SCIENCE PROGRESS

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OXFORD ON THE UPPER GRADE[1]

It is clear that the comprehensive message offered to Oxford by her Chancellor, diplomatically indefinite though it be in most respects, is bound to lead to changes of importance—such a ball having been set rolling will not be stopped until some of the existing asperities have been flattened out and the roads made more serviceable. The physician is so rarely prepared to heal himself that when he is seen to attempt an honest and thorough diagnosis of his ailments, any effort he may make to cure himself is bound to attract not only the sympathy but also the support of interested onlookers. On this occasion the onlookers are more than interested—those who have any eyes at all for the situation are aware that our Empire is most deeply concerned in a proper solution being found of the many serious problems which confront our two ancient Universities. The newer Universities alone cannot carry us to salvation: at present they are dependent, as all things educational in this country are more or less, on Oxford and Cambridge.

A few months ago, the writer ventured to say in presence of a select audience at Cambridge that nothing astonished outsiders like himself more than the masterly inactivity the University (and the sister University also) displayed in taking charge of her own affairs. We all know how a settlement of the Greek question has been rendered impossible, time after time, not merely by internal dissensions but mainly by the conservative

[1] "Oxford on the Up Grade." See *Nature*, June 16, 1904, vol. 70, p. 145. In this article the attempt was made to reproduce the argument of *An Oxford Correspondence of 1903*, by W. Warde Fowler, one of the most charming and valuable statements of opinion on the Oxford system published in recent years; this book should be read together with Prof. Gardner's *Oxford at the Cross Roads*, which is also an illuminating contribution to the discussion of the shortcomings of Oxford. The subject had been dealt with previously in articles which are to be found in the writer's collected papers on *The Teaching of Scientific Method* (Macmillan & Co.), cf. *The Need of General Culture at Oxford and Cambridge*.

action of outsiders—outsiders in the sense that they play no part in the work of the University—although, as a matter of fact, on the last occasion when the abolition of compulsory Greek was considered at Oxford, the proposition was defeated by the Ansonites in Congregation. Lord Curzon appears to have breached the wall at Oxford and the force of example, if not the stings of conscience or the fear that Oxford may steal a march upon her, is leading Cambridge also to organise an attack on the accumulated prejudice of ages. But, unless supports are brought up from all sides, it will be difficult for the party of reform at either University to effect the necessary changes.

The scare caused by the discovery that Germany is building "Dreadnoughts" faster than we are is proof that we are in some slight measure alive to the danger of attack upon our Naval power but, at the same time, it is proof also that our outlook is essentially a narrow one, as the alarmists are alarmed only at the growth of German naval and military power. We fail to understand or even to consider why it is that Germany has progressed in so wonderful a way and is become so dangerous a competitor: we do not see that she owes her present proud position and power primarily to the fact that her Universities are going concerns, whilst ours are not—institutions of higher learning, in touch with the people and mindful of the wants of the people, not wedded to the past and unable if not unwilling to consider the real requirements of the present in any proper manner. No one can assert that Oxford and Cambridge have not done much of late years to give increased facilities to learners—but the spirit of management is not altered; it remains academic and monastic as of yore and they are still little more than "superior boarding-schools."

Lord Milner in his speech at Nottingham in April last hit the nail on the head in saying that—"the potential strength of the Empire is immense but so also are its unreadiness and lack of organisation." This is true generally; not only of military and naval affairs. Huxley spoke to the same effect in 1886 when, in an impassioned letter to *The Times*, he called attention to the serious struggle for existence to which we were committed, ending with the statement "Many circumstances tend to justify the hope that we may hold our own if we are careful to organise victory." We are no better organisers, no more organised, than we were those twenty-three years ago—meanwhile, Germany has done nothing but organise, organise, organise—and against us in effect, whatever the intention may have been. The way in which we continue to cultivate loss of opportunity is maddening to many of us, especially when we see others who certainly are not more able so much more alert than ourselves—the Japanese, for example. But the reasons for this are not far to seek. A friend who has spent the greater part of his life amongst the Japanese told me only recently that when he congratulated some of his Japanese acquaintance on the wonderful success which had attended their efforts in the war against Russia and on the exceptionally clever way in which they had applied European experience, the reply made to him was in effect—"that no particular credit was due to them; they had but studied our methods and learnt the rules of the game we had devised; applying these rules strictly they could not do otherwise than succeed. If we had failed to obtain equally good results, it was because we had not worked according to the rules we had ourselves laid down but had been hampered by prejudices of which they had no cognisance." No doubt this has been the case and is still.

Prejudice or mere precedent seems to guide everything—no proper use is made of the wonderful store of experience we have accumulated. Our lack of outlook may be congenital, it may be that we are over imbued with philanthropic desires and in a sense over civilised or perhaps too democratic: it is certainly no longer true that we are good shopkeepers—in thought at least the good shopkeeper is always in advance of his customers' wants.

Probably one great cause of our difficulty arises from the fact that the bulk of the teaching staff at Oxford and Cambridge consists of men who have never held any post outside and have therefore no conception of what is being done in the world at large. Moreover the schemes under which these Universities are worked are so complex, being the growth of centuries, that no one can master them. Every change has to run the gauntlet of criticism by a series of disconnected bodies and no one person can well watch or undertake the supervision of a change through all its stages. Every opportunity is therefore given to malcontents to block progress. Autocratic action is almost imperative.

As we must look to the Universities to help us at least out of some of our difficulties, Lord Curzon's scheme is of no small importance as a public document. It is clearly the work of a diplomatist and is distinguished by its comprehensive consideration of all shades of University opinion; but from an educational standpoint it cannot well be regarded as otherwise than an incomplete statement of the case: the really important issues are scarcely touched upon. Lord Curzon does not recognise, perhaps it was scarcely to be expected that he would recognise, that the creation of a different intellectual atmosphere is required rather than any mere reorganisation of forces; the future aims and objects of the University are in no wise clearly or even nebulously defined in his document. In point of fact, the complexity of the problem to be solved is far greater than is either indicated or implied in the Chancellor's statement of principles and methods of University reform.

It is the curse of both Oxford and Cambridge that in so far as the Honours students are concerned the system is a prize system—one of downright open competition for place and for advertisement. We stand alone among the nations in this respect. But having given up the Bull-ring, Cock-fighting and Prize-fighting, we might well give up Memory-fighting; having abolished the system of payment by results from the elementary schools, we might well be content to abandon it at our Universities.

The work of the German Universities is carried on without scholarships, without Fellowships—entirely without prizes; it may be added also, without Convocations and entirely under the management of the Professoriate. As Professor Percy Gardner points out in his *Oxford at the Cross Roads*:

"The ultimate idea of the German University education is purely intellectual and scientific. To secure the most consummate masters of knowledge in its various branches and to set these proficients to carry on their own studies to the utmost point and to impart their results and methods to their pupils, such is the business of those who govern the Universities of Germany. . . . It is in the first place intellect which the Universities cherish and foster but intellect in close relation to fact and to reality. . . . The intellectual idea, the intense respect for fact as fact has never (he says) been dominant (in Oxford). . . . The

Universities (Oxford and Cambridge) have, for historic reasons, become closely connected with the public schools and are dominated by the spirit of the public schools. . . . But, unfortunately, the spirit of the public schools is obstinately set against intellect—intellectual things, to put it frankly, are unfashionable."

But our system is not only bad because it is a prize system—the evil influence of examinations is felt in even worse ways. Not only do these discourage all intellectual effort of an unremunerative kind but they establish an entirely false intellectual perspective in the student's mind—their effect is almost uniformly nothing short of demoralising. The attitude engendered is humorously pictured in a postscript to one of the inimitable letters by Edward Slade in *An Oxford Correspondence*:

" It was too late for post when this treatise—*dissertatio de examinationibus*—was finished. Wanting a light book after my exertions, I happened to dip into that evergreen trifle, *Vice Versâ*. There I found a remark of that light-headed youth Jolland, which struck me as to the point. He very properly objected to having to learn hymns on Sunday afternoons—but why? Because 'no one ever got marks for them in any exam he ever heard of.' "

The average good student not only does not want to learn hymns or their equivalent—he simply can't afford to do so if he wish to take rank in the final school or tripos.

Our system is an extraordinary one—throughout the undergraduate course, the student is only called upon to acquaint himself with what others have done and to do what others have done: but of such knowledge he must acquire an absolute temporary command to pass well; more often than not, he is trained only dogmatically and in no wise encouraged to be either critical or inquisitive. At the examination, to be successful, he must be able to reproduce what he has learnt with extraordinary skill and fluency. In a few exceptional cases, during an all too brief period of post-graduate study, the attempt is made to teach him to help himself by wet-nursing him through some piece of research work. The majority, however, leave the University without any such training—without any conception how things have been found out in the past and consequently, when it comes to be their turn to teach, they necessarily adopt a purely didactic unimaginative attitude.

An amusing proof of this is given in *An Oxford Corre-*

spondence in a P.S. to a letter to the tutor from his former pupil abroad: "I have been looking into the *Crossways* again. What does he mean when he says every one ought to do a piece of first-hand work? I am so ignorant that this puzzles me. Have you ever done such a thing?" The ingenuous simplicity of the question is delightful albeit truly typical of the average young English University graduate. It is inconceivable that a young German graduate could ask such a question. The tutor's reply, it may be added, is one to be studied with care. Professor Gardner puts the case pretty plainly in saying—"But taking matters as they stand, it can scarcely be denied that the atmosphere produced by the present type of classical study is one in which it is difficult for a spirit of research to live."

The course which has long been followed in the University in Germany is very different and in striking contrast to that we adopt—the period of didactic training is a brief one, the student being encouraged to undertake work for himself at as early a period as possible and to provide the material for a thesis which will give him the right to present himself for examination. At the examination, which is oral and personal, the attempt is made to give credit for what the student knows and can do; our practice unfortunately is one which tends always rather to condemn the examinee *because he does not know* this and that fact, little opportunity being given to find out *what he does know and really can do.* Passing in a creditable manner is too often a mere toss up.

The German system is one which tends to develop some elasticity of mind in the student by giving him a forward outlook and by encouraging him to take an interest in extending the boundaries of knowledge—he becomes of use to the employer: whatever its shortcomings may be, it has brought German commerce and industry to their present commanding position. Ours stands condemned by its failure to give results in any way comparable with those achieved under the German system. Here strangely enough the turned-up trouser hem is become symbolic of the public school and of the University—it may be taken as a clear indication of a tendency to conform to a rigid type and is practically now the registered trade mark of the "cultured" young Philistine.

Lord Curzon's tendency is rather to apologise for and justify the examination system; he has little to say of its many failings; in fact, he scarcely includes it in his scheme of reform. His references to the encouragement of research work are also somewhat bare and bald: the subject is one to which his informants have clearly not done justice and it is not to be expected that he could deal with it himself. It is men such as he, in fact, who clearly are suffering most from the shortcomings of our ancient Universities in this particular.

Unfortunately the examination system is become popular among us not only because it provides bold advertisement for Scholars, for Schools, for Headmasters and for Coaches and often affords satisfaction to fond parents but also because it pays so well—it is now an organised and most remunerative industry at our Universities and will be defended through thick and thin by those interested in its maintenance for purely financial reasons. The late Principal of the University of London, at a public meeting at Cambridge, took exception to the School-leaving-Examination scheme promulgated by the Consultative Committee of the Board of Education, a few years ago, on the ground that it would interfere with the University Matriculation examination—he implied clearly that the University could not afford to see such a scheme come into operation. Other public examining bodies damned the scheme with faint praise in a less open manner, obviously for the same reason. These are some of the difficulties to be encountered in connection with University reform.

