $S$ recurs rhythmically by virtue of the secondarily generated stimulus $\Sigma$. In other words, a "refractory period" in the $S$ reaction recurs rhythmically, the centres on which it plays becoming rhythmically irresponsive to it. The reflex inhibition by thus rhythmically introducing a refractory period in the $S$ reaction transmutes a continuous reaction into a rhythmic one; in the particular instance chosen it converts standing into walking or running.

Co-ordinations of this kind seem of wide occurrence as the basis of rhythmic alternating movements. The regulation of the wing-beats in bird flight, and of the side-to-side strokes of the tail in the swimming of fish, is probably of this nature. Even in cases where the actual production of the rhythm does not seem wholly explicable in this way the regulation of the *tempo* of the rhythm is found to depend on a reflex co-ordination of this kind. So with the respiratory movements of the chest. Expiration follows on inspiration. When the vagus nerves have been severed the rhythmic alternation of these two opposed movements becomes much slowed. With the vagi intact each remission of the activity of the inspiratory centre is due not to exhaustion of the discharge of the inspiratory centre but to its inhibition by a reflex mechanically generated by the expansion of the lung which inspiration itself produces. The secondary reflex acts upon the inspiratory centre through the afferent vagus. And the vagus at the same time as it quells the discharge of the inspiratory centre excites the activity of the expiratory centre. The inspiratory movement is thus cut short by a reflex inhibition generated by itself; and this inhibition runs *pari passu* with the initiation of a new movement of opposite sense to the old one.

### 6. Post-inhibitory Rebound

As in the above so also in a number of other important rhythmic reflex actions the actions are alternating in the sense that the part moved is first moved in one direction and then moved in the direction exactly opposite to the former movement. Stepping, cited in the last section, is an instance of this. The limb is flexed and extended alternately. In the flexing of the limb the spinal centres of the flexor muscles are excited and those of the extensors are inhibited. When the stimulus which produces this phase of the step ceases or falls into abeyance the opposite phase, that of extension, sets in; the excitation of the flexor centres then subsides and with it the state of inhibition of the extensor centres. This subsidence of the inhibitory state appears to be followed by a rebound of excitability, not merely to that degree obtaining in the centre before it was inhibited but beyond that. A post-inhibitory exaltation of activity occurs in the centre on its escape from the inhibition (fig. 7). On withdrawal of the inhibitory stimulus a seemingly spontaneous discharge of nervous impulses ensues from it *via* its motor fibres, causing its muscles to contract. In this way the application of a stimulus which excites flexion of the limb is followed on cessation of that stimulus by an active extension of the limb. The inhibition of the extensor centres during the flexion phase of the step favours and predisposes them for their

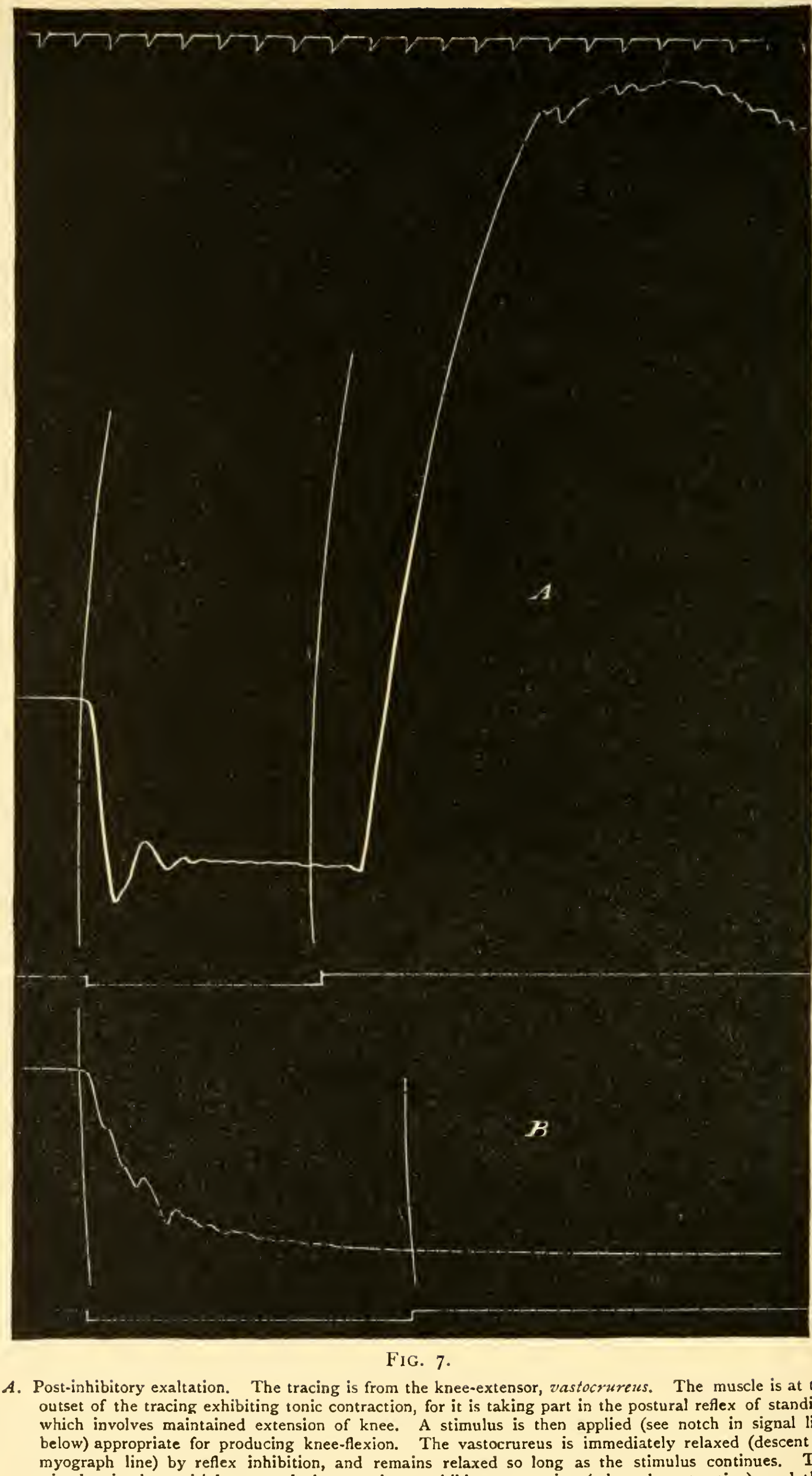


FIG. 7.

*A*. Post-inhibitory exaltation. The tracing is from the knee-extensor, *vastocrureus*. The muscle is at the outset of the tracing exhibiting tonic contraction, for it is taking part in the postural reflex of standing which involves maintained extension of knee. A stimulus is then applied (see notch in signal line below) appropriate for producing knee-flexion. The vastocrureus is immediately relaxed (descent of myograph line) by reflex inhibition, and remains relaxed so long as the stimulus continues. The stimulus is then withdrawn and the muscle re-exhibits contraction (rebound contraction), and this contraction is greatly in excess of the grade of the contraction it exhibited prior to the reflex inhibition. Time in quarter seconds above. *B*. When the inhibition is weak no post-inhibitory exaltation is as a rule evident, activity which executes the following or extension phase of the step. To such an extent is this the case sometimes that the extension or second phase seems self-induced by the inhibition. The central discharge ensuing on withdrawal of an inhibitory stimulus has been termed rebound discharge, and the contraction by which the muscles evidence it, rebound contraction. How potent it is in reflex stepping is shown by the fact that a stimulus which excites the muscle group figured in *A* of the annexed figure (fig. 8) is followed on its cessation by a rebound contraction of all the muscles figured in *B*. So that the flexion phase of the step evoked by the stimulus is succeeded by a complete extension phase without further stimulus at all. This extension phase is at once cut short by inhibition on reapplying the stimulus appropriate for flexion. A series of the complete diphasic acts of stepping can thus be produced by simple repetition of a single stimulus (fig. 9). The same phenomenon is observable in the rhythmic alternating reflex of respiratory movement of the chest (H. Head).

In these cases the inhibition is central—that is, has its seat in the central nervous organ, *e.g.* spinal cord or brain; and the post-inhibitory exaltation is an exaltation of the excitability and activity of an intra-spinal (or intra-bulbar) element of the reflex arc. But in cases where the seat of inhibition is peripheral there is some evidence of an analogous rebound, a post-inhibitory exaltation of activity on the part of the inhibited mechanism. Thus inhibition of the heart-beat by stimulation of the vagus nerve going to the heart is often followed on withdrawal of the stimulus by an exaggeration of the heart's action, the activity of the heart-beat becoming greater than that obtaining before the inhibition was provoked. If we consider that at the root of the antagonism between complemental colour sensations, such as red and green which neutralise each other so as to result when balanced in a colourless grey, there lies a nervous antagonism comparable with that between excitation and inhibition, it is noteworthy that stimulation by the one is followed on its withdrawal by an exalted sensitivity to stimulation by the other and even to a spontaneous appearance of the complemental colour (successive contrast). The increased tendency of a motor centre after inhibition to discharge impulses forms obviously a factor making for co-ordination in reflex sequences where alternate phases of inhibitory

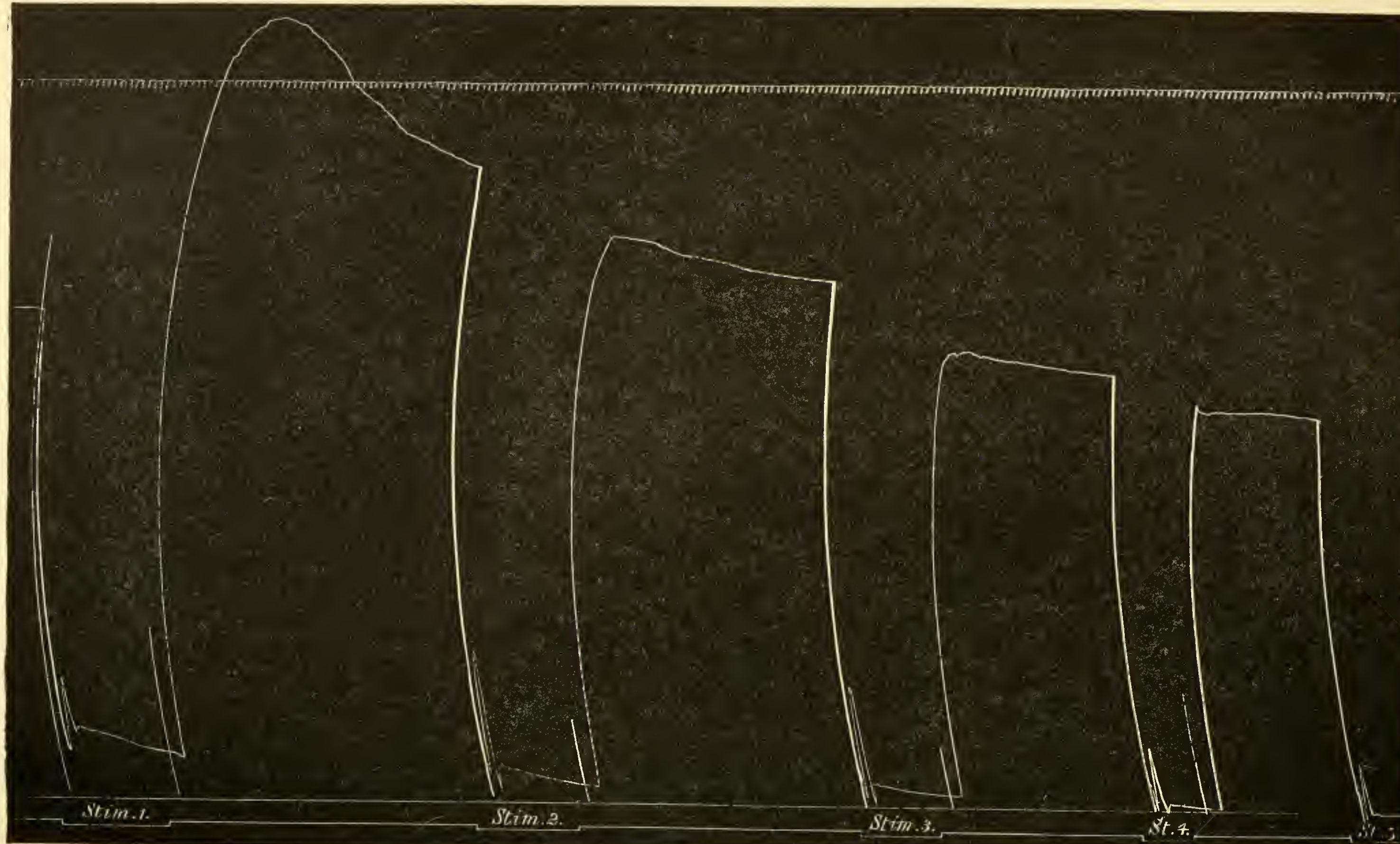


FIG. 8.—The extensor muscle of the knee.

At outset this muscle was in medium tonic contraction owing to its being employed in reflex "standing," supporting the extended knee in the "standing" posture. The central end of the popliteal nerve was then faradised for the time shown by the signal line; the muscle immediately relaxed by reflex inhibition. On discontinuing this stimulus the muscle immediately entered into a powerful contraction (post-inhibitory rebound). This recurs after each recurrence of the reflex inhibition. The post-inhibitory rebound expresses the extension phase of the movement of the "step." The limb was therefore made to execute the chief propulsive act of the step for a short series of steps not by applying a direct excitatory stimulus for that act but simply by removing an inhibitory stimulus which excites the opposite or flexor phase of the step. Time above in quarter seconds.

restraint from discharge and excitatory activity of discharge are demanded from the motor centre. And such alternating reflexes are common in occurrence and important in function—for instance, in locomotion, mastication, deglutition and respiration.

A condition greatly favouring reflex rebound—at least in the case of the extensor phase of the step—is the co-operation of the afferent nerves of the muscles which exhibit the rebound contraction, in the case in question the extensor muscles of the limb. If these nerves are severed the rebound is much less easy to evoke, less regular, and less pronounced. These same nerves are responsible for the reflex tonus of their muscles; this tonus, which forms so marked a feature of the postural tonus in the decerebrate animal, is lost on severance of these nerves. The disease known as tabes dorsalis is characterised by degeneration of afferent nerves, especially those of the muscles. A symptom of this disease is a peculiar imperfection in the management of the legs in walking; and it may well be that this is due to loss or grave defect of the post-inhibitory rebound which seems to play so important a part in the reflex execution of the step.

### 7. Inhibition in "Conditioned" Reflexes

There are other examples of inhibition accessible to physiological study exhibiting it in the rôle of a co-ordinator of nervous activities more complex than those above mentioned.

The simultaneous and successive co-ordination of the individual muscular events composing such acts as respiration and locomotion are innate and so fixed and rigid in character as to be unalterable save in detail by will and wholly unconscious in their working. In these cases the inhibition, like the rest of the reaction of which it is a part, lies in centres phylogenetically among the oldest of the nervous axis. But in the higher animals large masses of the central nervous system belong to the very newest acquisitions of the organism. Part of the utility of this new machinery lies in its plasticity; the comparative ease with which it responds to new needs by adapting its reactions to new situations. Especially is this true of the cerebral cortex and in greatest measure of its most recent portions, such as the so-called association areas of the neopallium. Their cortical reactions are to some extent subject to an adaptation attainable in the course of the animal's individual life and experience. Such cortical reactions, although in their essential outline inherited and instinctive, are yet capable of education and modifiable by training. In so far as they are conscious they are properly matter for psychology rather than physiology. Yet physiology can advantageously study them in regard to their sources in the reactions of afferent nerves, to the interaction of nervous centres which they involve and to their expression by effector organs as muscular and glandular acts.

A reflex is at its simplest a nervous reaction linking a particular muscular or glandular activity to a particular form of stimulus. Thus in reflex salivation the introduction of food into the mouth is linked to a secreting of saliva, the link itself being an intervening nervous reaction. This nervous reaction involves the brain but not the cortex of the brain. Yet the dog's cortex under ordinary circumstances becomes a factor conditioning the reflex. With it the centripetal channels of the reflex open up side connections and are largely extended thereby. Food when signalled not only by the tongue but by the eye, or nose, or ear becomes through the intermediation of the cortex able to call forth saliva. The cortex is a great nodal area where manifold influences meet and are liable to coalesce or conflict. Elicited through cortical channels the salivary reflex is liable to be disturbed by many conditions and therefore to be variable in reaction. Hence associate reflexes such as these obtainable through the cortex are termed "conditioned" (Pawlow), in contra-distinction from the practically inevitable reflexes obtained through the primal bulbar or spinal channels.

Not only are these conditioned reflexes less certain but they have plasticity. They can be modified by training and certain forms of them can be acquired by training. They can be acquired by training as so to say side attachments to the aboriginal reflex. Thus a visual stimulus such as a letter of the alphabet can by training be made to call forth a secretion of saliva. The letter, after being presented to the eye on a number of occasions as a regular precedent to the offering of food, becomes by association a visual stimulus able to excite secretion of saliva. A conditioned reflex is thus established. It is not innate but is acquired by the experience of the individual. Conditions of the moment particularly powerfully affect such reflexes.

Suppose a conditioned salivary reflex has been acquired for a visual stimulus—*e.g.* the letter A. Suppose another and

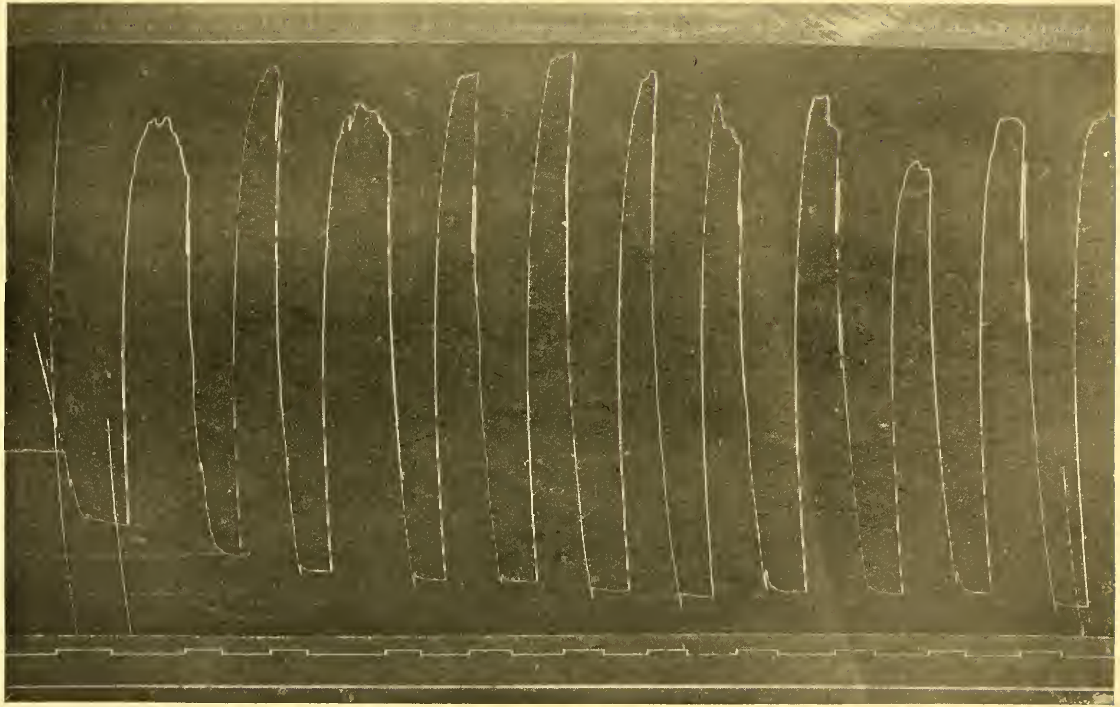

FIG. 9.—Knee-stepping by rebound: isolated extensor muscle.

On applying an electric stimulus (first rise of the signal line) to a foot-nerve, the myogram line falls and remains down during the stimulus, showing the reflex inhibition of the tonus. After 3 sec. the stimulus is discontinued (rise of signal line); a rebound contraction ensues; this is cut down by reapplication of stimulus, and re-ensues on removal of the stimulus. This is repeated (eleven steps). Observation by S. C. M. Sowton and C. S. S.

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unwonted stimulus—*e.g.* a musical tone—is presented along with the visual stimulus A. It is usually found that the reflex salivation then fails to occur. The new stimulus inhibits the conditional reflex to stimulus A.

Again, suppose a conditional reflex to stimulus A to have been established and that then some associate stimulus—*e.g* a musical tone—is added to A and on a number of successive occasions when this is done the feeding of the animal, which regularly follows the presentation of the stimulus A alone, is regularly withheld. Not only is there then no reflex salivation when the twofold stimulus is applied but on returning to the original procedure and presenting the visual stimulus A itself, the conditioned reflex acquired for that stimulus is found also to have been lost. By training it can be reacquired. But even when this reacquirement has taken place, the presentation once more of the note as an accompaniment of A is found to inhibit the conditioned reflex which A otherwise evokes.

In assessing the significance of such results it is helpful to remember that the conditioned reflex is acquired by training and that in the training inhibition is a notable factor. The first step is the directing of attention to the conditioned stimulus —*e.g.* to the visual symbol, which denotes that food will be offered. Unless that is successful the training cannot ensue. In attention, besides the peculiar and characteristic free accentuation of certain stimuli—*i.e.* those attended to—there is a concomitant blocking of other stimuli seeking entrance to the mind by other channels. This repression of reaction to other stimuli is an essential element in the attentive process and it amounts, as we know from our own experience, to a repression so complete that we do not for the time being even perceive other stimuli than the one attended to. And this concomitant inhibition, even after the acquisition of the reaction, still remains a *sine qua non* for the reaction; it is a condition for the conditioned reflex. Here, just as in motor reactions, excitation and inhibition are complemental processes, combined to ensure a unified response to the promptings of the outside world.

## 8. Conclusions

The power of the environment through the nervous system to incite this and that activity of the organism has long been known; its converse power to check and to call halt has only recently become recognised and studied. Reflex inhibition is the expression of this latter power. It is a great factor in the due grading of muscular contraction. It grades the degree of contraction of muscles in their execution of a particular act under particular combinations of circumstances. It avoids waste of nervous and muscular energy in the correlation of action of antagonistic muscles. In the sequence of reflexes of opposed effect it secures co-ordination by suppressing a pre-existent reflex to make room for a new one which employs the muscles differently. It plays an eminent part in the production of rhythmic refractory phase, thus cutting a continuous reaction into a series of intermittent ones; in this way it evolves alternating movements, *e.g.* stepping, from a tonic posture, *e.g.* standing. Lastly, it induces discharge as an after-effect. It interconnects the phases of diphasic reflexes by post-inhibitory rebound.

In all these uses of inhibition we see it as an associate of, and a counterpart or counterpoise to, excitation. Whether we study it in the more primitive nervous reactions which simply interconnect antagonistic muscles or in the latest acquired reactions of the highly integrated organism, inhibition does not stand alone but runs always alongside of excitation. In the simple correlation uniting antagonistic muscle-pairs, inhibition of antagonist accompanies excitation of protagonist. In higher integrations where, for instance, a visual signal comes by training to be associated to salivary flow, the key of the acquiring of the reflex and of its maintenance is attention. And that part of attention which psychologists term negative, the counterpart and constant accompaniment to positive attention, seems as surely a sign of nervous inhibition as is the relaxation of an antagonist muscle, the concomitant of the contraction of the protagonist. In the latter case the co-ordination concerns but a small part of the mechanism of the individual and is spinal and unconscious. In the former case it deals with practically the whole organism, is cortical and conscious. In all cases inhibition is an integrative element in the consolidation of the animal mechanism to a unity. It along with excitation composes a chord in the harmony which the healthy working of the organism exhibits always.

# THE NEED OF AFFORESTATION IN THE UNITED KINGDOM OF GREAT BRITAIN AND IRELAND

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## PART I

A NATURAL preliminary to the consideration of the question of extending forestry in Great Britain and Ireland is a brief study of the history of the forests which have existed hitherto.

In the time of the Romans Britain was densely wooded, Cæsar describing the ancient Britons as a true forest people. Under later Saxon rule more careful cultivation began to replace the chase as a means of subsistence, and led to more extensive clearances and their protection from wild beasts. It was only another step for forest areas to be reserved for the ruling classes.

Game laws existed in the times of the Saxons and Danes, although the first reputed regular statute is the Norman forgery known as the Charter of Canute, said to have been granted in 1018. Unenclosed land was appropriated to the King, and the law was manipulated to convey an exclusive right of chase to the King and those authorised by him. Great oppression was exercised in the name of this law, not only in the areas previously reserved, but over large tracts which were newly placed under ban: an instance was the New Forest, formed in 1079, an action which must have been most ruthless, even if the accounts are not fully credited. All this was done under the supposed law of Canute, and it was not until the Assize of Woodstock (1184) that the first genuine Forest Law was enacted. The most cruel punishments were enforced for offences which now seem trivial, and it appears that the administration of Forest Law was not without influence in bringing about the Magna Carta, 1215.

The Carta de Foresta followed ten years later, and it is interesting that Henry III. then laid down: "No man from henceforth shall lose neither life nor member for killing our Deer." This law applied to the royal forests only, while chases, parks, and warrens which the King granted to his subjects were governed by common law.

Abuses were not long in arising with the large number employed to administer a law not always very definite in application, and it was not until the Forest Ordinance of 1306 that the clamour for relief was for a time appeased, and not until 1327, in the reign of Edward III., that the oppression of Forest Law was finally broken.

In 1482 it was first recognised that a subject might own a forest, and the cutting and sale of wood was authorised. This was the signal for the wastage and destruction of much forest, as is testified in Holinshed's Description of England (1577). A consequence was that, in 1504, the planting of at least one acre of wood was compulsory where no great wood or forest existed on the estate, and in 1543 Henry VIII. ordered "the replantation of forest trees to cure the spoils and devastations that have been made in the woods." In 1534 the first work in the English language on the cultivation of trees was issued by Master Fitzherbert.

There is no very complete record of Forest Laws in Scotland and Ireland, where they probably did not exist as early as in England. Later many of the English laws were adopted in Scotland, but on the whole the provisions were more reasonable and the penalties less severe.

The clearing of woodland was still further encouraged by James I. and his successor Charles I., and in 1640 the Act for the Limitation of the Forests was passed, finally rendering any extension of the royal forests impossible.

In 1662 John Evelyn was selected, with a view to ensuring a more plentiful supply of oak for shipbuilding, to discourse upon the pleasures and profits of growing timber. His *Sylva; or, A Discourse of Forest Trees and the Propagation of Timber in His Majesty's Dominions*, is the great classic of British forestry. In spite of all endeavours, the supply of oak decreased, while the tonnage of the Navy had increased from 17,110 tons in 1603 to 413,667 tons in 1788. Fortunately the importation of teak staved off what would otherwise have meant disaster.

Increased demand for timber for other purposes enabled a fresh stimulus to be given to forestry at the beginning of the nineteenth century. A number of plantations made then are now mature, or becoming so, but want of scientific knowledge in establishing and tending the plantations has generally detracted greatly from their success. It is the inferior quality of the home-grown timber, together with the rapidly increasing imports of foreign timber, that has brought the consideration of forestry and its prospects in the United Kingdom to the front during the past few decades.

In 1885 a Committee was "appointed to consider whether, by the establishment of a forest school or otherwise, our woodlands could be rendered more remunerative." The Committee recommended the establishment of a Forest Board to organise instruction in forestry. Further they reported that having "had evidence that, apart from any immediate pecuniary benefits, there would be considerable social economical advantages in an extensive system of planting in many parts of the kingdom," the subject was one of great importance, and well worthy of early consideration. Little action was taken on this report, although some attempt was made to organise forestry instruction.

Again in 1902 a Committee was appointed to inquire into and report upon "the present position and future prospects of forestry and the planting and management of woodlands in Great Britain. And further to consider whether any measures might with advantage be taken for their promotion and encouragement." The report of the Committee again dealt chiefly with the question of forest education, recommending the provision of courses suitable for each class of trained forester required. To enable the training to be complete it was urged that two areas of forest of from 2,000 to 10,000 acres, one in England and one in Scotland, be acquired as Demonstration Areas. Apart from direct instructional purposes it was pointed out that these forests were required to provide practical demonstration of the most perfect technical and economic developments of forestry, of the expenses likely to be involved and profits to be realised. The Committee further stated that they did not feel justified in urging the Government to embark forthwith on any general scheme of State Forests, although they held the question of the planting of suitable lands under the control of the Crown to be worth attention, and recommended the compilation of a statement of areas presumably suitable for afforestation. Finally it was recorded that there were complaints as to the incidence of rates on plantations, and states that there was need for the immediate revision of estate duties.

As a result of these recommendations the Alice Holt woods in Hampshire have been brought under a working plan to serve as a demonstration area, and systematic courses of study in forestry are now provided at Oxford, Cambridge, and Edinburgh Universities and at seven other schools and colleges, not to mention lectures on forestry delivered elsewhere. More recently the Inverliever Estate of 12,530 acres has been acquired by the State with a view to experimental afforestation.

The next step taken was the appointment in 1907 of a Committee by the Department of Agriculture and Technical Instruction for Ireland to inquire into "(1) The present provision for State aid to forestry; (2) the means whereby in connection with the operation of the Land Purchase Acts existing woods might be preserved and land suitable for forestry acquired for public purposes; and (3) the financial and other provisions necessary for a comprehensive scheme of afforestation in Ireland."

The Committee in their report clearly showed the aid provided by the State to be inadequate, especially in view of the incentive to the clearance of forest arising from Land Purchase legislation. They gave actual figures to show how rapidly the already small area of forest, some 300,000 acres, was being reduced; and how, in consequence, the home timber industries which lack organisation will before long become extinct. They then pointed out that but little amplification of existing legislation was necessary in order to render the policy of Land Purchase peculiarly favourable to the initiation of a scheme of national afforestation, provided facilities were given by the State and funds made available. It was urged that at least 1,000,000 acres of forest was essential for the agricultural and industrial requirements of the country. The necessary land was shown to be available and 200,000 acres were estimated to consist of large blocks suitable for State afforestation, while the remainder might more satisfactorily be dealt with by county councils and private owners. Definite proposals were made for financing the separate schemes, and in the case of the

200,000 acres to be planted by State agency a return of 4½ per cent. on the total capital invested was anticipated in the eightieth and following years. Further proposals related to an extension of the existing training for foresters provided at Avondale and the establishment of an effective system of higher training for experts, and to the proper organisation of the timber trade. This report was made in 1908, and it is hoped definite action will be taken before long.

Finally, the Royal Commission on Coast Erosion was extended by Royal Warrant in March 1908 and directed to inquire "whether in connection with reclaimed lands or otherwise it is desirable to make an experiment in afforestation as a means of increasing employment during periods of depression in the labour market, and, if so, by what authority and under what conditions such experiments should be conducted." This Commission found afforestation in the United Kingdom to be practicable and desirable. But whilst holding that any scheme of national afforestation should be on an economic basis, the Commissioners were of opinion that such a scheme would contribute to the solution of the unemployed problem and that any additional expense incurred in so doing might be met from a separate account. It was estimated that 8,500,000 acres were suitable and available for afforestation without material encroachment on agricultural land, exclusive of at least 500,000 acres in Ireland. Proposals were made for the acquisition of this area by the State in all cases where the owners were not able and willing to afforest under State supervision, and estimates were framed based on the acquisition of the whole 9,000,000 acres at an average cost of £6 10*s.* per acre. In view of the urgency of the unemployed problem planting was proposed to be completed in sixty years, with a probable rotation of eighty years for 6,000,000 acres and forty years for the remainder. An alternative scheme dealt with 6,000,000 acres only, with the same proportion under each rotation. It was anticipated that from the eighty-first year a return of 3¾ per cent. on the costs accumulated at 3 per cent. compound interest would be realised.

The report concluded with recommendations that Commissioners should be appointed to determine by survey what land was suitable for afforestation, and to prepare a general scheme for its execution, and that the necessary powers and funds be placed at their disposal. The report has been abundantly criticised, but seems to have borne fruit, since in September 1910 the Commissioners appointed in connection with the Development Fund decided to postpone consideration of a scheme for Ireland until the proposals now in formation by the Board of Agriculture were before them.

Further proof of serious contemplation of practical steps for afforestation is furnished by clauses of the Development and Road Improvement Funds Act, 1909. This Act provides amongst other things for "the conducting of inquiries, experiments, and research for the purpose of promoting forestry and the teaching of methods of afforestation," and "for the purchase and planting of land found after inquiry to be suitable for afforestation."

## PART II

Passing from a résumé of the history of forestry in Great Britain, it is necessary to examine the effects of forest on a country and its inhabitants, and more particularly those which have recently brought forestry into prominence, or which justify the serious consideration of afforestation in the United Kingdom.

The investigation may conveniently be made by considering forestry in relation to—

(1) The employment of land. (2) The employment of capital. (3) The employment of labour, and (4) The indirect effects produced.