What is not sufficiently taken into account is the character of the influence of the examination system on the public service. Our examinations are almost entirely on paper and of a literary character—they therefore encourage the development of a particular class of study and of a particular habit of mind and they lead to the selection of a particular class of individual — a particular type of mind; they do little if anything to encourage the development either of the observing faculties or of reflective and reasoning power or of manual or artistic skill but place a high premium on mere lesson learning. The result has been gradually to fill the public offices with a class of men who, as a rule, are unpractical and too often destitute of individuality; the consequences of so short sighted a policy are becoming more and more obvious and serious every day—it is only too clear that the breadth and depth of understanding which are required to appreciate and cope with the complexities of modern civilisation are often altogether lacking in our officials. And not only in officials—as the higher journalism has also been very largely captured by Oxford, in consequence of the attention which is lavished there on the development of literary smartness and style.

The tendency referred to has undoubtedly been at work in the old Universities and is probably one of the chief causes of their comparative inefficiency. The control of affairs has fallen almost entirely into the hands of the literary class—the more practical and active minded students, whose influence would have been of such infinite importance, have been in no way attracted by the narrow choice of intellectual food which has been offered to them and have only availed themselves of the invaluable social training imparted at the University; these latter have gone out into the world and in virtue of their social qualities have helped us exceedingly but their power for good has necessarily been limited, owing to the narrowness of their education and their consequent inability to understand, let alone master, the problems with which they have been confronted. Our picture was clearly painted long ago by Matthew Arnold, after Heine, in an eloquent passage in which he calls attention to the poet's contempt for *British narrowness.*

"In truth, the English, profoundly as they have modified the old Middle-Age order, great as is the liberty which they have secured for themselves, have in all their changes proceeded, to use a familiar expression, by rule of thumb; what was intolerably inconvenient to them they have suppressed and as they have suppressed it, not because it was irrational but because it was practically inconvenient, they have seldom in suppressing it appealed to reason but always, if possible, to some precedent or form or letter which served as a convenient instrument for their purpose and which saved them from the necessity of recurring to general principles. They have thus become, in a certain sense, of all people the most inaccessible to ideas and the most impatient of them; inaccessible to them, because of their want of familiarity with them; and impatient of them because they have got on so well without them, that they despise those who, not having got on as well as themselves, still make a fuss for what they themselves have done so well without. But there has certainly followed from hence, in this country, somewhat of a general depression of pure intelligence:

Philistia has come to be thought by us the true Land of Promise and it is anything but that; the born lover of ideas, the born hater of commonplaces, must feel in this country, that the sky over his head is of brass and iron."

Since Arnold wrote the sky has hardened into adamant.

Having secured a prominent position at our Universities—first because literary studies were almost the only possible studies not so long ago, more recently, owing to the continued selection of the scholar rather than the man of action through the operation of the examination system—the classical and literary school have, to use Matthew Arnold's expression, suppressed what was intolerably inconvenient to them, viz. modern scientific studies: the times were against them and they have only succeeded in delaying their development but even now, such is their habit of mind, they have no sympathy with them. Men like Matthew Arnold have addressed them in vain—only here and there have they met with any cordiality of reception.

The late distinguished French savant, Mons. Berthelot, whose death took place recently at the advanced age of eighty years, has summed up the present situation in the statement—"La science domine tout: elle rende seule des services définitifs. Nul homme, nulle institution désormais n'aura une autorité durable s'il ne se conforme à ses enseignements."[1]

This, however, is not the position taken up in our Universities—"science" has no place in their entrance requirements and is in no way a necessary part in their curricula. Yet as Professor Gardner remarks in his *Oxford at the Cross Roads*, "The value of method, of organisation—in a word, of science—is every year becoming greater."

The time is come to take action. Lord Curzon publishes the following list of Scholarships and Exhibitions given at Oxford:

	Scholarships.		Exhibitions.	
	Number given yearly.	Total number.	Number given yearly.	Total number.
Classics	75	300	30	120
Mathematics	15½	62	5½	22
Science	14	56	13	52
History	14½	58	6½	26
Other subjects	7	28	2½	10
Total	126	504	57½	230

[1] *Science et Morale*, Paris, 1897.

The disproportion between the encouragement given to classics and to all other subjects is very striking; it is still more striking when the share of classics is contrasted with that of science and the preponderant value of the latter in the modern world is taken into account: if the order of encouragement given to the two subjects were reversed, a reform would be brought about which at no distant date probably would determine a complete change in attitude on the part of the University; the intellectual stimulus derived from a stay at the University would then be of a totally different order from that which the present conditions afford.

It is only necessary to note the difference between Oxford and Cambridge to see that this must be the case. The vigour and activity of the latter in comparison with the former is very striking—and we know the extent to which science is cultivated at Cambridge to be far greater than at Oxford; moreover that she has been largely influenced by active and broad-minded advocates of the value of scientific training, especially by men with ideas, such as the late gifted Sir Michael Foster. Oxford unfortunately has not yet had its Foster—a man knowing his subject thoroughly, enthusiastic on its behalf and loving to help young men. On the other hand, no school at Cambridge appears to be quite so philosophical in its tendency as the *Greats* school at Oxford; if this could be deprived of its one-sidedness, great things might come of it.

In the future, it is to be hoped the student will proceed to Oxford or Cambridge with the object of acquiring ideas and the scientific habit of mind, not mere technical proficiency, whatever the subjects of study he may select; at the same time he will be trained socially through the enjoyment of the opportunities which these Universities provide in such an incomparable manner—opportunities which are not found elsewhere.

It has been said that the function of criticism is "to see the object as in itself it really is." This may be applied to the teaching of science and indeed of all subjects at the Universities—we do not train our students at present in such a way that they learn to see clearly but too often obscure their vision by a mass of blinding detail bereft of nearly all principle; the object is seen not "as in itself it really is" but merely as the teacher chooses to present it, too often only as he thoughtlessly presents it. Hence it is that so much of our modern teaching is sterile.

Matthew Arnold has contended that "in the England of Shakespeare, the poet lived in a current of ideas in the highest degree animating and nourishing to the creative power; society was, in the fullest measure, permeated by fresh thought, intelligent and alive; and this state of things (he says) is the true basis for the creative power's exercise, in this it finds its data, its materials, truly ready for its hand; all the books and reading in the world are only valuable as they are helps to this." As remarked by "Jim" in one of the letters in *An Oxford Correspondence*, "Books may be made for men but I deny that man was made for books"—the corollary to which perhaps is that men made by books alone are not men.

The evolutionary wave which swept over us and permanently changed the direction of thought throughout the civilised world now nearly half a century ago has in a measure spent its vivifying force: to carry it forward again society needs to be made more intelligent and alive, to be permeated by thought, not merely by fresh thought; science, using the term in its broadest sense, alone can accomplish this end and it rests with the Universities to convey the message which science carries. But we must not expect too much. Again to quote Arnold: "The mass of mankind will never have any ardent zeal for seeing things as they are; very inadequate ideas will always satisfy them. On these inadequate ideas reposes and must repose the general practice of the world. That is as much as saying that whoever sets himself to see things as they are will find himself one of a very small circle; but it is only by this small circle resolutely doing its own work that adequate ideas will ever get current at all." The two ancient Universities are the small circles from which the wavelets must ripple outwards.

The growth of adequate ideas, of ideas adequate to the complex conditions of our modern civilisation, should be fostered by them. If they do not soon succeed in discharging their office in a manner more worthy of the position they have inherited from the past, their work can but end in failure and their continued failure must have most serious consequences; therefore the public cannot allow them to fail and must demand their revivification as centres of thought and of ideas.

Nothing is more desirable in the way of University reform than the modification in essential particulars of the conditions on which scholarships are awarded. Many of us feel that they are too often given on insufficient evidence and that scholars are too much an artificial product manufactured to order by the schools.

An entirely false standard is set, owing to the severity of the examinations; as a consequence the work of schools is interfered with most seriously. It is practically impossible for those who are proceeding to the Universities to obtain a satisfactory general education at school, a high degree of specialisation being necessary to insure success in the scholarship examinations. This is particularly the case in classics. The seeds of narrowness are therefore sown at school, under the all-compelling influence of the College scholarship examination; and the University entrance examination has no correcting influence. College and University, in fact, work almost at cross purposes: whilst it should be the object of the University to encourage the general efficiency of schools, the course of action pursued at the Colleges is such as to compel all who compete for scholarships to specialise during the later and more fruitful years of school life; and as the pace of a school is more or less set for its scholars, as the main object in view is to select out those who will eventually be scholars, the College policy has the most unfortunate tendency of preventing the application of the doctrine of the greatest good for the greatest number in very many of our schools. Another point of consequence to be mentioned is that the policy under discussion also has the effect of unduly forcing up the age of entry at the Universities—as only boys of ripened intelligence can compete successfully. The College policy unfortunately serves to give support to the policy favoured by schools of retaining boys as long as possible to "run" the games and to act as monitors and prefects; the result is our boys are kept under the narrowing conditions of school discipline far too long and when they enter the University as young men in age are still boys in habit and mode of thought: having been helped and ordered so long, they have lost much of their individuality and too often are unable to avail themselves properly of the opportunities of freedom which the University offers as they continue to need leading after having been led so long. In discussing the problem, it must not be forgotten that the

American youth, who will be among the most serious competitors of our youth in the future, are forced at an early age to help themselves and that the alertness of American in comparison with English workers has long been matter of comment. In taking command of a situation, when difficulties suddenly arise, the Englishman is probably without his equal—it is his congenital quality; but in seizing an opportunity he seems to be far behind his competitors of other nations—probably our school ways are much to blame for this and one duty of the Universities is to promote reform in this direction. It should, however, be added that the men who have displayed individuality and the faculty of assuming command under exceptional circumstances, as a rule, have not been University scholars but rather men who have escaped its influence on the intellectual side.

At Oxford and Cambridge it would probably be far better if scholarships were greatly reduced in number and the funds were applied in promotion of University teaching—if the University were made so attractive and efficient that it would be the desire of all parents of moderate means to secure a University education for their sons.

Without the assistance of scholarships, large numbers of students are now attending Universities other than Oxford and Cambridge, often with greater advantage and at a cost by no means so very far short of that entailed by residence at the older Universities. Now that lotteries of one kind or another are resorted to so freely by tradesmen of every shade to attract customers, the ancient Universities might well pursue a policy of altruism advisedly in deprecation of the growing selfishness of the age.

Lord Curzon in referring to the proposals that have been made to destroy the prize character of scholarships remarks that, if the course suggested were adopted and exhibitions were confined to poor men, it appears certain that the intellectual standard would deteriorate rapidly and in the long run it would be found that the fairly well-to-do men with some brains had been exchanged for poor men without them.

This argument would imply that brains are only to be had for money by the Universities—a sad confession if true; moreover that brains, as a rule, go with money. There is more truth in the latter conclusion perhaps than appears on the surface, as those who have risen in the social scale, as a rule, have had brains. The writer's experience and it is a considerable one would lead him to conclude that poor scholars are too often those who can be crammed to pass an examination but who are incapable of making any serious effort on their own behalf. The opinion is already gaining ground, apparently, that Mr. Carnegie's countrymen may have occasion to deplore his generosity in establishing scholarships for all and sundry. It is clearly undesirable to multiply unduly the number of those in attendance upon University courses—the practical training of the world is probably a far better discipline for the majority. Serious injury to the community may be done—is done—by placing a superficial veneer of University training upon those who cannot properly avail themselves of their opportunities—such men only degenerate into being inefficient teachers and then become in turn promoters of the failure of those who are committed to their care.

Some better system of selection is required—one which will make it possible to attract not mere learners but active-minded youths capable of being developed into thoughtful useful leaders. It may be questioned whether some form of probationary system should not be tried. To avoid undue specialisation at school, the examination might be of a more fundamental and general character, proper allowance being made for the prospective scholar's proclivities and aptitudes and the recommendations of his teachers. The final award of a scholarship might be made dependent on proof being given during the first year of study of more than ordinary ability in some one or other special direction. Really capable scholars should be allowed to continue their studies until qualified—in many branches of Science, for example, few become of any value as independent workers in less than five years. Such a system would throw great responsibility on teachers—but the office of a teacher should be a responsible one.