### The Employment of Land

According to the agricultural statistics for 1909 (Ireland 1908) the land in the United Kingdom is at present utilised as follows:

| | Dry Land. | Crops and Grass. | Woods and Plantations. (1905). | | Mountain and Heath Land. | Other Lands. |
|---|---|---|---|---|---|---|
| | Acres. | Acres. | Acres. | Percentage of total area. | Acres. | Acres. |
| England | 32,391,997 | 24,540,985 | 1,715,473 | 5·3 | 2,416,183 | 3,719,356 |
| Wales | 4,749,651 | 2,782,479 | 184,361 | 3·9 | 1,324,213 | 458,598 |
| Scotland | 19,070,182 | 4,859,609 | 868,409 | 4·6 | 9,102,876 | 4,239,288 |
| Isle of Man and Channel Islands | 185,453[1] | 125,188 | 1,168 | 0·6 | 30,843 | 28,254 |
| Ireland (1908) | 20,233,590 | 14,665,300 | 301,636 | 1·5 | 2,469,555 | 2,797,099[2] |
| United Kingdom | 76,630,873 | 46,973,561 | 3,071,047 | 4·0 | 15,343,670 | 11,242,595 |

[1] Including water in the case of Jersey.

[2] Includes 1,315,798 acres of turf-bog and marsh.

Thus there are at present 3,071,047 acres, or only 4 per cent. of the United Kingdom, devoted to the growth of forest, while there are 15,343,670 acres, or 20 per cent., mountain and heath land used for grazing. This latter figure is exclusive of 3,537,172 acres of deer "forest" or land devoted exclusively to sport, and 1,836,859 acres of barren mountain land, turf, bog and marsh in Ireland. Only 3 per cent. of the area of forest is the property of the State.

Varying views are expressed by writers on forestry for the United Kingdom as to the suitability of the climate, and it is not uncommonly made out that advocates of afforestation suppress facts as to the limitation of the extension of forests due to climate. In the circumstances it seems advisable to state concisely the limitations now deduced by Prof. Mayr from extensive investigations. Climate, that is to say, the external influences on the earth, its plant growth and animal life, is subject to gradual changes with which changes in vegetation correspond; its chief factors are temperature, moisture, wind and light.

*Temperature.*—Comparison of temperature in different regions definitely proves that extremes do not determine the existence of forest, but that it depends on a certain irreducible minimum temperature during the vegetative period. This period can hardly be less than six weeks, and is roughly four months at 50° N. latitude. Further examination of the variations from the minimum temperature over large areas have enabled Prof. Mayr to prove that it is chiefly on these that the natural distribution of species depends. The minimum temperature for four months is found to be 10° C., and to correspond in the northern hemisphere with a mean annual temperature of about 3° C. Temperatures so high during the period as to preclude the possibility of tree growth are nowhere known to exist.

The mean temperature of the months of May, June, July and August is about 12° C. in the United Kingdom, and although of course aspect, slope and the presence of water may considerably modify the altitude at which the vegetative minimum for trees is reached, 1,500 feet above sea level is the average in the extreme north of the United Kingdom (60° N. Lat.) and at 3,500 feet in the south (50° N. Lat.).

*Moisture.*—Insufficient moisture may preclude the growth even where suitable conditions of temperature exist. Destructive as snow may be in certain circumstances it cannot preclude the growth of forest, and is not, as is commonly supposed, responsible for the creeping of certain varieties of trees. Precipitation and humidity of the air may be equally important to tree growth. But it will suffice to say that the rainfall of the United Kingdom from May to August averages nearly 9 inches, while investigations have shown tree growth to be possible where 2 to 4 inches of rain falls in the vegetative period, provided, at the same time, that the humidity of the atmosphere is not less than 50 per cent.

Wind if strong and steady may render the growth of trees impossible, but light is nowhere known to be insufficient.

Contemplating these absolute climatic limitations it is apparent how little they come into effect in the United Kingdom, for only 3,537,172 acres lie above 1,500 feet in Great Britain, while the rainfall is ample, and steady strong winds do not prevail over large areas. On the contrary, the mild, moist summer climate is infinitely better, and gives much greater elasticity in the matter of selection of species than that of most of the well-wooded European countries with their continental climate of extremes. Gales, it is true, have made havoc of extensive areas of forest, but any excessive damage in the United Kingdom is ascribable to the way in which the forests have in the past been grown and treated. This can, and would be, removed once the study of forestry became more common, and owners relied less on learning each by his own experience. Forests are said to have been growing throughout the land in the time of the Romans, and a study of local conditions and the means of protection from wind should make the exclusion of but little land necessary.

Another not uncommon contention by writers on the British afforestation question is that much of the soil apparently available is too poor for the growth of trees. As a matter of fact this is never literally the case, for it is impossible to conceive of a soil, with the exception of that of volcanic origin, the composition or consistency of which prevents it from sustaining some species of trees; and thus where there is sufficient soil, say one foot or more, the question immediately becomes which of a limited number of species suitable is to be grown. Fortunately it is just the shallow or poorer quality soils which will produce a serviceable quality of the spruce, or two or three needle pine, timber so much in demand throughout the civilised world. For it is the ever-increasing demand and diminishing supply of these timbers, and not of the more valuable hardwoods, which makes the question of afforestation so pressing. It is a question of the quantity of serviceable timber and not one of the quality alone. In this lies the economic justification for afforestation and not in the value of selected crops grown under exceptional conditions, the success of which often does not bear proper financial analysis; nor is there justification to be found in the growing of exceptionally fine trees on soil which is arable, for this does not properly come within the sphere of economic forestry, which begins where agriculture ends. In now considering the land economically available for afforestation it is to be regretted that there is little more to go upon than the above-quoted agricultural statistics. In 1902 it was recommended by the Forestry Committee that a survey should be undertaken to ascertain what area of land might more suitably be employed for growing forests than for its present purpose; but the recommendation has not been carried into effect. Since the question of the employment of labour has to be taken into account, and it must be emphasised that it is not intended that afforestation should be undertaken at the expense of agriculture, it is better entirely to exclude from consideration the area now under crops, though it is doubtful whether some of the poorer village land would not yield better returns under forests. The area of mountain and heath land alone is that on which an estimate can be based except when it is definitely known that suitable areas are included under the heading of "other lands."

In Great Britain there are 13,074,115 acres of mountain and heath land, the rental value of which may safely be assumed not to exceed 1*s.* per acre on an average. Besides this there are 3,537,172 acres of deer "forest," or lands exclusively devoted to sport, so that after deducting 3,519,678 acres on account of excessive elevation, there remain in round figures 13,000,000 acres as possibly available for afforestation.

Now the opinions expressed by different authorities as to the area economically suitable for planting differ very widely, as is natural where such large figures are dealt with. Sir W. Schlich, who has done so much to bring the question before the nation, has assumed that at least 4,000,000 acres are suitable, whilst the Royal Commission of 1908 proposed 6,500,000 exclusive of 2,000,000 of poor tillage land. Widely as these estimates may differ they are only of importance in enabling us to realise the benefits of afforestation, for there can be little value in calculating on such a basis the cost of so vast and complete a scheme of afforestation as was contemplated by the Commission of 1908.

It suffices to know that there are available large suitable areas which would be more profitably employed for the growth of forest than for their present purpose. That this is the case is testified by a number of forest owners and experts well acquainted with the conditions prevailing in the most widely differing counties. Further and more specific evidence is afforded by the results of the inquiries instituted by the Board of Agriculture and Fisheries in certain representative counties into the adaptability for forestry of land below 1,500 feet elevation classed as mountain and heath land. The following are the summarised results of these inspections :

| County. | Total Mountain and Heath Land. | Land below 1,500 ft. inspected. | Land reported available. | Percentage of Land below 1,500 ft. available. |
|---|---|---|---|---|
| | Acres. | Acres. | Acres. | |
| Suffolk . . . | 29,873 | 27,280 | 20,000 | 73 per cent. |
| Derby . . . | 35,661 | 21,512 | 12,336 | 57 ,, ,, |
| Lancashire, N. } | 103,170 | { 57,050 | 12,734 | 23 ,, ,, |
| ,, S. } . | | { — | — | — |
| Glamorgan . . | 130,561 | — | 34,000 | 26 ,, ,, of total |
| Lanark . . . | 209,635 | 178,593[1] | 90,500 | say 50 per cent.[1] |

Reports on South Lancashire and Wiltshire, the latter with 20,584 acres of mountain and heath land, were to the effect that the amount of land that could be afforested with advantage was insignificant; but even if these and some other localities had to be omitted altogether—and further inspection of Wiltshire has at least left some doubt as to that county—the fact remains that very considerable areas throughout Great Britain are suitable. In the case of Ireland, the very conservative estimate of 700,000

[1] Below 1,000 ft.

acres by the Committee of 1908 may be taken as a minimum. This estimate was based on the minimum area of an economic forest as distinguished from a forest for purely domestic purposes, viz. 50 acres. Accordingly all holdings of under 100 acres were left out of account. A further exclusion of all land under crops and rotation was made, when it was assumed that 80 per cent. of the 3,315,836 acres of permanent pasture were necessary for agriculture, and that only 10 per cent. of the mountain grazed land were suited for economic afforestation. Thus Ireland, too, provides considerable scope for forestry so far as land, the natural factor of production, is concerned. In the case of both estimates the enhanced value of the land, if employed for the production of timber instead of for its present purpose, has been regarded as a *sine qua non*; and that a reasonable and business-like scheme of afforestation is not a gamble in futures is clear when it is borne in mind that the rental of the land contemplated rarely exceeds 2*s.* 6*d.*, and frequently is less than 1*s.* per acre per annum.

The bulk of this land is private property and it will be necessary later to consider how the difficulties that hinder afforestation by private agency can be remedied, or whether private initiative should be relied upon. One contention, however, may suitably be mentioned here, and that is that afforestation would dislocate the present means of utilising much other land; for instance, the loss of wintering for sheep would affect the value of the pastures to which it belonged. Undoubtedly this might be so if all afforestable land were planted forthwith; but presumably the private owner, for his own sake, would take such matters into account and no Government is likely to embark on the planting of any particular area without weighing the direct as well as the indirect effects of so doing; such consideration is hardly one which could be overlooked. Moreover, even the low estimate of 6,000,000 acres available admits of reduction and even then is not insignificant; and it must not be forgotten that the protection afforded by forest will in not a few cases increase the utility of adjoining land both for cattle and crops.

Finally, before leaving the subject of land, it is well to make a comparison of the proportion of land under forest and the area of forest per head of population in each of the European countries:

| | Percentage of total area under forest. | Area per head of population. Acres. | | Percentage of total area under forest. | Area per head of population. Acres. |
|---|---|---|---|---|---|
| Finland | 63 | 18·5 | Greece | 13 | 0·8 |
| Sweden | 48 | 9·4 | Luxemburg | 30 | 0·8 |
| Norway | 21 | 7·5 | Switzerland | 21 | 0·6 |
| Russia in Europe | 37 | 4·6 | Germany | 26 | 0·6 |
| Bosnia and Herzegovina | 50 | 4·0 | France | 18 | 0·6 |
| Bulgaria | 30 | 2·2 | Italy | 15 | 0·3 |
| Turkey | 20 | 1·7 | Denmark | 6 | 0·2 |
| Servia | 32 | 1·5 | Belgium | 18 | 0·2 |
| Roumania | 18 | 1·3 | Portugal | 4 | 0·1 |
| Hungary | 28 | 1·2 | Holland | 7 | 0·1 |
| Spain | 17 | 1·1 | Great Britain and Ireland | 4 | 0·07 |
| Austria | 33 | 0·9 | | | |

So that if forest be admitted essential for agricultural and industrial requirements, in no European country is the need for extension greater than in the United Kingdom, whether the basis of comparison be area or population.

### The Employment of Capital

In forestry, as in other undertakings, the employment of capital is necessary in order to render the land productive. The capital takes the form of roads, buildings and implements; but these are insignificant in comparison with the circulating capital, the growing stock.

This capital, comprising the cost of preparing, planting and protecting the area, steadily increases with the accumulation of compound interest on the original outlay. Apart from the initial cost the rate of interest employed and the length of the rotation chiefly determine the extent to which this capital accumulates. This intimate relation of forestry to the rate of interest was realised in England as long ago as 1623 when Thomas Culpepper pointed out that without a reduction in the customary rate no owner could afford to retain timber trees. Greatly as financial conditions have changed since then, the difficulty has not even now entirely disappeared, and the justification for forestry generally is still rather to be found in its indirect benefits and means it affords for the employment of much land and labour than in the actual return on a capital invested in it.

Without a doubt the existing 3,000,000 acres of woodlands are as a whole a financial loss, and where more business-like methods now prevail the figures available for short periods are too incomplete to be of much value. It is then to countries where forestry is established that one must look for the results obtainable under present circumstances. Thus in Germany careful calculations show that from 2 to 3 per cent. is as much as forests will yield, with the exception of some under short rotations, which may yield 4 per cent. It is sometimes forgotten that the extent of the latter forests is limited not only by the demand for the produce but more often by conditions of climate and soil, both of which must be particularly favourable if a rapid deterioration is not to be feared; 3 per cent. is then commonly applied for, and is reasonably to be expected from, forest enterprise when the rotation is about eighty years.

Compared with the premier Government securities which now yield: Great Britain, 3·1 per cent.; France, 3·1 per cent.; Germany, 3·6 per cent., forestry yields up to about ½ per cent. less. And with the higher rotations necessary—for the oak, for example—the accumulated capital is so great that 2½ per cent. is generally as high a rate as can be expected.

To repeat then, speaking generally, afforestation has at the present time no purely financial justification, practical proof of which is only too strikingly given by the failure of a number of forest companies formed in Austria towards the end of last century.

On the other hand, forestry has some financial advantages peculiar to it. It provides means of employing large sums of money for prolonged periods with great security, for, great as the disasters due to fire, wind, insects and diseases have been, these are only local where reasonable precautions are taken. The disorganisation thus caused is in inverse proportion to the extent of the property, and the development of means of communication and of the world's timber market enable the immediate loss to be greatly reduced where the crop affected is of a marketable size.

Intimately connected with the financial aspect of forestry is, naturally, the supply and demand of the produce. An investigation in the case of a number of the minor forest products would be interesting; but here it will only be necessary to consider timber in the chief forms in which it is placed on the British market.

The following statement compares the net imports of 1890 with those of 1909:

| | 1890. | 1909 | 1890. | 1909. |
|---|---|---|---|---|
| *Hewn.* | Loads. | Loads. | £ | £ |
| Fir . . . . . . | 2,004,196 | 3,180,004 | 3,248,753 | 4,096,077 |
| Oak . . . . . . | 141,404 | 155,369 | 804,356 | 952,448 |
| Teak . . . . . . | 54,467 | 29,494 | 604,955 | 470,040 |
| Unenumerated . . . | 65,716 | 55,618 | 233,714 | 172,524 |
| | 2,265,783 | 3,420,485 | 4,891,778 | 5,691,089 |
| *Sawn.* | | | | |
| Fir . . . . . . | 4,502,632 | 5,543,169 | 10,545,602 | 14,715,459 |
| Unenumerated . . . | 256,337 | 156,429 | 473,015 | 650,098 |
| | 4,758,969 | 5,699,598 | 11,018,617 | 15,365,557 |
| Staves . . . . . | 153,471 | 117,773 | 642,569 | 479,973 |
| *Furniture Woods.* | | | | |
| Mahogany . . . . | 38,133 | 41,184 | 340,357 | 293,176 |
| Unenumerated . . . | 76,051 | 186,628 | 564,661 | 1,069,615 |
| | 114,184 | 227,812 | 905,018 | 1,362,791 |
| Wood pulp . . . . | 129,829 | 741,945 | 700,276 | 3,450,662 |
| House frames, etc. . . | — | — | 565,606 | 1,852,100 |
| Total loads . . . | 7,422,236 | 10,207,613 | 18,723,864 | 28,202,172 |

No account is taken in the above of the insignificant amount of home-grown timber exported; it was returned as £108,673 in value in 1909.

The most striking features of the comparison of the first and last years of twenty years' timber trade are that the value of the imports has risen 51 per cent. and the quality 38 per cent.; and that the home production has remained about the same, and probably does not exceed 2,000,000 loads, or say 16 per cent. of the total consumption.

A comparison of the individual classes of imports shows the enormous increases of 271 per cent. in the case of household frames, wood turnery, etc., and 471 per cent. in the case of wood pulp; whilst large increases are shown in the case of fir, which constituted 88 per cent. of the timber imported in 1890 and 85 per cent. in 1909.

These figures prove the growing need for timber, in spite of the numerous substitutes now employed, and are in themselves a great argument in favour of afforestation.

But, again, the majority of the land available for afforestation is naturally poor, or has deteriorated with exposure and so is for the most part only suitable for conifers. It is then just to meet the demand for fir, forming 85 per cent. or more of the imports, that afforestation would chiefly contribute. Not only this, but the demand for coniferous timber is increasing in all civilised countries, while, although a timber famine may be far removed, there is no doubt much of the more accessible forest is worked out.

That this is generally being recognised is shown by the restrictions now imposed on the lumbering trade in several countries where there is definite proof of excess felling in the past, whilst in others, too, active steps are being taken to preserve and ensure the proper working of existing forests and, in not a few, no opportunity is lost of extending their area.

More particular evidence of the exhaustion of the supplies of fir timber in the most accessible countries is furnished by a comparison of the origin of imports. The following compares the average quantities of fir timber annually imported from each country between 1890–4 and 1905–9, and shows the supplies to have decreased, or at least not to have risen in proportion to the demand, excepting in the case of Russia:

FIR IMPORTS

| | 1890–4. (1,000 loads.) | | | 1905–9. (1,000 loads.) | | |
|---|---|---|---|---|---|---|
| | Hewn. | Sawn. | Total. | Hewn. | Sawn. | Total. |
| Sweden | 380 | 1,503 | 1,883 | 427 | 1,380 | 1,807 |
| Russia | 318 | 1,283 | 1,601 | 1,134 | 2,283 | 3,417 |
| Canada | 99 | 1,091 | 1,190 | 24 | 1,091 | 1,115 |
| Norway | 313 | 421 | 734 | 247 | 468 | 715 |
| U.S.A. | 75[1] | 322 | 397 | 32[1] | 437 | 469 |
| Germany | 217 | 73 | 290 | 127 | 78 | 205 |
| France | 588[2] | — | 588 | 776[2] | — | 776 |
| Other countries | 55 | 25 | 80 | 331 | 55 | 386 |
| Total (1,000 loads) | 2,045 | 4,718 | 6,763 | 3,098 | 5,792 | 8,890 |

Large and sufficient supplies of coniferous timber exist in Canada, if not in Russia, but assuming that a reasonable financial proposition for home afforestation can be based on present market conditions, it does not seem unlikely that the justification will greatly increase before much more timber can be exported from these countries.

Again, the increased demand is well illustrated by the imports of fir timber into the United Kingdom. The following

[1] No pit props. [2] Pit props only.

are the average annual quantities and value for each of the periods of five years since 1890, and for many years previous to that a steady increase is also recorded :

FIR IMPORTS

| | Hewn. | | | Sawn. | | |
|---|---|---|---|---|---|---|
| | Loads. | £ | Per load. £ s. d. | Loads. | £ | Per load. £ s. d. |
| 1890–4 . | 2,045,175 | 3,015,360 | 1 9 6 | 4,718,284 | 10,355,450 | 2 3 11 |
| 1895–9 . | 2,199,590 | 3,143,440 | 1 8 7 | 6,062,000 | 13,904,503 | 2 5 10 |
| 1900–4 . | 2,708,759 | 3,098,203 | 1 9 1 | 6,278,419 | 16,410,303 | 2 12 3 |
| 1905–9 . | 3,098,203 | 4,212,908 | 1 7 2 | 5,792,561 | 15,423,050 | 2 13 3 |
| Increase or Decrease | + | + | − | + | + | + |
| 1890–4 to 1905–9 | 50 per cent. | 40 per cent. | 9 per cent. | 23 per cent. | 49 per cent. | 21 per cent. |

It is seen the imports have risen 50 per cent. in the case of hewn timber and 23 per cent. in the case of sawn, and whilst the price of the latter has increased 21 per cent. a fall of 9 per cent. is recorded for the hewn variety. The set-back in the quantity of sawn fir in the last quinquennium is attributable to trade depression. The average price of hewn timber is lower entirely owing to a disproportionate increase of pit props.

This definite evidence of the trend of supply and demand, and the characteristic increasing value of all produce of the soil, apart from the fall in value of money and the economic law of falling rate of interest with progress of culture, all give reason to believe that forestry will yet come within the scope of purely financial enterprise.

## THE EMPLOYMENT OF LABOUR

It is in a possible solution of the very serious labour questions of the present day that a number of people see the chief recommendation for forestry. Many of the arguments adduced are no doubt very potent and it stands to reason that every increase in the demand for labour must reduce the number of unemployed, but it takes only a brief examination of the practical side of the case to show how little afforestation can be applied to the direct relief of the unemployed.

In 1901 there were 16,395 woodmen in the United Kingdom or, compared with the 3,030,008 acres of woodlands returned, one to about every 185 acres. Considering the poor state of a large proportion of the woodlands it might be expected that the area per woodman would be much higher than in countries where more scientific and intense forestry is established. This, however, is not so marked: in Germany insurance statistics show that from 125 to 175 acres of State forest provide employment for one labourer for a year of 300 working days. Some explanation is no doubt to be found in the fact that those returned in the United Kingdom as woodmen are employed on work not strictly speaking of a forest nature, or on areas not included in that returned as woodland, but the fact remains that established forests do not in themselves provide employment for much labour. But this is not the whole case for afforestation. In the first place much of the mountain and heath land which might be afforested now only provides employment for one man to every 1,000 or 2,000 acres, while the preparation and initial stocking of this land with trees requires more labour than forest land which can be re-stocked by natural regeneration, sowing or indeed even by planting. Taking the average cost of clearing the ground, fencing and draining where necessary, raising the plants, putting them out and replacing any casualties at £6 10*s*. per acre, £4 10*s*. of this may not unfairly be taken to be spent directly on labour. The Royal Commission assumed £54 to be a reasonable annual remuneration for forest labour including housing and other incidental expenses, so that based on these figures the initial work of afforestation on 6 acres should provide employment for one man for the six months' duration of the planting season. With, say, 6,000,000 acres of land to be planted in sixty years, employment for six months would be provided by the initial work of afforestation for 16,000 men, or double of what the whole 6,000,000 acres are providing now. Great as this increase is, the number of persons dealt with by the distress committee has often recently exceeded 100,000, so that even if such labour were suitable, relief could only be provided for a small proportion. Apart from this, the margin of profit in forestry necessitates the greatest economies, particularly at the beginning of the rotation; for with an eighty-year rotation, the original outlay mounts up to nearly eleven-fold by the time the crop has reached maturity, when calculated with 3 per cent. compound interest. And apart from the probable increased cost of unemployed labour as such, there can be no doubt that the greatest economy is effected by initial success in planting and it requires but little practical experience of this operation to know that success is only ensured when skilled labour is employed. This may not be forthcoming in the United Kingdom to the extent necessary for a large scheme of afforestation and selected men might and would be taken from the ranks of the unemployed as well as from other occupations to meet the demand until a supply of skilled labour was established. This would take more than five years or so in each district and then whatever the degree to which direct relief had previously been afforded to those out of work, future relief would be solely in the expansion of the labour market. So that it seems out of place to associate the present problem of the unemployed directly with the question of afforestation dealing with sixty years or more; indeed to do so in practice would be to attempt to carry out relief work at the expense of an undertaking which in present circumstances is known not to do more than pay its way.

Other difficulties lie in the majority of the unemployed having congregated in the cities, far away from land which can profitably be planted, where they often have acquired habits unsuiting them to rough outdoor work.

Comparatively small then though the direct and immediate effects of afforestation on labour may be, there are others, however, which leave no doubt as to the ultimate benefits accruing, fully justifying the greatest efforts being made for their realisation.

In the first place it has been shown to what extent the demand for labour could permanently be increased; year by year this would be added to until, once the whole area say 6,000,000 acres were stocked, permanent employment would be provided for 40,000 men. Now this labour is for the most part required during the months October to March, just when the demand is smallest generally, particularly for agricultural labour, and the distress greatest. So that the relief afforded is greatest in the season when it is most needed; but not only this, it makes possible occupations which would otherwise not be so, and thus the relief extends beyond the actual labour employed. Throughout the country rural depopulation is in progress: the temporary labourer on the large farms is attracted by the hope of higher wages to the town where, however, the long roll of unemployed clearly shows the supply to exceed the demand, and where on closer examination the higher wages only too often prove illusory.

Much the same applies to the tenants of small holdings, whose profits are frequently insufficient to provide the wants throughout the year. It is in such cases that forestry, by supplying occupation in the winter months, will stem the migration to the towns.

If further proof be sought of the complementary nature of the relief afforded by forest work, it is readily found in German statistics. While in 1895 only 58,711 obtained their livelihood from forest work alone, 228,847 officials and workmen were in 1899 employed in the State forests alone, exclusive of labour for planting or for engagements for very short periods.

So far only the forest labour proper has been considered; there remains the labour employed in sawing and manufacturing the timber. £15,469,624 worth of sawn timber alone and £3,509,400 of wood pulp were imported into the United Kingdom in 1909, most of which timber was grown in approximately the same climate to the United Kingdom. To make a very conservative estimate the afforestation of 6,000,000 acres could provide half, or say £10,000,000 worth, and at least £3,000,000 now paid for foreign labour employed in the preparation of the timber or pulp might be providing a livelihood for 50,000 British workmen.

These are merely instances, and relate to the initial preparation of wood only; the demand for labour in other branches too would be materially increased with a regular and greater supply of home-grown wood of all qualities, as is apparent from the following return of imports for 1909:

| | £ |
|---|---|
| Manufactures of wood | 2,054,258 |
| Wood-pulp board and millboard | 535,614 |
| Matches | 598,660 |
| Brooms and brushes | 370,352 |
| Baskets and basket-ware | 213,885 |
| Toys and games | 1,438,606 |

Even admitting then the limitations of forestry in relation to the immediate labour problems, the case for it is overwhelming on the grounds enumerated.

### The Indirect Effects

Repeated attempts have been made to ascribe to forests, in an excessive degree, various effects on climate. Proof has generally been found in seemingly convincing facts; but an unbiased examination of the views held at different times leaves little doubt that they varied directly with cycles of meteorological conditions.

The difficulties of discriminating between cause and effect render it advisable to restrict any conclusions as to the influence of forest to what is proved beyond doubt. The mean annual temperature is found to be slightly lower (about ·1 to 1° C.) in forest than in the open country and the extremes both of heat and cold are reduced. No difference is to be found in the absolute humidity of the air within and without forest, so that only the somewhat lower temperature in forest gives rise to greater relative humidity.

This leads at once to the increase in rainfall due to forest which is popularly supposed to be considerable, and in which some people, not unnaturally, see a great drawback to afforestation in the United Kingdom. Experiments are still being made with a view to gauging to what extent this is the case. An increase is probably owing to the higher relative humidity in forest and the fact that the presence of the trees forces the clouds to rise, and that the trees with their branches and foliage mechanically sift the moisture-laden atmosphere.

On the other hand, air currents are constantly reducing the excess in relative humidity in the comparatively small volume of air within the forest, and, comparing the height of forest trees with the extent of atmosphere in which precipitation originates, the mechanical influence is not likely to be very noticeable.

In short, experiments have so far shown a slight increase in precipitation extending only a very short distance beyond the forest's limits: the influence ascribed to the presence of forest is more often than not due to the configuration of the ground, forests being now largely restricted to hilly districts unsuitable for agriculture. All fears, then, of a considerable increase of the rainfall of the United Kingdom owing to afforestation may be dismissed as unfounded.

Other influences of forest in the circulation of moisture are much more marked. Only a part of the precipitation actually reaches the soil, whilst a larger quantity of water is absorbed by trees than by crops or grass; this is counteracted to some extent by less evaporation of water directly from the soil; but, speaking generally, the fact that forest soil is only moister down to six inches in depth and drier than that of other land at a greater depth, seems to prove a balance capacity for consuming water. This is of the greatest importance, as it provides a means of reclaiming swampy land, of which there is much in the United Kingdom, particularly in Ireland. Again, where the rainfall is irregular or periodically scarce, the retarding of drainage by the trees themselves, the litter and undergrowth and the penetration of the soil facilitated by the loosening effect of the roots prolong the benefits of precipitation; this is particularly the case in hilly country where otherwise the drainage is rapid.

Other, and often very important, purposes for which forests are grown are the prevention of erosion, land-slips, and the silting-up of rivers and lowlands. This is due to the influence of forest on drainage and the stability afforded by the roots of trees. Although there may not be any great cause as yet for afforestation in Great Britain or Ireland on this account, yet instances are not wanting in which great havoc or more expensive and unproductive works might have been avoided had trees been planted.

The value of the protection from wind afforded by forest is generally admitted, and the purity of the forest air and the larger quantity of ozone it contains are advantages which cannot be overlooked in considering the benefit to all who work in or visit the forests, for unhealthy conditions associated with the greater relative humidity of the atmosphere do not arise in the temperate climate of the United Kingdom.

## PART III

The steady progress of opinion, from a desire to improve the condition of existing forests to the development of schemes for their extension, is strikingly illustrated in the findings of successive committees. The chief reasons for the need of more extended forests have been recapitulated; it is proposed now to describe how practical progress can best be made.

It is natural to inquire the reasons for the unsatisfactory conditions of so many of the forests, as this is advanced by many, who have not thoroughly studied the question, as indicating probable failure of any attempts at further afforestation. The reasons are many. In the first place the rapidly increasing demand for timber before the development of means of communication placed supplies from other countries at disposal was the incentive for the most reckless destruction of forest. Trees were felled regardless of their immaturity, and, if any were retained, they were those which were of little or no service. This occurred in other countries too, even where the present condition of forests is far superior to that of the United Kingdom, but circumstances were different. In the United Kingdom development began earlier, forests were more accessible, the supply of coal caused greater independence of the yield of fuel from forests and there were no striking examples of erosion consequent on disforestation. The financial aspect of forestry was of course not recognised and the only real stimulus to afforestation was provided by shortage of timber for the Navy. In consequence, oak was sown in a large number of areas, often regardless of the conditions of the locality, as is shown by some of the forests now surviving. Again, bent stems and branches had a particular value in shipbuilding, and to meet the requirements the oak were frequently grown far apart. Admirably, however, as this served the purpose, while still admitting of the production of a number of fine straight oak, the treatment could naturally not be applied to other species without producing results commensurate with their natural power of development. But identical treatment was applied, and, promoted by the not unsatisfactory results in the case of larch, is the cause of rejection of so much home-grown timber to-day. Further, a more permanent damage has been done by the inadequate protection afforded to the soil and its consequent deterioration, especially where grazing was permitted. A striking example of this is furnished by the Forest of Dean, where occasional fine old oak-trees surviving from a time when the species was mixed with beech are found amongst the open and poor crops of oak grown pure during the last century when grazing was permitted.

Very considerable influence has also been exercised upon the treatment of forests, more particularly in the past few decades, by considerations of sport. The abundance of natural undergrowth, a condition favouring both ground and winged game, has exactly the reverse effect in regard to the success of modern systematic forestry.

Finally, a cause of many more recent, and, it is feared, future, failures has been an attempt to grow species, often exotic, which happen to be in demand, or otherwise to find favour, regardless of suitability of locality. Such plantations can no more be said to come within the sphere of economic forestry than can areas managed chiefly for sport. Experiments with exotic species are only justifiable on a strictly experimental scale where, after careful examination, conditions similar to those of their natural habitat are found to exist, and where the exotic species more fully meets a requirement than indigenous trees. Obvious as this may seem, how frequently is the failure of a plantation ascribable to want of its appreciation, how many poor oak forests or diseased larch crops are now striking proofs of its disregard, and how many more plantations of Japanese larch and other exotics may only swell the number? Similar experience is not wanting in other countries where more advanced forestry is practised, but it is particularly striking in the United Kingdom, where much more intense management is necessary before the best developments of indigenous species are attained.

These fundamental causes of deterioration in the condition of the forests and in the quality of their produce have had the further consequence of irregular yield and poor financial returns. Thus it is that traders have looked to other sources for their supplies and the forest-owners have found in the amenities of forestry the only incentive for extending, or justification for preserving, their forests. The dependence on imported timber has grown further with want of organisation of the home timber market and the exclusion of home-grown timber from concessions granted by railways to the regular traffic of imported timber; while the attitude of the owner has been strengthened by unfair taxation based on a fallacious discrimination between the factors of production and produce.

In these circumstances it is most imperative not only that instruction in theoretical forestry should be provided, but that a more general appreciation of the possibilities should be cultivated by application of its principles to existing forests.

Some progress has been made in the matter of forest education, as has been shown, and a number of forests, both private and State property, have been put under regular working plans. Unfortunately, however, these forests are not always very accessible, or their condition was such when taken in hand that the effects of proper treatment are not yet so striking as is necessary to carry conviction. Consequently a number of those people who have been trained in forestry or have taken an interest in its development are inclined to stamp the teaching as "continental" and inapplicable to British conditions. And instead of examining the conditions and seeking the cause of any need for modification in the application of the theoretical principles they have learnt, they have been content to adopt only so much of the theory as takes their fancy and for the rest to adhere to old methods, arguing that, after all, practical experience is the surest guide. This want of confidence is most difficult to overcome, particularly as practical experience is invaluable so long as it is properly understood and suitably applied.

Herein lies one of the chief duties of a State anxious to improve the condition of forests and to extend their area, unless it is seriously contemplated to restrict all such enterprise to State initiative.

The first need, then, is a clear discrimination in the case of all State forests between those which are to be treated on an economic basis and those whose chief object is to provide other amenities. The former should then be placed under proper management and treated under the approved systems of modern sylviculture. Where, however, for reasons already mentioned, these do not provide adequate and suitable illustrations of the most advanced methods, other forests in suitable condition should be acquired and brought under proper management.

Far-reaching as the indirect effect of such practical proof of State appreciation of sylviculture would be, knowledge of how in each case the system of treatment adopted has been determined would add greatly to the conviction such examples carried. Moreover, with a staff of experts employed to prescribe suitable treatment and superintend its application, expert advice could easily be made available for owners who desired it. Finally, apart from influence on the management of other forests, the advantage of converting unproductive State property into productive itself justifies the action suggested.

Essential as this step would seem to be for any permanent progress of forestry in the United Kingdom, it is strange that opinion has jumped from apathy to a demand for more adequate forest instruction and then directly to the consideration of schemes for planting up new areas, often of vast extent.

It has been shown how inadequate the present area of forest is and not only what scope for extension exists, but also what benefits will accrue from an increase of forestry; it is now proposed to consider how action can best be taken in this matter.

In the first place, the average returns from forests in the United Kingdom are not such as to encourage landowners to invest further in them; and the increasing taxation of land, the unfair incidence of taxes on forest property, and the time for which capital employed in producing timber is locked up, finally preclude, in existing circumstances, any extensive afforestation by private initiative. The demonstration of proper treatment of State forests as proposed may encourage some, and their number will increase if advice is forthcoming, taxation is revised, and financial assistance possibly granted; but it seems hardly reasonable to trust to private enterprise in a matter which, based on present returns, shows a narrow margin of profit and has other objects than purely financial ones. In any case a lead is necessary, and this can best be given in the shape of practical example.

There is no need for Government to commit itself at the outset to any such vast scheme as that proposed by the recent Royal Commission, for not only has much practical experience in organised afforestation yet to be gained, but it is at all events doubtful whether suitable labour is now available to the extent necessary, and certainly the plants are not at a reasonable price.

The chief value of such schemes lies in their demonstration of the possibilities of afforestation, but it is a mistake to hold nothing short of them to be worth attempting and to let this, and a desire for regular sustained yield nearly a century hence, cause more cautious persons to withhold their support.

The first step should be the compilation of a statement of areas suitable for afforestation as recommended by the Committee of 1902, but with additional information, so far as a preliminary examination will permit, as to the area suitable for each species, the quality of locality, and the probable effect of afforestation on the utility of the remaining land and on the local population.

This should be carried out by forest experts in Government employ, but the assistance of owners and local authorities should be solicited, and the progress of inquiries and their results should be made known. Crown property and surrounding land should first be dealt with, and the inquiries should then be extended to areas where conditions are thought likely to be favourable, and the possible acquisition of a workable area of land by the State least difficult. The collection of information would in no way preclude planting from proceeding as suitable areas were made available; indeed, the experience would be of the greatest value in future surveys if the staff employed for the earlier were also employed to supervise the later planting operations. In this way no time would be lost in making a start, work could be gradually extended as opportunity offered and as the supply of skilled labour increased and forests became available, and there would be less chance of those failures which must be feared if the ideal complete and symmetrical scheme is aimed at from the start. Another great advantage will be the better position of Government to judge to what extent private initiative can be counted on and how the deterrents now complained of can best be overcome.

Having outlined the procedure to be followed, it is necessary to consider the question of species and treatment best calculated to produce large quantities of serviceable timber.

The selection of species is a matter which can only be decided after careful observation of the conditions of each locality to be dealt with. Climate must be carefully considered, and not, as is so often the case, the decision based almost entirely on the nature of the soil, or possibly even fancy for a particular species. It is not within the scope of this paper to go into the requirements of each of the indigenous forest trees or species the introduction of which may be said to have passed the experimental stage, but a list may be given of the more important ones in order of the amount of warmth required for their best development. The order must be understood to be only roughly relative and indicative of the species suited from the warmest localities in the United Kingdom to where the limit of tree growth is reached:

Chestnut (*Castanea vesca*), Oaks (*Quercus sessiliflora* and *Q. pedunculata*), Ash (*Fraxinus excelsior*), Hornbeam (*Carpinus Betulus*), Corsican Pine (*Pinus Laricio*), Austrian Pine (*Pinus austriaca*), Beech (*Fagus sylvatica*), Douglas Fir (*Pseudotsuga Douglasii*), *Picea Sikkaensis*, Scotch Fir (*Pinus sylvestris*), *Pinus Banksiana*, Willows (*Salix* spp.), Birches (*Betula* spp.), Alders

(*Alnus* spp.), Poplars (*Populus* spp.), Weymouth Pine (*Pinus Strobus*), Silver Fir (*Abies pectinata*), Spruce Fir (*Picea excelsa*), Siberian Cedar (*Pinus Cembra*), Larch (*Larix europæa*).

Given a suitable soil, a number of these species may be grown almost throughout the United Kingdom and the warmer the locality the less exacting they will be as to quality of soil and quantity of light, although greater moisture of soil and humidity of air will be necessary. The higher the temperature the more rapid too will be their development at first, both as regards height, girth and volume, but the reverse is the case after the first few decades. Again, although the toughness and flexibility of the timber will be greater, all the other qualities will be best where the development is best sustained. Thus it is seen how necessary it is to endeavour to select the species to which the local conditions are best suited and not to be deceived by the early growth of any species.

As regards soil, the quantity, consistency and water capacity are of first importance. Generally speaking the broad-leaved trees are most exacting and the two or three needled pines least, while intermediate between the two are *Pinus Strobus*, *Pinus Cembra*, Picea, Abies, Populus and Salix and Betula.

It is evident from these considerations that conifers will generally have to be selected for the purpose of afforesting the poor and high-lying land, which is that chiefly available, and to do so will be quite in keeping with the object of reducing the dependence on foreign imports.

The greatest security is to be found in establishing each species where conditions indicate it as likely to give the best results without impoverishing the soil, and not in the indiscriminate mixture of a number of species which may thrive. If this be the guiding principle, the variety of species cultivated is still not unduly restricted and full account can be taken of changes in the quality of the soil. The development of the individual trees is better, as they generally have to contend with their own species and not with others on which the conditions may have different influences. In this way the land is utilised to its fullest extent and, whilst the chief advantages of a mixed crop are attained, the struggle for existence does not leave the same doubt as to the composition of the final crop, nor does it necessitate constant intervention as in the case of crops in which there is an intimate mixture of different species.

Passing to the question of treatment, in the first instance the three chief systems of coppice, coppice with standards, and high forest present themselves. The partial or wholly coppice systems have found favour in the past for a variety of reasons, the chief amongst which were:

(1) More frequent returns and the consequent employment of less capital.

(2) The provision of conditions more suitable for the rearing of game.

(3) The simplicity of regeneration and management.

(4) The larger volume production of wood within a given period.

Added to these the common error of miscalculating the financial yield from coppice with standards gave this system an apparent advantage over high forest.

As regards considerations of sport, these only find place in any scheme for economic afforestation to the extent to which the effect on the sporting rental influences the financial yield. There are hardly any areas suitable for forests that will not yield larger returns under proper treatment than if managed solely for sport; where sporting considerations are permitted to determine the method of treatment, the adverse effect on the forest yield must be treated as the cost of so doing.

Frequency of returns and the employment of less capital are undoubted attractions to the private owner, but do not carry the same weight in a national scheme in which the first financial consideration must be highest sustained yield on the investment. It needs only a brief examination of the principles of coppice systems to show that they do not ensure this to the same extent as high forest.

In the first place the treatment is applied chiefly to broad-leaved species, the demand for which does not show an increase approaching that for coniferous timber; and again the yield is not of the same variety as in the case of high forest, and so the financial success is dependent on the demand for a restricted variety of produce. The far-reaching effect of this is illustrated by the poor returns now obtained from coppice forests in most countries owing to the substitution of other fuel for firewood and the employment of more powerful tanning materials than the bark of oak coppice. Generally speaking, although coppice systems will find place in a national scheme of afforestation, greater caution is required in their prescription owing to the danger of over-production of a limited number of products, and, what is of even greater importance, the possible exhaustion of the soil consequent on more complete utilisation. Finally much of the area contemplated for planting in the United Kingdom has not the warm climate so necessary for the successful growing of coppice and in most cases the soil has already deteriorated from over-exposure.

High forest must then be the system chiefly employed and it remains to consider the question of establishing, tending and harvesting the crops.

These must primarily depend on the quality of locality and species in each area, but seeing how abundant the growth of heather, bracken and other weeds is over most of the land, and how necessary it is to protect the soil as soon as possible, planting in one form or another will generally be advisable. This is more expensive than sowing and still more than natural regeneration, but it has already been pointed out how the initial expenditure mounts up at compound interest by the end of rotation, and, while natural regeneration is obviously out of the question in areas which are not wooded, planting, being more likely to prove successful from the outset, is likely to be less expensive in the end.

In future working the attainment of natural regeneration should be sought, for not only is it the cheapest method, but it most effectually reduces the liability of the soil to deterioration from the time of removal of the old crop until the new one is established. To ensure the greatest chance of success in this the crops will have to be tended with this object in view. One of the chief requirements for natural regeneration is a receptive condition of the soil and this is seldom naturally provided when required in forests which are managed to meet certain economic requirements to their fullest extent. For instance, a closely grown crop of shade-bearers does not sufficiently permit of the disintegration of litter to enable seedlings to reach the mineral soil, whilst in a crop of light-demanding species the natural early opening out of the canopy will induce conditions suitable for regeneration before seed is produced abundantly, or before the parent crop can be removed and the seedlings allowed to develop. In either case it must be the object of the forester to remedy these conditions. In the case of the light-demanding crop, this is best done by underplanting with a shade-bearing species, preferably the soil-improving beech, which will preserve the condition of the soil and prevent the growth of weeds, and in the case of shade-bearers the same treatment may be applied when, after the chief height growth is completed, a heavy thinning is made; or the thinnings may be regulated to produce similar results without underplanting. In either case failure to treat the crops as described may preclude the possibility of natural regeneration, or at any rate may so prolong the period of regeneration that the expedience and economy of the method will be doubtful.

Finally the question of rotation has to be considered. In the case of the common conifers the best financial results are generally found to be obtained when the final crop is removed at an age of from 75–90 years, while in the case of oak 120 years, and in the case of beech 80-100 years may be taken as average. It is impossible accurately to fix the best rotation when a crop is first established, indeed, it is unnecessary to do so. The financial rotation is the only one properly employed in economic forestry, and this depends chiefly on the growth of the crop and the demand for various classes of timber. This rotation corresponds with the age at which after calculating all revenue and expenditure at a fixed rate of compound interest the largest surplus or soil value remains. Practically it is best determined by means of the "Weiser-prozent" which may briefly be stated to be the rate of interest which at any given time a crop is producing on the capital it represents. The rate of interest fixed for the working capital has considerable influence on the rotations so determined, and 3 per cent. is most suitably employed.

Indisputable as the necessity for such a sound financial basis may seem, it is curious that there are still supporters of methods having as their object the maximum annual returns regardless of cost, and that in the United Kingdom there are advocates of afforestation with the chief object of producing pit timber. The demand for this commodity has greatly increased, but the increase in the price is not in proportion to that for other coniferous timber, thus showing the supplies to be less restricted. This, indeed, is to be expected, seeing that even forests grown for longer rotations than necessary for pit timber yield a large quantity of suitable materials in thinnings; moreover, should occasion arise, there is no difficulty in recalculating the financial rotation as circumstances change. Even under the most favourable conditions the best rotation cannot be determined to a year, and so success can be assured independently of a temporary state of the timber market.

In connection with the financial rotation it must be pointed out how necessary it is that the results of forest enterprise should be uniformly stated in land values obtained on a fixed basis of interest, most suitably 3 per cent. The want of this not only encourages a number of critics to produce seemingly conclusive proofs of the probable failure of afforestation, but results stated in a form not readily understood or compared may do more harm than their very attractive appearance may do good.

If, then, there is a need of afforestation in the United Kingdom of Great Britain and Ireland, as has been shown and as is now generally admitted, its realisation should primarily be based on sound sylvicultural principles and the best sustained financial returns. Speculation as to future conditions and over-anxiety for symmetry in establishing and managing the forests are out of place.

# THE CORROSION OF IRON AND OTHER METALS[1]

"But though such is my view, I put it forth with all the reservation made on former occasions. I do not pretend to explain all points of difficulty. . . . I profess rather to point out the difficulties in the way of the views which are at present somewhat too easily accepted and to shake men's minds from their habitual trust in them; for next to developing and expounding that appears to me the most useful and effectual way of really advancing the subject:—it is better to be aware or even to suspect we are wrong than to be unconsciously or easily led to accept an error as right."—FARADAY, *On some Points of Magnetic Philosophy*, 1854.

ON account of the ever-increasing use that is made of steel not only in constructing machinery, rail- and tram-ways, bridges and ships of every kind but also as the framework of buildings often of huge dimensions, it is most important that engineers and all who are responsible for the care of iron and steel structures should be fully alive to the conditions under which rusting takes place and to the precautions that must be taken if the metal is to be preserved from decay. The subject was discussed in this Journal in 1907, in the January number, with reference to experiments carried out by Dr. G. T. Moody, whose results had been made public in the previous year. In the interval the conclusion Moody arrived at that rusting is necessarily and always *primarily* conditioned by the presence of acid has either been challenged by more than one observer or the issue obscured by the introduction of other considerations. It therefore appears to be

[1] *The Corrosion and Preservation of Iron and Steel.* By Allerton S. Cushman, A.M., Ph.D., and Henry A. Gardner. (McGraw-Hill Book Co., 6, Bouverie Street, London, E.C. 1910. Price 17*s*.).—"The Corrosion of Iron and Steel." By W. H. Walker, A. M. Cederholm, and L. N. Bent, *Journ. Amer. Chem. Soc.* 1907, **25**, 394.—"The Rusting of Iron." By William Augustus Tilden, *Chem. Soc. Trans.* 1908, 1365.—"The Rusting of Iron." By J. Newton Friend, *Journ. of the Iron and Steel Institute*, 1908, No. 11.—"The Preservation of Iron and Steel." By A. S. Cushman. "The Electrolytic Theory of the Corrosion of Iron and its Applications." By W. H. Walker (with discussion), *ibid.* 1909, No. 1.—"The Rusting of Iron." By J. Newton Friend, *Chem. Soc. Proc.* 1910, 179.—"The Wet Oxidation of Metals: I. The Rusting of Iron." By Bertram Lambert and J. C. Thomson, *Chem. Soc. Trans.* 1910, 2426.

desirable to reopen the discussion from this point of view and to consider the cogency of the arguments advanced, especially as Moody's contention is not accepted by Messrs. Cushman and Gardner in their recent comprehensive book on the subject of the corrosion of metals, a work which will doubtless be regarded by engineers and builders as authoritative on account of the position one of its authors occupies in the United States Department of Agriculture and the attention he appears to have given to the subject.

The historian of the future considering the growth of scientific knowledge during the latter half of the nineteenth century will be sorely puzzled probably to account for the slowness with which chemists arrived at clear conceptions of the nature of chemical change and of the conditions that determine it. Perhaps he will be forced to conclude that it is not within the compass of average human nature to be scientific—that dogma must necessarily be preferred above science. It is certainly strange that a problem of such importance as that afforded by the rusting of iron should be a subject on which opinions can differ to the extent apparent in recent discussions.

If the conditions that determine the attack of metals are to be appreciated, there are certain fundamental facts upon which, in the first instance, attention should be centred.

When common Spelter—the crude Zinc of commerce—is placed in dilute sulphuric or muriatic acid, it is at once attacked and dissolved. In most text-books the interaction is represented, without further remark, by the expression :

$$Zn + H_2SO_4 = H_2 + ZnSO_4.$$

The student is allowed to believe that the metal actually displaces hydrogen from the acid ; usually no account is taken of the part played by the impurities in the zinc. But as zinc is freed from the impurities which are associated with it in spelter —arsenic, lead, graphite, etc.—it becomes less and less readily attackable by acids and ultimately all but insoluble. All who have worked with carefully purified material agree that this is the case : for example, Lieut.-Colonel Reynolds and Professor Ramsay (*Chem. Soc. Trans.* 1887, **51**, 857) in speaking of a specimen of the metal which they had separated from most carefully purified zinc sulphate by electrolysis and then volatilised in an exhausted tube of hard glass, say : "The sublimed metal appeared to be so pure that it was nearly unacted on by sulphuric or hydrochloric acid."

Such is the experience of every one who has had occasion to purify zinc. It is only logical to assume therefore that zinc *pure and simple* would not be attacked by such acids.

It may be desirable to insist here that a pure material must ever remain an abstraction, taking into account the fact that any and every substance is necessarily in some degree subject during its preparation to contamination by its environment.

It has long been known that even highly impure zinc may be "protected" by amalgamation with mercury, that is to say, by rubbing this metal over the surface of the zinc after it has been a short time exposed to the action of acid. The property is one of extreme value and is shared with zinc by no other metal except the allied element cadmium—hence it is that amalgamated zinc was so long made use of in voltaic batteries instead of the cheaper iron, which approaches zinc in electromotive efficiency.

Faraday explained the effect of impurities in the zinc in causing it to dissolve in acid in the following lucid terms (*Experimental Researches in Electricity*, 1834, Series VIII., § 998).

"The cause is, that when ordinary zinc is acted upon by dilute sulphuric acid, portions of copper, lead, cadmium or other metals which it may contain are set free upon its surface; and these, being in contact with the zinc, form small but very active voltaic circles, which cause great destruction of the zinc and evolution of hydrogen, apparently upon the zinc surface but really upon the surface of these incidental metals. In the same proportion as they serve to discharge or convey the electricity back to the zinc do they diminish its power of producing an electric current which shall extend to a greater distance across the acid and be discharged only through the copper or platina plate which is associated with it for the purpose of forming a voltaic apparatus."

The explanation Faraday gave of the effect of amalgamation is as follows (*ibid.* § 1,000):

"It is probable that the mercury acts by bringing the surface, in consequence of its fluidity, into one uniform condition and preventing those differences in character between one spot and another which are necessary for the formation of the minute voltaic circuits referred to. If any difference does exist at the first moment, with regard to the proportion of zinc and mercury, at one spot on the *surface* as compared with another, that spot having the least mercury is first acted upon and, by solution of the zinc, is soon placed in the same condition as the other parts and the whole plate rendered superficially uniform. One part cannot therefore act as a discharger to another; and hence *all* the chemical power upon the water at its surface is in that equable condition which, though it tends to produce an electric current through the liquid to another plate of metal which can act as a discharger, presents no irregularities by which any one part, having weaker affinities for oxygen, can act as a discharger to another."

No better explanation has yet been advanced. We are still content to think of the zinc surface as reduced to "an equable condition"; whatever the explanation, the effect produced is very remarkable.

Although the presence of mercury serves to protect the zinc against acid, action sets in immediately the amalgamated surface is touched by any "metallic" conductor relatively negative to zinc. It is not improbable that the zinc is protected because it is covered with a liquid film of a saturated solution of zinc in mercury so constituted that the uppermost layer consists of mercury only, much as an aqueous solution may be thought of as covered with a tenuous film of pure water; the electro-positive zinc may be thought of as brought to the surface immediately contact is made with the relatively electronegative conductor: then as being at once gripped by the acid radicle and drawn into solution.

The essential act by which attack is determined is the formation of a conducting circuit of three components, two being metals, the third an acid; all are conductors of electricity but one of them, the acid solution, is decomposed or electrolysed in conducting—that is to say, it is an electrolyte. These are the conditions apparently that obtain in any and every case of chemical change—in other words, the occurrence of chemical change is dependent on the production of an electric current but this current is only produced as the change is consummated: the two phenomena are interdependent and inseparable. Such may be said to be the electrolytic theory of chemical change—the theory being that change only takes place when, to use Faraday's words, "active voltaic circles" are formed.

The evidence brought forward in support of this view during the past five-and-twenty years is remarkably complete and cannot well be gainsaid. In case after case it has been shown that the implication constantly set before students of chemistry that combination or interaction takes place between two compounds A and B is incorrect and that in all instances, to promote change, a third substance must be coupled with these, the third "substance" being of such nature that, if not itself an electrolyte, it is one from which an electrolyte is produced by association with either A or B, the presence of an electrolyte being an essential feature in the occurrence of change.

The student is invariably told that oxygen is a supporter of combustion and is led to think that oxygen and the burning substance are the two factors in the process. But as a matter of fact, carefully dried carbon and phosphorus do not burn in carefully dried oxygen ; they will take fire only in the moist gas. And although ammonia and hydrogen chloride prepared in the ordinary way combine with the greatest readiness, forming solid ammonium chloride, if very special care be taken to dry both gases nothing happens when they are mixed.

Again, an ordinary mixture of hydrogen and oxygen is exploded with the greatest readiness but if prepared with special care by electrolysing an alkaline solution of baryta, so that it contains no trace of acid, the mixture, even if dried simply by freezing out water from it by means of liquid air, is no longer explosive. A coil of silver wire may be heated to redness in the dried mixture without firing it—yet liquid water gradually makes its appearance, the interaction taking place slowly and without explosive violence. This result is one of the very greatest importance, as it is a proof that the presence of water is not in itself sufficient to bring about the change: nor should it be, from the electrolytic point of view advocated above, as water is not an electrolyte; only "dirty" water behaves as an electrolyte—that is to say, water containing some substance dissolved in it—a trace of an acid, of an alkali or of a salt, as the case may require. There can be little if any doubt that in cases in which moisture alone has been observed to condition change, it is because traces of "impurity" were present together with water; obviously it is impossible to avoid such and it must therefore always be impossible to prevent a change such as that involved in the formation of water from the elementary gases hydrogen and oxygen from taking place to some slight extent.

It would be ungracious to pass from this subject without referring to the great debt which chemists owe to Dr. Brereton Baker for his work on highly purified materials—work which he has carried on with unwearied perseverance and with a degree of skill that appears to be altogether peculiar to himself. Strangely enough, such work has only been done in this country and but little attention has been paid to the subject elsewhere; the significance of Baker's results in relation to the problems of chemical change is certainly in no way properly appreciated at present.

In the discussion that has taken place since the results of Moody's experiments were made known, it has not been denied that iron dissolves in presence of the carbonic acid which is necessarily present in ordinary water and in the film of water which condenses on the surface of the metal whenever it is exposed in a moist atmosphere at a sufficiently low temperature. What is asserted is that corrosion may and does take place in the absence of carbonic or any other other acid. This assertion may be considered from two points of view—firstly, with reference to the observations on which it is based: that is to say, with reference to the facts; secondly, from the point of view of the hypothesis by which it is supported.

The character of the evidence put forward will be best understood if a few quotations be given from Cushman and Gardner's work descriptive of experiments affording results which they claim are proof that iron is dissolved without the intervention of acid and that rusting may be conditioned by water and oxygen alone:

"The two clean Jena flasks A and B are three-quarters filled with pure freshly distilled water. Two drops of an alcoholic solution of phenolphthalein indicator (1 gram in 100 cc. pure alcohol) are added to the water in each of the flasks. The beaker C is more capacious than the flasks A and B. The flasks D and E are used in each experiment as blanks to check the results obtained. After connecting up as shown, the water in each vessel is simultaneously boiled very vigorously until about one-quarter is boiled off. The rubber stopper in A is then lifted and clean, polished strips of iron quickly slipped in. The stopper is again tightly inserted and the boiling continued for about fifteen minutes. The lamps under A and E are then extinguished, while the water in B, C and D continues to boil. As soon as flasks A and E have sucked back into boiling water so that they are completely filled, the lamps under flasks B and D are also extinguished. When B is quite full, flasks A and B are quickly cooled by surrounding them with cold water. The valve at F is then closed. By this means the bright specimens are immersed in water practically free from air, oxygen or carbonic acid and may be kept under observation for any desired length of time. This experiment has been repeated a great number of times with different samples of iron and steel and no rusting has ever been observed unless air was allowed to enter."

"It has been shown that the electrolytic theory of the wet oxidation of iron is based on the premise that iron must first go into solution, an equivalent amount of hydrogen being set

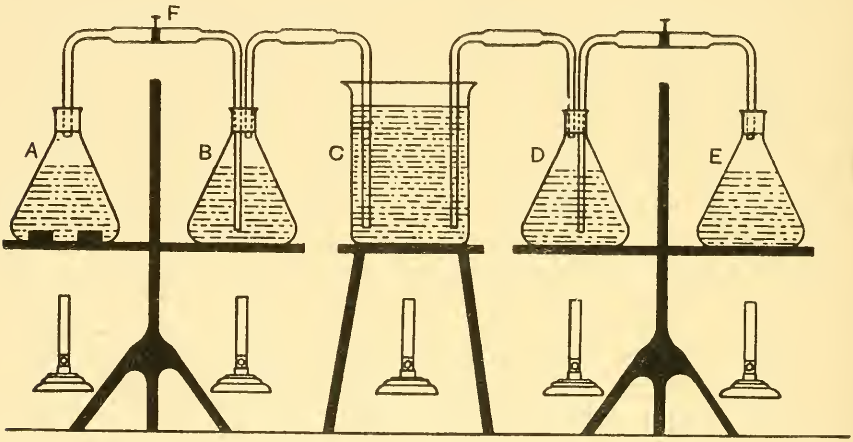



FIG. 1.

free. The resulting ferrous hydroxide in solution betrays its presence by producing a pink coloration with the phenolphthalein indicator. In every experiment made the pink colour was seen, although in some cases the colour developed slowly and only after the lapse of a number of hours. That the colour was not due to the action of water on the Jena glass is shown by the fact that no colour appeared on the blank side of the experiments."

In other words, it is argued by Cushman and Gardner that a certain amount of iron was dissolved and that it was dissolved by the action of water alone. Further proof that iron had been dissolved is given by their statement made with reference to other similar experiments that on allowing oxygen carefully purified from carbonic acid to bubble through the flasks rust appeared on the bright metal surfaces in five minutes or less and in one hour was deep and heavy.

There is no reason to dispute the conclusion arrived at by Cushman and Gardner that whilst some iron is dissolved no rusting takes place under conditions such as were realised in their experiments until oxygen was admitted: it may well be that boiling the water in the manner described would have the effect of so nearly removing the whole of the oxygen dissolved in the water that there should be no obvious rusting under such conditions. It is known, however, that the complete displacement of carbon dioxide from water is a very difficult task, if not an impossibility; it cannot be admitted that the treatment described would be effective in removing it entirely.

Cushman and Gardner themselves admit that it may be doubted whether it is possible to boil out all carbonic acid from the water contained in the apparatus described. They contend, however, that "granting this is the case in regard to the last traces, it is easily shown that the hydrogen ions which would be supplied by a minute quantity of carbonic acid are of no more importance than the hydrogen ions supplied by the normal dissociation of pure water and that the assumption that carbonic acid must be present is quite unnecessary."

They then call attention to Whitney's argument that the concentration of "hydrogen ions" in a solution of carbon dioxide in equilibrium with ordinary air at 15° is 16 grams per 10,000,000 litres of water—"or only sixteen times as many as perfectly pure water contains; and that, at the boiling temperature, the carbon dioxide dissolved would probably yield a concentration of hydrogen ions even less than in pure water, for not only is the solubility of the gas greatly diminished but the dissociation of water is greatly increased by rise of temperature."

We are here brought into the region of pure speculation, not to say obfuscation; a definite issue is raised, however, that can be discussed.

Nothing could be more complete than the following statement by Dr. Cushman:

"It appears to the writer to be demonstrated that Whitney was right in his assertion that iron goes into solution up to a certain maximum concentration in pure water without the aid of oxygen, carbonic acid or other reacting substances.

"*Rusting of Iron primarily due to Attack by Hydrogen Ions.*—This point established, it becomes apparent that the rusting of iron is primarily due *not to attack by oxygen but by hydrogen ions.*"

In the interest of engineers and others not versed in the scientific jargon of the day, who wish to have explanations given in plain terms, it is clearly desirable to consider what is meant by such statements.

The cult of the free and independent ion has so dominated most branches of physical and biological science during the past twenty-five years that it may almost be ranked with influenza and plague as one of the infectious complaints that have been brought into prominence in modern times. The manner of its development is very remarkable.

Having made the fundamental discovery that compound substances which are decomposed by an electric current are resolved into two parts, Faraday in 1834 coined the word *ion* simply to express "those bodies which can pass to the electrodes in electrolysis."

It was not until 1857 that Clausius postulated the conception that probably some of the encounters that take place between the moving compound molecules in a liquid are so violent that some of the molecules are broken up into their constituent atoms. To explain electrolysis, he assumed that when an electromotive force is applied to a liquid it does not produce disruptions and reunions but finding these already going on influences the motions of the parts during their intervals of freedom, so that the positive move more in the positive direction and the negative move more in the negative direction than would be the case if they were uninfluenced. Clausius apparently did not suppose that compounds were broken up only into their ions; indeed, on his kinetic hypothesis it is difficult to see why all of the atoms in a compound should not make their separate appearance at the electrodes—why all compounds which undergo electrolysis should always be broken up in a particular way into two ions only, the one being always hydrogen or a metal, the other whatever is associated with the hydrogen or metal in the compound; furthermore why so many compounds—organic compounds in particular—should not be in the least affected by an electric current. The fact that, in so many cases, when substances are mixed which by themselves have no electrical conducting power, solutions are obtained which conduct readily is also in no way accounted for by the hypothesis.

In 1883, without considering any of the difficulties inherent in the Clausius hypothesis, Arrhenius put forward the proposition that not only was the conductivity of solutions to be ascribed to the presence in them of the dissociated, electrically charged *ions* of the dissolved substance but that it was proportional to the degree of dissociation and he assumed that the dissociation was practically complete in dilute solutions of strong acids. He recognised, moreover, that there was a close correspondence between chemical activity and electrical conductivity in the case of acids; in fact, his conclusions were summarised in the pregnant sentence: "L'activité électrolytique se confonde avec l'activité chimique."

The conclusions at which Arrhenius arrived were so fascinating and the correlation of chemical with physical activity was so welcome, that the hearts of chemists were captured forthwith; it was a case of unreflecting love at first sight. The fact was overlooked that the solvent was in no way taken into account; the violence of the assumption made in supposing that practically all of the molecules of the dissolved substance "went smash" while those of the solvent remained unaffected was not remarked. To the present day, conductivity is very generally ascribed to the presence of free dissociated ions and to these alone.

After this digression, consideration of Cushman and Gardner's argument may be resumed. Having stated that three "theories" of rusting are to be considered: (1) The carbonic acid theory; (2) the peroxide theory; (3) the electrolytic theory, they proceed to dismiss (1) and (2) on various grounds, and then state the third in the following terms:

"*The Electrolytic Theory.*—From the standpoint of the modern theory of solutions, all reactions which take place in the wet way are attended with certain readjustments of the electrical states of the reacting ions. The electrolytic theory of rusting assumes that before iron can oxidise in the wet way it must first pass into solution as a ferrous iron. The subject has been interestingly treated by Whitney, who discussed it from the standpoint of Nernst's conception of the source of electromotive force between a metal and a solution. When a strip of metallic iron is placed in a solution of copper sulphate, iron passes into solution and copper is deposited, this change being of course accompanied by a transfer of electrical charge from the ions of copper to those of iron. Hydrogen acts as a metal and is electrolytically classed with copper in relation to iron. If, therefore, we immerse a strip of iron in a solution containing hydrogen ions, an exactly similar reaction will take place, iron will go into solution and hydrogen will pass from the electrically charged or ionic to the atomic or gaseous condition. In such a system the solution of the iron and, therefore, its subsequent oxidation, must be accompanied by a 'precipitation' or setting free of hydrogen. It is very well known that solutions of ferrous salts, as well as freshly precipitated ferrous hydroxide, are rapidly oxidised by the free oxygen of the air to the ferric conditions, so that if the electrolytic theory can account for the original solution of the iron the explanation of rusting becomes an exceedingly simple one.