Not only the entrance but also the final examinations need reforming: at present they encourage little else but cram and the attitude which was fatal to Lot's wife; they should be modified so as to encourage a forward and independent outlook. And the German example should be followed of trusting the teachers to examine those whom they have taught, so that justice may be done to the students' efforts.

But the pass men and those who seek a general education rather than professional proficiency will after all be those who most deserve attention at Oxford and Cambridge—as they are the men who should go forth persuaded of the value of education, reclaimed from Philistinism, although the majority may not attain to any very high intellectual standard. For such men, everything has to be done—we have not even begun to think out proper methods of educating them. There should be no delay, however, in doing this, as in the future the ancient Universities must be judged according as they succeed or not in dealing with this class of student.

Throughout Lord Curzon's document, there is, to use his own words, an earnest desire to broaden the basis and to raise the intellectual standard of the University at every point. This can only be accomplished if the problem be studied both within and without the University from an educational standpoint. The Universities are not independent corporations but are closely interlocked in all their operations with the schools; but as they occupy the superior position, they must take upon themselves the functions of legislators and lead the schools—as these apparently will never change of their own accord, and at present tend to dominate, owing to the insensate competition among the Colleges, for their best pupils.

While placing the system of training pursued at all our Universities upon a far broader and philosophical basis, we need to widen the conditions of entry, in order that the preliminary instruction given in our schools may be made more practical, more thorough, more consonant with the requirements of the times. From the point of view adopted in this article, the reform of the old Universities can only be effected from within by those engaged in the work: once guided by true ideals they will be successful; until, however, they are dominated by a new current of animating ideas there will be no real progress.[1]

H. E. A.

[1] "Oxford is probably unique of its kind in the world; its many old and characteristically beautiful and well-preserved buildings, with trim grass lawns and handsome trees, are all stately to a degree and very magnificent. It is quite impossible to picture it at home until one has seen it and I now understand the devotion of an Englishman to his University. The system works admirably for the education of 'Gentlemen' but it cannot lead to much in science and it needs an extraordinary interest in science to prevent a Fellow from sinking into indolence."—HELMHOLTZ, in a letter to his wife.

PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES

By W. J. SOLLAS, D.Sc., F.R.S.
Professor of Geology in the University of Oxford

Magdalenian Man and the Eskimo

In caves where the succession of deposits is complete a comparatively thin layer of loam, often not more than twenty to thirty inches in thickness, is all that separates the Magdalenian stage from the underlying Solutrian; yet the change in the general character of the industrial cult is complete. The flint implements are less elaborated, ruder in style and lacking in finish; the elegant Solutrian laurel-leaf points have disappeared, and we meet instead with long thin flakes and splinters which have been converted by a minimum amount of dressing into scrapers, gravers, drills, and other simple tools. It is not to these flints, however, that we must look for the distinctive character of the Magdalenian industry; they still played an important part, not directly as weapons of the chase, but rather as the implements by which those weapons were made. The new kind of material which had come into use—bone, reindeer's horn, and mammoth's ivory — possessing very different properties from flint, and requiring a different kind of workmanship, effected a revolution in the arts. The arms it furnished to the hunter increased in the number and complication of their forms, and new kinds of implements were devised which added to the comforts of daily life. The stimulus of discovery led to rapid progress in the new industry, and the deposits in the caves reveal three stages in its development, succeeding one another in a definite order from the simpler to the more complex: thus as the characteristic of the first stage we have the simple point, of the second the harpoon with a single row of barbs, and of the third the harpoon with two rows of barbs, one on each side.

The simple forms of arrow-head and spear-head which came in with the first stage, but persisted throughout the remainder of the period, are simple cylindrical rods of various dimensions, terminating at one end in a conical point, and at the other in a base for attachment to the shaft. The base is fashioned in several different ways: very commonly by slicing off the head obliquely to its length, so as to afford a surface for making a simple splice with the shaft; sometimes, though almost exclusively in deposits of the first stage, it is excavated by a wedge-shaped fissure, evidently intended to fit on to a shaft with a correspondingly wedge-shaped extremity; in some cases this last relation is reversed and the base forms a solid wedge, which was probably inserted into a slit at the end of the shaft. In a few rare examples the wedge is converted into a tongue by which a square-shouldered joint is produced; there is no better joint, so far as security is concerned, than this, and it is the kind exclusively adopted by the Eskimo and some other hunting tribes at the present day. The union of the head with the shaft was no doubt secured by threads of sinew tightly bound round the joint. Finally there are some simple points with a base which truncates the head transversely; perhaps with a view to providing a loose joint, so that the head might readily break off in the wound, its connection with the shaft being maintained by a separate cord.

Both arrow-heads and spear-heads, especially the latter, are usually adorned with some simple incised design, such as a series of transverse lines, zigzags, or scroll work. This, as Lord Avebury has pointed out in the case of Eskimo weapons, served no doubt as a means of identification. Such marks of ownership are commonly met with on the arrows of existing wild races; they provide a useful arbiter in the settlement of disputes, such as arise from time to time in battle or the chase. In fig. 5, on the right, two Eskimo are represented as quarrelling over the carcase of a walrus which one of them has slain; it is to be hoped that the arrow bears the owner's mark.

Some of the simple points are scored with a deep longitudinal groove, sometimes called the blood-channel; it has been suggested that this may have been intended to carry poison. In this connection it may be mentioned that some of the interior tribes of British North America make use of poisoned arrow-heads. The poisons are of various kinds, that obtained from the fangs of the rattlesnake being the commonest and most deadly.[1]

[1] C. Hill Tout, *British North America*, p. 132: London, 1907.

The simple point presented itself almost ready-made as one of the prongs of the reindeer's horn; the harpoons of the succeeding stages required more elaborate workmanship. The form with uniserial barbs often ends below in a conical point with a flange on one side only, and in some cases two or three of these heads may have been bound together at the end of the shaft to form a bident or trident for spearing fish. In some well-made examples from Castillo, in Santander, a perforation exists near the base—probably intended for a connecting thong (fig. 1).

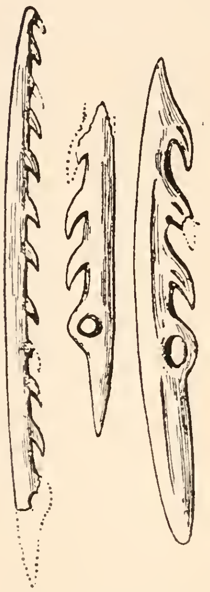

Fig. 1.—Harpoon-heads from Castillo, in Santander, Spain.

The harpoons with biserial barbs take a great variety of forms, and near the base frequently swell out into an annular ridge, or two opposed lobes, before terminating in a blunt cone. This, again, is suggestive of a loose union with the shaft, and in one instance the upper angles, where the lobes spring from the head, are deeply incised as though to afford a notch for a connecting thong. The double-barbed harpoons of the Azilian stage, which succeeds the Magdalenian, are perforated with a fairly large hole, obviously intended for the passage of such a thong.

No bows have been discovered in any Magdalenian deposits; this weapon, if it existed, was almost certainly made of wood. Some of the simple bone-points are of such comparatively small size that they could not have served for spears, and can only be interpreted as arrow-heads.

Whatever doubts may be entertained as to the existence of the bow, there can be none as to the "propulseur" or spear-thrower, an instrument still in use among several wild hunting tribes, including some who at the same time are also in possession of the bow. The spear-thrower reduced to its simplest terms is a stick with a recurved tooth at one end; the spear is laid parallel with the stick, its butt-end resting against the tooth. It is differently held by different races; the Eskimo rest it between the root of the forefinger and thumb, the ends of these digits holding the spear. By a sweeping movement of the wrist and forearm the spear is discharged, and as the fingers close over the handle of the throwing-stick this is swept forwards with great force and rapidity, following and accelerating the spear in its flight (fig. 2). Several Magdalenian spear-throwers have been discovered, chiefly in the caves of Dordogne. They are carved in one piece out of bone or ivory, and adorned with engravings or finely sculptured after some animal form. One of the finest specimens is that represented in fig. 3—a spirited study of the forepart of an antelope in its skilful rendering, its vigour, and its truth. This is a master-

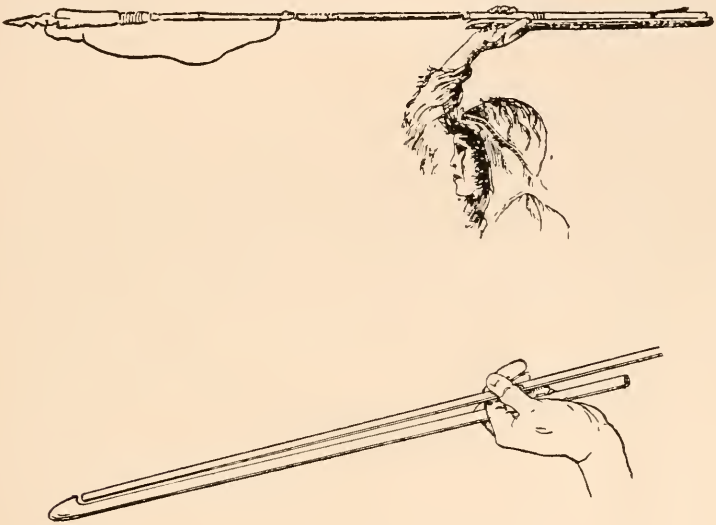

FIG. 2.—The upper figure illustrates the use of the spear-thrower by the Eskimo, the lower one by the Australian.

piece of art: to put it to common use would to our eyes seem nothing less than a desecration.

There are several objects among the Magdalenian bone implements to which it is difficult to assign a use. One of the most interesting of these is the *bâton de commandement*, as it is termed by De Mortillet. In its simplest form this is a rod of reindeer's horn, perforated with one or more cylindrical holes; very commonly it consists of a part of the stem of an antler bearing one of the tines. Sometimes it is carved into a simple symmetrical form devoid of ornament, at others it assumes a more elaborate character, and is adorned with incised designs. In several instances the extremity just beyond the perforation is sculptured to represent two heads adossée, a motive not infrequently met with in primitive art. In one instance, on the other hand, the two heads, in this case of mammoth, are opposed face to face.

De Mortillet's explanation of the *bâton de commandement* is implied in its name, translated "sceptre" by some English writers. One of the commonest forms bears some resemblance to a symbol of office carried by some North American chiefs, and known among them as a *poga magan*, but this always lacks the characteristic perforations. By some authors it has been regarded as a tent-peg, by others as a drum-stick; but perhaps the strangest suggestion of all is due to Dr. Shoetensack,[1] who imagines that it was used as a rude kind of fibula. This view has been hailed by Dr. Klaatsch[2] as a "glucklicher gedanke," and it seems to be widely accepted in Germany. That a people who had achieved such a mastery over the carving of bone and ivory as the Magdalenians, and who showed such a keen sense of the appropriate in art, should have fastened their garments by such a clumsy device seems at least unlikely, and expert hunters would scarcely choose to start on the chase with a piece of bone about a foot and a half long dangling from their necks. The Magdalenians were quite capable of making respectable buckles or fibulæ, but they probably fastened their dress in quite another fashion. A more plausible explanation, as it seems to me, is that proposed by Prof. Boyd Dawkins, who has compared the bâton with the Eskimo's arrow-straightener. For some reason this view has not been very favourably received by anthropologists either at home or abroad,[3] possibly because most of the Eskimo arrow-straighteners exhibited in our museums have been brought

FIG. 3.—Throwing-stick from Mas d'Azil.