"*Pure Water a Solvent of Iron.*—As iron has been shown by Whitney, Dunstan and one of the authors, to rust in the presence of pure water and oxygen alone, the electrolytic theory as a fundamental cause of the wet oxidation of iron must stand or fall on the determination of one crucial question, viz. Does iron pass into solution, even to the slightest extent, in pure water? If iron does dissolve, the electrolytic theory is so far satisfactory; if it does not dissolve, we must conclude that the oxygen finds some way of directly attacking the metal.

"Almost every one will admit that in the case of impure iron, with its unhomogeneous physical and chemical constitution, electrolysis will supervene, but it must be remembered that we are now concerned with the underlying cause of the wet oxidation or hydroxylation of iron, regardless of its state of chemical purity.

"According to the dissociation theory, even the purest water contains free hydrogen ions to the extent of about 1 gram in 10,000,000 litres. If iron dissolves in the purest water it should be by interchange with hydrogen and as Whitney has pointed out, pure water is to this extent an acid."

Then follows an account of experiments such as have been referred to, showing that no rusting took place in boiled-out water until oxygen was admitted, although iron had been dissolved prior to the entry of the oxygen. It will not be easy for the average reader interested in the problem under discussion to disentangle the meaning from the quotation just given. The implication is firstly that *pure* water ($H_2O$) is to some extent "dissociated" or resolved into the ions H and OH and that when iron is placed in contact with *pure* water iron atoms or ions enter into solution in exchange for the hydrogen ions, hence the appearance of iron in the solution in the experiments referred to.

The answer to these contentions is that no one has yet dealt with *pure* water and that it is practically inconceivable that any one ever should.

What has been done is to take ordinary water and purify it, after boiling out dissolved volatile "impurities," by repeated distillation *in glass vessels*: as purification proceeds, the power of the liquid to conduct an electric current diminishes and becomes very, very slight; it is only logical to assume that if pure water could be obtained, *i.e.* water free from every other substance, it would have no conducting power. As even the hardest glass is to some slight extent attacked by water, it is inconceivable that pure water should ever be obtained at all events by the use of glass vessels; and if vessels, say of iridium—the least attackable of metals—were used, there would still be the difficulty, again all but insuperable if not entirely so, of getting rid of surface impurities derived from the environment during working. The conductivity test is of such delicacy that the most minute impurity tells.

No "electrolytic theory" can apply to water if water be a non-conductor, the essence of the electrolytic theory being that action takes place because an electrolyte is present which is resolved by the passage of the current through it into its ions, one of which at least is active towards the electrode at which it is delivered.

That two substances, such as iron and water, neither being an electrolyte, should interact (without the intervention of a third) is improbable, to say the least, also for another reason. When two conductors are brought into contact, it is well known that they are at a different electrical potential at the junction: years ago there was much discussion as to the origin of this difference—whether it might be regarded as an actual contact difference or whether it should not be supposed that some minute amount of chemical change took place at the junction in which a film of moisture condensed on the surfaces brought into contact was concerned. The question has never been settled and there are almost insuperable difficulties in the way of settling it experimentally. But assuming that such an electrical difference exists at the junction of two substances, on joining them at the other extremity an equal but oppositely directed difference of potential would be developed: consequently, a circuit could not be established in which an electric current would flow. To secure a flow of current, the introduction of a third component into the system is required to afford the necessary slope of potential.

For the various reasons given it may be contended therefore that the answer to Dr. Cushman"s "one crucial question"—"Does iron pass into solution, even to the slightest extent, in pure water?"—must be, on the practical side at least: "That it is not proven that it does"; on the theoretical: "That it is highly improbable that it does, not to say impossible that it should." Or perhaps it should be said, as it cannot be denied that, strictly speaking, no substance is insoluble in water, that if iron dissolved it would dissolve mechanically in the water and in the absence of acids it would not rust, even if oxygen were present. But such an ideal condition of purity is only conceivable; it could not be realised.

Taking Moody and Friend's observations into account, it may be said to be proved that whenever iron is dissolved acid is present and active. Objection has been taken to Moody's conclusion on the ground that the inactivity of the iron he used towards water was due to the fact that by cleaning the iron in chromic acid before exposing it to oxygen and water he rendered it *passive*—but the account he gives is full of proof to the contrary. Thus he states that rusting began when ordinary air was allowed to enter into the tubes in which iron had been exposed during long periods in contact with purified air and water without any rusting being apparent; also that the iron at once began to rust when and where it came into contact with the glass tube in which it was enclosed. I still have in my possession tubes prepared by Moody several years ago in which iron is sealed up in contact with water and oxygen and not the least rusting is apparent, except at one spot where, owing to careless handling by students, the V-shaped tube having been turned upside down, the iron rod was allowed to come into contact with the glass and to remain in contact with it during a short time. Friend has also observed that contact with glass promotes rusting. This observation is in itself most significant as showing the influence that acids have in promoting rusting—the effect cannot well be ascribed to any other cause than to the action of silicic acid present at the glass surface.

Friend, however, has met the criticism applied to Moody's results in a most ingenious way, by carrying out experiments with the simple apparatus shown in fig. 2.

The thimble-shaped tube shown within the flask is of iron and can be cooled by circulating water inside it in the manner indicated; the flask contains strong caustic potash solution and above this air. At the outset, some of the air is expelled or withdrawn and the flask closed by sealing the side-tube at

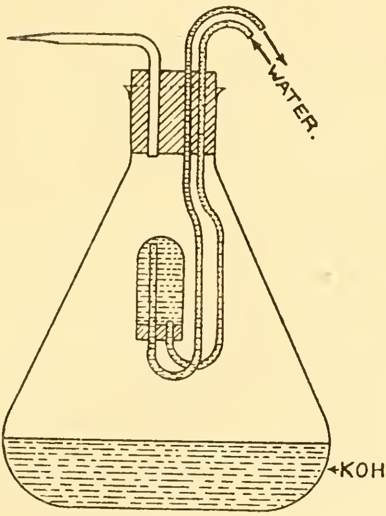



FIG. 2.

its point; after shaking well, so as to wash down all the surfaces exposed within the flask with alkali, to remove all traces of adherent carbonic acid, the flask is immersed in water at 100° and cold water circulated through the iron thimble; as the steam condenses on the outer iron surface, this becomes cleansed of alkali; when the cleansing process is judged to be complete, the flask is set aside. Operating in this way, Friend found that no rusting took place, except on one occasion when a couple of rust spots were formed at the top of the thimble; as the spots appeared at the same place after the rust had been removed, there can be no doubt that the rusting was due to the presence of "slag" at the point affected. Moody had a similar experience and advisedly used chromic acid as a means of removing manganese sulphide—a common impurity in iron, which apparently undergoes oxidation with particular readiness, giving rise to acid. It should be added that Friend cleaned the iron he used by mechanical means and assured himself at the close of each experiment that the alkali had been washed off the surface.

Taken in conjunction with Moody's experiments, Friend's observations afford the clearest possible proof that the presence of acid is essential to rusting. The results obtained by others which appear to contravene their conclusions can only be ascribed to lack of success in effecting the complete removal either of carbonic acid or of slag from the iron.

It is quite clear that the engineer and builder need not concern themselves in the very slightest with ions—least of all whether they enjoy independent existence or not in solution: this may well be left a subject of academic dispute; indeed the proper protection of iron against decay is of such supreme public importance that any attempt to complicate the problem by the introduction of a pretentious jargon, which even the expert can fathom with difficulty and which serves mainly to cloak ignorance, deserves to be visited with the most absolute disapproval. Suffice it for the engineer to know that, in the first place, he should "keep his powder dry"—in other words, he should take every possible precaution to prevent liquid films of water from collecting on iron surfaces, as such films necessarily contain carbonic acid in solution, carbon dioxide being present in the air at all times; secondly, that the access of acid to the iron is to be avoided on all occasions but more especially if the conditions be such that water can condense readily on the iron.

The extraordinary slowness with which ideas are spread and clear conceptions are arrived at is well illustrated by the publication recently by Messrs. Lambert and Thomson of an account of experiments on the oxidation of iron which they have carried out with the greatest perseverance and manipulative skill but obviously without any clear understanding of the conditions underlying the processes of chemical change and without any clear conception of the problems to be solved.

The aim of the investigation is said to have been "to bring together, under the simplest possible conditions, the purest obtainable water, oxygen and iron in vessels which would be least likely to be acted on by any of these substances."

Now, although it may be true that "to the pure all things are pure," to the chemist who thinks all things are impure and must ever remain so—some contamination is unavoidable; the important question to consider is whether or no a particular impurity can be counted as harmful. Pure is defined in the dictionary as—"separate from all heterogeneous or extraneous matter"; a pure thing is the thing in itself. There can be no degrees of purity—pure is to be qualified only by such words as "nearly," "far from," etc. At most, we can speak of "highly purified" or of "very nearly" or "all but pure" substances. Messrs. Lambert and Thomson misuse and misapply the word "pure" throughout their communication.

The results they have obtained show, they say:

"that chemically pure iron will not undergo visible oxidation even after long exposure to pure water and pure oxygen in vessels made of clear fused silica. Further, that a very small trace of impurity in the iron is sufficient to cause oxidation under exactly the same conditions, where there is not the remotest chance of any acid substance either being present or being formed during the reaction."

It is worth while considering to what extent these statements are justified. "Chemically pure," unfortunately, is a phrase to which no precise meaning can be attached—it means a material purified by a chemist, whose degree of success will depend on the methods he may adopt in purifying the material. Messrs. Lambert and Thomson tell us that to prepare "pure iron" they took a "a pure specimen" of ferric chloride from a certain dealer—at the best a very indefinite description. Metallic iron was separated from the salt by electrolysing it between iridium electrodes. The metal was then made into nitrate, the which salt was purified by recrystallisation from purified nitric acid; the salt then obtained was colourless, whereas ferric nitrate prepared from "ordinary pure iron" is pale violet; this is probably the most interesting and valuable observation made in the course of the inquiry and is alone well worth all the trouble they expended on the preparation of the salt. Ultimately the nitrate was decomposed by heat and the oxide reduced to metal by "pure hydrogen"—whether ordinary pure or chemically pure we are not told.

Iron so prepared was not visibly oxidised under the conditions arranged by Lambert and Thomson even after several months.

On the other hand, iron made in precisely the same way from "ordinary pure iron" invariably showed signs of oxidation in two or three hours. In like manner, when platinum vessels were used, particularly when a platinum boat was substituted for one made of iridium to contain the iron oxide (prepared from the most purified materials) while it was submitted to reduction in hydrogen, the iron produced underwent oxidation readily in two or three hours, rusting taking place invariably at those parts of the metal which had been heated in contact with the platinum tube. All kinds of commercial iron rusted readily in their apparatus; they even go so far as to state that commercial electrolytic sheet iron cleaned by Moody's method with chromic acid was readily attacked, thus disposing again of the contention that chromic acid necessarily renders iron passive.[1]

It is clear that Lambert and Thomson unwittingly arranged matters so that iron would rust in their apparatus *whenever it was coupled with the necessary electro-negative conductor*; by using iridium vessels, which apparently are not in the least subject to attack even by chlorine and strong acids, they were able to prepare an iron free from electro-negative impurity—as is to be expected on all grounds, such iron did not rust. This result is an interesting verification of the only theory of chemical change that is in accordance with present knowledge but it is nothing more. The work affords no disproof of Moody's argument; on the contrary, it supports it most strongly. There can be little doubt that although Lambert and Thomson were successful in carrying the purification of iron very far, they were not sufficiently careful to secure the removal of carbon dioxide from their apparatus. In view of the results obtained by others, it is inconceivable that they would have arrived at results such as they describe had they done so. And it is not difficult to see where they went astray. Whilst they took great care to prepare oxygen free from acid impurity by electrolysing a solution of baryta and all water introduced into the apparatus was carefully distilled from an alkaline solution, they evidently were not alive to the difficulty of removing

[1] Compare Brereton Baker, *Chemical Society's Report*, 1910, p. 38.

carbon dioxide entirely from glass surfaces, although this has long been recognised; a very large area of glass was exposed within their apparatus, especially in the vessel in which the oxygen was stored. A simple apparatus such as Friend used is in reality far better suited to such an inquiry than the highly complicated apparatus used by Lambert and Thomson. But simple conditions are usually adopted by all experimenters only as a last resort.

H. E. A.

# VERTEBRATE PALÆONTOLOGY IN 1910

By R LYDEKKER

So far at least as the highest class of animals concerned is concerned, the most important palæontological work published during the year under review is undoubtedly Professor H. T. Osborn's *The Age of Mammals*.[1] To give an adequate summary of the contents of this valuable work is quite impossible in this article; but it may be mentioned that the whole subject is treated in an original and to a great extent novel manner, special attention being directed to the palæogeography of the countries of the world, and to former connections between areas now widely sundered. Whilst urging the existence of a former land connection between Australia and South America, by way of the Antarctic, the author refuses to admit any such connection between South America and Africa, believing that the former country received its early vertebrate fauna from the north.

In regard to the puzzling problem of the origin of the marsupials of Australia, Professor Osborn argues that this fauna must have come either from south-western Asia or from South America. The first immigrants into the country were, it is urged, almost certainly arboreal opossum-like animals, from which the rest of the marsupial fauna developed within the country itself. On this hypothesis the fossil thylacines of Patagonia (assuming that American palæontologists are justified in regarding the Patagonian carnivores as such) must either have come from Australia or have been independently developed in South America. If independently developed, which seems unlikely, they afford, of course, no evidence as to an Australian-American connection. On the other hand, if they came from Australia they indicate, in the first place, the Asiatic origin of the marsupials of that island-continent, and also that the ancestral Australian marsupials reached their present habitat long before the Miocene; this being practically the view expressed by myself in 1896.[2]

[1] New York, the Macmillan Company, 1910.

[2] *A Geographical History of Mammals*, p. 56. Cambridge.

In connection with the above may be mentioned an article on the continuity of development by Dr. W. D. Matthew, published in the *Popular Science Monthly* for November 1910 (pp. 473-8). After discussing the geological sequence and development of the so-called "ruminating hogs," or oreodonts, in North America, the author concludes that if we regard the record as continuous where there is no apparent break, "and that the known record really represents what was going on over the entire continent of North America, I do not see that we can fairly escape from the conclusion that new species, new genera, and even larger groups have appeared by saltatory evolution, not by continuous development.

"But—and here lies the crux of the whole question—we have no right whatever to make either of these assumptions. And without them the argument from palæontology for discontinuous development is almost or quite worthless."

Later on in the article Dr. Matthew makes the following important observations with regard to the probable places of origin of the oreodonts, the horses, and certain other groups:

"I assume that since the oreodonts and peccaries never reached the Old World, and the camels did not reach it till the Pliocene, their centres of dispersal were well to the south of the Bering Sea connection with the Old World. I assume that since the horses are represented by a double evolutionary series, one in Europe, a closer one in North America, their centre of dispersal lay far enough north to spread into Europe on the one hand, and North America on the other, but that the latter was nearer or more accessible, *i.e.* their centre of dispersal was north-eastern Asia or Alaska. On similar grounds the centre of dispersal of most of the Tertiary ruminants might be located in north-west Asia, of rhinoceroses in north-east Asia and Alaska, of dogs in north-west Canada."

This suggestion as to the single dispersal-centre of the horse-group in high latitudes seems, on the face of it, much more probable than Professor Cope's theory that the group had a dual origin and development, one in North America and a second in the Old World.

An important advance in our knowledge of the early Tertiary fauna of the Fayum district of Egypt has been made by Dr. Max Schlosser, who in the *Zoologischer Anzeiger* for 1910, vol. xxxv. pp. 500-508 announced the discovery in the Oligocene of remains of Primates which are referred to three new genera, viz. *Mœripithecus*, *Parapithecus*, and *Propliopithecus*. The last, as represented by *P. haeckeli*, is a small ape intermediate in size between the species of the American genera *Chrysothrix* and *Cebus*, with the normal simian lower dental formula; the canine and premolars being vertical, and the two branches of the lower jaw running nearly parallel, and forming a firm symphysis. This genus is regarded by its describer as the ancestor, not only of the *Simiidæ*, but probably also of the *Hominidæ*. *Parapithecus fraasi* is a small species of the size of a squirrel-monkey, with the lower dental formula *i.* 1, *c.* 1, *p.* 3, *m.* 3, regarded as connecting the Eocene *Anaptomorphidæ* with the modern *Simiidæ*, and perhaps also with the *Cercopithecidæ*. *Mœripithecus markgrafi* is too imperfectly known to admit of its systematic position being determined, but appears to have been about the size of a spider-monkey. Dr. Schlosser also describes a number of new types of hyracoids, among which *Bunohyrax* is based on some of the species included by Dr. C. W. Andrews in *Geniohyus*, both genera being regarded as bunodont hyracoids. The fauna is likewise shown to include representatives of the Chiroptera and Insectivora. To the former belongs the humerus of a large vampire-like bat described under the name of *Vampyravus*, while a lower jaw referred to under the new generic title of *Metoldobotes* pertains to the latter. Of minor interest are remains of certain creodont Carnivora.

The author's views as to the affinity of some of these Fayum fossils have not been allowed to pass without challenge, for in the *American Naturalist*, vol. xliv. pp. 700–703, Dr. W. D. Matthew states, in reference to the remains of Primates, that the material is insufficient to permit of determining whether the specimens are really referable to the Anthropoidea, still less for the statement that *Propliopithecus* is the ancestor of the *Simiidæ* and probably also the *Hominidæ*. "If the author had stated," observes Dr. Matthew, "that the Oligocene ancestor of man probably had lower teeth like those of *Propliopithecus*, the conclusion might well be accepted. But the corollary of Schlosser's statement is that we have found the Oligocene ancestor of man, and that he lived in Africa. The evidence is not adequate to warrant any such conclusions."

It was pointed out by Dr. Schlosser that the Fayum hyracoids differ markedly from their modern representatives in the form and proportions of their skulls, and likewise in the somewhat more specialised characters of their feet; and it therefore seems that they are not directly ancestral to existing hyraxes, although they may stand in that position to the Pliocene *Pliohyrax* of Samos, which may be derived from *Sagatherium*. The supposed hyracoid affinities of *Arsinoitherium* are not admitted by Dr. Schlosser, who considers the genus to be connected with the Amblypoda, and regards relationship between hyraxes and proboscideans as limited to a common descent from condylarthrous ancestors.

Although the presence of bats in the Fayum fauna is undoubted, Dr. Matthew has some hesitation in admitting whether the same can be affirmed with regard to Insectivora, for *Metoldobotes* is referred to the so-called Proglires, a group whose affinities are doubtful. If, however, the Fayum genus be really related to *Olbodotes* and *Mixodectes* of the basal Eocene of the United States, it will be a point of some interest.

Another important faunistic paper is a preliminary account by Dr. G. E. Pilgrim, published in *Rec. Geol. Survey India*, vol. xl. pp. 63–7, of new genera and species of mammals from the Tertiary formations of India, especially those of the Bugti Hills, in Baluchistan. In many instances, at any rate, the diagnoses of the new genera and species are very short. The new forms include three monkeys, one of which is referred to the European Miocene *Dryopithecus*, as *D. punjabicus*, while the second, also from the Siwaliks of the Punjab, is believed to represent a new genus and species—for which the name *Sivapithecus indicus* is proposed—allied to the gorilla; the third, from the same district, being a langur, *Semnopithecus asnoti*. The Carnivora comprise two new species of *Palhyæna* and an *Amphicyon*. The artiodactyle ungulates include a new *Tragoceros*, several new genera and species of giraffe-like ruminants, a new type of traguloid described as *Dorcabune anthracotheroides*, which is believed to connect the modern *Tragulidæ* with the *Anthracotheriidæ*, two new genera of the last-named family, and a new species of *Tetraconodon*. Among the perissodactyles we have two new species of *Aceratherium*, the first recorded Indian species of *Cadurcotherium*, species of *Diceratherium* and *Telloceras*, and the new genus *Phyllotillon* for the species originally described as *Macrotherium naricum*, together with a new species of the equine genus *Hippodactylus*. Finally, it is suggested that certain teeth from the Bugti Hills may indicate an Indian representative of the Fayum genus *Mœritherium.*

A short notice must suffice for a paper by Mr. W. Granger, published in the *Bull. Amer. Mus. Nat. Hist.* vol. xxviii. pp. 235–51, on the Tertiary faunistic horizons of the Wind River basin of Wyoming. Descriptions of several new Eocene mammals are included in the paper, among which the new genus *Shoshonius* for a member of the *Anaptomorphidæ* may be mentioned. Fuller mention is made later on of the fifth part of Dr. H. G. Stehlin's memoir on the mammals of the Swiss Eocene, published in the *Abh. Schweiz. Pal. Ges.*, vol. xxxv. pp. 691–837, 1909, to which I had not the opportunity of referring in my review for that year. A reference may likewise be made here to a paper by Dr. M. Boule, *Compt. Rend. Ac. Sci.* Paris, vol. cl. pp. 812, 813, on certain vertebrate remains from southern Tunisia.

Turning to systematic work, it is important to refer to a novel classification of so-called tetrapodous vertebrates proposed by Dr. O. Jaekel in a paper contributed to the *Zool. Anzeiger*, vol. xxxvi. pp. 113–24, which to some extent affects mammals. The author adopts the following seven classes, viz.: (1) Mammalia, (2) Paratheria, (3) Aves, (4) Reptilia, (5) Amphibia, (6) Microsauria, and (7) Hemispondyla. The Paratheria are taken to include the monotreme mammals, the more typical anomodont reptiles (in the wider sense of that term), and chelonians. The class as thus constituted is considered to be intermediate between the Mammalia and the Reptilia, but nearer the former than the latter, and containing among its earlier representatives the ancestors of the mammalian line. The author's definition of the class is, with some omissions, as follows:

So far as known, paratherians are short-limbed, egg-laying vertebrates with a short body, moderately long neck and tail, flattened, clumsy feet, and a covering of horny scales, spines, or hair. The head is depressed, with a small brain, forwardly directed, and for the most part narrow, nostrils, large and laterally directed eyes, and a single or no temporal fossa. In most cases the fore part of the palate is double-roofed, the choanæ are mesially united, and the vomer and pterygoids well developed. The dentition may be simple, mammal-like, or aborted; and the teeth, when present, are generally socketed, with single roots. In the lower jaw the dentary is the dominant bone, and there is usually a coronoid process. The occipital condyles are either tripartite or double. In the shoulder-girdle the scapula and coracoid are distinctly separate. Except in the marine turtles, the phalangeal formula is 2 . 3 . 3 . 3 . 3, when the feet are fully developed. The group is an ancient one, attaining marked development in the Permian and Trias.

Although anthropology does not generally come within the scope of this series of reviews, it may be well to mention that *Homo heidelbergensis*, Schoet, has been referred by Mr. G. Bonarelli, in *Riv. ital. Paleont.*, vol. xv. p. 26, 1909, to a new genus, under the name of *Palæanthropus*.

Reference has been made already to the discovery of remains of Primates in the Siwaliks and the Fayum Oligocene.

To the *Anales del Museo Nacional de Buenos Aires*, ser. 3, vol. xiii. p. 317, Dr. F. Ameghino has contributed a note on certain teeth from a cavern in Cuba, which are referred to a large monkey. The dental formula is identical with that of the *Cebidæ*, but the cheek-teeth are stated to approximate to those of Old World monkeys and man. For this monkey the new generic and specific name of *Montaneia antropomorpha* is proposed. It is noteworthy that no wild monkeys are found in Cuba at the present day.

The systematic position and dentition of *Microchœrus erinaceus*, from the Hordwell Eocene, are discussed in the *Annals and Magazine of Natural History*, ser. 8, vol. vi. pp. 39–43, by Mr. C. Forster Cooper, who comes to the conclusion that the genus probably represents a lemuroid, whose nearest affinities are with *Necrolemur* of the French Oligocene.

In the first part of a paper published in the *Bull. Amer. Mus. Nat. Hist.*, vol. xxviii. pp. 33–42, Dr. Matthew describes a skull of the insectivorous genus *Apternodus* from the Lower Oligocene of Wyoming. Together with the contemporaneous North American *Xenotherium* and the South American *Necrolestes*, the genus is referred to the so-called zalambdodont insectivores, represented at the present day by the Malagasy *Centetidæ*, the African *Potamogalidæ* and *Chrysochloridæ*, and the West Indian *Solenodontidæ*. Among the tenrecs (*Centetidæ*), *Apternodus* shows marked resemblance in dental characters to *Ericulus*, but differs from that and the other genera of the family in the extraordinary features of the mastoid region of the skull. The author regards *Apternodus* as representing a special subfamily (*Apternodontinæ*) of the *Centetidæ*—a reference which, if trustworthy, is of great interest and importance from the point of view of geographical distribution.

The phylogeny of the *Felidæ* forms the subject of an important article by Dr. W. D. Matthew, in vol. xxviii. pp. 289–316 of the *Bulletin* of the American Museum of Natural History. It is stated that the majority of the extinct members of the family, including the oldest species, are characterised by a more or less pronounced development of the upper canines into long, flat-sided tusks. These sabre-tooths, or machærodonts, date from the Lower Oligocene, but typical cats with relatively short upper canines are not known till the Pliocene. The early sabre-tooths are divisible into two series, one characterised by the length and slenderness of the tusks and the large size of the protecting flange on the lower jaw, and the other by the shorter tusks and smaller flange. *Hoplophoneus* and *Dinictis* respectively represent the two series in North America. The derivation of the large Pliocene and Pleistocene sabre-tooths from *Hoplophoneus* has been accepted, but the relations of modern cats to *Dinictis* have been overlooked. The evidence seems to indicate that the *Dinictis* phylum led directly into modern *Felidæ*, the canines having reverted from the machærodont specialisation to the normal type of carnivorous mammals. The series *Dinictis*, *Nimravus*, *Pseudælurus*, and *Felis* forms a direct succession, structurally and geologically.

Dr. Matthew considers that the origin of the family cannot be carried back further than the Oligocene sabre-tooths; derivation through *Ælurotherium*, a genus based on the milk-dentition of a species of the same group, from the Eocene creodont *Palæonictis* being inadmissible.

On the other hand, it may be noted that Mr. R. O. Peterson has described, under the name of *Daphænodon* (*Mem. Carnegie Mus.*, *Pittsburg*, vol. iv. No. 5), the skeleton of a dog-like carnivore of the size of a leopard from the Miocene of Nebraska, which, together with the older *Daphænus*, he regards as in some degree intermediate between dogs and cats, although the skull and teeth are essentially dog-like. In many respects *Daphænus*, of which the skeleton is known, is very cat-like, especially in the long, leopard-like tail. A cat-like feature is the partially retractile structure of the claws. In concluding his description, Mr. Peterson observes that the model "is instructive, as it furnishes at least a conception of a primitive form ancestral to cats and dogs."

Later in his article Dr. Matthew replies to critics who have doubted the theory that the sabre-tooths attacked by dropping the lower jaw into a nearly vertical position and stabbing with the upper tusks. After supporting the theory by additional evidence, he remarks that most of the early large ungulates were of the "pachyderm" type, and thus fitted to receive this method of attack, while they would not succumb to the mode practised by lions and tigers.

Finally, he observes that "with the rise and dominance of the large light-limbed ruminants and horses, some of the early sabre-tooths were correlatively adapted into the modern type of felines, while as the surviving pachyderm phyla became larger, thicker-skinned, and more powerful, the sabre-tooths likewise became progressively larger, more powerful, and developed heavier weapons to cope with and destroy them. The final extinction of the machærodont phylum was probably largely conditioned by the growing scarcity and limited geographic range of the great pachyderms."

Dr. Matthew protests, however, against the idea that the huge Pleistocene sabre-tooths died out as the result of over-specialisation.

Carnivora from the Pliocene and Pleistocene of Mexico form the subject of a paper by Dr. E. Freudenberg, published in *Geol. u. Pal. Abhand.*, ser. 2, vol. ix. pp. 195–231. The Pleistocene forms appear identical with existing species, but a new *Felis* and a new *Hyænognathus* are described from the Pliocene.

In the above-mentioned article in the *Memoirs of the Carnegie Museum*, vol. iv. pp. 205–278, on the Miocene Carnivora of Western Nebraska, Mr. Peterson, in addition to the description of *Daphænodon* and notes on its affinities, records a new genus and species of the *Amphicyoninæ*, under the name of *Borocyon robustum*. This animal, which was of the approximate size of a lion, appears to have been probably related to *Daphænodon*, but with non-retractile claws. He likewise describes a new genus and species of mustelines, *Paroligobunis simplicidens*. The genus, which is evidently akin to *Oligobunis*, of the John Day formation, appears to have affinities to both otters and badgers, and to be related to the European Miocene otter-like *Potamotherium*.

To the *Bulletin of the Royal Academy of Belgium*, *Class des Sciences*, 1910, No. 5, Mr. A. Rutot contributes an article on the occurrence in Belgian caverns of layers containing remains of Arctic rodents. Similar layers have been identified in Swiss and German caves in association with those containing the mammoth-fauna, which indicates a moderately cold climate, and includes the Aurignacien and Solutreën stages. One of the rodent zones contains a fauna comparable to that of the modern European and Asiatic steppes, while a second includes one of the type of the Siberian tundra. Both belong to the reindeer epoch, but the tundra-like fauna alone indicates Arctic conditions, *Dicrostonyx torquatus*, *Microtus gregalis*, and *Lagomys pusillus* representing Arctic types of rodents. All this indicates the former prevalence of very similar climatic conditions over a large extent of Europe.

In North America the late Tertiary rodent fauna of Virgin Valley and Thousand Creek, Nevada, forms the subject of an article in the *Geological Bulletin of California University*, vol. v. pp. 411–37, by Miss Louise Kellogg. Some of the species—among which several are new—belong to existing genera, such as *Arctomys*, *Peromyscus*, and *Lepus*, but others are referable to the extinct *Mylagaulus*, *Entoptychus*, etc. The new genus *Diprionomys* is proposed for two species related to *Perognathus* and *Perodipus*.

The voles and lemmings of the late Pleistocene of the British Isles are discussed in the *Ann. Mag. Nat. Hist.*, ser. 8, vol. vi. pp. 34–9, by Mr. M. A. C. Hinton, who describes new species of *Arvicola*, *Microtus*, and *Dicrostonyx* from the Ightham fissures, Kent. Of these, *A. abbotti* is allied to the Scandinavian form of the water-rat, now termed by advanced systematists *A. terrestris*, but displays still more specialisation for burrowing. *Microtus corneri* is nearly related to the Orkney *M. orcadensis*, while *M. anglicus*, displays affinity to an Asiatic group separated by Mr. Kastchenko as *Stenocranius*. Finally, *Dicrostonyx henseli*, which also occurs in caverns in Derbyshire and Clare, in place of being related to the Old Word *D. torquatus*, is allied to the American *D. hudsonianus*.

An important contribution to the relationships and phylogeny of the families of rodents is made by Dr. W. D. Matthew in an article on the osteology and relationships of *Paramys* and the affinities of the extinct family *Ischyromyidæ*, of which it is one of the constituents. The article is published in the *Bull. Amer. Mus. Nat. Hist.*, vol. xxviii. pp. 43–72. The family *Ischyromyidæ* includes the five genera *Ischyromys*, *Paramys*, *Sciuravus*, *Mesops*, and *Prosciurus*. The family is considered to belong undoubtedly to the Sciuromorpha, among which it is classed, together with the existing *Haplodontidæ* and the extinct *Mylagaulidæ*, in the group Haplodontoidea (or Aplodontoidea), while the Sciuroidea is taken to include the *Sciuridæ*, *Castoridæ* (with *Castoroididæ*) and *Geomyidæ* (with *Heteromyidæ*). The inclusion of the *Geomyidæ* in the Sciuroidea is a new departure, as is also the reference of the extinct *Meniscomys* and *Mylagaulodon* to the *Haplodontidæ*.

On the other hand, the primitive Tertiary families *Theridomyidæ* and *Pseudosciuridæ*, although closely related to the *Ischyromyidæ*, are classed, together with the modern *Anomaluridæ*, in the Hystricomorpha. The first, at least, of these families appears to have undoubted hystricomorphine affinities, and must be regarded as approximately ancestral to the group, in spite of the difficulties in accounting for the geological and geographical distribution of the known members of the *Theridomyidæ* and Hystricomorpha. In this classification Dr. Matthew follows the one proposed by myself in the British Museum "Catalogue of Fossil Mammalia."

The first paper on ungulates for notice is one by Dr. Max Hilzheimer, published in the Hanoverian journal *Jahrb. Wiss. u. prakt. Tierzucht*, vol. v. pp. 42–93, under the title of "Wie hat der Ur gesehen?" The special subject is the authenticity and value of the various portraits and other representations of the aurochs; but attention is also directed to the supposed evidence of the former existence in Germany of a short-horned race of the species. In a second paper, *Sitzber. Ges. naturf. Freunde*, Berlin, 1910, pp. 136–46, the same writer describes new forms of extinct European and Siberian bisons.