[1] O. Schoetensack, "A quoi servaient les 'batons de commandement,'" *L'Anthr.* vol. xii. p. 140, pl. iii. 1901.

[2] H. Klaatsch, *Weltall und Menschheit*, edited by H. Kraemer, vol. ii. Berlin, no date, p. 276.

[3] See M. Hoernes, *Der Diluvial Mensch in Europa*, Brunswick, 1903, p. 72.

from Greenland or other regions where this instrument has obtained its most perfect form and development. Such examples are generally of comparatively small size, skilfully carved out of ivory, and especially distinguished by the form and other characters of the perforation, intended for the insertion of the arrow. It is invariably lozenge-shaped, and, as Mr. H. Balfour points out with just insistence, it passes obliquely through the implement. Both the form and direction of the perforation ensure a good grip of the arrow-shaft, and at the same time in such a manner as to minimise the chances of bruising it during the operation of straightening. In the Magdalenian implement, on the other hand, the hole is always circular or cylindrical, and takes a straight course, at right angles to the two faces. This difference, which impairs to some extent the usefulness of the Magdalenian implement, seemed to me at one time to offer a fatal objection to the identification suggested by Prof. Dawkins; but it now appears that the Greenland form, with which we are most familiar, is not universal among the Eskimo. Boas has figured an example from Baffin Land, in which the hole is cylindrical, and apparently takes a direct and not an oblique course. Between this and the Magdalenian bâtons there is no essential difference; both are arrow-straighteners. There are some other Magdalenian implements perforated by several holes, which I should have regarded as problematical, but for the fact that Boas also describes a piece of bone, similarly perforated, as an arrow-straightener, and expressly mentions that it is provided with several holes of various diameters in adaptation to the various thicknesses of the arrow-shafts.[1]

Although the Greenland arrow-straightener is a much superior instrument to the Magdalenian, yet a remarkable resemblance may sometimes be traced in their decorative form, the heads adossée already referred to as a motive in Magdalenian art being a frequent feature in the Eskimo examples. In both cases also the handle of the straightener is incised with line engravings representing animal forms.

As connected with the chase, we may mention the bone pins not uncommonly met with in Magdalenian deposits. These,

[1] Franz Boas, "The Eskimos of Baffin Land and Hudson Bay," *Bull. Amer. Mus. Nat. Hist.* vol. xv. p. 84, fig. 117, New York, 1901; W. J. Sollas, *Nature*, vol. 74, p. 372, fig., 1906.

though inappropriately thick, are supposed to have served for dress fasteners; but it is extremely unlikely that a people, who were evidently adepts in the art of sewing, would show so great a disregard for valuable skin garments as to drive such rude pegs as these "pins" through them. We shall find a more probable explanation by reference to the Eskimo, who possess similar pins, which they call "taa-poo-ta," and use for skewering together the sides of the wounds inflicted in killing seals or other large animals, with the object of securing the blood, not a drop of which is willingly lost.[1] The Algonkian Indians, who live inland, next to the Eskimo, have the same custom. Occasionally the Eskimo make use of a bone plug instead of the "taa-poo-ta"; it is inserted in the wound as a kind of stopper.[2] An ivory peg figured by Piette from Brassempouey, with the remark "use unknown," may perhaps have served the same purpose.[3]

Whistles made from the phalange of a reindeer, such as in use by North American Indian tribes, have been found in Magdalenian deposits of several caves.

A variety of evidence leads to the conclusion that the clothes of the Magdalenian people were made from the skins of animals killed in the chase; the reindeer probably furnished some of the warmest and most resistant to the weather. That these, after dressing and trimming, were sewn together is suggested by the abundant bone needles which are found strewn through Magdalenian deposits. The needles are remarkably well made, straight and slender, with sharp points and round or elongated eyes. Their variety in size—the length ranging from 37 to 72 mm.—seems to show that the seamstress was particular as to the fineness of her work. In making a needle the first step was to obtain splinters of bone from a reindeer's shoulder-blade, or to cut strips out of the cannon-bone of a horse or deer; these were then scraped into shape with a flint flake, rubbed smooth and pointed by a grooved piece of sandstone, and finally drilled by means of a delicately chipped flint awl. The awl was no doubt mounted in some manner, probably by binding it with sinew on to a rod of wood or bone. In drilling holes for arrow-straighteners a large flint borer was

[1] W. J. Sollas, "On some Eskimo Bone Implements from the East Coast of Greenland," *Journ. Anthr. Inst.* vol. ix. pl. vii. 1880.

[2] F. Boas, *loc. cit.*

[3] Piette, *L'Anthrop.* vol. vi. p. 135, fig. 6.

necessary, and the question arises whether any accessory apparatus was used, such as the bow drill, so common among many primitive people at the present day. The Eskimo use an ivory bow drill, and if a similar implement had been known to the Magdalenian men we might expect to find examples preserved in the cave deposits; none, however, have so far been met with. The bow is not the essential part of the bow drill, but merely a mechanical refinement, ensuring that the bow string is maintained in uniform tension. The string, twisted round the borer, may be employed alone, its ends being held one in each hand and pulled alternately in opposite directions. This simple method of obtaining rotation, still in use among various wild tribes, was possibly first introduced by the Magdalenians; the addition of the bow would then have followed later.

Domestic utensils are not numerous. A shallow bowl, made out of a fine close-grained sandstone, was found in the cave of La Mouthe, Dordogne; it is produced at one side into a kind of shelf, which affords a handle, and the base is engraved with a rough sketch of a wild goat. It has been interpreted as a lamp, and it is certainly not unlike some of the stone lamps used by the Eskimo to warm their winter houses. These lamps give no smoke when carefully tended, and this suggests the question whether they may not have been used to illuminate the dark recesses of those caves which are adorned by mural paintings.

Personal ornaments have been found in great variety. In addition to the teeth of bear, horse, and reindeer, sea-shells, and even fossils, all perforated for suspension, we encounter pendants of various forms carved out of bone or ivory, some of which are of especial interest on account of their precise resemblance to similar ornaments in use among the Eskimo, who attach them to needle-cases, housewife bags, and sometimes as tassels to their dress. Long, thin bone or ivory rods also occur, very carefully shaped and bearing incised designs; they closely resemble in form and ornament the hairpins still in use among the Eskimo. A small broken ornament with little pit-like markings found in the Magdalenian of Kulna,[1] Moravia, recalls some objects of unknown use which the Eskimo women carry attached to their "housewives."

The art of the period is most fully expressed in sculpture

[1] J. Knies, *Časopis muzejního Spolkn v Olomuci*, taf. xiv.

and line engraving. It seems to be generally assumed that some of the later paintings on the walls of caves already referred to really belong to the Magdalenian age, but this is a question which awaits further investigation. The line engravings, sometimes deeply cut, sometimes faintly scratched in, are frequently met with on the sides of bone implements, more rarely on stones; towards the close of the period the designs become conventional and geometric, but the earlier drawings, which are fortunately the most numerous, are faithful delineations of the contemporary animals; one of the earliest discovered and best known is the famous mammoth from the rock shelter of La Madeleine, which has always been regarded with especial interest, not only as an evidently faithful portrait of an extinct animal drawn from life, but as confirming in an unexpected manner the conclusion obtained from other evidence that palæolithic man was familiar with this animal in the living state. None of the characteristic features of the mammoth have escaped the artist's observation: the profile of the head, the great curved tusks, and swinging trunk, the coating of long hair, the little eye and large half-opened mouth, and the peculiar gait indicated by the position of the kneeless hind-legs have all been rendered with convincing truth—so much so that we feel as though an apology were due to the artist when we add that the fidelity of his sketch is confirmed by independent evidence, including that afforded by the complete and well preserved specimens of the mammoth found in the frozen soil of Siberia.

The reindeer is a favourite subject, and has provoked some of the cleverest sketches. The horse, supposed to be Przevalsky's species, is frequently represented, and its frisky colt is drawn in characteristic attitudes. Several studies are known of the bison, and one in particular from Laugerie Basse is of special interest, since it represents, behind a grazing bull, unconscious of impending evil, a Magdalenian hunter, crawling on the ground with a spear in his right hand which he is about to throw. The human figure is not well drawn, so that no trustworthy conclusion can be deduced from it; it shows a large powerful lower jaw with an angular chin, and a curiously peaked roof to the skull; a hatching of simple lines represents the hair of the head, and since similar lines are distributed over the legs and body it has been conjectured that these parts of the body also were hairy. Sketches of several other naked human figures are known—as, for instance, the *femme au renne* from Laugerie basse, which again shows indications of a growth of hair over the thighs and abdomen. A broken arrow-straightener from La Madeleine bears a sketch of a standing human figure, evidently naked; it is diagrammatic, but faithful, and shows a complete absence of any tendency to steatopygy. The profile of a man-like form found at Mas d'Azil is distinguished by such an extraordinary projecting face that Piette thought it might represent an anthropomorphic ape; it has a projecting muzzle not unlike that which we may attribute

FIG. 4.—Reindeer and Salmon incised on a piece of horn from Lorthet.

to Neandertal man, but is without any other features of resemblance. A human face with very oblique eyes cut on a piece of reindeer's horn found in the cave of Rochebertier recalls the faces which figure on the doorposts of some of the houses of the North American Indians. Of the remaining animal forms which find representation we may mention the ibex, seals, geese, trout, pike, and salmon. An admirable drawing of salmon, in various attitudes, lazily disporting themselves in the water, is reproduced in the illustration (fig. 4).

The sculptures in bone and ivory afford some of the finest examples of Magdalenian art; the bone dagger from Laugerie basse, with its lifelike rendering of the reindeer, artistically adapted to form the handle, is a famous example. The same cave has furnished several other daggers; one has the figure of a mammoth for the hilt, another that of some great carnivore. Some of the sculptured figures which have been regarded as the handles of daggers are possibly not of this nature; the Abbé Brueil believes that they were all intended simply and solely as images of the animals they represent, and he is inclined to think that magic influence was attributed to them: there is no doubt a considerable amount of truth in this view, and it may very well apply to a sculptured mammoth found in the cave of Bruniquel, but scarcely to the reindeer hilted dagger of Laugerie basse. · As another instance of an animal form carved without any apparent ulterior purpose may be mentioned the head of a musk ox, found in the Kesserloch, near Thayngen, Switzerland. Most of the sculpture, however, is decorative; as additional instances we may cite a pendant carved with the figure of a Saiga antelope, the same animal as that which is sculptured in so masterly a manner on the spear-thrower mentioned on p. 20 (fig. 3). The apposed heads of bison at the extremity of an arrow-straightener may also be recalled here.

The engravings and carved figures illustrate in a remarkable manner the natural history of the Magdalenian age; and their evidence is in complete harmony with that derived from a study of the associated bones. The fauna includes among others the following: Reindeer, stag (*Cervus elaphus*), the great Irish deer (*Cervus megaceros*), bison, horse, ass, musk-ox (now confined to arctic North America), Saiga antelope (now confined to the steppes of Russia), glutton (now distributed over lands bordering the Arctic Ocean), arctic hare (Alpine and Arctic regions); piping hare (*Lagomys pusillus*, an inhabitant of the Asiatic steppes), lemming (restricted to the northern parts of Europe). It is evidently a mixed fauna, containing some forms characteristic of the steppe, others of the tundra. At an early period in the study of palæolithic remains observers were led by the presence of the cold-loving species of the tundra to look to the Arctic regions for the surviving representatives of reindeer men. Pruner Bey was one of the first to identify the Magdalenians with the Mongolians, though on somewhat insufficient grounds. He was followed by Hamy,[1] who asserted that it is solely among Arctic people, Lapps, Eskimos, and Chukchis, that we find the same customs, weapons, and im-

[1] E. T. Hamy, *Précis de Paléontologie Humaine*, Paris, 1870, p. 366.

plements as those of the Magdalenian age. These races, he remarks, continue down to our own days, in the circumpolar regions, the age of the reindeer as it existed in France, Belgium, and Switzerland.