Fossil *Cervidæ* from Swabia form the subject of an article by Dr. W. D. Dietrich, published in *Mitt. Kgl. Naturalienkabinet*, Stuttgart, 1910, pp. 318–36; the specimens described being referable to the red deer, elk, and reindeer. *Cervus lydekkeri*, from the Pleistocene of Java, forms the subject of a paper by Mr. K. Vogel von Falckenstein in the *Sitzber. Ges. naturf. Freunde*, Berlin, 1910, pp. 319–33. It is allied to *C. axis* and also European Pleistocene forms.

Various new Indian *Giraffidæ* are referred to in the notice of Mr. Pilgrim's paper.

To the *Proc. Amer. Phil. Soc.*, vol. xlix. pp. 196, 197, Mr. W. J. Sinclair contributes an account of the restored skeleton of the Tertiary cameloid *Leptauchenia decora*; and in another paper, *Amer. J. Sci.* vol. xxix. pp. 297–394, Mr. F. B. Loomis discusses the osteology and affinities of the allied genus *Stenomylus*, figuring an entire skeleton of a new species, *St. hitchcocki*, from Nebraska. From this it appears that *Stenomylus* was a slenderly built tylopod, adapted to an upland life and furnished with the full typical series of forty-four teeth. It had a long neck, two-toed, digitigrade feet, and the navicular and ectocuneiform bones of the carpus free. *Eotylopus* is a new cameloid from the Oligocene of Wyoming, described by Dr. Matthew in *Bull. Amer. Mus. Nat. Hist.* vol. xxviii. p. 36; and on p. 248 of this volume Mr. Grainger proposes the name *Camelodon* for a species from the Eocene of the same State. The latter is allied to *Protylopus* and *Leptotragulus;* differing from the first by its more selenodont molars, and from the second by the more compressed anterior and more complex last premolars. *Eotylopus* is referred by its describer to the *Hypertragulidæ*, on account of possessing four digits and separate metacarpals in the front limb, only two functional digits in the hind-foot, the metapodial keels restricted to the palmar aspect, and the dentition primitive, unreduced, and brachyodont. The *Hypertragulidæ* is divided into the subfamilies (1) *Leptomerychinæ*, (2) *Hypertragulinæ* and *Leptotragulinæ*, of which the first is believed to exhibit signs of affinity with the *Cervidæ*, the second with the *Tragulidæ*, and the third with the *Camelidæ*.

In the *Anthracotherium* group much interest attaches to a description by Dr. Marcellin Boule, in a paper on certain fossil vertebrates from southern Tunisia, published in vol. cl. (pp. 812, 813) of the *Comptes Rendus* of the Academy of Sciences, Paris, of remains of a species of the Siwalik genus *Merycopotamus* from Upper Tertiary strata. This new species, *M. africanus*, as it is named, is of importance as affording evidence in favour of Dr. Arldt's theory as to the migration of the Siwalik fauna through a forest-tract to Africa.

Mention of the preliminary description by Dr. Pilgrim of two new genera of *Anthracotheriidæ* has been already made in the notice of his paper. In addition to these, Dr. H. Stehlin, in the course of a revision of the European anthracotheres, *Verh. naturfor. Ges. Basel*, vol. xxi. pp. 165–85, has described two new species of the type genus. In his above-quoted memoir on the mammals of the Swiss Eocene (*supra*, p. 664), the same writer discusses the phylogeny of the early Tertiary *Suidæ* and their relatives, and also describes several new species belonging to that family and the *Anthracotheriidæ.* Of special interest to English palæontologists is his reference of a skull from Hordwell which I identified many years ago with *Anthracotherium gresslyi* (Meyer) to a new species of *Haplobunodon*, under the name of *H. lydekkeri.* Reference may again be made here to Mr. Pilgrim's *Dorcabune*, believed to connect the *Anthracotheriidæ* with the *Tragulidæ.*

In a paper on a new genus of peccary (*Pediohyus*) from the Tertiary of Wyoming, published in the *Amer. Journ. Science*, vol. xxx. pp. 381–84, Mr. Loomis discusses the phylogeny of the family. The group, although distinctly American, and dating from the Oligocene, is considered to be of Asiatic origin, and derived from a primitive type of the Old World *Hyotherium.* Starting in America from the Middle Oligocene *Perchœrus*, the group, when it reached the Lower Miocene *Desmathyus*, split into three branches, of which one culminated in the modern peccaries, while the other two died out in the Pleistocene, one in the form of *Mylohyus* and the second in *Platygonus.*

Passing to the *Equidæ*, Dr. Boule, as the result of researches undertaken at the expense of the Prince of Monaco, has been able to furnish information with regard to the members of the family inhabiting France during the Pleistocene; his paper on equine remains from the Grottes de Grimaldi being published in the *Annales de Paléontologie*, vol. v. part 3, pp. 1-23. The remains represent a horse specifically identical with *Equus caballus* and approaching in character the large modern Percheron breed. This race was larger than the ordinary cave-horse, which is allied to the wild Mongolian tarpan. The cavern also contained skulls and bones of a smaller and more delicate character, some of which appear to be referable to the wild ass (*Equus asinus*), and not to the onager (*E. onager*). Since, however, other authors have identified the latter species in certain caves, it would seem that the African wild ass and the Asiatic onager lived side by side in southern Europe during the Pleistocene. In addition to remains of the horse and ass, there are fragments of jaws at Grimaldi referable to the extinct *Equus stenonis* of the Upper Pliocene and Lower Pleistocene. In the opinion of the author, *E. stenonis* and the allied Indian *E. sivalensis* represent a stage intermediate between the modern *Equus caballus* and the extinct *Protohippus*, which (and not the three-toed *Hipparion*) was the proximate ancestor of the existing members of the family. *Protohippus*, said to be known only from America, although it may have existed in Asia,[1] was three-toed, but had the lateral digits functionless. In the aforesaid extinct species of *Equus* these bones became reduced to mere splints, much as in the modern members of the family.

The hyracoids of the Fayum are discussed by Dr. Schlosser in the paper already quoted, which contains descriptions of new genera and species.

One of the most important pieces of palæontological work in the year is Dr. W. B. Scott's account of the Litopterna—a group typified by the well-known *Macrauchenia*—of the Santa Cruz beds, published as vol. vii. pt. 1 of the palæontological series of the *Reports of the Princeton University Expeditions to Patagonia*. In regard to the affinities of the group, Dr. Scott, as he himself states, arrives at a conclusion very similar to one expressed by myself several years previously. The Litopterna, in the opinion of Dr. Scott, are more nearly related to the Toxodontia than to the Perissodactyla, the striking resemblances to the latter being largely due to parallelism of development, although in part to the retention of primitive features common to all early ungulates, if not indeed to mammals generally. This conclusion as to the absence of near affinity between Litopterna and Perissodactyla receives strong support from the fact that the earlier members of the two groups show no signs of closer approximation than is exhibited by their later and more specialised representatives. In fact, the exact converse of this is true, since the closest resemblance between the two groups is displayed by the *Proterotheriidæ* on the one hand and the *Equidæ* on the other. It may accordingly be assumed that the Litopterna and Toxodontia form two sections of an assemblage of ungulates peculiar to South America which originated in that continent from some at present unknown ancestral type or types. "The primitive features of the Lito-

[1] It was recorded from Russia by Madame Pavlow in 1903.

pterna," remarks Dr. Scott, "such as the semi-taxeopod carpus and tarsus, extensive articulation between the fibula and calcaneum, and the inadaptive method of digital reduction, are all as distinctly marked in the highly specialised and monodactyle *Thoatherium* as in the tridactyle and isodactyle genera. When it is remembered that *Thoatherium* greatly surpasses the horses in completeness of digital reduction the retention of so many primitive characters becomes all the more remarkable and significant." Several new forms are described.

With the exception of a paper by Madame Pavlow on the fossil elephants of Russia (*Nouv. Mem. Soc. Imp. Nat. Moscow*, vol. xvii. pt. 2) nothing of any importance with regard to the palæontology of the Proboscidea has come under my notice.

As it deals largely with sirenians and zeuglodonts, reference may be conveniently made in this place to a paper by Dr. E. Stromer, in *Fortschritte naturwis Forschung*, vol. ii. pp. 83–114, entitled "Neue Forschungen über fossile Lungenatmende Meeresbewohner," in which the phylogeny, adaptations, etc., of the various groups are discussed in a specially interesting fashion.

A paper by Mr. A. Issel on certain fossil Italian mammals, published in *Mem. R. Ac. Lincei*, ser. 5, vol. viii. pp. 191-224, is largely concerned with Sirenia, and contains a full account of the osteology of *Felsinotherium*.

As regards cetaceans, considerable importance attaches to a paper by Prof. F. W. True, *Bull. Amer. Mus. Nat. Hist.*, vol. xxviii. pp. 19–32, on the skeleton of a species from the Patagonian Miocene, which has been described as *Diochotichus vanbenedeni*, and also as *Argyrodelphis benedeni*. In the opinion of Mr. True, the genus is referable to the *Squalodontidæ*, of which it constitutes a specialised type, characterised by the single-rooted and nearly simple-crowned teeth.

To vol. xii. pp. 139–4 of *Proceedings and Transactions of the Nova Scotian Institute of Science* Prof. G. H. Perkins communicated a memoir on cetacean remains from the superficial deposits of Canada. The main subject of the paper is a skeleton in the museum at Halifax, discovered about 1873 on the Jacquet River, New Brunswick, and identified as that of a narwhal. Other skeletons, respectively in the museums of MacGill University, Montreal, and Montpelier, belong to the white whale described as *Delphinapterus vermontanus*; but whether this is more than a large race of the existing *D. leucas* is left doubtful.

The only paper on fossil edentates that has come under my notice is one by Mr. J. Sinclair, published in *Proc. Amer. Phil. Soc.*, vol. xlix. pp. 191–5, on dermal bones of *Paramylodon* from the asphalt deposits near Los Angeles, California. These bones agree very closely with those of the Patagonian *Grypotherium.*

A few extinct marsupials are described as new by Mr. L. Glauert in an account of the so-called Mammoth Cave in Western Australia, published in *Rec. W. Austral. Mus.* vol. i. pp. 11–36. Some interest also attaches to an announcement by Mr. H. H. Scott in the *Launceston Examiner* of August 23rd of the discovery of remains of *Diprotodon* in Tasmania.

The question of the proper systematic position and feeding-habits of the African genus *Tritylodon* and its European and American relatives *Plagiaulax* and *Ptilodus* has been reopened by Dr. R. Broom in the *Proceedings of the Zoological Society of London*, 1910, pp. 760–8. The author has no doubt as to *Tritylodon* being a mammal, while as the only known specimen is from the Stormberg beds, it must be regarded as of Lower Jurassic, and not Triassic, age. Dr. Broom refuses to admit that Mr. Gidley is justified in including the three genera among the diprotodont marsupials, remarking that the dentition, both structurally and numerically, is of a different type, whilst the presence of a well-developed septomaxillary in the African genus suggests monotreme rather than marsupial affinities. It is also pointed out that, as believed by Osborn, there is considerable probability of diprotodonts having originated in Australia, in which case it is obvious that the Secondary genera must form an early parallel development. The author, therefore, concludes that "in the present state of our knowledge it seems wisest to leave the Multituberculata as a distinct independent group with no very near affinities with the living monotremes, marsupials, or eutherians." As regards the food of these mammals, it is pointed out that fruits were non-existent in Jurassic times, while if, as Dr. Broom considers probable, *Tritylodon* and its relatives were carnivorous, they must have fed mainly on reptiles, which would require a type of dentition different from that of mammal-eating species.

Remains of wading-birds from the Pleistocene asphalt-beds of Rancho la Brea, near Los Angeles, California, form the subject of a paper by Mr. L. H. Miller, published in the *Geological Bulletin of the University of California*, vol. v. pp. 439–48. Several of the specimens described are referable to the typical group of storks (*Ciconiinæ*), which is nowadays unknown in the district, and represent an extinct species of *Ciconia* and the living jabiru (*Mycteria americana*). The stork (*Ciconia maltha*) is in some respects intermediate between *Ciconia* and *Euxenura*, thereby suggesting that these two genera might well be merged in one. The other bones belong to two cranes, one of which (*Grus minor*) is extinct. In a second communication on the subject published in the same journal (vol. vi. pp. 1-19) Mr. Miller describes remains of condors, in which group the formation is remarkably rich. On the evidence of the metatarsus alone, four species are recognised, of which the first is identified with the Californian condor (*Gymnogyps californianus*). The second type is referred to an extinct species (*Sarcorhamphus clarki*) a true condor, while the third and fourth are assigned to new genera, under the names of *Cathatornis* and *Pleistogyps*. The last was a gigantic bird, equal in size to the previously described *Teratornis merriami*.

Since its original description by Sir R. Owen in 1873 the imperfect skull of the saw-billed bird (*Odontopteryx toliapica*) from the London Clay of Sheppey, preserved in the British Museum, has remained the sole evidence of its genus and species. When complete this skull probably measured something like six inches in length. The discovery has, however, been announced by Mr. B. Spulski, in the second number of *Der Geologe*, of the skull of a much larger species of the same genus in Tertiary strata in Brazil, the total skull-length being about 20 inches. The name *O. longirostris* is proposed for the Brazilian species.

Fossilised birds' feathers have been recorded from about fourteen localities—with one exception of Tertiary age; and on account of this rarity reference may be made to Mr. F. Chapman's notice in vol. xxiii. part i. of the *Proceedings* of the Royal Society of Victoria, of a fossil of this nature from the Tertiary ironstone of Redruth, Victoria. Determination of the genus of the specimen, which is in the form of impressions on the two halves of a split nodule, seems impossible, but it is suggested that it may have belonged to one of the smaller waders, such as an ibis.

To the *Ibis* for 1910 (ser. 9, vol. iv. p. 759), Mr. E. Bidwell contributes a note on some fragments of the shell of an ostrich obtained from superficial deposits in the Banda district of the United Provinces of India. Although somewhat stouter, the shell corresponds very closely in structure with that of the egg of the Somali ostrich, but the describer considers himself justified in referring the Indian fragments to a new species, under the name of *Struthio indicus*. Although this is not mentioned in the note, there appears to be historical evidence that ostriches formerly ranged over a considerable area in Central Asia, not improbably inclusive of Baluchistan; and their occurrence in India during the early Pliocene is definitely proved by the Siwalik *S. asiaticus*. Moreover, at a much later epoch, as is demonstrated by subfossil egg-shells, there was an ostrich in the Government of Cherson, southern Russia; and during the Pliocene there was a third in Samos.

One other matter in connection with fossil birds demands brief notice. In my review of vertebrate palæontology for 1907 reference was made to the description by Dr. O. Abel of certain remains from the Eocene of Alabama which he regarded as indicating a new type of large bird (*Alabamornis gigantea*). These remains, which Dr. Abel regarded as coracoids, had been identified by American palæontologists as the pelvis of a zeuglodont skeleton with which they were found in association; and I am informed by Dr. F. A. Lucas—as mentioned in *Nature* for December 31, 1910—that not only is the American determination indisputably correct, but that the bones have been mounted in their proper position in the zeuglodont skeleton, which is now exhibited in the United States National Museum. "*Alabamornis*" therefore apparently disappears from the list of extinct birds.

Extinct reptiles form, as usual, the subject of a large number of papers; but among these it is possible to refer only to the more important which have come under my notice. The proposal of Dr. O. Jaekel to remove the anomodonts (in the wider sense of that term) and chelonians from the Reptilia, and to brigade them with monotreme mammals in a class by themselves, has been already mentioned. Reference may, however, be made to an earlier paper by the same author, published in the *Zool. Anzeiger*, vol. xxxv. pp. 324–41, in which a revised classification of Reptilia is suggested. According to this arrangement reptiles, exclusive of chelonians and most of the anomodonts, are divided into four sub-classes: (1) Protorosauria; (2) Enaliosauria; (3) Lycognatha, and (4) Hyperosauria. The first includes the orders Protorosauri, Naosauri (=Pelycosauria), Procolophonii, Sphenodonti =(Rhynchocephalia), and Champsosauri; the second embraces the Mesosauri, Ichthyosauri, Sauropterygii, and Placodonti; in the third are placed the Lacerti, Ophidii, and Mosasauri; while the fourth is represented by the Dinosauri, Loricati (=Crocodilia, inclusive of Parasuchia), and Pterosauri.

Commencing a review of systematic work with the group last mentioned, that is to say the one commonly known as Ornithosauria, the first paper for notice is an elaborate description, by Mr. G. F. Easton, published in the *Mem. Connecticut Acad. Sci. and Arts*, vol. ii. pp. 1–38, of the osteology of the giant toothless American Cretaceous pterodactyles of the genus *Pteranodon.* This memoir is illustrated by 31 plates, the last two of which are devoted to restorations of the entire skeleton. The second of these illustrates the extraordinary length of the skull, which, inclusive of the supra-occipital prolongation, is about one-third greater than that of the body and tail. This supra-occipital spine is an ultra-development of that which occurs in chameleons and the *Chelydridæ*; and, as in those groups, is doubtless for the purpose of affording attachment for the powerful temporal muscles necessary to work the huge jaws. Equally noticeable in the figure is the extraordinarily small size of the pelvis, which, according to the author, appears to have been connected by means of cartilage with the sternum. In *P. ingens* the wings, even when slightly flexed, had a spread of no less than 22 ft. 3 in., while, if stretched to their full extent, the measurement would be about 5 per cent. more. Possibly even these enormous dimensions were exceeded in another species.

Very interesting is a paper given by Dr. R. S. Lull, published in *Amer. J. Sci.*, vol. xxix. pp. 1–39, on the distribution of dinosaurs. At present dinosaurian remains are unknown from Central and North-eastern Asia, but this may be attributable either to lack of knowledge of the palæontology of that area or to the circumstance that these reptiles never occurred there. The theropod group is believed by the author to have originated in North America, whence it migrated in one direction, probably at a late epoch, into South America and in another by way of

Greenland and Iceland to Europe, and so on to India, Africa, Madagascar, and Australia. The Sauropoda, on the contrary, appear to be an Old World group which migrated early in the Jurassic into "Gondwanaland," and likewise into the New World. In the southern hemisphere this group had a distribution nearly as extensive as that of the carnivorous theropods, and survived long after the latter had disappeared from the north, occurring in India during some part of the Cretaceous, and in Patagonia during the Laramie or topmost Cretaceous. On account of their semi-aquatic habits these dinosaurs were independent of complete land-connection, and able to extend their migrations across areas impassable to the terrestrial iguanodon and its relatives. This may be the reason why the latter group, so far as is known, never succeeded in reaching the southern hemisphere, although it is possible that the date of their radiation may have been later, when communication between Europe and Gondwanaland was interrupted. It has also to be borne in mind that, judging from their dentition, iguanodonts were dependent upon a particular kind of food. Originally the Ornithopoda were probably North American, and among them the Ceratopsia appear to have been always restricted to the western continent.

The pose of the limbs in sauropod dinosaurs was much discussed in 1909, and the subject has been resumed in the year under review.

In the *American Naturalist*, vol. xliv. pp. 259–83, for instance, Dr. W. J. Holland discussed the views of Dr. Tornier and others on the pose of the limbs of *Diplodocus*. In his criticism the author emphasised the marked distinctness of the Dinosauria from other reptiles, and suggested that this might in some degree be sufficient to render it probable that their limbs approximated to the mammalian type in regard to the relative position of their bones. Evidence of this is afforded by the compressed, instead of depressed, form of the thoracic cavity, which appears incompatible with limbs arranged in crocodilian fashion. It is shown that if the femur is placed, as suggested by Dr. Tornier, in a horizontal plane, its head cannot be made to enter the acetabular cavity of the ilium, while, on account of projections, no movement would be possible. In this mode of restoration the distal articular surfaces of both humerus and femur project at right angles to the axes of the bones of the lower segment of the limbs without being opposed to the corresponding articular surfaces of the latter. After a reference to the extraordinary position which would be assumed in certain circumstances by the fore-limbs of *Diplodocus* according to Tornier's restoration, Dr. Holland maintains that the form given to the limbs in the restored skeleton in the museum under his charge is, in all essential features, true to nature.

A second and more elaborate contribution to the subject was made by Dr. W. T. Matthew in the *American Naturalist*, vol. xliv. pp. 547–60. That sauropods walked, instead of crawling, the author considers fully proved, their limb-structure, as already pointed out by Dr. Abel, displaying a remarkable parallelism to that of proboscideans. This rectigrade type, in which the whole limb is pillar-like, with the foot short, rounded, and heavily padded, and the toes reduced or rudimentary, appears to be an attribute of animals of great bodily size, the movements being chiefly restricted to the upper joints, and the foot serving mainly as a cushion to minimise the shock. A structure of this kind will obviously occur only among animals which habitually rest their weight on the limbs alone. A limit is, however, soon reached in regard to the weight which even the most powerful limbs are capable of supporting in the case of a purely terrestrial animal, and this limit appears to have been attained among the elephants. If this be so, there arises the question why sauropod dinosaurs, with their less perfectly formed limbs, vastly exceeded the largest elephants in bulk and stature. The answer, according to Dr. Matthew, is that these reptiles were aquatic, and adapted to wading. "A wading animal," he observes, "has the greater part of its weight buoyed up by the water, and might attain a much larger size without transcending its mechanical limitations, just as the whales and some true fishes attain a much larger size than any land animal."

On the other hand, Dr. O. Jaekel, in a paper published in the *Monatsbericht. deutsch. geol. Ges.*, vol. lxii. pp. 270–77, maintains that the foregoing views are quite wrong, and that sauropod dinosaurs moved on their limbs after the fashion of lizards; this opinion being apparently based in part on a skeleton of the hind-limb of *Plateosaurus*, in which the bones are believed to retain their natural position, and in which the femur is nearly horizontal.

Somewhat similar views are expressed by Mr. O. P. Hay, *Proc. Washington Acad. Sci.*, vol. xii. pp. 1–25, who, after discussing the theory of the nearly upright position of the limbs in sauropods, observes that he "is not willing to assert that *Diplodocus* and its relatives never straightened out their legs, thus lifting themselves well above the ground, and never walked thus. Even crocodiles have been known to do this on rare occasions. . . . What is disputed by the present writer is that this was the customary attitude of the sauropods; and their great bulk makes it doubtful if it ever was assumed."

Towards the close of the article the author expresses his dissent from the opinion of Dr. von Huene that the sauropods are descended from the theropod dinosaurs. According to his own view, it seems much more probable that just the reverse has been the case; as "it appears more reasonable to suppose that the Sauropoda were a more primitive stock than the Theropoda, and that the latter were derived from the early Triassic representatives of the former."

In the *Anat. Anzeiger*, vol. xxxiii. pp. 401–405 (published in 1908, but not referred to in my review for that year), Dr. von Huene discusses the prepubis of dinosaurs and other reptiles. He is of opinion that this bone is a distinct pelvic element. A pubis + a prepubis are found in ornithopod dinosaurs, crocodiles, and birds. In crocodiles and the ceratopsian section of the Ornithopoda the pubis is rudimentary, whereas in birds it is the prepubis which aborts. Much the same condition obtains in crocodiles and pterodactyles, in which the prepubis becomes anchylosed to the other pelvic elements. The presence of a prepubis is further evidence of the close affinity of the four groups mentioned.

Mere mention will suffice for a paper by Mr. P. Larkin, *Journ. Geol.*, vol. xviii. pp. 93–8, on remains of a sauropod from the Cretaceous of Oklahoma; and the same course may be adopted in regard to a note by Baron Nopcsa, *Geol. Mag.*, decade 5, vol. vii. p. 261, on the systematic position of *Titanosaurus*.

On the other hand, more than ordinary interest attaches to a paper by Dr. Smith Woodward, published in the *Quart. Journ. Geol. Soc.*, vol. lxvi. pp. 111-14, on a skull of *Megalosaurus* from the great Oolite of Minchinhampton, Gloucestershire. The special interest of the specimen, which is made the type of the new species *M. bradleyi*, is the presence of a nasal horn, as in the American *Ceratosaurus*, from which genus *Megalosaurus* is distinguished by the presence of four, in place of three, pairs of teeth in the premaxillæ.

Another paper of special interest to British palæontologists is one by Dr. von Huene on a primitive dinosaur (*Saltopus elginensis*) from the middle Trias of Elginshire, published in *Geol. u. Pal. Abhand.* ser. 2, vol. viii. pt. 6. Although the new genus presents certain resemblances to *Thecodontosaurus*, it cannot be placed in the same family, and perhaps it will prove to be more nearly related to *Ammosaurus* and *Tanystrophæus*, although referable to another family. The Elgin dinosaur, which had saltatorial habits, appears to have been about a couple of feet in length, with relatively long neck and tail.

A magnificently preserved skull from the Cretaceous of New Mexico has afforded Mr. Barnum Brown, *Bull. Amer. Mus. Nat. Hist.*, vol. xxviii. pp. 267–74, the opportunity of describing a new genus, *Kritosaurus* (preferably *Critosaurus*) of trachodont dinosaurs.

Passing on to the stegosaurian group, it has first to be mentioned that Dr. von Huene has recorded, in the *Neues Jahrbuch f. Min. Geol. u. Pal.*, 1910, vol. i. pp. 75-8, a femur of *Omosaurus* (= *Dacentrurus*) from the Forest Marble of Enslow Bridge, Oxfordshire.

The nature and arrangement of the bony armour of the American *Stegosaurus* are discussed by Dr. R. S. Lull in two papers published in the *Amer. Journ. Science*, vol. xxix. pp. 201-210, and xxx. pp. 361–77. In the specimen restored by Prof. Marsh a number of ossicles were found adhering to the under-surface of the lower jaw, which, in the opinion of Dr. Lull, formed a gular shield, and also extended over a considerable part of the body, as it is unreasonable to suppose that any portion of the skin of an armoured reptile would be unprotected. The great vertical dorsal plates and caudal spines, the former of which were originally supposed to constitute a single series, appear to have been arranged in a double row. The plates are considered to represent an ultra-development of the longitudinal vertical ridge on the horizontal scute of a crocodile or an unspecialised dinosaur like *Ancylosaurus*. Throughout the back the ribs are T-shaped in section in order to bear the weight of the plates. In the neck the latter are borne on short and notched transverse processes, but in the back the processes become longer and stouter, while in the sacral and anterior caudal region the bases of the plates are approximated and supported on the summits of the tall and expanded neural spines. The terminal third of the tail seems, however, to have formed a flexible aggressive weapon, in which the laterally divergent spines were inserted in the muscles between the neural spine and the centrum. Although the caudal spines of the English Kimeridgian *Omosaurus* are structurally identical with those of one species of *Stegosaurus*, the author is indisposed to admit the generic identity of the Old World and American types, as vertical plates have not been discovered in the former. In a note to *Nature* Dr. F. A. Lucas states that while Dr. Lull believes the plates to have been arranged in pairs, his own opinion is that although, reasoning by analogy, they should have been thus arranged, the facts point to their having been placed alternately on opposite sides of the median line. No pair of plates has ever been found, and, making the greatest allowance possible for individual variation, it seems incredible that differences of several inches should exist between the plates from the two sides of the body if they were arranged in symmetrical pairs.

Little work has been done among the more typical Crocodilia; but an unusually well-preserved crocodilian skull from the Ceratops beds of Wyoming, in the United States National Museum, has been referred by Mr. C. W. Gilmore, *Proc. U.S. Nat. Mus.*, vol. xxxviii. pp. 485-502, in spite of its later geological horizon, to a second species of the genus *Leidyosuchus*, originally described from the Judith River Beds of Alberta, Canada. The new species is named *L. sternbergii.* A second skull, from the Hell Creek beds of Montana, which came under the author's notice after the original paper was written, is also described and figured.

*Leidyosuchus* is a short and broad skulled crocodile, with the nasals apparently not reaching the nares, the posterior nostrils enclosed by the pterygoids (instead of being behind them, as in *Crocodilus*), the mandibular symphysis short and formed in part by the splenial, the upper teeth more numerous than the lower, the first lower tooth received into a pit, and the third and fourth—which are about equal in size—into notches in the skull. The centra of the vertebræ have the cup in front; and there was armour on the lower as well as on the upper surface of the body. Many of these characters connect the genus with *Crocodilus* on the one hand, and *Alligator* and *Caiman* on the other, although their preponderance is with the first genus; and there are also indications of affinity with the extinct *Diplocynodon*. The position of the posterior nostrils—intermediate between those of modern and Jurassic crocodiles—is what might have been expected from the geological horizon of the genus.

Somewhat curiously, these early types of crocodilian palates had been sketched by Dr. O. Jaekel, probably without seeing Mr. Gilmore's paper, in an article on a new phytosaurian from the Buntersandstein of Bamberg, as those which ought to occur in the group intermediate between Triassic and modern crocodiles. In this paper, *Sitzber. Ges. naturfor. Freunde*, 1910, pp. 197-229, the author describes, under the name *Mesorhinus fraasi*, a new type of parasuchian, differing from the European *Phytosaurus* and *Mystriosuchus* by the nares being partially in advance of the front border of the preorbital vacuity, although not to the same extent as in the North American *Palæorhinus*. In its comparatively straight profile the skull of the new genus approximates to that of *Phytosaurus*. In the opinion of Dr. Jaekel, the primary choanæ of the parasuchian crocodiles were covered by a membrane, and their opening thereby converted into secondary choanæ placed much farther back; such an explanation, affording, at all events, the best mode of explaining the evolution and development of the modern crocodilian palate.

In connection with the foregoing, it may be mentioned that in a paper published in the *Centralblatt f. Min. Geol. u. Pal.*, 1909, pp. 583–92, on a huge skull of *Phytosaurus plieningeri* from the Swabian Trias, Dr. von Huene expresses the opinion that the genus should not only also include *P. kampfi*, but likewise the above-named *Mystriosuchus planirostris*, as well as the American *Rhytitodon carolinensis*.

A second addition to the list of reptiles from the Trias of Elgin has been made during the year by Dr. von Huene in a paper published in the *Neues Jahrbuch f. Min. Geol. u. Pal.*, 1910, vol. ii. pp. 29–62. The new form, which is described as representing a genus by itself under the name of *Brachyrhinodon taylori*, is a small, short-skulled rhynchocephalian, probably allied to the Upper Jurassic Continental forms. The paper concludes with a review of the extinct rhynchocephalians and their relationships.

Before noticing papers on marine reptiles, I may remind my readers that interesting observations on mosasaurians, ichthyosaurs, turtles, etc., will be found in Dr. Stromer's article on marine lung-breathing animals already quoted (p. 673) in connection with sirenians and cetaceans.

Of the mosasaurian genus *Tylosaurus* Dr. von Huene describes a finely preserved skeleton from the Cretaceous of Kansas in *Geol. u. Pal. Abhand.*, ser. 2, vol. viii. pt. 6. Several osteological questions are discussed; and since *Tylosaurus* and another mosasaur possess rudimentary nasals, it is evident that these bones are not involved in the premaxillary rostrum of that group. The evidence of the existence of distinct nasals is, however, questioned by Mr. S. W. Williston in a note on a skeleton of the allied genus *Platecarpus* which appeared in the *Journal of Geology*, vol. xviii. pp. 537–41.

The first part of the British Museum *Catalogue of the Marine Reptiles of the Oxford Clay, based on the Leeds' Collection*, by Dr. C. W. Andrews, is a work of first-class importance. The remains of crocodiles, ichthyosaurians and plesiosaurians from the Oxford Clay near Peterborough collected by the Messrs. Leeds are noted for their wonderful state of preservation, which admits of the osteology of these groups being studied in nearly as much detail as if we had recent skeletons. Although the deposits at Peterborough were probably laid down in fairly deep water, the association of bones of land reptiles with those of the marine types indicates that the coast was not far distant. Among the ichthyosaurs, the single large-eyed representative of the genus *Ophthalmosaurus* was, however, a pelagic reptile with habits doubtless very similar to those of the whales and porpoises which have taken its place in the seas of the present day. The absence of teeth may be due to the circumstance that *Ophthalmosaurus*, like the modern beaked whales (*Ziphiidæ*), which are also nearly edentulous, fed on squids and cuttles—a suggestion confirmed by indications in the skull of diving powers on the part of these reptiles.

The plesiosaurs of the Oxford Clay, in place of being driven through the water at a rapid pace by means of a screw-like tail-fin, after the fashion of the ophthalmosaur, appear to have rowed themselves along on or near the surface in a more leisurely style by means of their four paddles, of which the hind pair is relatively larger than in the ichthyosaurs, whereas the tail, which probably carried little, if anything, in the way of a fin, is proportionately reduced. Dr. Andrews has worked the osteology of both groups with great care.

Fine skeletons of plesiosaurians from the Upper Lias of Holzmaden are described by Dr. E. Fraas in the *Palæontographica*, vol. lvii., where attention is directed to the rarity of plesiosaurian remains in the German Lias as compared with what obtains in the corresponding English formation. Their abundance in the latter indicates that these saurians were common in the Liassic seas, although they did not, in all probability, congregate in such large shoals as the commoner ichthyosaurs. The majority of the English specimens come from the Lower Lias of Lyme Regis, Street, and Charmouth, an horizon represented by a different type of strata at Holzmaden. Dr. Fraas refers his specimens to *Plesiosaurus gulielmi-imperatoris*, named by Prof. Dames in 1895, and to a new species of *Thaumatosaurus*, proposed to be called *T. victor*. This reptile was about 10 feet in length, with a relatively small head, short and thick neck, plump body, slender and nearly equal-sized paddles, and short, powerful tail.