A similar view was subsequently supported by Prof. Boyd Dawkins,[1] who pointed to the Eskimo as the one race which makes the closest approach to the Magdalenian in the character of its art, implements, and mode of life. Of late years, however, this conclusion has been strongly contested. Laloy remarks: "Cette théorie est absolutement contredit par les faits"[2]; Steensby, the latest writer on the origin of the Eskimo, dismisses it as fantastic and impossible.[3] In face of such conflicting judgments it becomes necessary to examine this question in some detail. If we can find an existing race which may fairly be regarded as the lineal descendants of the Magdalenians, we shall have connected two dissevered ends in human history, thus linking together by a single explanation the fate of one race and the origin of another; but the very consciousness of our desire for continuity must warn us against too facile an acceptance of testimony.

As a useful preliminary to our inquiry we may begin with a brief sketch of the habits and mode of life of the inhabitants of the North American tundra. The belt of barren land which is known as the tundra borders the Arctic Ocean both in the Old World and the New: it supports a scanty vegetation of mosses and lichens, together with a few trees, such as the arctic willow, dwarf birch, and two species of conifers, which are chiefly found in the neighbourhood of lakes and water-courses. Towards the interior the tundra is succeeded by a forest zone characterised by pines and other conifers, but including patches of willow, poplar, and birch. Beyond the forest follows the great prairie or steppe. The men who inhabit these regions are the Red Indians[4] and the

[1] W. Boyd Dawkins, *Cave Hunting*, London, 1874, p. 353 *et seq.*

[2] Laloy, *L'Anthrop.*, 1898, vol. ix. p. 586. This author is mistaken in asserting that in Greenland decoration is confined to lines and points.

[3] H. P. Steensby, *Om Eskimokulturens Oprindelse*, Copenhagen, 1905, pp. 1–219. This work contains a very full bibliography.

[4] A pedantic objection has been raised to the use of this name on the ground that it is applied to a people who are neither Indians nor red: "red," however, is a term with very wide meaning, and there is a good historic reason for "Indian"; the nomenclature is consecrated by usage, and cannot lead to any serious misconception.

Eskimo,[1] both alike members of the Leiotrichi. The Eskimo occupy the Arctic coast from Greenland to Alaska, and even beyond, extending into the Aleutian Islands and the extreme north-east of Asia, as far as Kolyuchin Bay.[2] They number, all told, according to Kurl Hassert's estimate made in 1891, about 40,000 individuals. The Chukchi and Kamchadals, characterised by similar habits and mode of life, but belonging to a different race, are found in Kamtchatka and the north-east extremity of Siberia.

Wherever they occur the Eskimo are distinguished by a remarkable uniformity in bodily characters, habits, implements, language, and mode of life. Yet they have no national unity, and completely realise the anarchic ideal of government; they are without chiefs, and even the "angakok" or medicine-man possesses far less authority or influence than his nearest homologue, the Asiatic shaman. The only differentiation of labour is that between men's work and women's work.

Some fifty dialects have been distinguished in their language, but the most unlike of these, *i.e.* the dialect spoken on the east coast of Greenland and that on the Asiatic side of Behring Strait, do not differ more than, say, English and German. Thalbitzer,[3] the latest writer on the subject, remarks that the Eskimo language, so far as it is known, stands apart from all others. No one has yet succeeded in discovering any language, either in Asia or among the American Indians, which might possibly have been originally related to it.

Their physical characters bear the same testimony, and stamp them as a race apart; their resemblance to the Mongolians, though marked in many respects, is no greater than might be expected to exist between two races which are both included within the Leiotrichi.

The Eskimo are of short stature, the mean height of the Greenlanders being 1,621 mm. Their hair is absolutely black, coarse, and straight like a horse's mane. Their skin is reddish brown in colour; smooth and full to the touch, like a Negro's.

[1] As in the case of many a Scottish clan, the Eskimo owe their name to their enemies, the adjacent Indians: it means "eaters of raw flesh," though as a matter of fact the Eskimo generally cook their food, unless prevented by necessity. Their own name for themselves is Innuit—*i.e.* men.

[2] W. H. Dall, *Journ. R. Geogr. Soc.* vol. iii. p. 568, 1881.

[3] W. Thalbitzer, *A Phonetic Study of the Eskimo Language*, Meddelelser om Grønland, Hefte 31, Copenhagen, 1904.

Their eyes are dark brown ; the orbit is wide and high. The face is long and orthognathous ; the nose both long and narrow : it is indeed the most leptorhine as yet observed. The head is long, high, and wall-sided, with a pent-roof-like summit. The cranial capacity is great ; according to Duckworth, 1,550 cc., thus surpassing some of the most civilised peoples of Europe.[1]

The Indians, who succeed the Eskimo towards the interior, occupy a broad belt of wood and tundra stretching right across the continent ; they are divided into two great races—the Algonkian on the east and the Athapascan on the west. In mode of life there is a considerable amount of resemblance between the Eskimo and these northern Indians ; and some of the Algonkians possess very similar bodily characters, except as regards stature, the Algonkians being a tall people. They are also less dolichocephalic, though towards the east they make a close approach to the Eskimo in this respect.[2]

The other animals which inhabit the tundra and the pine woods are the fox, wolf, bear, and marten ; squirrels, hare, beaver, and beaver-rat ; the bison, which is restricted to the tundra, and never enters the woods ; the mountain sheep, which is found in the Rocky Mountains, the elk or moose and the reindeer. There are also abundant water-fowl, and the waters swarm with fish, especially salmon, sturgeon, pike, and the white fish (*Coregonus albus*). The last named, much esteemed for its fine flavour, contributes largely to the sustenance of the Indians during the winter ; it is the chief food of the Ojibways (Algonkian), who call it the " reindeer of the water."

[1] Brierly, however, from an examination of seventeen skulls found in Greenland, obtained an average of only 1,357 cc. J. Brierly, *Journ. Anthr. Inst.* 1906, vol. xxxvi. p. 120.

[2] The taxonomic position of the American races may be indicated by the following attempt at classification. The Leiotrichi include two groups, one characterised by finer and the other by coarser hair (Deniker, " Essai d'une Classification des Races Humaines," *Bull. Soc. d'Anthr.*, 1889). We will distinguish them as the Leptocomæ and the Pachycomæ. The Pachycomæ may be subdivided into the Mongoloids, with a small and depressed nose, and the Americans, with a large and salient nose. The Americans then fall into the following groups :

Dolichocephalic ; long face ; short stature		Eskimo
Mesaticocephalic ; „ „		Fuegian, Botocado
Brachycephalic ; nose aquiline; tall or medium height		Redskins (the Eastern Algonkians are dolichocephalic)
„ nose straight or upturned	tall	Patagonian
„ nose straight or upturned	short	South American Indians.

The passage of the sun across the equator sets a great part of this animal world in motion. The reindeer, on which the very existence of man depends in these inhospitable regions of the north, leaves the forest belt at about the end of May and travels northward over the tundra in search of fresh vegetation. It marches in herds numbering many thousands of individuals, reaches the margin of the Arctic Ocean just before the winter ice breaks up, and finds a passage over this to the islands lying off the coast, which furnish its most northerly feeding-grounds. There, isolated from the continent after the disappearance of the ice by the open sea, it enjoys the short Arctic summer, and fares well, growing sleek and fat, till on the approach of winter it turns south again, crosses the sea as soon as the surface is covered with fresh ice, and regains its home in the woods. In these annual oscillatory migrations it is exposed to continual danger: wolves are never very far off; from the woods through the tundra the Indian follows the herds as far as the limits set by the Eskimo occupation, or if farther at his own peril; beyond this limit the hunt is continued by the Eskimo himself. There is no close time for the reindeer, but it is more particularly during the return journey, when the animal is in good condition, and accompanied by its newly-foaled young, that its flesh is sought. In the case of the reindeer both Eskimo and Indian pursue the same methods of capture: it is waylaid at spots where its trail crosses a river, or it is driven by noise and alarms in the direction of convergent stone fences, which extend for great distances, and lead to a lake or watercourse, where the hunter waits concealed in his birch-bark canoe or his kayak, ready to dispatch victim after victim with his spear. By this latter method, when the plot is well arranged and the herd not too large, not a single animal will escape. The reindeer flesh is the favourite meat of Indian and Eskimo alike: every part of the animal is eaten, even the contents of the stomach; the blood is boiled, and makes a rich brown soup, greatly esteemed as a dainty; sometimes the half-digested vegetable food from the stomach is mixed with the blood before boiling—a welcome addition in a region where plants edible by man are scarce or altogether absent. The marrow is extracted from the bones, which are then pounded small and the fat boiled out.

The autumn hunting affords a rich store of reindeer meat, which is dried and set aside as provision for the winter. The mode of curing, at least among the Indians, is as follows: The flesh is first cut in thin slices and dried in the sun, or over the smoke of a slow fire. It is then pounded between stones, and finally a quantity of melted fat—about one-third of its bulk—is poured over it. The result is the well-known pemmican. If carefully protected from damp it will keep good for several years. The horns of the animal are used to make fishing-spears and fish-hooks, ice-chisels, and other implements. The skin is carefully dressed, cut into shape, and made up into winter clothing. A shin-bone, split longitudinally, is used as a scraper to remove superfluous hair and fat. The undressed hide furnishes a substitute for rope. It is cut into long strips of various thicknesses, and twisted into thongs for deer-snares, bow-strings, net-lines, fishing-nets, and snow shoes. The tendon of the dorsal muscle is split up into fine threads for sewing. During the absence of the reindeer—*i.e.* for about eight or nine months of the year—the Indians of the tundra live chiefly on white fish, which is caught by hook or net: in winter, when all the lakes and waterways are thickly frozen over, the nets or hooks are introduced through holes broken in the ice.

The Eskimo hunter, while possessing much in common with the Indian, is distinguished by greater aptitude and by special methods of his own. He represents the triumph of human adaptation to the changing conditions of a rigorous climate; by the variety and ingenuity of his implements, weapons, and devices he has brought the art of hunting to its very highest state of differentiation, and in the exercise of this art he stands supreme among all the hunting races of the world.

In summer (July to September), when the sea is open, he lives along the coast, dwelling in tents made of reindeer skin or seal's skin, and hunts the seal with harpoon and bladder from his kayak, using a spear-thrower to hurl the harpoon. In some localities, as at Point Barrow, he also goes a-whaling at this season. The whales migrate towards the north at the beginning of summer, and return about the end of August, moving southwards to the Mackenzie: on the return journey they are attacked from umiaks (large skin-covered boats), containing as many as twelve men, all armed with harpoons. When a whale appears, as many harpoons as possible are cast into it, and endeavours are made to drive it towards the shallow water off the shore. The whale is valued not only for its flesh and blubber, but for a variety of useful purposes: threads of "whalebone" are used for making nets, its jaws serve as runners for sledges, and, when wood is scarce, its ribs are used for rafters or tent-poles. Fishing is also carried on in the inland waters, chiefly by children, women, and old men: the fish are taken by hooks, nets, and barbed spears or harpoons. In dangerous places, such as rapids or whirlpools, the sport requires great skill and nerve, and is undertaken by able-bodied hunters. Birds are shot with a fowling spear, or captured by a kind of miniature bolas; their eggs are collected by the children.

In autumn (August and September), when the reindeer are on the homeward road, the best hunting of the year begins, and a heavy tax is levied on these animals, to provide not only for present eating, but also a sufficient store for the winter season. Salmon fishing is also actively pursued, and large quantities of these fish are preserved for future use.