It may be added that to the *Geological Magazine*, decade 5, vol. viii. p. 110, Dr. Andrews has contributed a note on a mounted skeleton of the small Oxfordian plesiosaurian *Peloneustes philarchus*.

Fossil chelonians do not loom large in the work of the year; but British palæontologists should be interested in the redetermination by Mr. D. M. S. Watson, *Geol. Mag.*, decade 5, vol. vii. p. 311, of a species from the Purbeck of Swanage described by myself under the name *Thalassemys ruetimeyeri*. Mr. Watson finds that the species had a mesoplastron, and is therefore a member of the Amphichelydia, referable apparently to the North American Jurassic genus *Glyptops*.

Several new species of American fossil terrapins—all referable to previously known genera—are described by Mr. O. P. Hay in the *Proc. U.S. Nat. Mus.*, vol. xxxviii. pp. 307–236; and in vol. ix. of the *Bull. Soc. Acad. Boulogne-sur-Mer* Mr. H. E. Sauvage briefly discusses the Jurassic Chelonia of the Boulonnais.

The reptiles and amphibians of the Trias and Permian have received much attention during the year. To vol. xxviii, pp. 127–239 of the *Bull. Amer. Mus. Nat. Hist.* Dr. R. Broom contributed, for instance, an article on the relationship of the Permian reptiles of North America to those of South Africa. After reviewing the leading types of each, he concludes that in the Upper Carboniferous northern South America was the home of a primitive vertebrate fauna from which originated the North American Pelycosauria and the African Anomodontia (in the wider sense of the term). In the Permian this fauna invaded North America, where it soon became isolated. Early in the same epoch the Brazilian genus *Mesosaurus* reached Africa by a land-bridge, and later on appeared other types, which may have developed in the area now occupied by the South Atlantic. When sundered, the North American and African faunas underwent development in divergent directions, the former displaying many strange specialisations—notably in vertebral spines—while the latter showed a tendency to increase in the size of the limbs. This limb-lengthening, accompanied by the alteration of the phalangeal formula from 2.3.4.5.4 to 2.3.3.3.3, started the mammalian line of evolution, for directly the more specialised anomodonts raised their bodies above the ground they were well on the way to become mammals. Birds, in fact, adds the author, "are reptiles that became active on their hind limbs; mammals are reptiles that acquired activity through the development of all four."

The same subject is continued by Dr. Broom on somewhat different lines in the *Trans. R. Soc. S. Africa*, vol. i. pp. 473–77, in which the relationship of the American pelycosaurs to the African anomodonts is again emphasised. It is added that during most of the Permian the southern continent was separated by sea from Europe, but that towards the close of this period a land-connection, probably in Asia, was established, by means of which pariasaurians and apparently dicynodonts effected an entrance into Russia. By the Lower Jurassic the European connection had become well established, as the Stormberg dinosaurs are in some cases closely allied to European types, while the African *Tritylodon* is near akin to the European *Triglyphus*.

Intimately connected with the two last is a paper by the same author on the nomenclature of the elements of the amphibian shoulder-girdle, published at Grahamstown, but in which of the South African journals there is no clue in my copy. After discussing the elements of the frog's shoulder-girdle, Dr. Broom refers to the cleithrum, or clithrum, as it should be spelt, of the labyrinthodonts, pelycosaurians, pariasaurians, and dicynodonts.

Further reference to the constitution of the amphibian and reptilian shoulder-girdle occurs in a paper by Mr. Williston in the *Journal of Geology*, 1910, pp. 585–600, under the title "New Permian Reptiles and Rhachitomous Vertebræ." In this communication the author adopts the view (already accepted by Ameghino) that the element commonly termed precoracoid, or procoracoid, is really the coracoid, while the posterior bone generally designated coracoid is a metacoracoid. In referring to the origin of this revised nomenclature, he states it is "after Howes and Lydekker, the former of whom reached the same conclusion from the study of mammals." As a matter of fact, I suggested the redetermination—*Proc. Zool. Soc.* 1893, p. 172—from an examination of the shoulder-girdle of sloths and monotremes, but my conclusions were disputed by Prof. Howes.

As regards the rhachitimous type of vertebræ, the author strongly suggests the view, in the paper last quoted, that the hypocentum is the element from which the centrum of higher vertebrates has been evolved.

Of the new forms described in the same paper, perhaps the most interesting is *Arœoscelis*, which is regarded as typifying a new family, of uncertain affinity. The one species appears to have been a long-necked and long-legged reptile of about 18 in. in length.

Certain specimens of South African fossil reptiles in the collection of the British Museum form the subject of a short article by Dr. R. Broom in the *Trans. R. Soc. S. Africa*, vol. ii. pp. 19–25; the species referred to including *Galesaurus*, *Scaloposaurus*, *Titanosuchus* (of which an imperfect skull is for the first time described and figured), *Theriognathus*, *Ælurosaurus*, *Lycosaurus*, and *Cistecephalus*.

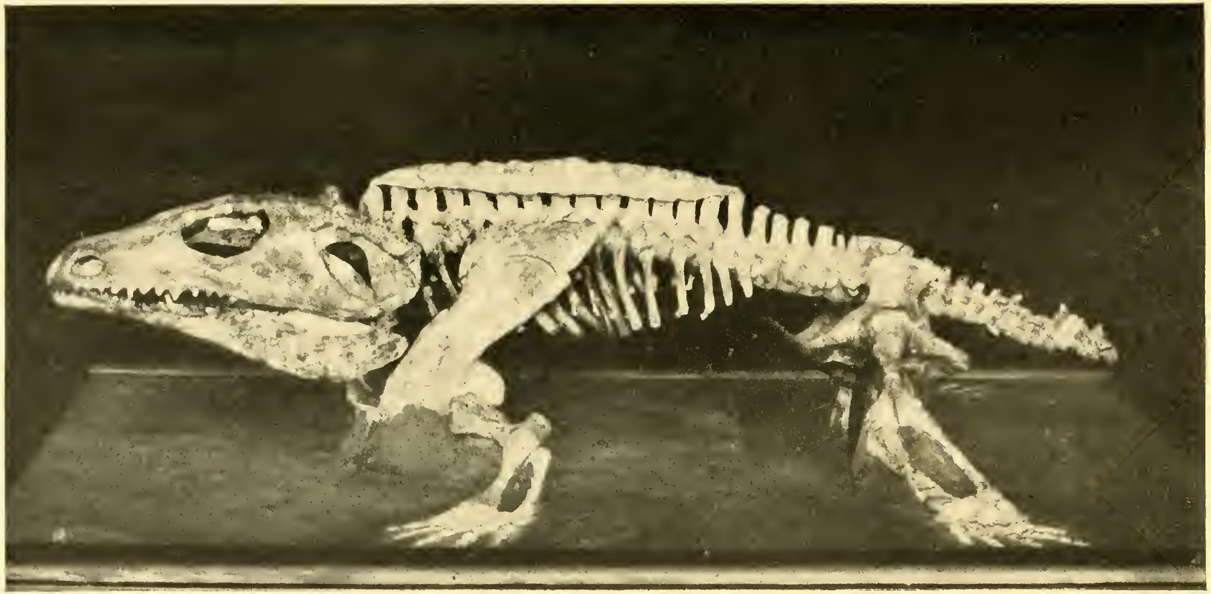
The earliest known Tetrapoda of France are discussed in an article communicated by M. Armand Thevenin to the Paris *Annales de Paléontologie*, vol. v. pt. 1. The review, which is well illustrated, includes such forms as *Protriton*, *Pelosaurus*, *Actinodon*, and *Archegosaurus*. It concludes with a discussion whether the Microsauria should be classed as reptiles or amphibians, special mention being made of the fact that the British *Ceraterpetum galvani* tends to connect typical stegocephalians with microsaurians, as it retains the sculptured and grooved skull, the pectoral girdle, and the cartilaginous carpus and tarsus of the former, combined with the elongated body and short ribs of the latter.

To the *American Naturalist*, vol. xviii. pp. 367–75, Mr. R. L. Moodie contributes a note on the alimentary canal of a branchiosaurian salamander from the Carboniferous shales of Mazon Creek, Illinois, for which the generic name *Eumicrerpeton* is proposed. The specimens are preserved in nodules, and were it not that soon after death the œsophagus became loosened and displaced, the viscera would recall those of a modern salamander. The viscera were compared with those of several modern salamanders, with the result that they are closest to those of an immature *Diemyctylus torosus*, the next nearest types being *Desmognathus*, *Spelerpes*, and *Hemidactylus*. From this, it is possible that the adults of the last three retain an ancestral condition of the intestine which is transient in *Diemyctylus*; and the resemblance of the viscera of the fossil to the recent forms tends to confirm the theory of the author that modern salamanders are descended from branchiosaurians.

The small, carapaced, amphibian-like creature from the Permian of Texas described many years ago by Prof. Cope under the name *Dissorophus* and an allied genus for which the author proposes the name *Cacops* form the subject of two papers by Mr. S. W. Williston, respectively published in the *Bull. Geol. Soc. America*, vol. xxi. pp. 249–48, and the *Journal of Geology*, vol. xviii. pp. 526–41. They are regarded as indicating a family—the *Dissorophidæ*—of uncertain affinity.

As the general characters of the skeleton are well shown in the annexed illustration, it will suffice to quote Mr. Williston's definition of the family, which is as follows:

Skull with the otic notch completely enclosed, so as to form a large ear-cavity. Palate with only two large teeth on each side, one at the anterior inner margin of the nares, the other at the posterior margin; mandibular and maxillary teeth of nearly equal size. Parasphenoid reduced. Twenty-one presacral and two sacral vertebræ; tail short. A dorsal carapace, composed



Skeleton of *Cacops aspidophorus*. After Williston.

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of lateral expansions of the spines of the vertebræ, with overlying intercalated dermal plates. Cleithrum very large and expanded above. Clavicles small, without external pittings. Interclavicle smooth on the dermal surface, small, with a short posterior process. Humerus without ectepicondylar process.

In the typical *Dissorophus* the carapace occupies the full width of the body, with a broad and elongated shield in front overlying several vertebræ; but in *Cacops* it is only slightly broader than the vertebræ, becomes narrower in front, and is not fused into an anterior dorsal shield.

In reference to *Cacops*, Mr. Williston observes that "the creature, as mounted, presents an almost absurd appearance, with its large head and pectoral region, absence of neck, and short tail. It is certain that it possessed no other dermal ossifications than those of the median dorsal carapace, and it would seem almost as certain that the creature was aquatic or largely amphibious in its habits. Almost frog-like in appearance, it doubtless had more or less frog-like habits. What was the significance of the dorsal carapace I am at a loss to suggest. That it could have been a protection seems more than doubtful, whatever may have been its use in *Dissorophus*; but this coincidence is remarkable: with an external turtle-like ear-opening, it had also the beginning of a turtle-like carapace. And this parallelism is also seen in *Diadectes*, a reptile with dorsal dermal plates and turtle-like ears. . . . That the animal was a swimmer I do not doubt, and in all probability the feet were webbed—they were certainly not clawed. Whatever may have been its habits, the creature, with the nearly related *Dissorophus*, must be classed among the oddities of vertebrate palæontology."

In a paper on reptiles and amphibians from the Permian of Texas, published in *Bull. Amer. Mus. Nat. Hist.*, vol. xxviii. pp. 163–88, Mr. E. C. Case gives a restoration of the skeleton of the cotylosaurian genus *Diadectes*. These reptiles appear to have been sluggish, harmless and perhaps burrowing in their habits, carrying the body close to the ground. A special suborder, *Gymnarthria*, is suggested for the new genus *Gymnarthrus*. Several new amphibians are named. In a second paper, *l.c.* pp. 183–88, the same writer describes, under the name of *Pœcilospondylus francisi*, a new genus and species of pelycosaurian from the Texan Permian; while in a third communication, *ibid.*, pp. 187–96, he gives a very full account of the skull and skeleton of *Dimetrodon incisivus*—one of those extraordinary reptiles with enormous dorsal spines to the vertebræ.

In *Geol. u. Pal. Abhandlungen*, ser. 2, vol. viii. pp. 33–46, Dr. von Huene redescribes and figures the typical remains of the well-known labyrinthodont *Dasyceps bucklandi* from the Permian of Kenilworth, preserved in the museum at Warwick. The skull is compared with those of other stegocephalians, and an appendix added on the homology of the stegocephalian "epiotic" and "supraoccipital" with the corresponding elements of reptiles, especially *Pariasaurus*, *Ichthyosaurus*, and *Placochelys*. The restoration of the occipital aspect of the ichthyosaurian skull taken as a basis of comparison is the one made by Baur, which differs in some details from that given by Andrews in the memoir already cited (p. 684).

Of papers devoted mainly or exclusively to fishes, the first for notice is one by Dr. E. Stromer on reptilian and fish remains from the marine older Tertiary strata of southern Togo, German West Africa, published in *Monatsber. Deutsch. geol. Ges.*, vol. lxii. pp. 478–507. With the exception of *Varanus niloticus*, none of the few reptiles is specifically identified; but the fishes include species of pycnodonts, rays, and sharks, several of which are new, while the palate of one ray is made the type of the new genus *Hypolophites*, whose affinities are with *Myliobatis*.

Fish-remains from strata of apparently Lower Cretaceous age in the Manse Valley, Cameruns, form the subject of an article by Dr. Jaekel in *Beiträge zur Geologie von Kamerun*, Berlin, vol. x. pp. 329–98. The one form fully described, *Proportheus kameruni*, represents a new genus and species, regarded by its describer as an annectant type. The preservation of the skull is remarkably fine, all the constituent bones being displayed.

Two other papers on African fish-remains, by Mr. F. Priem, have been published in *Bull. Soc. Géol. France*, ser. 4, vol. ix. p. 315–26. In the first of these the author describes a number of fish-teeth from the Eocene phosphates of Tunis and Algeria, many of which approximate to those from the Eocene of Egypt. The second is devoted to a new pycnodont (*Cœlodus bursauxi*) from the upper Senonien of Tunis. Here also may be mentioned a note by Mr. Wood in the *Geological Magazine*, decade 5, vol. vii. p. 402, on a new sole (*Solea eocenica*) and eel (*Mylomyrus frangens*) from the Egyptian Eocene; the second of these also representing a new genus.

Remains of various fishes from the Upper Silurian of Portugal, of which one is new, form the first portion of a paper by Mr. Priem published in *Communicaçoes Serv. Géol. Portugal*, vol. viii. 10 pp.

The Oligocene fishes of Belgium are described at considerable length by Mr. Maurice Leriche in the *Mém. Mus. R. Hist. Nat. Belgique*, vol. v. pp. 231–363. The species are ranged under two series, namely a small one from the Tongrien, and a larger one from the Rupelien horizon. None of the species is new; and those of the Rupelien stage indicate a littoral fauna, containing representatives of several genera still living on the coast. As a whole, the fish-fauna of the Oligocene of Belgium is of a somewhat more temperate nature than that of the Eocene, which is distinctly tropical. Most of the Oligocene forms appear to be new types, although a considerable number are derivatives from those of the Eocene. A few species are common to both horizons. Reference may likewise be made to the continuation by Dr. Smith Woodward of his monograph of the fishes of the English Chalk in the volume issued by the Palæontographical Society for 1910; chimæroids and elasmobranchs forming the subject of this fasciculus.

Another fish-paper, dealing mainly with sharks and rays, by Mr. G. Stefanu, entitled "Osservazioni sulla Ittifauna Pliocenica di Orciano e San Quirico in Toscana," is issued in the *Boll. Soc. Geol. Ital.*, vol. xxviii. p. 539–648.

An equally brief notice must serve in the case of a memoir by Prof. G. de Alessandri on the Triassic fishes of Lombardy, published in *Mem. Soc. Ital. Sci. Nat. e Mus. Civ. Storia Nat. Milano*, vol. vii. pp. 1–145, and containing descriptions of a few new species and notes on many others.

Otoliths and other fish-remains from the pisciferous shales of Transbaikalia form part of the subject of a memoir by Mr. O. M. Reis, published in part 29 of *Récherches géologiques et minières le long du Chemin de Fer, Sibérie.* These shales are largely developed on the left bank of the We'tiser River, and appear to be of Mesozoic age. A nearly complete fish is made the type of a new genus and species, under the name of *Stichopterus woodwardi.*

The bituminous shells of Albert and Westmorland counties, British Columbia, which are extensively worked for commercial purposes, contain certain layers abounding in remains of fishes of the family *Palæoniscidæ*. These Albert Shale fishes, according to an elaborate account by Dr. L. M. Lambe, published in *Contributions to Canadian Palæontology*, vol. iii. No. 3, appear to have lived in lagoons, cut off to a great extent from the sea ; and the abundance of their remains in some of the layers can be explained only by occasional destruction on a wholesale scale. With the exception of an *Elonichthys*, none of the species recorded by Dr. Lambe is new. In the Monographs of the Palæontological Society for 1910 Dr. R. S. Traquair continues his account of the *Palæoniscidæ* of the British Carboniferous.

Remains of fishes from the Devonian of New York are described by Mr. B. Smith in the *Proc. Acad. Nat. Sci. Philadelphia*, 1900, pp. 656–63. They include spines of *Machæracanthus* and a skull of *Dinichthys*.

The last paper for notice, published in *Festschrift zum sechzigsten Geburtstage R. Hertwigs*, Jena, 1910, vol. ii. pp. 611–22, is one by Dr. E. Stromer on the dentition of the *Lepidosirenidæ* and the distribution of Tertiary and Mesozoic lung-fishes. In the second part of this article the author refers to the occurrence of remains of Tertiary lung-fishes allied to *Protopterus* in districts considerably to the north of the present habitat of that genus, and discusses their bearing on the theory of a former land-connection between Africa and South America. In noticing the former range of *Ceratodus* in space, Dr. Stromer observes that palæontology furnishes no clue as to the relationship between the *Lepidosirenidæ* and *Ceratodontidæ*.

# MOLECULAR ARCHITECTURE

## PART II

By R. T. COLGATE and E. H RODD

### The Crystalline Structure of Binary Compounds

In the first part of this article[1] it was pointed out that with very few exceptions the elements crystallise in forms belonging either to the holohedral cubic or hexagonal systems; it was shown that these two forms correspond to the cubic and hexagonal modes of packing equal spheres closely. The crystal structures of the elements correspond in their symmetry to such close-packed assemblages of equal spheres.

We have now to consider the crystal structure of compounds of a very simple type—the binary compounds of the type $X\ Y$, where $X$ and $Y$ are two atoms generally of equal valency. From the conclusion drawn by Barlow and Pope that atoms of the same valency appropriate approximately equal domains or spheres of influence when present in the same compound, it follows that in order to represent the crystal structure of such a binary compound by the same method, two kinds of spheres will have to be used not exactly but nearly equal in individual volume and otherwise distinguishable the one from the other. A symmetrical distribution of the two kinds closely packed to form an assemblage of spheres of such a nature as will faithfully represent the distribution of the atoms in a crystal of the compound must, it is evident, present nearly the same relative dimensions as would be the case if the spheres arranged were all equal; the symmetry of the arrangement, however, will be a function not merely of the kind of close-packing but also of the nature of the distribution of the two sorts of spheres. Two new factors are thus introduced, (*a*) the presence of some sort of pattern in place of the complete uniformity of an assemblage composed of but one kind of sphere, (*b*) a slight departure from the simplicity of

[1] Part I. appeared in Science Progress, January 1911, vol. v. pp. 345-70.

arrangement of the sphere-centres in the assemblage traceable to the existence of small inequalities between the distances separating nearest sphere-centres. The united result of these two factors is a degradation of symmetry and whilst it is found possible under the new conditions to obtain cubic or hexagonal symmetry as in an assemblage of uniform spheres, the symmetry is of a somewhat lower order. The degradation of the symmetry of the assemblage finds expression in the disappearance of certain of the elements of symmetry which characterise the cubic and hexagonal closest-packed assemblages of equal spheres. These latter are said to present holohedral cubic or hexagonal symmetry; the suppression of elements of symmetry referred to leads to the production of the so-called hemihedral and tetartohedral types of cubic and hexagonal symmetry.

Now let us look at the facts. A large number of binary compounds such as oxides and sulphides of bivalent elements and chlorides, bromides and iodides of univalent elements have been examined crystallographically and almost without exception these are found to crystallise in hemihedral cubic or hexagonal forms. Further in the case of binary substances crystallising in the hexagonal system, as in the case of elements presenting this type of symmetry, the axial ratio $a:c$ approximates to $1:1{\cdot}6330$ or $1:0{\cdot}8165$.

How near the actual observed value of $a:c$ approaches to the theoretical value is well shown in the following table:

| Compound. | $a:c$ (observed). | $a:c$ (ideal). |
|---|---|---|
| Beryllium Oxide, BeO | $1:1{\cdot}6305$ | $1:1{\cdot}6330$ |
| Zinc Oxide, ZnO | $1:1{\cdot}6077$ | ,, |
| Zinc Sulphide, ZnS | $1:0{\cdot}8175$ | $1:0{\cdot}8165$ |
| Cadmium Sulphide, CdS | $1:0{\cdot}8109$ | ,, |
| Silver Iodide, AgI | $1:0{\cdot}8196$ | ,, |

The compounds in this table are practically the only binary hexagonal compounds known. The great majority of binary substances are cubic, very few crystallising in other systems. Among these last may be mentioned tetragonal mercurous chloride and iodide (if these be given the double formulæ $Hg_2Cl_2$ and $Hg_2I_2$ they are not strictly binary, as in such compounds the mercury atomic sphere of influence may have a valency volume = 2); orthorhombic litharge, PbO; monosymmetric mercuric oxide, HgO and a few more.

The slight difference in size between the volumes of the spheres of influence of the atoms of two kinds in a binary compound would account for the slight divergence in hexagonal crystals of the axial ratio $a : c$ from the ideal value.

Barlow and Pope (*Trans. Chem. Soc.* 1907, 1170 *et seq.*) have worked out in great detail the spacial distribution of the atoms in a number of binary compounds, particularly in the cases of silver iodide and a number of halogen salts of the alkali metals. Only a few of the results of this investigation can be quoted.

The case of silver iodide is particularly interesting. This substance is dimorphous, crystallising at the ordinary temperature in the hemimorphous pyramidal class of the hexagonal system, the ratio $a : c$ being 1 : 0·8196; whilst above 147° C. it is stable in a cubic modification. These two modifications of course correspond to the hexagonal and cubic closest-packed assemblages of equal spheres and since the axial ratio $a : c$ of the hexagonal form is very nearly identical with the ideal value 1 : 0·8165, it is highly probable that the silver and iodine atomic spheres of influence are very nearly equal in size.

It has long been known that when a substance is dimorphous, its two crystalline forms are only stable the one above and the other below a certain temperature termed the transition temperature. If the modification stable at lower temperatures be gradually heated, when the transition temperature is reached and passed a change occurs—sometimes suddenly and with violence as in the case of silver iodide—into the modification stable at the higher temperature. It is of great theoretical importance that Barlow and Pope are able to show that in the case of silver iodide, the two modifications, one hexagonal and the other cubic, are very closely related. A cubic assemblage of spheres is deduced representing silver iodide and it is shown that by a slight shear of alternate layers this assemblage becomes transformed into a hexagonal form possessing hemimorphous symmetry corresponding to the observed hemimorphism of silver iodide. Similarly, of course, a shear in the reverse direction changes the hexagonal back into the cubic assemblage. It may well be that when hexagonal silver iodide is heated, the change in temperature produces a small change in the relative volumes of the spheres of atomic influence of the two kinds, as a result of which, above 147° C., change takes place by means of the slight shear already described into the more stable cubic form. The present writers have ascertained that in the case of several dimorphous organic compounds the two forms of these are also very closely related (p. 710).

It is also apparent from a study of the two forms of silver iodide, that the homogeneous partitioning of the hexagonal and cubic assemblages of spheres simulating these two forms can be so carried out as to give exactly the same aggregate or unit in both cases. The dimorphism of the compound is therefore to be attributed to the existence of alternative methods of close-packing units of this form. Polymorphism in general may be defined as the existence of alternative methods of homogeneously close-packing the same set of spheres of atomic influence under the condition that the homogeneous partitioning of the assemblages shall yield the same form of unit or molecule identical in composition, constitution and configuration in all the cases thus related. This definition gives us for the first time a clear idea of the nature of polymorphism and is of considerable importance.

Another exceptionally interesting point arising out of this investigation has reference to the abnormal coefficient of expansion of silver iodide. It is well known that hexagonal silver iodide has a negative coefficient of cubic expansion, a large negative coefficient of linear expansion in the direction of the principal axis and a small and positive coefficient in directions at right angles to this axis. Supposing that in the hexagonal silver iodide assemblage the silver and iodine spheres of influence which are slightly unequal have different relative thermal coefficients of expansion, such that as the temperature rises the volumes of the spheres of the two kinds become more nearly equal, heating will cause a closer approximation to the ideal closest-packed assemblage and will actually produce a contraction of the mass as a whole corresponding to the observed negative coefficient of cubic expansion of the crystals.

There seems, in fact, to be no end to the fruitfulness of the new theory nor to the diversity of problems which it helps to elucidate. The study of the geometrical properties of assemblages representing the halogen compounds of the alkali metals potassium, rubidium and cæsium has led to many interesting results. For instance, assemblages have been derived to represent the crystalline forms of these substances which show peculiarities corresponding to the observed properties of the crystals as regards symmetry, cleavage and gliding planes, modes of twinning, etc. From an examination of a large number of trihalogen compounds of the metals potassium, rubidium and cæsium, it is concluded that the sphere of influence of the metallic atom in these compounds has the same volume as in the monohalogen compounds. This is in accordance with the theory of multivalency already put forward. It is interesting too to note that thallic iodide is almost identical crystallographically with rubidium triiodide. It is well known of course that thallous salts are isomorphous with potassium salts and the existence of crystallographic similarity between $TlI_3$ and $RbI_3$ serves but to emphasise the relationship between thallium and the alkali metals. It seems indeed that thallium is out of place in the third group of the periodic system and that its fundamental valency is unity.

The halogen salts of ammonium crystallise in forms belonging to the cubic system and although the structure of these salts is obviously more complex than that of the corresponding salts of the alkali metals, yet it has been found possible to account satisfactorily for their crystalline structure. The deduction of the crystalline structure of these compounds depends upon the geometrical properties of close-packed assemblages already referred to.

So far only inorganic substances have been discussed here from the Barlow-Pope standpoint; but as a matter of fact, the first paper published by these authors dealt principally with the application of the theory to organic compounds, the work on the elements and binary compounds appearing later. Inasmuch, however, as the latter work is simpler and more elementary, it has been introduced here first. In view of the overwhelming influence which the study of organic chemistry has had upon the development of chemical theory, the application of this new theory of the atomic structure of crystals to organic compounds is however of great importance. Many of the most fruitful ideas which have found their way into chemistry have had their origin in the study of organic chemistry. The idea of valency itself was not first developed, as might have been expected, from the study of simple inorganic substances but grew in the mind of Frankland from consideration of more complex organic bodies. During recent years too, a great amount of time has been spent upon the study of the relationships existing between the chemical constitution and various physical properties of organic substances, such as their refractive index, optical dispersion and magnetic rotation in solution. The great majority of such investigations deal with substances in the liquid form or in solution. Very few attempts have been made to correlate the physical properties of solid bodies with their chemical constitution. Consequently if it be found possible to discover a more intimate connection between chemical constitution and crystalline form than heretofore, a great advance will have been made.

The study of organic compounds with the idea of investigating their ultimate crystal structure will be simplified by the accurate knowledge we possess of the constitution of many. The constitution or mode of linking of the atoms in inorganic compounds other than the very simplest is at present very obscure; should it be found possible in the case of organic compounds to discover the connection existing between chemical constitution and crystal structure, it might well become possible to apply the knowledge thus gained to the solution of the problem of the constitution of inorganic bodies.

The fact is well known that benzenoid organic compounds crystallise with greater facility than paraffinoid derivatives, many of these latter being oils which crystallise only at low temperatures. The benzene derivatives on this account afford the more suitable material in commencing the crystallographic study of organic compounds. In their first paper (*J.C.S. Trans.* 1906, p. 1675) Barlow and Pope have considered at length the crystal structure both of benzene and of a number of its derivatives.

## The Crystalline Structure of Benzene

We have to consider first the data from which the internal structure of the crystalline hydrocarbon is determined, regarding it, of course, as a homogeneous close-packed assemblage of atomic spheres of influence of carbon and hydrogen. The composition of benzene is $C_6H_6$, so there are to be in the corresponding assemblage of spheres equal numbers of spheres of different kinds representing respectively carbon and hydrogen. Further, as the fundamental valencies of carbon and hydrogen are in the ratio 4 : 1, by hypothesis the volume of each carbon sphere of influence is to be taken as four times that of each hydrogen sphere. Again, it must be possible to fashion the structure homogeneously into similarly constituted units having the composition $C_6H_6$. These units must have such a constitution that they conform precisely to the known chemical properties of benzene. This last point, of course, is most important. Among such chemical properties must be noted the possibility of forming but one mono- substitution product of benzene, three isomeric di- substitution products, etc. Also it should be noted that benzene derivatives do not exist in enantiomorphic forms.

Benzene crystallises in the holohedral orthorhombic system, having the axial ratios $a:b:c = 0{\cdot}891 : 1 : 0{\cdot}799$ (Groth). If the equivalence parameters of benzene be calculated, according to the method already given, from these axial ratios, they are found to be $x:y:z = 3{\cdot}101 : 3{\cdot}480 : 2{\cdot}780$, the valency volume being 30. These three numbers represent three translations mutually at

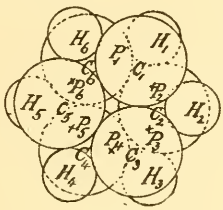



FIG. 1.

right angles in the crystalline structure of benzene. Hence any assemblage of spheres derived to represent crystalline benzene must exhibit holohedral orthorhombic symmetry and at the same time must possess three translations at right angles corresponding in dimensions to the above equivalence parameters.

Such are the data with which the problem has to be solved. The solution at which Barlow and Pope have arrived in the paper referred to is as follows. Six spheres of volume 4 are placed so that their centres lie at the apices of a regular octahedron. Six spheres each of volume 1 are then placed in six of the eight similar hollows lying round the octahedral group in such a manner that the two unoccupied hollows are diametrically opposed. Up to the present then we have a structure (see fig. 1) representing the complex $C_6H_6$ and it remains to pack a number of these together in homogeneous close-packed arrangement to arrive at the structure corresponding in symmetry and dimensions to crystalline benzene. A method of packing in harmony with the data is arrived at as follows:

The complexes are piled in vertical columns in such a manner that the faces which do not carry a small sphere are in contact. The general arrangement in the complexes persists in the resulting structure, each small sphere being in contact with three large ones. The close-packing of the structure is then somewhat increased by compressing the columns slightly and so shortening their height; after this slight distortion the small spheres touch four large ones instead of three, as in the initial arrangement. Figs. 2 (*a*) (*b*) and (*c*) show the resulting structure in plan and elevation. The small and the large spheres alternate regularly in the vertical columns (fig. 2, *c*) and by geometrical partitioning these columns they may be divided into segments each consisting of two consecutive layers, as shown in fig. 2 (*b*). The shortening of the column as described does not lead to any considerable distortion.

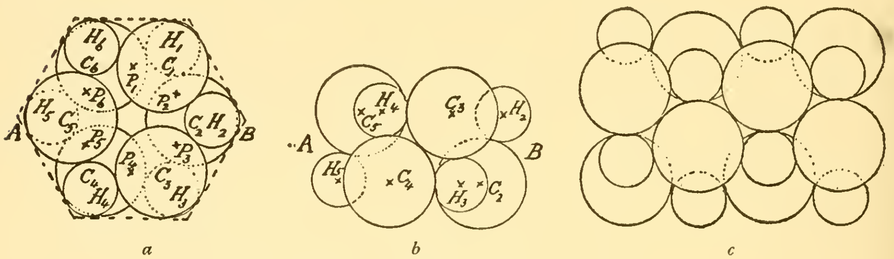



FIG. 2.

Next the vertical columns are placed in contact side by side and it is found that, in order to secure the closest possible packing, half the columns must be raised slightly with respect to the rest; the walls of the columns then interlock. A further slight distortion increases the closeness of the packing and would appear to render it as close as possible—*i.e.* to give a minimum of interstitial space. As already explained, this interstitial space is merely a geometrical feature; no separation of space into two kinds, enclosed and unenclosed, as in a system of spheres, is to be regarded as obtaining in the actual crystal. Indeed, the conditions supposed to prevail are more closely represented if the spheres used are deformable and the assemblage they form is compressed so as to distort them into polyhedra and annul the interstitial space altogether.