At the beginning of winter (October) the Eskimo go into their winter house, a solidly constructed dwelling capable of containing several families. It is sometimes built of stones, sometimes of timber, and in each case thickly covered over with a layer of earth. The wooden house is ingeniously designed, with a skeleton of upright pillars and transverse balks, to which the boards forming the walls and roof are affixed. The timber is furnished by driftwood found on the coast: in some localities this driftwood is so scarce that it may take three or even five years to collect as much as will build a single house or provide the framework of a boat. It is said that these winter houses are the best that could be devised, under the circumstances, to meet the rigours of an arctic climate. They are entered by a long covered passage, and warmed by blubber lamps (these are simple variously shaped bowls of soapstone, sandstone, or other rock, in which blubber, usually obtained from the seal, is burnt). The houses are so proof against cold that, with these lamps, a temperature of 20° C. is maintained. Speaking of the Greenland houses, which are built of stone, Hans Egede remarks: "I cannot forbear taking Notice, that though in one of these Houses there be ten or twenty Train-Lamps, one does not perceive the Steam or Smoak thereof to fill these small Cottages: The Reason, I imagine, is the Care they take in trimming those Lamps—viz. they take dry Moss, rubbed very small, which they lay on one Side of the Lamp, which, being lighted, burns softly, and does not cause any Smoak, if they do not lay it on too thick, or in Lumps. This Fire gives such a Heat, that it not only serves to boil their Victuals, but also heats their Rooms to that Degree, that it is as hot as a Bagnio. But for those who are not used to this Way of firing, the Smell is very disagreeable, as well by the Number of burning Lamps, all fed with Train-Oil, as on account of divers Sorts of raw Meat, Fishes and Fat, which they heap up in their Habitations; but especially their Urine-Tubs smell most insufferably, and strikes one, that is not accustomed to it, to the very Heart."[1]

On entering into winter quarters the Eskimo begins to reward himself for the labours of the year: reindeer meat, seal's blubber, and dried salmon furnish forth a long succession of Gargantuan feasts, which continue as long as the provisions last. When they give out—and in good times this will not be till the darkest days are past—hunting must perforce begin again. By this time the ground has long been frozen hard; rivers, lakes, and the sea are covered with a continuous sheet of smooth winter ice. Hares may now be trapped; the musk-ox, which never leaves the tundra, is an easy prey, but never eaten, except as a last resort; the arctic bear may be engaged in fight, and this calls for all the skill and courage shared by the two men who undertake the combat. But the main food of many Eskimo tribes, both now and all through the greater part of the year, is provided by the seal. There are four kinds of seal in the Arctic Ocean, and two of them extend northwards beyond the Arctic Circle, as far as Grinnell Land. One or other species is fairly plentiful up to lat. 60° N.; its favourite haunts are deep fjords, covered for nine months of the year with smooth ice. It makes holes in the ice in order to obtain air to breathe, and in summer it crawls up through larger holes on to the ice to bask in the sun. In spring it feeds its young in a hole under the snow, and when the snow has melted away it returns to the ice. The walrus, which affords a favourite food, is far less widely distributed. It is most dainty in its choice of a dwelling-place; the sea must not be too deep, the bottom must be covered with abundant shellfish, and certain relations must exist between the sea-currents and the ice.

[1] Hans Egede, *A Description of Greenland*, London, 1745, p. 117.

In late winter and spring, the Eskimo, for the most part, leave the land and spread in small groups over the ice, travelling by dog-sledges along the coast, and never remaining very long in one place. They live at this time in snow houses, warmed by blubber lamps, and hunt seals, chiefly by the "maupak" method—that is, the hunter sits down by the side of an air-hole and waits till a seal comes up to breathe, when he despatches it with a harpoon: as the year advances, the "arpok" method is also used, the seal in this case being killed as it lies basking at midday in the sun.

The dress of the Eskimo, which is much the same for the women as the men, consists of short trousers and a tunic ending above in a hood to cover the head. The trousers are sometimes continued downwards into stocking feet. Of boots, which are well made, they have a great variety, to be worn according to the weather. Shoes with very ingeniously contrived soles are made for walking on the ice. Fur gloves or mittens are also worn. An overall for use in wet weather is made from the intestines of the seal. The intestine is thoroughly cleaned, inflated with air, and hung up to dry. It is then carefully flattened and rolled up tight, like a spool of ribbon. When required for use it is slit up longitudinally, and makes a strip about three to five inches wide. The margin is pared, and several strips are sewn together into the desired form. These overalls are extremely light, not above six or seven ounces in weight. The transparency of the seal's gut renders it useful for other purposes: it makes an excellent substitute for glass as a window-pane.

The Eskimo wear their dress only when out of doors; in their houses they go stark naked, and the first hospitality offered to a visitor is an invitation to strip.

Notwithstanding the hardships of the struggle which the Eskimo wage with reluctant Nature for their existence, they were at one time by no means a miserable race; they made themselves comfortable in a frozen region where other men would have perished, took a healthy enjoyment in life, and were distinguished by many estimable domestic and social qualities. The intrusion of the white man has brought with it its usual evil blight—poverty, sickness, selfishness, and loss of self-respect. It would be beyond our province to give instances, but one case where a different result might have been expected may be cited from Rink. He writes :—

"On approaching these places [Ny Herrnhut and Lichtenfels] the visitor, on being told that each of them contains about a hundred natives and two or three missionary families, will be at a loss to make out where the former have their abodes. The mission lodges are pretty spacious, and for Greenland even stately in appearance. The stranger will probably be surprised on being informed that these buildings are only inhabited by missionaries, because he discovers nothing like human dwellings anywhere else. Then his attention will be called to something resembling dunghills scattered over low rocks and partly overgrown with grass, and he will be surprised to learn that the native population live in these dens."[1] At one time these people had good winter houses.

The number of Eskimo is diminishing, especially in Greenland, and if the race should become extinct, the country will remain uninhabited, for white men alone could not live there.

Detailed descriptions of the implements, weapons, and miscellaneous possessions of the Eskimo may be found in the Annual Reports of the Bureau of Ethnology, published in Washington: a brief enumeration will suffice for our purpose. The kayak, umiak, salmon-fork, bird spear, spear-thrower, bow and arrow, bird bolas, and skin tent are chiefly used in summer; dog sledges, harpoons, spears, winter houses, and blubber lamps during the winter; besides these there are bow drills, arrow straighteners, needles and needle-cases, bone pins, tool-bags with bone handles, buckles, belt fasteners, snow picks, hair combs, and a vast variety of other miscellaneous objects.

The adjacent Indians possess the birch-bark canoe in two forms, a larger corresponding to the Eskimo's umiak, and a smaller corresponding to the kayak, which is sometimes covered in for as much as three-quarters of its length; snow shoes, sledges for travelling over snow, drawn by women assisted by dogs, the bow and arrow, spear-thrower, ice-chisel, fish-hooks, nets, and fishing-spears: to ensure their recovery the arrows are sometimes attached by a long thread to the bow, and a line held at one end in the hand is sometimes attached to the fishing-spear. In some cases, indeed, as among the Ojibways and Shoshones, a rudimentary harpoon was

[1] H. Rink, *Danish Greenland*, London, 1877, p. 181.

used, provided with a point which became detached from its immediate union with the shaft on entering its victim, a connection however being still secured by a long intervening line. The Eskimo harpoon is a further development of the same device: it is distinguished from all others by the introduction of an additional movable segment between the detachable point and the shaft. This intermediate piece is articulated with the shaft by a ball-and-socket joint, and held in position by two stout thongs of reindeer hide which pass through holes drilled in it and the shaft.

If now we turn to the Magdalenian implements, we must admit that a large number of those most characteristic of the Eskimo are not to be found among them. The sledge, the kayak, and the fully developed harpoon are all missing, and since in each of these bone or ivory occurs as an essential part, they should have left some trace of their existence, had the Magdalenians possessed them. This argument does not apply, however, to the birch-bark canoe and wooden sledge of the Athapascans and Algonkians, for, as we have seen, wood is a perishable material.

The sledge, the kayak, and the harpoon of the Eskimo are all highly specialised instruments, and we should scarcely expect to find the remote ancestors of the race in full possession of the completely developed Eskimo cult as it now exists.

When we examine the various kinds of objects which are common to the Eskimo and Magdalenians, we cannot fail to remark a surprising amount of resemblance between them in detail. There is no essential difference between the more primitive Eskimo arrow-straighteners and those of the Magdalenians; the bone arrow-heads are often strikingly similar, and this similarity extends to those used by the Indians, especially as regards the character of the ownership marks; the bone hairpins of the Magdalenians may be matched among those of the Eskimo, and the lobate ivory pendants, sometimes heart-shaped, which both races possess, are almost identical in form. These are used by the Eskimo as ornamental appendages to fur bags, "housewives" or clothing. Not much stress can be laid on the bone needles, for these are of almost world-wide distribution, but the bone pins or taapootas seem to be more characteristic. The barbed bone spear-head of the Magdalenian more closely resembles that of the Eskimo than of any other people: that in use among the Fuegians is simpler and ruder in form; but it is by no means certain that the Fuegians should be omitted from this comparison.

The spear-thrower is common to the Magdalenians, Eskimo, Indians, and many other races, including the Australians, and thus does not count for much; nor should we omit to point out that the form of the Magdalenian implement is very different from that of the Eskimo.

The sculpture of figures in the round presents many remarkable analogies, the reindeer, mammoth and musk-ox of the Magdalenians finding parallels in the whales, seals, and bears of the Eskimo, though on the ground of art superiority must be allowed to the more ancient race. The same is true of the line-engravings, with which both adorned their implements. The Magdalenian sketches are always the more realistic, the Eskimo the more conventional. There is also a difference

FIG. 5.—Drawings on Eskimo bow-drills.

On the left a man gathering berries, in the middle two boys playing football, on the right hunter quarrelling over the possession of game.

in motive. The Magdalenian artist was an artist for pure love of art—he took pleasure in the graceful form and attitudes of the reindeer and delighted in representing it; the Eskimo, on the other hand, is more interested in story-telling, his drawings show a strong tendency towards picture writing, and almost achieve it (fig. 5). The difference will be perceived at a glance on comparing the well-known figure of a feeding reindeer from the Kesslerloch, near Thaingen, with the drawings engraved on an Eskimo arrow-straightener preserved in the British Museum and represented by Prof. Boyd Dawkins in *Cave Hunters*, p. 354. In the one our admiration is aroused by the truthful outline and artistic feeling of the sketch; in the other our pleasure is less æsthetic but perhaps more intellectual: we are impressed by the skill with which the animals are generalised—the detail is as sparing as in Egyptian hieroglyphs and the symbolisation is just as correct—but our chief interest is in the event which the drawing records. In the one case the object of the drawing is a reindeer, in the other a reindeer hunt. The hunters, disguised with reindeer horns, are stalking the unsuspecting herd. This difference is essentially similar to that which we have already observed in the case of Solutrian and Bushman art, though the Bushmen have not carried generalisation so far.

We should perhaps scarcely have expected this order of development, though now that it is suggested good reasons can be found for it.

Children often display a remarkable aptitude for rough portraiture; the illustration (fig. 6) records the spontaneous efforts of an untaught English girl at the age of seven or eight. They are admitted by their victims to be excellent caricatures, but the artist showed no signs of unusual ability with her pencil

FIG. 6.—Artistic efforts of childhood.

in later years. A stage of imitative art may thus occur in the childhood both of the individual and the race.

On a general review of the facts it would appear that, allowing for the long interval which separates the Magdalenian from the Eskimo in time, there is a sufficient degree of similarity between the products of their industry and art to furnish a *prima-facie* case in favour of an alliance by cult. The evidence is by no means convincing; and if, proceeding a step further, we begin to speculate on the consanguinity of the two races, we are met with geographical difficulties, not to mention others, which are amply sufficient to justify those who maintain a sceptical attitude.

There still remains, however, one class of evidence to which as yet we have made no allusion: it is that relating to the bodily characteristics of Magdalenian man. Such of his skeletal remains as are preserved in our museums were, until recently, surprisingly few; many more, no doubt, have been encountered by explorers of caves, but unfortunately many of these persons were more intent on enriching their collections with "curiosities" than on scientific investigation, and we have to deplore, in consequence, the loss of much precious material, which has been ruthlessly destroyed because it was not fitted to adorn a cabinet. Of late years, however, the systematic excavation of the caves at Mentone under the generous patronage of the Prince of Monaco has put us in possession of several well-preserved skeletons, so that our knowledge of one part of the Magdalenian population of Europe has now been placed on a secure basis.

The material at our disposal indicates the contemporaneous existence of two distinct races, one represented by the giants of Mentone, the Crô Magnon race; and the other by a man of comparatively low stature, whose skeleton was found at Chancelade.

The Crô Magnon race was recognised as Mongoloid by Pruner Bey,[1] but it presents several very remarkable characters which do not find any close analogy among any existing people. Skeletons belonging to it were first discovered in 1868, in making a cutting for a railway line from Limoges to Agen at Crô Magnon (whence the name), near Les Eyzies, in the valley of the Vézère; additional discoveries have since been made from time to time, the most recent being afforded by the grottes de Grimaldi, which have yielded six additional skeletons.[2] These were found under circumstances which show that the Crô Magnon people buried their dead; some were interred over a hearth, others in a grave, or in a rudimentary tomb, made by placing stones on edge for the walls, and roofing over with slabs. The corpse was buried, possibly dressed in the clothes, and certainly adorned with the ornaments, which had been worn during life; these include perforated shells of *Nassa neritea*, perforated teeth of deer, vertebrae of fish such as salmon, and carved pendants, representing together the remains of a necklace or collar. The perforated shells are sometimes found on the skull, and seem to have been sewn on to a cap. Flint implements of Magdalenian type are also found in the burial place.

[1] Pruner Bey in Lartet & Christy, *Reliquia Aquitanica*, 1868, p. 88.

[2] Verneau, *Les Grottes de Grimaldi*, 1906.

The bodily characters presented by all the remains of skeletons are of a very uniform kind. The stature ranged from 1,750 mm. to 1,890 mm., with a mean of from 1,820 to 1,870 mm. (6 ft. to 6 ft. 3 in.). The cranial capacity is very great, ranging from 1,590 to 1,715 cc. The head is dolichocephalic, and thus not in harmony with the face, which is broad and short (index from 63·2 to 63·4); the glabella and brow ridges are well marked, the orbits rectangular and very deficient in height (index from 61·4 to 66·7); the nose is depressed at the root, but rises rapidly, and is long and narrow, or leptorhine (index from 45·9 to 56·9).

A race distinguished by tall stature, a short face and depressed orbits was certainly not Eskimo, nor does it appear to be represented among the Athapascans or Algonkians, whether living or fossil.

The Crô Magnon was the first discovered of the two races, and for a long time afforded the only evidence we possessed as to the physical characters of Magdalenian man. Our knowledge of the Chancelade race is based on a single skeleton found on October 10, 1888. It lay buried in the deposits of a rock shelter on the left bank of a rivulet called the Beauronne, 7 kilometers north-west of Perigueux, in the commune of Chancelade. The remains of a rich Pleistocene fauna, flint implements of Magdalenian type, as well as implements of bone and reindeer's horn, were found associated with it. It rested on a rocky floor at a depth of 1·64 meters from the surface of the soil; overlying it were first a hearth and associated débris, 37 cm. thick; then a sterile layer, 32 cm. thick; next another hearth and débris, 40 cm. thick; and finally a superficial layer of cave earth, 53 cm. thick.

We owe a masterly anatomical study of the skeleton to Dr. Testut, who states that it represents a man of low stature, only 1,500 mm. in height, with a large skull (capacity 1,700 cc.) having the characteristic Eskimo form: a comparison which is borne out by every feature in detail; it is wall-sided, with a pent-like roof, and dolichocephalic, with an index (72·02) almost the same as that of the Eskimo (mean value 71·72); the face is remarkable for its length, and there is a close correspondence in the relation between the length and the breadth, or the facial index, which amounts to 72·8 in the Chancelade and 72·2 in the Eskimo skull; the nose also is long and narrow, its index (42·5) agreeing closely with that of the Eskimo (42·62); the orbit is wide and high, just as in the Eskimo, its index being 86·97, and that of the Eskimo 87·8; the palate is fairly long in comparison with its breadth, with an index of 67·9, that of the Eskimo being 68·4; finally the naso-malar angle of Flower, which measures the recession of the face behind the orbits, is very large, attaining the value of 145: in this respect also it makes a nearer approach to the Eskimo, with a value of 144, than to any other known race.

The evidence could scarcely be more definite; the osteological characters of the Eskimo, which are of a very special kind, are repeated by the Chancelade skeleton so completely as to leave no reasonable doubt that it represents the remains of a veritable Eskimo, who lived in southern France during the Magdalenian age.[1]

In North America, as we have seen, a tall Indian race immediately succeeds the Eskimo towards the interior; and in Europe a tall Crô Magnon race was associated with the short Chancelade people. If we have rightly identified the two short races one with the other, we shall next be tempted to suppose that some close bond of blood may have existed between the two tall ones. There are, indeed, some characters which they possess in common, the Algonkians, in the eastern part of the continent, having long heads, like the Crô Magnon men, and this in itself appears to be a remarkable fact, when we consider the rare occurrence of dolichocephaly among the Leiotrichi. The short faces and depressed orbits of the Crô Magnon men mark them off, however, as a distinct race.

The Magdalenian cult extended east from Mentone, through France, Switzerland, Germany, Bohemia, Moravia, and as far as Russian Poland, and it has been traced northwards to Kent's Hole in Devon and Creswell Crag in Derbyshire. Future discoveries alone can inform us as to the relative distribution of the two races, who probably shared this territory between them, but it is safe to suppose that the Chancelade race occupied the more northern stations, though all that is certainly known is its occurrence in southern France. The question next arises as to how the existing Eskimo acquired their present distribution.

The Magdalenians are the latest completely palæolithic race which inhabited Europe: their successors on this soil, apart from the Azilians, were the neolithic folk, who brought with

[1] L. Testut, "Recherches Anthropologiques sur le Squelette Quaternaire de Chancelade, Dordogne," *Bull. de la Soc. d'Anthr. de Lyon*, tom. viii., 1889.

them a pastoral or agricultural mode of life. It is highly probable that these neolithic folk were already in existence, previous to their entrance into the Magdalenian area, and if so, the time was almost certain to arrive when by a natural increase in numbers they would begin to exert a pressure on adjacent tribes. The chase is extravagant in the demands it makes upon territory: possibly a hundred farmers could exist on the land which would only support a single hunter. Thus, from the very nature of their industry the neolithic people could scarcely fail to grow strong numerically, and consequently capable of forcing their way into fertile regions in face of whatever resistance the hunters might oppose. Simultaneously with this pressure from behind, an attraction may well have arisen in front, for towards the close of the Magdalenian age a steady amelioration of climate was in progress which especially affected the temperate zone; as a consequence the sub-arctic fauna which supplied the Magdalenian hunters with so large a part of their food, especially that important member of it, the reindeer, so highly esteemed by Indian and Eskimo alike, was shifting its limits towards the north. In this connection we may recall the fact that Magdalenian stations are known to occur well within the limits of the greatest extension of the ancient ice, as for instance at several localities in Switzerland, and at Cresswell Crag in England. The cold fauna, represented by fossil remains of the reindeer, musk-ox, and walrus, is found in North America as far south as southern New Jersey, or in the adjoining region to the south and west; and it seems to be confined to superficial gravels, a fact which points to a comparatively late immigration. Possibly it was followed or accompanied by Magdalenian man.

Ingress to the North American continent might take place over Behring Strait and the Aleutian Islands, or across the Icelandic bridge. At first sight the latter route appears most promising. It is doubtful, however, whether at this time it was still standing; it had possibly ceased to be intact during Miocene times, and is generally supposed to have completely broken down before their close. Besides this, no relics of Magdalenian man have been discovered on those remnants of the bridge which still stand above water, nor on the neighbouring shores. Scotland has yielded none,[1] and the earliest human remains

[1] Perforated bone harpoons like those of the Azilian stage have been found at Oban. Joseph Anderson, *Proc. Soc. Antiq. of Scotland*, vol. xxix. p. 211, 1895.

found in Scandinavia date from the neolithic period. The more probable route would therefore appear to have lain over Behring Strait or the Aleutian Isles.[1]

A general consideration of all the facts might, then, lead us to some such hypothesis as the following. During the Magdalenian age two races of dolichocephalic Leiotrichi, differing greatly in stature, extended from western Europe to the east, across the entire breadth of Asia, occupying a zone which included much of the tundra and the steppes. They possessed a common Magdalenian culture, and resembled in their mode of life the Algonkians and Athapascans of the tundra as they existed before the advent of the white man, feeding on reindeer and mammoth, horse and bison, together with various kinds of fish.

The taller, and probably more powerful, race held possession of the more favoured regions in the south, where the climate was less rigorous and game more abundant; the shorter race, hemmed in by its tall relations in the south and the ocean or the ice in the north, had to make the best of its inhospitable surroundings, and developed, thanks to its great intelligence, a special mode of life. No doubt other Leiotrichous races, but distinguished by broad heads, were in simultaneous existence in the more southern parts of Asia.

As the climate became warmer, the pressure of the rapidly increasing neolithic people began to make itself felt, acting probably from a region somewhere between the Carpathians and India. A movement of the Leiotrichi was thus set up towards the north; but as there was no room for expansion in that direction, it was diverted towards the only egress possible, and an outflow took place into America over Behring Strait or the Aleutian Islands. The primitive Eskimo, already accustomed to a boreal life, extended along the coast. The primitive Algonkians, following close upon their heels, occupied the southern margin of the tundra, and extended east as far as the Atlantic Ocean. The broader-headed Athapascans came next, and gradually acquired possession of the western half of the southern tundra. The Eskimo were rigidly confined to the coastal regions, but there was nothing to arrest the progress of the primitive Red Indians towards the south—everything, indeed,

[1] See A. Hamberg, *Om Eskimoernas härkonish och amerikas befolkande*, Ymer, 1907, p. 15.

seemed to invite them in that direction. No geographical barriers rise across the path, and game of all kinds was abundant, so that in no very long time the primitive Indians may have populated both the American continents throughout their whole length, from north to south. It is interesting to observe in this connection that at the southern extremity of South America we still find a dolichocephalic Leiotrichous race, the Fuegians, who, though very inferior to the Eskimo in all respects, yet present some very striking resemblances to them in bodily structure, implements, and mode of life.

The subsequent differentiation of the original Red Indian races—*i.e.* the primitive Algonkians and Athapascans—may have given rise to all the existing races of both the American continents, except along the western coast, where the occasional stranding of vessels from the east of Asia or the islands of the Pacific may have added a foreign element.

That the Algonkian and Athapascan races once occupied a far larger area than they do now, or rather did before the invasion by modern Europeans, is shown not only by fossil remains found outside their present boundaries, but by circumscribed areas still inhabited by them, which are isolated from the main body of their race by alien tribes.

Recurring for a moment to the Eskimo, we may mention that Steensby,[1] as the result of a very interesting investigation, is led to conclude that the origin of the fully developed Eskimo cult must have occurred somewhere near the region of Coronation Gulf, where the conditions are peculiarly favourable for an "emancipation from forest life" and an adaptation to the environment provided by the Arctic coast. This view would not be wholly inconsistent with that which we have just sketched out; but it rests on resemblances between the implements and mode of life of the Eskimo and Indians which are susceptible of a different explanation.