The arrangement arrived at is shown in plan in fig. 3. It has now to be shown that this assemblage corresponds in symmetry and dimensions to crystalline benzene and that by homogeneous partitioning of the assemblage units are derived which conform to the known chemical constitution and properties of benzene.

The structure depicted in fig. 3 possesses holohedral orthorhombic symmetry and, moreover, three dimensions can be selected in the structure at right angles corresponding to the equivalence parameters $x : y : z = 3{\cdot}101 : 3{\cdot}480 : 2{\cdot}780$. The $x$ and $y$ dimensions are marked in fig. 3 and the $z$ dimension

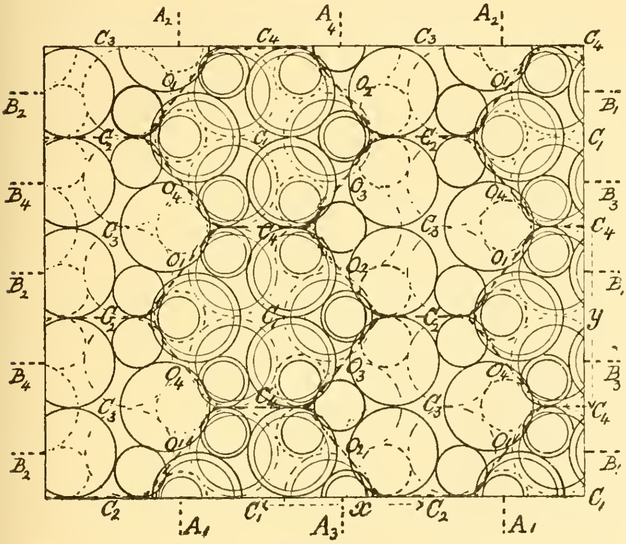



FIG. 3.

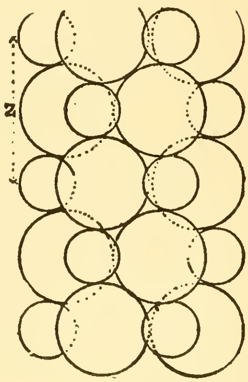



FIG. 4.

shown in fig. 4 is seen to correspond to the height of two layers in the vertical columns. The structure then corresponds geometrically to the known crystalline form of benzene and is so far satisfactory.

The structure can be homogeneously partitioned into units of the composition $C_6H_6$ having the form depicted in fig. 2 ($a$). This partitioning is effected by first breaking up the structure into the vertical columns of molecules already described and then subdividing these columns by a series of horizontal planes. The partitioning by horizontal planes can be accomplished in two ways. Thus if fig. 5 represents a skeleton column in the assemblage, the numerals 1, 2, 3, etc., indicating the hydrogen spheres, the partitioning planes can pass between the layers *a* and *b*, *c* and *d*, *e* and *f*; or alternatively between *b* and *c*, *d* and *e*. The units of two kinds formed are, however, identical.

When, however, some of the spheres representing hydrogen are removed and others introduced in accordance with one of the various cases of chemical substitution, the two kinds of partitioning sometimes produce different isomers, the difference consisting in some cases of an enantiomorphous relation, in others amounting to a more remote isomerism. In regard to cases in which partitioning in the two different ways gives two kinds of units enantiomorphously related, the fact that the

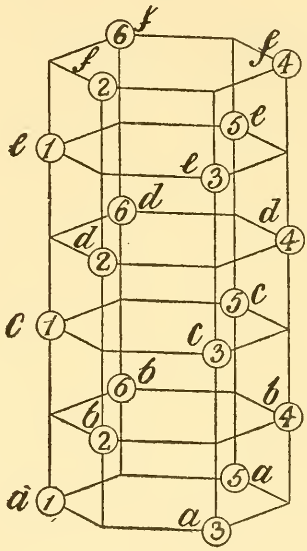



Fig. 5.

existence of this possibility has not yet been experimentally verified may be attributed to the law of chances, since partitioning of both kinds will take place to an equal extent. The resulting enantiomorphous derivatives will be externally compensated. Perhaps some day they will be disentangled and thus their existence established.

To compare the benzene units obtained by partitioning with the usual hexagon formula of the hydrocarbon, we have to remember, in the first place, that but one mono-substitution product can be formed by substitution of one hydrogen atom in each alternate layer of the columns. As to those obtained by further substitution: in fig. 5 the hydrogen atoms are numbered in the same way as in Kekulé's formula. Consideration of this figure will show that three di- derivatives are possible, namely, the 1 : 2, 1 : 3 and 1 : 4 isomerides. If the first substituent be in position 1, then the second may enter into position 3 or 5 in the same layer as the first, forming a metadi-derivative; or it may enter, in the alternate layers, into positions 2 or 6, forming ortho- or into 4 forming para- di-derivatives. We thus see here a distinct geometrical difference between the derivation on the one hand of meta- di- derivatives and on the other hand of ortho- or para- di- derivatives. Supposing that the substituent X displaces the hydrogen atoms $a_1$, $b_4$, $c_3$, $d_6$, $e_5$, $f_2$, etc., then one kind of partitioning leads solely to the production of a 1 : 4 di- derivative and the other kind to the 1 : 2 di- derivative only. This is, of course, completely in accord with the known chemical laws of substitution in the benzene nucleus and gives an entirely novel geometrical explanation of the known simultaneous production of 1 : 2 and 1 : 4 di- derivatives without any appreciable quantity of 1 : 3 isomeride. It may therefore be said with truth that the $C_6H_6$ units arrived at by geometrical partitioning of the assemblage described above conform to the known chemical properties of benzene. And as the structure as a whole agrees in symmetry and dimensions with crystalline benzene, it furnishes us with a very vivid picture of the molecular condition of this hydrocarbon in the solid state.

Before proceeding to discuss the crystalline form of benzene derivatives with respect to that of the parent substance, it is necessary to mention two alternative methods of packing together the benzene units to form a homogeneous close-packed assemblage. The arrangement depicted in fig. 3 possesses orthorhombic symmetry; one alternative arrangement possesses rhombohedral symmetry (*J.C.S. Trans.* 1906, p. 1699), the other is pseudo-cubic (*J.C.S. Trans.* 1910, p. 2380). Only one crystalline modification of benzene is known but of the three assemblages referred to, two, viz. the first and the third, present nearly the same dimensions, so that indeed it is not at present certain which of them is exemplified by the known crystal (cf. *J.C.S. Trans.* 1906, fig. 3, p. 1694, and *ib.* 1910, fig. 57, p. 2386). The reason of this resemblance is that in two respects they are practically alike in that in the direction of the *y* axis strings of large (carbon) spheres in contact are present and that in the direction of the $z$ axis strings are found formed of large and small spheres placed alternately in contact. A considerable number of derivatives crystallise in a form corresponding to one or other of the three theoretically possible crystalline modifications of benzene, as will be shown later.

The first two alternative arrangements of crystalline benzene are conveniently distinguished according to their geometrical derivation. Barlow and Pope show that the arrangement first described corresponding to orthorhombic benzene can be derived from the hexagonal close-packed assemblage of equal spheres. This arrangement is said to possess *hexagonal* marshalling. The arrangement of rhombohedral form can in a similar manner be derived from the cubic close-packed assemblage of equal spheres; the structure is in this case said to possess *rhombohedral* marshalling.

The third variety of arrangement is also derived from the cubic closest-packed arrangement but in a quite distinct manner; it involves a considerable distortion of the benzene unit not however amounting to the remarshalling of its atoms.

The scientific worker is ever on the alert to discover hidden relationships between different objects, whatever may be his sphere of work. To the chemist or the physicist the properties of a substance are not of such great interest in themselves but there is a great fascination in comparing them with those of some other substance, whether related to it or not. In the same way, although it is a great achievement to have solved the problem of the internal structure of crystalline benzene, it is of far more absorbing interest to trace this crystalline form or the basis of it through a whole series of benzene substitution products. Work of this kind, the way having been pointed out by Barlow and Pope, has been carried on by other observers.

It may be inquired, how are the structural relationships between benzene and its substitution products to be recognised? In the first place, we know that the dimensions of the hexagonal mode of marshalling benzene units and of the pseudo-cubic mode are both practically given by $x : y : z = 3{\cdot}101 : 3{\cdot}480 : 2{\cdot}780$. Now it is conceivable that in a substitution product only one or two of these dimensions should be changed and consequently in the dimensions of the derived assemblage we might expect to find one or two of these numbers recurring. The dimensions of the alternative rhombohedral marshalling are not known; but on theoretical grounds the $z$ value should be slightly less than 2·780. In the pseudo-cubic mode of packing the spheres, on the other hand, we may look for pseudo-cubic symmetry, that is to say, two or all three equivalence parameters may be approximately equal. Under these circumstances the axial directions would be so chosen that the usual $z$ value could not appear. Further, the ratio $x : y$ in rhombohedral crystalline benzene would be 0·8660 : 1; and finally the ratios 0·8165 : 1 or 1·414 : 1 might appear in compounds possessing pseudo-cubic marshalling.

## The Crystalline Structure of Benzene Derivatives

It has been shown how relationships are to be recognised between benzene and its crystalline derivatives. Many different types of benzene substitution products have already been examined crystallographically from the point of view of the new theory and many remarkable results have been obtained from the study of equivalence parameters. In some of these compounds the hexagonal benzene marshalling, in some the pseudo-cubic marshalling, is traceable; in many cases one of the equivalence parameters is approximately 2·780, the $z$ parameter of benzene; other compounds show the rhombohedral marshalling and have one parameter slightly less than 2·780, *i.e.* corresponding with the $z$ value characteristic of the (unknown) second alternative benzene assemblage.

Before proceeding to discuss the results which have been obtained, it will be as well to mention one or two difficulties which stand in the way when we wish to compare the crystal constants of different substances. In any crystallographic system except the cubic, the observer has a certain range of choice of axial ratios, the particular axial ratios deduced depending upon the indices ascribed to particular faces appearing on the crystal. The lower the type of symmetry the greater is this freedom of choice, until in the monosymmetric system the interaxial angle $\beta$ becomes a matter of choice and in the anorthic system all three axes are chosen arbitrarily. Now suppose that on an orthorhombic crystal, for instance, two forms $m$ and $n$ occur either of which may be designated $\{111\}$, that is to say, we assume that any face of the crystal form if produced will intercept each axis at its own axial length from the centre. Now if $m$ be made {111}, $n$ becomes {121} and the ratios $a : b$ and $c : b$ have a certain value. But we might equally well have called $n$ {111}, in which case $m$ would be {212} and the values of the two ratios $a : b$ and $c : b$ would become just double what they were before.

If we are to compare the dimensions of the crystal structures of different bodies, it is obvious that we must make certain that the dimensions compared are really comparable. The dimensions we compare are those given by the equivalence parameters and these are calculated from the axial ratios. The axial ratios must therefore be comparable and in order to make them so it frequently becomes necessary to modify the values given by different observers by multiplying them by some simple whole number or fraction. Some objection has been raised to this treatment of axial ratios but from what has been said it will be obvious that some such treatment must sometimes be resorted to.

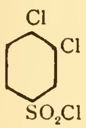
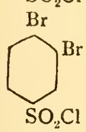
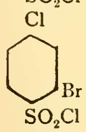
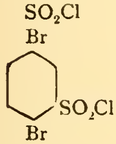
In the case of monosymmetric crystals the choice of the $\beta$ angle offers some difficulty. It is found, however, that most benzene derivatives may be regarded as in some manner derived from orthorhombic benzene, viz. from the first or third form of benzene assemblage, one right angle being destroyed through a shear; it is usual to chose the angle which is nearest to 90° on the zone parallel to the axis of symmetry for the interaxial angle. When this rule is followed the results obtained are generally satisfactory. At present, however, little can be done with anorthic crystals.

The simplest derivatives of benzene are those in which one or more hydrogen atoms have been displaced by other univalent atoms, *e.g.* atoms of halogen elements. In accordance with the principles already put forward halogen derivatives of benzene possess the same valency volume as benzene itself; hence they should exhibit a crystal structure almost identical with that of the parent substance. This is indeed found to be the case. Hexachloro- and hexabromo-benzene exhibit rhombic symmetry, whilst several other halogen-substituted benzenes exhibit rhombohedral symmetry. The following table shows the equivalence parameters of a number of these substances:

| Substance. | $x$ : $y$ : $z$. | $\beta$. | Type of Symmetry. |
|---|---|---|---|
| $C_6H_6$ | 3·101 : 3·480 : 2·780 | 90°0′ | Rhombic. |
| $C_6Cl_6$ | 3·017 : 3·589 : 2·772 | 87°50′ | ,, |
| $C_6Br_6$ | 3·018 : 3·610 : 2·759 | 87°18′ | ,, |
| 1 : 4 $C_6H_4Cl_2$ | 3·131 : 3·683 : 2·646 | 100°25′ | Rhombohedral. |
| 1 : 4 $C_6H_4Br_2$ | 3·227 : 3·597 : 2·611 | 98°11½′ | ,, |
| 1 : 4 $C_6H_4I_2$ | 3·140 : 3·616 : 2·642 | 90°0′ | ,, |
| 1 : 2 : 4 : 5 $C_6H_2Cl_4$ | 3·241 : 3·585 : 2·617 | 99°26′ | ,, |

The general similarity of the dimensions is very noticeable in all the above compounds and the accord of the symmetry with one or other of the three assemblages described; this bears out the hypothesis that the hydrogen and halogen spheres of influence have approximately equal volumes in all these compounds.

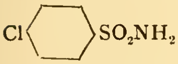
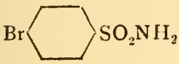
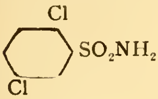
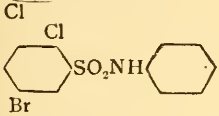
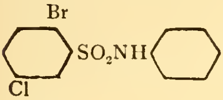
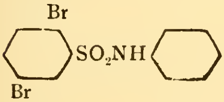
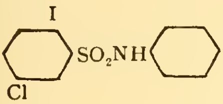
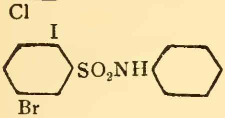
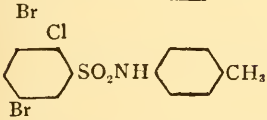
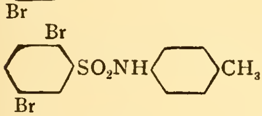
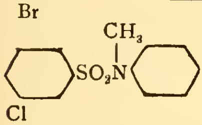
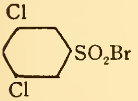
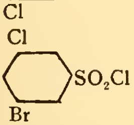
Similar facts have been established through the examination of a large number of halogen-substituted benzene-sulphonic chlorides and bromides. Some thirty of these compounds have been examined in Professor Armstrong's laboratory and the present writers have been able to show that the majority possess the same general crystalline structure whatever be the position of the halogen atoms in the nucleus. Most of them exhibit rhombohedral marshalling, a few are pseudo-cubic, a few rhombic. Not infrequently the connection between the different series of compounds is not immediately obvious from inspection of the axial ratios but when the parameters are calculated, according to the rules already given, the close relationship becomes obvious; typical examples of such cases are included in the following table:

| Type. | | Axial ratios. $a$ : $b$ : $c$. | $\beta$. | Fractions of axial ratios chosen. | Equivalence parameters. $x$ : $y$ : $z$. |
|---|---|---|---|---|---|
| I. |  | 1·633 : 1 : 1·154 | 105°2′ | $\frac{3}{2}a$ | 5·786 : 2·363 : 2·727 |
| II. |  | 1·280 : 1 : 0·576 | 104°0′ | $\frac{b}{2}$ | 5·960 : 2·324 : 2·678 |
| III. |  | 0·813 : 1 : 0·569 | 90°0′ | $3a$, $2c$ | 5·731 : 2·349 : 2·675 |
| IV. |  | 2·476 : 1 : 1·144 | 95°26′ | — | 5·787 : 2·337 : 2·674 |

In the table on the opposite page a series of examples are given showing that very frequently the relationship between allied substances, if not at once apparent in the parameter values deduced directly from the axial ratios, becomes so after but very simple modification, such as, *e.g.* the interchange of $x$ and $y$ in the three sulphonamides. All the compounds included in the table on page 709 exhibit pseudo-cubic marshalling and may be regarded as derived from rhombohedral benzene.

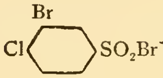
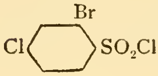
Many of the data from which the above results have been deduced had been known for a long time but publication had been withheld as there appeared to be no point in publishing mere figures. But now, since the advent of the new theory, we are beginning to see how the data which have been accumulated may be applied to the solution of the problem of correlating crystalline form and chemical constitution. One thing is certain from the above results, namely that crystallographically hydrogen and the halogens are approximately equivalent. But not precisely so; and it is hoped that when we see our way more clearly it will be possible to determine the differences in volume between the spheres of atomic influence of hydrogen, chlorine, bromine and iodine which cause the small differences observed between the equivalence parameters of such related substances as those given in the above tables. These differences, be it noted, though small, are beyond the limits of experimental error. Barlow and Pope have already concluded from a study of the crystallographic constants of the trihalogen compounds of the alkali metals that iodine possesses a larger sphere of atomic influence than either bromine or chlorine. However, the mathematical problems involved in the determination of the magnitudes of these differences would appear to be of a high order of difficulty.

The benzenesulphonic chlorides and bromides which have been examined present many cases of polymorphism. In a few cases two forms of one substance have been measured and the results obtained for these bear out the statement made during the discussion of silver iodide that the different crystallographic modifications of a substance are probably very intimately related. In the two following cases it can be seen that there is very little difference between the equivalence parameters of the two forms of each substance and it can

| Compound. | $a : b : c.$ | $\beta.$ | System. | Fractions of axial ratios chosen. | $x : y : z$ |
|---|---|---|---|---|---|
|  | 2·4314 : 1 : 1 | 90°0′ | T | — | 7·380 : 3·035 : 3·035 |
|  | 0·629 : 1 : 0·603 | 97°48′ | M | — | 4·746 : 2·987 : 2·848 |
|  | 0·628 : 1 : 0·601 | 98°56′ | M | — | 4·749 : 2·982 : 2·859 |
|  | 0·423 : 1 : 0·421 | 91°44′ | M | $\frac{2}{3}b$ | 4·642 : 2·943 : 2·929 |
|  | 2·678 : 1 : 1·058 | 98°7′ | M | — | 7·752 : 2·894 : 3·062 |
|  | 2·676 : 1 : 1·047 | 98°30′ | M | — | 7·776 : 2·906 : 3·043 |
|  | 2·658 : 1 : 1·025 | 97°58′ | M | — | 7·793 : 2·932 : 3·005 |
|  | 2·700 : 1 : 0·997 | 97°15′ | M | — | 7·943 : 2·942 : 2·933 |
|  | 2·658 : 1 : 0·979 | 97°18′ | M | — | 7·909 : 2·976 : 2·913 |
|  | 0·767 : 1 : 0·959 | 98°25′ | M | 4$a$ | 9·022 : 2·940 : 2·820 |
|  | 0·764 : 1 : 0·959 | 98°32′ | M | 4$a$ | 9·001 : 2·943 : 2·824 |
|  | 2·694 : 1 : 0·965 | 100°15′ | M | — | 8·269 : 3·070 : 2·962 |
|  | 1·309 : 1 : 0·961 | 90°28′ | M | — | 4·005 : 3·060 : 2·937 |
|  | 1·353 : 1 : 1·018 | 90°51½′ | M | — | 4·015 : 2·968 : 3·021 |
|  | 1·496 : 1 : ·985 | 91°4′ | M | — | 4·569 : 3·054 : 3·010 |

T = Tetragonal. M = Monosymmetric.

well be imagined that a slight shear in one direction might determine the passage of one form into the other:

| Substance. | Axial ratios. $a : b : c$. | $\beta$. | Fractions of axial ratios chosen. | Equivalence parameters. $x : y : z$. |
|---|---|---|---|---|
|   | 1·601 : 1 : 1·141 | 105°29′ | $\frac{3}{2}a$ | 5·736 : 2·389 : 2·726 |
| | 1·293 : 1 : 0·585 | 102°30′ | $\frac{b}{2}$ | 5·949 : 2·300 : 2·694 |
|   | 0·813 : 1 : 0 569 | 90°0′ | $3a$, $2c$ | 5·731 : 2·349 : 2·675 |
| | 0·835 : 1 : 0·563 | 97°27′ | $3a$, $2c$ | 5·872 : 2·343 : 2·639 |

Particularly in the case of the second compound is the similarity between the two modifications brought out. The equivalence parameters of the two forms are almost identical, the only appreciable change involved being the shear of the first orthorhombic form into the monosymmetric form with $\beta=97°27'$.

Many of these dimorphous compounds have their transition point very near to, frequently slightly above, common room temperature. In such cases it is possible to observe directly the change from one form into the other. A crystal of the substance is melted upon a microscope slide beneath a cover slip. On cooling, it crystallises first in the form stable at the higher temperature. After some time, however, a change into the form stable at lower temperatures begins to set in, usually starting at one or two points and spreading gradually, becoming complete perhaps in a few minutes, perhaps only after several hours. The change can be very well followed under the microscope or photographs of the specimen can be made at intervals—a method by which very interesting records of the phenomenon can be obtained.

The morphotropic study of substituted benzenesulphonic acid derivatives has brought to light other relationships of so interesting a character that the writers have been induced to revert to unsubstituted benzenesulphonic acid and accumulate data with respect to derivatives of this acid. Already relationships between derivatives of the substituted and unsubstituted acids are coming to light; a few of these may be hinted at here.

The sulphanilides obtained by the interaction of paradihalogen benzenesulphonic chlorides and aniline are found frequently to be dimorphous. The labile form is evidently derived from the rhombic type of benzene, whilst the stable form exhibits pseudo-cubic symmetry. The following are the data for paradibromobenzenesulphanilide :

| | $a : b : c.$ | $\beta$ | $x : y : z.$ |
|---|---|---|---|
| Labile form . . | 1·372 : 1 : 1·163 | 97°50′ | 7·631 : 2·781 : 3·234 |
| Stable form . . | 1·329 : 1 : 1·025 | 97°58′ | 7·793 : 2·932 : 3·005 |

The close similarity between the equivalence parameters of the two forms is again noticeable. Now when benzenesulphanilide is examined, it is also found to be pseudo-cubic ; in fact it crystallises in the tetragonal system, the constants being :

| | $a : c.$ | $x : y : z.$ |
|---|---|---|
| Benzenesulphanilide . . . | 1 : 2·4314 | 7·380 : 3·035 : 3·035 |

The parameters reveal the close connection between the crystal structures of benzenesulphanilide and the stable form of the dibromo-derivative and again we observe how small is the change involved in substituting hydrogen by bromine.

It may be remarked here that Jerusalem[1] has studied the crystals of a large number of picrates and styphnates and finds that all of them can be regarded as derived crystallographically from one or other form of the benzene assemblage.

From what has been said and from the examples which have been given, it will be obvious that the remarkable work of Barlow and Pope has made it possible to connect together in a wonderful way the crystallographic data concerning derivatives of benzene. Although but a limited amount of work has yet been done in this direction, the results so far obtained indicate that, at any rate in simple derivatives, the crystalline structure of benzene is retained in that of these derivatives modified by the introduction of substituent groups in place of hydrogen atoms. It does not appear that the relative positions of the carbon atoms of the ring suffer displacement ; or in other words, there is no reshuffling of the whole structure in order to accommodate new groups. The number of cases in which the positions of the substituent groups have been completely worked out is of course small ; but then there are few men who can grapple with such involved geometrical problems in the way that Barlow and Pope do.

It should be possible now to solve the problems of the crystalline structure of other ring compounds such as pyridine,

[1] *J.C.S. Trans.* 1909, 1275.

thiophene and of their derivatives, taking benzene as a model. The more difficult task of dealing with open chain carbon compounds has already been undertaken by Barlow and Pope (*Trans.* 1910, p. 2308). It is not possible here to discuss in any detail this most recent work. Starting from the fundamental principles with which the reader is by now familiar, they have deduced assemblages which, they claim, exhibit geometrical peculiarities characteristic respectively of paraffinoid, ethenoid and acetylenic hydrocarbons. Thus certain peculiarities in ethenoid and acetylenic assemblages are taken as representative of the double and triple bonds in these hydrocarbons. Of course there are no crystallographic data available for these hydrocarbons, as they are all liquid or gaseous; however, crystals of their halogen derivatives have been measured and the data afforded by these appear to support the theoretical deductions regarding the hydrocarbons. It will be remembered that hydrogen and the halogen atoms have approximately equal atomic spheres, therefore the displacement of one by the other should produce little morphological change. A cubic marshalling is deduced for methane and in accordance with this, it is known that carbon tetrachloride and tetriodide are cubic whilst the tetrabromide is dimorphous, one form being cubic, the other monosymmetric but pseudocubic. The structures for higher paraffins are characterised by long chains of carbon atoms, each attached to two other carbon atoms and to two hydrogen atoms, the points of attachment on each carbon atom being arranged tetrahedrally, in accordance with the Le Bel-van t'Hoff theory. The carbon atoms in these straight chains are arranged zig-zag fashion; consequently the fourth or fifth atom does not approach the first, as Baeyer supposes in his strain theory, which so admirably explains the formation of $\gamma$ and $\delta$ lactones, etc.

A few words upon the bearing of the results of crystallographic research upon the unique chemical position of carbon may not be out of place here. It may be stated at once that it appears probable that no element except carbon possesses so high a fundamental valency as 4. Chemically, silicon is the nearest relative of carbon but the differences between the two are great, far greater than those between, say, boron and aluminium or nitrogen and phosphorus. Carbon and silicon appear never to be isomorphously interchangeable. It might be expected that carbonates and metasilicates would be isomorphous but they are not. Further, Barlow and Pope have been able to explain the two crystalline forms of silica, namely quartz and tridymite, on the assumption that in the assemblages representing the crystal structure, the volume of the silicon sphere of atomic influence is but slightly larger than that of oxygen, the mean valency volume of all the spheres being 2. Other crystallographic evidence too supports the assumption that the volume of the sphere of atomic influence of silicon is 2, not 4. If then carbon be unique among the elements in having a fundamental valency of 4, it is not surprising that it should be unique in so many other respects.

Sufficient has now been said to give the reader an idea of the progress of chemical crystallography during the last few years. The Barlow-Pope theory is unquestionably a great step forward and its advent should stimulate endeavours to elucidate the relationship undoubtedly existing between chemical constitution and crystalline form.

# REVIEWS

**The Building of the British Isles.** 3rd edition. By A. J. JUKES-BROWNE, B.A., F.R.S., F.G.S. [Pp. xvi + 470, 80 maps and figs.] (London: Edward Stanford, 1911. Price 12*s*. net.)

WE welcome a new edition of Mr. Jukes-Browne's book. It is eighteen years since the previous edition appeared; and so great has been the advancement of our knowledge since then, that the author has "found it necessary to re-write every chapter of the book"; this edition therefore is essentially a new work.

It would be easy to cavil and there are doubtless many utterances in the book to which exception might be taken, yet to lay stress on such utterances without judging of the work as a whole would be most unfair to the author. Every one who has tried knows how difficult it is to attempt restoration of land and sea at different geological periods. The lecturer can give vague indications of coast-lines with a broad sweep of the chalk over the blackboard and these vague lines give truer indications of our present knowledge than continuous fine-drawn lines. But in a book the latter must be drawn and if it be remembered that they are but approximations to what may be regarded as the mean conditions prevalent during a long period of minor fluctuations, no harm can come of it. Mr. Jukes-Browne has given us a series of maps illustrating such mean conditions during different geological periods; the bulk of the work is a luminous description of the data upon which these maps are based, with an account of the general physical conditions which prevailed over the British area during the successive periods.

The book is an honest attempt, after a most careful study of the old and modern literature, to work the countless details of the stratigraphical geology of the area into a connected story; any student who is repelled by the sinister array of apparently meaningless facts will turn with pleasure to this work to find that they are so marshalled that each has its meaning and forms a definite link in the chain which the author has so ably put together; a chain of which the lowest link is fixed to those ancient rules which have been termed "the foundation-stones of the earth's crust," whilst the uppermost marks the rocks which are being formed to-day.

In the introductory chapter, the author explains the principles which must be applied when attempting the restoration of the physical geographies of various past times and indicates the necessary precautions which must be observed in this task.

He applies the term Archæan Rocks to all rocks of pre-Cambrian date—a use of the term which is not generally admitted. He adopts a suggestion of Prof. Bonney that these rocks may be grouped into Eparchæan, Mesarchæan and Protarchæan divisions.

Great caution is exercised by the author in discussing the probable physical conditions of these times. "It is of course impossible to read the history of any period until the succession of the deposits has been ascertained; at present all that can be done is to build up an inferential and theoretical succession on the most probable basis and to indicate the probable history of events in the order thus suggested."

Nor is the author overbold in his attempts at restoration of the conditions of Lower Palæozoic times and it is not until the less highly disturbed rocks of Upper Palæozoic age are considered that he speaks with more certain voice. The chapter on the Carboniferous Period shows the great progress that has been made since the appearance of the last edition and incidentally illustrates the author's careful consideration of the most recent works on the subject.

The term Vralo-Permian Period applied to the time which elapsed between the close of the Carboniferous and that of the Permian Periods seems to us an improvement on the unsatisfactory expression Permo-Carboniferous Period which has been so much used. The latter expression should, strictly speaking, be applied to both Carboniferous and Permian Periods.

The chapter on the Vralo-Permian and Triassic Periods show how fascinating a story can now be woven about a time when the deposits formed in our island have a somewhat monotonous character.

In treating of the Jurassic and Cretaceous Periods, Mr. Jukes-Browne is concerned with a subject of which he is a master. The two chapters which are devoted to the rocks of this period are of very high interest and merit.

In Tertiary times the existing features were gradually coming into shape and in the latter part of the book the work of sea, rivers and land ice in modifying the surface of the ground is clearly indicated.

The style of the book is straightforward and clear ; the maps and diagrams are excellent ; lastly, the value of the work is increased by the admirable reproductions of photographs which occur at frequent intervals.

The work is one which we regard as essential to the student of the Geology of the British Isles, in order that he may understand the significance of the facts recorded in his stratigraphical text-books. Nor should it be neglected by the geographer. It may be regarded as a connecting link between the ordinary stratigraphical text-book and a purely geographical work such as Mr. Mackinder's well-known book on *Britain and the British Seas*. To geologist and geographer alike we cordially recommend it.

JOHN E. MARR.

**The Physiology of Reproduction.** By F. H. A. MARSHALL, M.A., D.SC., with a Preface by PROFESSOR E. A. SCHÄFER, F.R.S., and Contributions by W. CRAMER, D.SC., and J. LOCHHEAD, M.D., etc. [Pp. 706; 154 figures.] (Longmans, Green & Co., 1910. Price 21*s.* net.)

IN the short preface which he contributes to this book, Prof. Schäfer writes, "This is the first time that the Physiology of Reproduction has been presented in a complete form." A reader, meeting this statement in the first lines of the preface, is inclined to suspect the writer of exaggeration ; but as he goes through the book chapter by chapter and finds no diminution in the wealth of matter treated under each heading, he comes to agree that the adjective "complete" is very nearly justified. Absolute completeness in such a subject is impossible of attainment. The author states in the introduction that he has confined himself chiefly to the reproductive phenomena of the Vertebrates, particularly of the Mammals and has not attempted to deal, except very cursorily, with the related phenomena of Variation, Heredity, Cytology, etc. No reasonable person could expect all these subjects to be treated even in the 700 pages of which the volume is composed; as a summary of our knowledge of the reproductive and related processes in the Mammalia the book fully justifies Prof. Schäfer's description.

The first chapter gives a short account of the phenomena of breeding seasons in the various groups of the animal kingdom; and it is concluded that generative activity in its widest sense (including migration, etc.) occurs as a result of definite stimuli, which are partly internal and partly external. It is also suggested that the failure to breed in captivity, which is so often noticed, is possibly due not to adverse influences acting on the generative organs but to the absence of the needful external stimuli.

The œstrous cycle in the Mammalia is next discussed; the meaning of "Œstrus," "Proœstrum," "Metœstrum" and so forth is carefully defined and the various types of œstrous cycle in the different orders of mammals are described. It is shown how the cycle may be affected by environment, so that domestic animals often differ in this respect from their wild progenitors. The relation of the œstrous cycle in the lower mammals to the menstrual cycle of the human species is discussed; and in the succeeding chapter, after fully describing the changes undergone by the uterus in the various phases of the cycle in man and various mammals, it is concluded that menstruation is homologous in all essential respects with the proœstrum of the lower mammals.

The full discussion of the purpose of the proœstral and menstrual changes is again postponed until the phenomena of oogenesis, ovulation, formation of corpus luteum, etc., have been described; it is then concluded that the most probable hypothesis is that the object of these changes is a preparation of the uterus for the reception of the fertilised ovum—though exactly why such extensive preparation should be necessary is not quite clear. Mammalian spermatogenesis, the structure and movements of spermatozoa and natural and artificial insemination are considered, after which a chapter is devoted to the phenomena of fertilisation, including a short account of various theories on the subject and of telegony and artificial parthenogenesis. The Mendelian Theory is outlined and some account given of the possible rôle in heredity of the nucleus and chromosomes; Verworn's objection to the Mendelian conception of unit characters is quoted with approval, on the ground that it is inconsistent with the intimate organic correlation so familiar to the physiologist and "is too morphologically conceived." To the present writer it appears that this objection is valid not against the Mendelian conception in its proper sense but against the assumption, too often carelessly expressed, that a particular "factor" is borne by a particular morphological unit; if such a unit exists (in a chromosome or elsewhere) it must exist in relation to the rest of the cell or organism and any change in that relation results in the change from one Mendelian "character" to another. Interesting suggestions for experiment on selective fertilisation are also given under this head.

A detailed description follows of the accessory male reproductive organs and glands and of the nervous mechanism of insemination, succeeded by a chapter on the Biochemistry of the Reproductive Organs contributed by Dr. Cramer. One of the most valuable chapters in the book deals with the ovary and testes as organs of internal secretion, showing how they are correlated with secondary sexual characters and with the ductless glands and proving that the secretion of the follicular or interstitial cells of the ovary provides the stimulus for the proœstral or menstrual changes in the uterus. The origin and structure of the corpus luteum has been described in a previous chapter; its function in maintaining fœtal nutrition is here fully discussed. The two long chapters by Dr. Lochhead on Fœtal Nutrition and on Changes in the Maternal Organism during Pregnancy provide more difficult reading than the rest of the book and one leaves them with the impression that there is a perhaps unnecessary use of technical terms.

Space will not permit more than passing reference to the remaining chapters dealing with Parturition, Lactation, Fertility, the quite excellent summary of facts and hypotheses on the vexed question of sex-determination and the short concluding account of phases in the life of the individual. It can only be said that these are fully up to the standard of the earlier part of the book. Mention must not be omitted of the excellent illustrations, the thoroughness of the references given in footnotes and last but not least, the well-chosen quotations in many languages at the heads of the chapters.

It is inevitable that in such a book some mistakes and misprints should occur but they seem on the whole few and unimportant. A few misprints occur in specific names, *e.g.* those of *Polypterus* on p. 17; the account of the chromosomes in polar body formation, while correct in general, is misleading in detail (p. 130); it is unfortunate that in the discussion on sex the author has overlooked the papers on *Hydatina* of Whitney and Shull (if indeed the latter appeared in time to be included), which would have made his account of this animal complete. The mention of such small defects only emphasises the general excellence of the book, which includes accounts of the most modern work in a surprising variety of fields.

L. DONCASTER.

**Plant Life in Alpine Switzerland.** By E. A. NEWELL ARBER, M.A. [Pp. 355.] (London: John Murray, 1910. Price 7*s*. 6*d*. net.)

TO write a popular scientific book that shall really interest the general public and at the same time shall satisfy the scientific man is a task of such difficulty that works of the kind are among the rarities in modern English literature. In electing to write on Swiss Alpine Plants, Mr. Arber has chosen a subject excellently adapted for popular treatment, for these plants offer many interesting features and problems that may be observed and appreciated by the numerous holiday-seeking visitors to Switzerland. Moreover, there was definite room for an English work on the natural history of alpine vegetation. So far as popular interest is concerned, it may at once be stated that Mr. Arber has succeeded in writing a book which will not only interest those who care to examine the particular species described but may also induce climbers to do more than merely glance with admiration at the flowers around them. Observation is also encouraged and facilitated by the truly excellent photographs that liberally illustrate the text, as well as by the arrangement of the subject-matter—for the author largely groups the Swiss alpine plants according to habitat. But these merits in encouraging observation are hardly balanced by equal ones conducing to clear thinking and reasoning. For when Mr. Arber attempts the scientific *rationale* of the form and working of alpine plants he occasionally falls into inadequacy or inaccuracy of statement. Among such statements are: "The leaves of all members of the Heath family (*Ericaceæ*) are evergreen"; ". . . Conifers do not bear flowers." Misleading, too, is the assertion that "With alpine plants the tendency to excessive transpiration in summer is very marked owing to the high day temperature and the dryness of the air." It should be pointed out that the temperature of the air is low and that the relative humidity of the atmosphere at alpine altitudes is not low but fluctuating; and the causes promoting excessive transpiration are rarefaction of the air coupled with intense solar radiation, *periods* of extreme atmospheric dryness and winds. Moreover, attention should be directed to the retardation of water-absorption associated with coldness of the soil during night-time and in the shade during day-time. Again, in treating of the special habitats of alpine plants, Mr. Arber writes: "It may be assumed that such species are specially suited to the particular conditions under which they live and are not adapted to circumstances which prevail elsewhere." Apart from the vague use of the word "adapted," it is not pointed out that one most important factor included under the head of "circumstances" is competition and that many extreme alpine species can be successfully cultivated in the lowlands, whilst some naturally grow near the seashore. Throughout the book "adaptation" is somewhat too freely invoked as a mystic *deus ex machina* by which to explain features whose nearer cause lies in direct action of the environment or in untested or unknown influences. A climax is reached when the author writes: "It is conjectured that the earliest primitive flowers were wind pollinated, but owing to the probability that they were soon visited by insects, who robbed them of their pollen, the plants appear to have determined that, if they must suffer robbery, they might as well make use of the insects in some way, and consequently they hit on the happy idea of making them the pollen distributors. So successful was the move in this direction, that it quickly became 'fashionable' to adapt the flowers especially to insect visitors." Such anthropomorphic misrepresentation cannot be too strongly condemned and is somewhat of a shock when coming from a competent scientific author, for it is not passively unscientific but is actively anti-scientific. The hope may be expressed that a new edition of the book will soon be called for and that the author will then so retouch the text as to make it match the admirably selected and executed illustrations and the general soundness of the subject-matter.

**History of the Theories of Æther and Electricity.** By E. T. WHITTAKER, F.R.S. [Pp. 475 + xiii.] Dublin University Press Series. (London: Longmans, Green & Co., 1910. Price 12*s.* 6*d.* net.)

THE beginning of the twentieth century, witnessing, as it has, the passing away of Stokes and Kelvin, the last of the older school of great physicists and the rise and rapid development of a new school under the work and teachings of J. J. Thomson and his pupils, forms a fitting time to review the theories by which attempts have been made to explain the phenomena connected with light and electricity. Until the seventeenth century, the only influence which was known to be capable of passing from star to star was that of light. Newton added to this the force of gravity and since his time, the power of communicating across vacuous space has been shown to be possessed by electric and magnetic attractions. The vehicle of this conveyance of energy is the *æther*, occupying all but that infinitesimal fraction of space which is occupied by ordinary matter. Hence arises the problem as to the relation between æther and matter, a problem not yet completely solved. The history of this problem, beginning with Descartes in the earlier half of the seventeenth century and ending with a brief account of the latest work of the Cambridge school, has been admirably traced in the book under notice and the author is to be congratulated highly on the painstaking manner in which he has compiled a very interesting work.

The first few chapters are devoted to an account of the early discoveries in Light, Electricity and Magnetism, followed by a chapter on the theories of the æther in which its properties are compared with those of an elastic solid. A description of the epoch-making discoveries of Faraday and the work of the mathematical electricians of the middle of the nineteenth century leads up to Maxwell's Electromagnetic Theory and its elaboration by later workers. A perfectly continuous narrative could not be expected in a subject consisting of at least three different branches but by following up one branch through a definite period of development and then returning to another, the author has produced a very readable book, which preserves the interest throughout. The controversies which have raged about opposing theories, such as the one-fluid and two-fluid theories of electricity and the corpuscular and wave theories of light and the crucial experiments devised to decide between them are told in a clear and concise manner. Due importance is given to such fundamental questions as the existence of a pressure due to light, the direction of the vibrations of æther particles in a polarised beam relative to the plane of polarisation, anomalous dispersion and the motion of the æther, their influence on the construction of different theories being carefully pointed out. No statement is made or opinion attributed without quotation or full reference, a feature to be highly commended and the whole forms a review of this department of physical science which will receive a welcome, not from the historical standpoint only but from the point of view of those interested in further developments of the subject. As one reads of the early wave theory of light abandoned for a century owing to its imperfections and yet so much nearer the truth than the accepted corpuscular theory or as one reads of Bernoulli, who in 1736 forestalled some of the ideas of Maxwell, one is compelled to wonder whether any of the dead and abandoned theories of the past will yet rise again as our knowledge advances and, corrected, displace those we now accept. Certain it is that no claim can be made for any theory, including those now generally accepted, that it is a complete explanation of the known experimental facts.

One suggestion only we have to make. The value of the book would have been enhanced if the author had concluded his detailed history with a short summary, free from the theory and proofs previously fully dealt with, giving our *present* conceptions of the æther and electricity, with a statement of the points on which they are imperfect. Such a crystallisation of present opinion may give a misleading appearance of finality but it is preferable to a diffuse ending in which accounts of the present state of thought on different phenomena are scattered over many pages; an ending which is not apt to complete that feeling of enjoyment with which a reader should lay down a work of this character. Apart from this minor point, the book is one in all respects to be commended and will form a valuable addition to our scanty literature on the History of Physics.

H. MOSS.

**Induced Cell-Division and Cancer.** The Isolation of the Chemical Causes of Normal and Augmented Asymetrical Human Cell-Division. By HUGH CAMPBELL ROSS, M.R.C.S., L.R.C.P. [Pp. 291.] (London: John Murray, 1910. Price 12*s*. net.)

MR. H. C. ROSS claims to have discovered that practically the whole of the existing descriptions of the phenomena connected with the multiplication of animal cells are misinterpretations of facts, due to faulty methods of investigation.

He believes that he has proved that cell-proliferation is normally due to certain chemical substances which he has isolated and consequently that he is able to cause and control cell-division. These, with the causes of cancer and healing, are among the most important of his numerous claims to discoveries.

The author begins by assuming that no selective power exists in living cells with regard to the absorption of dissolved substances from the solution in which they are immersed. The whole process of absorption by living cells is, according to him, governed by the simple laws of fluid diffusion. Basing the whole of his work upon this fundamental fallacy, he goes on to describe what he calls the "coefficient of diffusion" of different classes of living cells. For the purpose of determining this "coefficient" he evolves an equation which decides the composition of the medium in which the cells, almost invariably leucocytes, are placed for microscopical observation. This medium is an agar jelly containing certain arbitrary "units" of stain, salts, alkali and, as far as can be ascertained from the text, of time and heat also. Having postulated that the penetration of the stain into the leucocytes depends directly or inversely upon each of the other units contained in the jelly, instead of multiplying together the values of those factors to which diffusion is directly proportional and dividing by those to which it is inversely proportional, the author proceeds by the more easy mathematical process of addition and subtraction. In attributing to any one so unprecedented a method as adding together such dissimilar quantities as cubic-centimeters of solutions, degrees of temperature and minutes of time, it is only fair to him to quote enough to show that no mistake has been made. In the equation given $cf$ represents "coefficient of diffusion."

$$\text{“}cf = (5\overset{\text{stain}}{s} + 3\overset{\text{alkali}}{a} + 3\overset{\text{heat}}{h} + 2\overset{\text{time}}{t}) - (c + 2\overset{\text{salts}}{n})\text{.”}$$

"A letter by itself means one unit of a factor; a number before a letter means that there is that number of units of the factor: $c$ means a unit of sodium citrate, $3c$ would mean three units of it, and so on" (p. 58). "The unit of heat is 5° C.; unity is 10° C., because one cannot conveniently work at a temperature below this point; 20° C. is three units, etc." (p. 60). "One unit of time is ten minutes" (p. 60). "The unit of alkali is 0·1 cc. of a 3 per cent. solution of sodium-bicarbonate" (p. 60).

This is reminiscent of the schoolboy, who, when told that he could not add 6 pecks of carrots to 4 lb. of butter, replied, "Perhaps not; but I *know* that I can take 4 quarts of milk from 1 cow."

It is possible, however, that a man, even holding Mr. Ross's views with regard to diffusion and whose mathematical attainments were less than his, might make observations of considerable value upon living cells.

Mr. Ross says "there is no doubt that the observation of the living cell is a new study" (p. 10). Similar statements to this effect are frequent and emphatic in his book. Unfortunately for him, cytologists know that the study of the living cell is almost as old as the cell theory itself. He makes it very clear that the careful search he says he has made in the literature of his subject has been a most unsuccessful one, for he is obviously ignorant of elementary facts which have been the common property, not only of cytologists but of zoologists and botanists for many years. That he should proclaim as something new, demonstrated by his methods, that the nucleus is not flat but more or less spherical (p. 10), is in itself sufficient to illustrate his very slight acquaintance with the literature relating to cells. It is also clear that he has never seen a properly prepared cytological specimen under appropriate conditions, for he says, "when a cell is stained and fixed, it appears as if the nucleus is a mass of chromatin" (p. 149). His wholesale condemnation of fixed and stained specimens shows that he is not aware that the effects of reagents have been constantly in the minds of observers and that observations upon such specimens have frequently been checked by comparisons with the living cells. Further examples are unnecessary to show that Mr. Ross has not taken sufficient trouble in finding out what had been done before he began to make investigations upon his own account. He has also either read carelessly or failed to understand the few authors whose writings he quotes, even with regard to cancer. He quotes Farmer, Moore and Walker on more than one occasion. According to him, the great point made by these authors was that "asymetrical maiotic mitoses" were found in cancer cells (p. 135). Apart from any consideration of the observations of Farmer, Moore and Walker, their whole point was the occurrence of "symetrical" reduced mitoses in cancer cells, in addition to the asymetrical divisions previously described by Klebs, von Hansemann and Galeotti. The author defines a "mutation," emphasising the fact that the word is used in the technical sense, as "an acquired characteristic suddenly becoming hereditary for all succeeding generations" (p. 245), the context showing that he uses "acquired" as opposed to inborn. Such mistakes are almost as numerous as his quotations.

Mr. Ross makes general statements with regard to cancer which, though they agree with his theory, will not be readily accepted. For instance, he says that scirrhus of the breast is commoner in women who have had children than in those who have not. Statistics show exactly the contrary. The only recognisable illustration given as a section of a malignant growth is evidently chronically inflamed skin, and he has labelled it scirrhus of the breast.

It is evident, therefore, that though Mr. Ross began his investigations with an open mind, untrammelled by any preconceived ideas, he was also at a considerable disadvantage. The gamekeeper can see a hen-pheasant sitting on her nest which is invisible to the town-bred man with equally good eyesight. So it is with all kinds of observations and though Mr. Ross deserves credit for finding out for himself that the nucleus is spherical and not flat, his observations cannot be accepted as readily as those of men whose previous observations have been confirmed by trained investigators. That he did not know how to use his microscope to the best advantage is suggested by the fact that he employed an eye-piece unsuitable for use with an apochromatic objective. This also accounts for the badness of his photographs.

To put it briefly, Mr. Ross has produced certain changes in dying leucocytes which he interprets as mitoses. Granules in the cytoplasm of the cells which he identifies as "Altmann's granules," he says form the chromosomes. This is not the place to discuss what name should be applied to these cytoplasmic structures. Whether they are called Ehrlich's or Altmann's granules or microsomes, chondriosomes or mitochondria, is immaterial—it is quite certain that they are not what Mr. Ross says they are, for they can be demonstrated in a dividing cell with its full complement of chromosomes. Under the conditions of the author's experiments the leucocytes are placed in media which vary to a greater or lesser degree from the isotonic and this in itself would be sufficient to account for the crowding together of the granules, for alterations in the shapes of the cells and of their nuclei and for other appearances which would surprise the untrained observer. Complications are added through the presence of poisons and through the variations which must occur in the conditions of osmosis in the cells themselves as they die. The technique is full of sources of error, almost as serious as those involved by the equation upon which it depends and so it is difficult to speculate as to what he really has seen. The combinations of agar, stain and various other substances used by the author are original, though films of agar jelly have been used for ten or fifteen years, polychrome methylene blue has long been a favourite stain for living cells and the effects of poisons and other substances upon living cells have been studied by a large number of men for more than twenty years. While it is of course impossible to state definitely that mitoses have not been produced by Mr. Ross's method, it is extraordinarily improbable that they have. Certainly there is nothing in the book which suggests that he has ever seen a mitotic figure and cytologists will be convinced of the inaccuracy of his observations and his interpretations thereof by his statements that "the Altmann's granules form the chromosomes, the nucleolus the centrosomes and the nucleus forms the spindle" (p. 166), "that the so-called nuclei of leucocytes ought, we think, in reality always to be called the centrosomes and the word nucleus deleted from their morphology" (p. 13). The sub-title of his book involves the belief in normal asymetrical mitoses. It is common knowledge that normal mitoses are symetrical.

Cytologists will not accept his discoveries as easily and carelessly as he attributes opinions to them, for what he states as the generally accepted opinions with regard to mitosis and reduction are at best but a caricature of the truth.

C. E. W.

**Modern Methods of Water Purification.** By John Don, F.I.C., A.M.I.Mech.E., and John Chisholm, A.M.I.Mech.E., engineer and manager of the Airdrie, Coatbridge and District Waterworks. [Pp. xiv + 368; 98 illustrations.] (London: Edward Arnold, 1911. Price 15*s.* net.)

The success of Mr. Don's well-known essay at the Institution of Mechanical Engineers on "The Filtration and Purification of Water Supplies" has led him to bring out, in collaboration with Mr. Chisholm, the present treatise on "the most efficient appliances for the treatment of water, with details of working and costs of construction and management," subjects which are here admirably described and fully illustrated. Particularly interesting also are the sections on the rationale of sand-filtration—Dunbar's theory of absorption or *ad*sorption, Kemna's (not *Kenna*) and Pennink's views, and the scattered paragraphs on non-submerged filters (pp. 67, 77, 85, etc.). Whilst not disputing the frequent usefulness of the top "filtering skin," the authors point out its many faults as distinguished from the action of the slimy coating on the lower sand grains which entangles organisms. A summary is given of Dr. Houston's researches on the "safety change" of river waters under storage; and it is remarked that "theoretically the water is stored for about fifteen days at Chelsea and Lambeth and for ninety-five days at Staines; but it can easily be understood that the water which is brought to the filter-beds may have been impounded for very different periods, according to the exigencies of supply and demand." We must not forget that it was just this urgency of supply that in 1866 caused unpurified water, contaminated with cholera by one immigrant family, to be distributed in East London, occasioning 16,000 cases and 6,000 deaths. The precautions for general efficiency in filtration are dealt with at length in this volume; but it is recognised that a rapid process for the destruction of pathogenic organisms is safer and can now be made economical. Ozone treatment is rather too diffusely discussed in a chapter of twenty-nine pages, whilst chlorine is imperfectly dealt with on three pages (198-200), with a short reference on p. 47. Household purification has only three and a half pages of text, in a chapter containing a good description of water-softening machines, including Dr. Ganz's "Permutit" process.

A review of bacterial and chemical testing is given in Chap. X. (pp. 260-285) and Chap. XI. describes conductivity tests, standards of purity and microscopical examination. Chaps. XII. and XIII., on the problems of distribution, treat ably on lead and iron corrosion and solvency, on "Water Hammer" and enter briefly also into special questions of public health and water supply. But we meet with a surprising sentence on p. 348: "To the oft-discussed question whether sewage-polluted water is able to cause typhoid in the absence of the specific germ of the disease, the majority of bacteriologists would probably answer that it is not." In other places the wording is not happy—*e.g.* "the seeds of bodily ailments" (p. 1), "general tone is under par" (p. 3), "beyond the demesne (domain?) of agricultural activity" (p. 8), "traces of the bacillus" (p. 24), "they merit no ban on hygienic grounds" (p. 14), "the consumers of the metropolitan area" (p. 27), "iron is taken up by water with more or less reluctance" (p. 330), "the activity is in full swing" (p. 86). "Water undertakers" (pp. 5, 13, 18, etc.) is inelegant, "farmsteading" is ugly, "defilement" is continually repeated (pp. xiii, 8, 11, 13, 14, 15, 22 and others). We have also too much repetition in the matter and such redundancy as "wells in districts adjacent to the metropolis," instead of "wells near London." "Terrain" (pp. 18, 146) is hardly an English word. In the eighteen pages of description of plankton (pp. 293-310) much irrelevant material is introduced: the figures are excellent but of course can only cover a small portion of the ground. The bibliography repeats the names of works two or three times. Of misprints, there are "prfeerence" (p. 3), "Palæozic" (p. 11), "infectionskh" (p. 17); in fig. 84 two of the reference letters are omitted. Titles of periodicals might have been much abbreviated. The test on p. 24 is inexcusably dangerous, so is the suggestion (p. 42) to *anticipate* development of algæ in deep well waters by adding copper sulphate. It is rather unfortunate to mention Cladothrix as connected with iron difficulties when a footnote says that it thrives in iron-free water (p. 332).

These minor points of criticism derogate very little from the great utility of this work and can be easily amended in the new edition which will probably be demanded early. Useful tables relating to water filtration and distribution and a good index terminate a volume which is full of practical experience.

SAMUEL RIDEAL.

**An Introduction to Zoology.** By ROBERT W. HEGNER. [Pp. xii + 350; 161 figs.] (New York: The Macmillan Company, 1910. Price 8*s*. net.)

THERE are evidences that the teaching of zoology at the present time is undergoing many changes and Dr. Hegner's text-book is a striking instance of departure from the older school. The author claims no originality for his work; nevertheless it must be accorded him, as he has certainly struck off from the beaten track and leads his readers into many fascinating and intensely interesting byways of zoological science.

The title is somewhat misleading, as the subject is practically confined to invertebrate zoology.

Commencing with a general introduction on the phenomena of life, the author passes on to a consideration of the cell and cell theory; although both are brief, they are well written. Nine chapters are devoted to types and a general consideration of the phylum to which each belongs. The mode of treatment is excellent; throughout structure and function, ecology and behaviour are co-ordinated, whilst short accounts of the embryology and bionomics add greatly to the interest.

There is much to be said in favour of even the beginner extending his knowledge beyond the actual "type" and the method advocated by Dr. Hegner strongly commends itself to us. The various "types" are treated as living organisms whose activities are of fundamental importance—hence the morphology, although fully dealt with, is not specially emphasised.

There is an interesting chapter dealing with historical zoology and a final one treating of general considerations of zoological facts and theories. This latter is perhaps the weakest; but we fully realise the difficulties of doing justice to the subject in thirty pages. We welcome the chapter on historical zoology, which might have contained more information even in fewer words.

On the whole, Dr. Hegner's work deserves every praise and zoologists generally will welcome so interesting and lucid an exposition in remarkably small space.

WALTER E. COLLINGE.

**The Geology of Water Supply.** By HORACE B. WOODWARD, F.R.S., F.G.S. [Pp. xii + 339.] (London: Edward Arnold, 1910. Price 7*s*. 6*d*. net.)

IN a long life spent in the service of the Geological Survey, the author has had exceptional opportunities of obtaining information on the difficult subject of the distribution and movements of underground water, knowledge of importance for the health of the community and the prosperity of its industries. That it is still very imperfectly understood, the unexpected irruption of great volumes of water into the sinkings for coal in the south-east of England is sufficient proof. In the present volume we have, so to speak, the contents of the author's capacious note-book placed unreservedly at our disposal, so that it constitutes a mine of wealth for those who are seeking for information. One could wish he could have seen his way to discard some of the technical terms at present in general use, which, embodying as they do the incorrect ideas of the past, only serve to mislead the student. Why, for instance, should we refer to the underground surface of saturation as a 'water table,' when it is well known that it is rarely truly horizontal; or as a 'plane of saturation,' when it is usually distinctly curved? If a short expression is desired, water surface would answer every requirement.

Of especial value is the systematic account of the water-bearing characters of the different formations in this country, which is as detailed and reliable as we expect to be the case when we know it is from the pen of the author of the "Geology of England and Wales."

In his preface the author pays a well-deserved tribute to Mr. Whitaker, to whom we owe so much in this country for our knowledge of the relations of underground water to the geological structure of the rocks.

JOHN W. EVANS.

**Phases of Evolution and Heredity.** By BERRY HART. [Pp. xii + 259.] (London: Rebman Ltd., 1910. Price 5*s*. net.)

TO the person who requires a knowledge of these two all-important scientific problems without wishing to enter into the elaborate and complicated details to be found in the standard works upon these subjects, this book will be of great service. It is written throughout in a style which will claim the interest of the reader.

The first chapter, which deals with Weismannism and Darwinism, is very clearly written and puts forth the views of these two great scientists with a simplicity which cannot fail to please, at the same time with a vividness which will give the reader a good grasp of the fundamental principles.

The chapter on Mendelism is very good so far as it goes but it would have been better had the author devoted some attention to the discussion of the results obtained when dealing with pairs of allelomorphic characters. His amended Mendelian scheme is ingenious and we have not been able to detect any glaring flaws in his argument. In it he gives the results of the crosses in terms of the zygote and puts forth the view that when pure tall peas and pure dwarf peas are crossed zygotes of three kinds appear in $F_1$, viz. (1) One quarter having tallness pure both in the propagative and the somatic part. (2) One quarter having dwarfness pure in the propagative part and tallness in the somatic part. (3) The remaining half with tallness and dwarfness present in the propagative part and only tallness in the somatic part. Hence in $F_1$ the zygotes are all pure talls in the somatic part and the plants are therefore all talls. The quarter with dwarfness pure in the propagative part produce pure dwarfs in $F_2$; the quarter with tallness pure in the propagative part breed pure to tallness and the remaining half behave as in $F_1$.

The author objects to the use of the terms dominant and recessive and substitutes for them the terms "first character" and "second character." This suggestion appears to us to be very good and deserving of serious consideration by Mendelian workers.

He gives very good accounts of Biometry, Mnemism and Mutation and in his Intrinsic Theory of Variation attaches great importance to the reducing division of the chromosomes on the formation of the gametes, urging that when the primitive germ cells are undergoing mitosis the "determinants of the unit characters in them are being arranged according to the Law of Probability" and hence that this is a step in the process of producing variation.

The chapter on Heredity is remarkably good but his definition of it is somewhat lax and it would have been much better if the author had stated that heredity was merely a convenient term for expressing the genetic relation between successive generations.

For the rest of the book we have nothing but praise; the chapters on a Modern Observation Hive and the Evolution of the Honey Bee are remarkably well written and extremely interesting. The book is full of interesting and useful information and should take a place in the front rank of the semi-popular works of this nature.

J. W. CHALONER.

**Arnold's Geological Series**: A Textbook of Geology. By PHILIP LAKE, M.A., F.G.S., and R. H. RASTALL, M.A., F.G.S. [Pp. xvi + 494.] (London: Edward Arnold, 1910. Price 16*s*. net.)

IT is some years since a new textbook of geology has appeared and the present publication, which is the work of two well-known Cambridge teachers, will be examined with interest. In the editor's preface Dr. Marr tells us that physical geology and stratigraphy, the two subjects with which the work deals, should be mastered before the detailed study of rocks and fossils is undertaken. It may be questioned, however, whether this dictum will meet with general acceptance. All will agree that attention should be given first to the operations of the natural forces that are ever moulding and transforming the earth's crust but there seems good reason for postponing serious work at stratigraphy, the crown and end of geological research, till the student has a fair acquaintance with both petrology and palæontology. That stratigraphy is not an elementary department of the science, that can be conveniently yoked with the principles of dynamical geology, is strikingly illustrated by the volume before us ; for it is emphatically not a single textbook which is the joint work of two authors but two distinct textbooks in a single binding.

In the fifteen chapters on physical geology, which are contributed by Mr. Rastall, the treatment of the whole subject is sound but it is especially effective and up to date when it deals with the processes which can now be observed in operation on the surface of the globe ; we may instance the description of the phenomena characteristic of arid conditions, glaciation and volcanic activity. In the more speculative aspects of the subject the author is more open to criticism. In explaining the frequent association of lakes with evidence of glacial action he never alludes to the view which is now widely held that they are in many cases at least the result of differential elevation and depression while the action of running water was suspended by frost or the accumulation of ice. At the same time the different explanations which have been advanced of the occurrence of glacial periods do not receive even a passing allusion. The brief treatment of the igneous rocks is on the whole satisfactory ; but why are the terms "alkaline" and "subalkaline" employed to express the difference between syenite and diorite? The prefix "alkali-" has a recognised use as denoting rocks in which felspars containing an appreciable amount of lime are practically absent and in which the pyroxenes and amphiboles are represented by soda-bearing varieties. One would certainly not understand the expression "alkaline rocks" to include a normal syenite. In concluding the notice of this portion of the book, a word of praise must be given to the excellent reproductions of photographic plates which illustrate in a most effective manner the phenomena described.

The remaining fifteen chapters which are devoted to the stratigraphy of the British Isles are by Mr. Lake, who must have experienced some difficulty in compressing the subject-matter into the two hundred pages which are allotted to him. It is perhaps for this reason that references to foreign geology are as a general rule absent. Even the fragment of France which regularly appears in the south-east corner of the maps showing the extension of the different formations is always left blank, in spite of the opportunity it affords of indicating the continuation of the Wealden anticline across the Channel. We can only hope that this exclusion of foreign stratigraphy points to the early appearance of a work specially devoted to the subject by the same author, who possesses exceptional qualifications for the task.

In stratigraphy, probably more than in any other department of geology, there is room for difference of opinion and it is natural that there should be a number of points in which Mr. Lake is not in agreement with other authorities. We need not be surprised, for instance, that the Ingleton slates, which Mr. Rastall has given us good reason to believe are of pre-Cambrian age, find no place in the chapter dealing with these early rocks or the map in which the localities where they occur are indicated; for the independence of the two authors is firmly impressed upon us in their preface. Sufficient stress is hardly laid on the character of the Tremadocs as passage beds from the Cambrian to the Ordovician, especially in view of the fact that it is only in this country that they are placed in the former instead of the latter. The separation of the Lower Devonian into Dartmouth slates, Meadfoots and Staddon grits, which is now recognised by the Geological Survey, is ignored, while many far less important divisions are elsewhere recorded. The author does not recognise a continental period between the Cretaceous and Eocene, though he admits that the Eocene sea probably "did not extend much beyond the present chalk escarpment." One would have thought that the smallness of the area in which the Thanet sands appear to have been laid down was satisfactory evidence that in early Eocene times the sea was confined within much narrower limits.

A word of protest must be entered here against the author's inclusion of Pleistocene and Recent deposits in the Tertiary, an innovation that does not apparently meet with his colleague's approval, for the latter speaks on p. 217 of "Tertiary and post-Tertiary eruptions." A Quaternary or Anthropozoic period can be justified on the score of both convenience and principle. The post-Pliocene deposits that are open to our observation are as a rule superficial deposits in terrestrial areas and most of a widely different character from those which have been preserved to us from earlier times. The epoch when man began to exercise an important influence on the world's history marks the inauguration of a series of changes as great as those which divide the Palæozoic from the Mesozoic or the latter from the Kainozoic. There is every reason to believe that even during the course of the Pleistocene he was the cause of some striking modifications in the distribution of life upon the globe and this process has since proceeded with ever-increasing rapidity and extent of operation down to the present. With the increase of population, of agriculture and of mining (more especially the exploitation of alluvial deposits), even purely physical changes have become more and more pronounced. The commencement of the glacial period, during which the pre-existing surface accumulations over a large portion of the earth's surface were removed and redistributed directly or indirectly by the action of ice, has been generally adopted as a suitable point at which to draw a dividing line between the Tertiary and Quaternary; there seems no good reason for disturbing this convention and with it the accepted grouping and nomenclature of the later deposits.

These criticisms must not, however, be understood as detracting to any appreciable extent from the value of what is a thoroughly readable and useful account of British stratigraphy.

John W. Evans.

**A Course of Practical Work in Agricultural Chemistry for Senior Students.** By T. B. Wood, M.A. [Pp. 56.] (Cambridge: At the University Press, 1911. Price 2*s*. 6*d*. net.)

In his preface, Prof. Wood states that the subject-matter of this booklet has been used at Cambridge, in the form of type-written sheets, for the last ten years. If that be so, he should have published it long ago, for there is no doubt that its usefulness will be appreciated in a much wider sphere than the Cambridge laboratory.

Every department of practical agricultural chemistry is well represented and it is impossible to quarrel with the material chosen, even though some of the experiments seem hardly up to a standard for "senior students." The greater portion of the course is, rightly, devoted to experiments with soils and manures. The subject-matter here is excellent and well preserves the balance which it is necessary always to maintain between science and its application.

In most respects the arrangement leaves nothing to be desired. It is not obvious, however, why after dealing with the mechanical analysis of the soil in Section II., one should have to wait for Sections XII., XIII. and XIV. before experimenting with sand, clay and humus.

The little book will be found extremely valuable both to students and teachers of agricultural chemistry and it can be cordially recommended.

S. J. M. AULD.

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*The entries in italics refer to reviews of books.*
*The names of the authors of papers are printed in capitals.*

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