If the views we have expressed in this and the preceding articles are well founded, it would appear that the surviving races which represent the vanished palæolithic hunters have succeeded one another over Europe in the order of their cranial capacity or intelligence: each has yielded in turn to a more highly developed and more highly gifted form of man. From what is now the focus of civilisation they have one by

[1] H. P. Steensby, *Om Eskimokulturens Oprindelse*, Copenhagen, 1905.

one been expelled and driven to the uttermost parts of the earth: the Mousterians survive in the remotely related Australians at the Antipodes, the Solutrians are represented by the Bushmen of the southern extremity of Africa, the Magdalenians by the Eskimo on the frozen margin of the North American continent, and as well, perhaps, by the Red Indians. It is a singular fact, when considered in connection with the claims sometimes asserted in favour of the dolichocephalic skull, that in each of these ancient races, marked by so many primitive characters, a long head is distinctive. Perhaps this also is to be numbered among the primitive characters?

Finally we may ask in the light of this history what part is to be assigned to justice in the government of human affairs? So far as the facts are clear they teach in no equivocal terms that there is no right which is not founded on might. Justice belongs to the strong, and has been meted out to each race according to its strength; each has received as much justice as it deserved. What perhaps is most impressive in each of the cases we have discussed is this, that the dispossession by a new comer of a race already in occupation of the soil has marked an upward step in the intellectual progress of mankind. It is not priority of occupation, but the power to utilise, which establishes a claim to the land. Hence it is a duty which every race owes to itself, and to the human family as well, to cultivate by every possible means its own strength: directly it falls behind in the regard it pays to this duty, whether in art or science, in breeding or organisation for self-defence, it incurs a penalty, which Natural Selection, the stern but beneficent tyrant of the organic world, will assuredly exact, and that speedily, to the full.

THE DELETERIOUS EFFECTS OF BRIGHT LIGHT UPON THE EYES

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It is well known that bright light under various conditions is detrimental to the sight. In recent times great progress has been made in the development of the many methods of artificial illumination. Gas light, although at one time threatened with supersession by the electric light, has taken a new lease of life owing to the introduction of incandescent mantles, whilst a secure and unassailable position has been gained for the electric light by its perpetual modification and improvement. Each subserves a useful function in civilised life, each being adapted to requirements which are less efficiently satisfied by the other. It must be admitted that there has been something haphazard in the development of these illuminants for the use of mankind. Inventors and engineers have held only one chief problem before their minds—the provision of maximum illumination at minimum cost. They have until recently not troubled themselves at all about the physiological aspects of the question; but it has now become imperative that this view of the problem shall receive due attention. It is the object of this paper to review briefly the recorded cases of the manifold deleterious effects of bright light upon the eyesight, to analyse them as far as may be so as to discover the conditions which predicate detriment to the eyes, to consider how far these conditions prevail in the use of modern artificial methods of illumination and to demonstrate the precautions which must be adopted to avoid danger.

Even a scanty acquaintance with the diseases of the eye which follow exposure to intense light suffices to show the multiplicity of its effects. Attention was first prominently drawn to the subject by cases of partial or complete blindness resulting from the observation of eclipses of the sun with the unprotected eyes. Every one knows that looking at a bright light is followed by an unpleasant blurred spot in the centre of the field of vision. This spot, which is known as a negative after-image, lasts for a variable period according to the intensity of the light and the length of time during which it is fixed with the eyes. The commonest sequel of observing an eclipse of the sun with unprotected eyes, the so-called eclipse blindness, is a pathological exaggeration of this physiological phenomenon. The after-image, instead of gradually dwindling and finally disappearing, persists as a permanent blind spot in the centre of the field of vision. In technical language this is described as a permanent positive central scotoma. It has an extremely disastrous effect upon vision because it affects the most sensitive part of the sentient layer of the eye or retina. It abolishes vision in that part which is used for the things we are actually looking at. The surrounding retina is still functional but vision is normally far less acute and is useless for the observation of the fine details of things seen. The total effect is that, while the patient is quite able to walk about, everything is seen dimly and he is unable to read or do other near work. If the retina of such a patient be examined by the ophthalmoscope the central area or macula lutea is seen to be diseased. Instead of being red it is white in the early stages but later on shows minute spots of black pigment. As already mentioned, the condition is permanent in the majority of cases. Other cases occur in which the damage is still greater. Whether or no the changes mentioned appear, vision gradually fails not only in the central areas but over the whole field of vision. Inspection with the ophthalmoscope then shows that the optic nerve, which conducts the visual impulses from the retina to the brain, is atrophic and dead. Other rarer results of the action of bright sunlight have been noticed but need not detain us.

Nearly comparable with eclipse blindness is the effect sometimes observed after exposure of the eyes to the bright flash which occurs when a powerful electric current becomes short-circuited. Here the duration of the stimulus is momentary but the intensity of the light is extreme. The changes in the eyes which have generally been seen in these cases are almost identical with those following exposure to bright sunlight.

Allied to these conditions again are the cases of injury to the eyes from lightning stroke. Here, however, we are dealing not only with bright light—using that term in its broadest sense—but also with other forces, mechanical, electrolytic and so on. The results are correspondingly complex. In addition to changes in the retina, inflammation or atrophy of the optic nerve, inflammation of the vascular coats of the eye (iris, ciliary body, and choroid), etc., a very large proportion of the cases show the development of cataract, *i.e.* opacification of the crystalline lens. It is improbable that the cataract produced in this manner is due merely to the effects of exposure to the sudden intense light. It is more likely to be caused by the concussion of the stroke, being thus brought into line with other well-known examples of concussion or traumatic cataract. Some of the changes in the eye are, however, almost certainly due to light and are exactly comparable with those found in short-circuiting an electric main.

There is another condition under which, after exposure to bright light and heat, cataract develops—viz. amongst glass-workers. The cataract in these cases is of a very characteristic type, the opacity being limited in the early stages to a small circular area in the posterior part of the crystalline lens. The men who most suffer from this disease are elderly men who have pursued the trade from boyhood. They have generally passed through all the stages in the manufacture—usually of ordinary bottles such as are used for beer, etc. Most of them are expert workmen who are engaged in "finishing" bottles, *i.e.* putting a ring of molten glass round the mouth of the bottle. The performance of this operation demands keeping the eyes fixed almost continuously upon the molten glass, either in the process of taking the requisite amount of "metal" out of the furnace or in fashioning the ring. Whether this form of cataract is caused by the exposure to the intense light or to heat or other concomitant factors is at present unknown. It is occupying the attention of a committee of the Royal Society which has been appointed for the elucidation of the problem.

The conditions hitherto considered of affection of the eyes after exposure to bright light have dealt only with serious defects of vision resulting from disease of the deeper structures. The more superficial parts are not infrequently affected, fortunately with less disastrous consequences. Snow-blindness is an example of this kind: in this case, after exposure to the bright light reflected from the surface of the snow, the superficial parts of the eye become intensely inflamed. The part most affected is the conjunctiva or mucous membrane which covers the inner surfaces of the lids and is reflected from them on to the outer parts of the eyeball. The inflammation sets up a most severe irritation, just as if sand or dust were present in the eyes. As a result the lids are tightly closed and the least attempt to open them is accompanied by pain and violent spasm. This condition is generally known as "photophobia" but it is very doubtful whether the dread of light plays any part in its production.

Almost identical with snow-blindness is the condition called "ophthalmia electrica." It occurs after prolonged exposure of the eyes to the naked arc light, to other forms of electric light, to the light emitted by mercury vapour lamps and so on. There is the same inflammation of the conjunctiva and spasm of the lids (blepharospasm). Sometimes, as also in snow-blindness, the cornea or clear anterior membrane of the eye which is so important for vision shows excoriations of the surface and even deeper parts may be affected, hyperæmia or actual inflammation of the iris, etc., sometimes occurring.

In these various causes which are productive of injury to the eyes and sight it is obvious that bright light, though itself complex, is not the sole factor to be considered. Thus it is fairly certain that light plays a relatively small part in the development of lightning cataract, for this disease is seldom if ever present in cases of short-circuiting, unless indeed an actual electric shock has been received by the patient. If we analyse the physical factors we shall find that in some, as in glass-workers' cataract, there is also intense heat. In almost all various forms of radiant energy are at work, not only all the waves of the visible spectrum but also the very active chemical or actinic ultra-violet rays. It is not impossible that in some cases X-rays or radium emanations may play a part.

In the complete elucidation of the subject it is necessary to determine not only what force is the actual cause of the lesion but also whether it acts directly on the structure injured or affects it secondarily by interfering with its nutrition, either by interfering with the normal functions of other structures of the eye or by affecting the general bodily health. In most cases the exact method of action is very imperfectly understood and consequently we are unable to apply any very rational or effectual method of cure. We are here, however, specially concerned with prevention and need not dwell on these more obscure problems.

There is no doubt that snow-blindness and ophthalmia electrica are caused by the action of ultra-violet rays. If sunlight is split up into its component parts by being passed through a glass prism a spectrum is obtained. If this is focussed on a screen by lenses, a sharply defined band consisting of all the colours of the rainbow is seen. The part of the band on the side of the base of the prism, *i.e.* the part which is least refracted, is red; the other end is violet. The various colours are due to differences in the wave-length of the light, the extreme red end having a wave-length of 766 $\mu\mu$ (millionths of a millimeter), the extreme violet 397 $\mu\mu$. If the spectrum is photographed it is found to be much longer on the violet side than that seen by the eyes. There are, therefore, rays beyond the violet end which are capable of producing chemical changes in the sensitive plate. These are called the ultra-violet rays. They may be made visible by placing a uranium glass in their course, when the uranium salt which impregnates the glass is said to "fluoresce." If a prism and lenses made of quartz instead of glass are used to form the spectrum the ultra-violet part is still longer. Glass, therefore, has the property of absorbing part of the ultra-violet rays. It is found that a foggy atmosphere has much the same effect as glass, so that sunlight in clear high altitudes contains far more ultra-violet rays than elsewhere. Moreover, snow surfaces and the surfaces of sheets of water reflect ultra-violet rays without causing any appreciable absorption. The eyes are protected to a large extent from direct sunlight by the overhanging brows and headgear. They are unprotected from the reflected light off the ground surface. Ultra-violet rays are not only responsible for these inflammations of the eye but are also the cause of so-called sunburn. If the skin be exposed sufficiently long to the ultra-violet rays only of the spectrum it becomes "sunburnt." Similarly, it has been shown experimentally that conjunctivitis exactly similar to snow-blindness or ophthalmia electrica is caused by exposure of the unprotected eyes of animals or men to intense ultra-violet rays.

In seeking the cause of glass-makers' cataract it is obvious that the question of heat must be taken into consideration. This is the more forcibly impressed upon us by the fact that workers in certain forms of glass manufacture appear to suffer little or not at all from the complaint. These men are engaged in the manufacture of flint glass bottles, pressed glass articles, and so on and the heat of the furnaces is much less than in ordinary bottle making. If heat, however, were an important factor, we might expect iron workers to suffer in a similar manner but such is not the case. Moreover, it is difficult to imagine that heat can penetrate far into the eye when we consider that water is a bad conductor and that the space between the cornea and the crystalline lens is filled with watery fluid. In fact, the lens is admirably protected from heat by a water bath. Cases, too, occur in which molten metal at an extremely high temperature is splashed into the eye without causing the development of cataract. If, therefore, such intense degrees of heat as those under consideration fail to produce deleterious effects upon the deeper structures of the eye, it is probable that we may with safety eliminate this factor as of any importance when we come to deal with sources of illumination in which the output of heat is incomparably less.

It is a striking fact that the attempt to obtain greater efficiency in artificial methods of illumination has resulted almost invariably in the production of sources of light which are extremely rich in ultra-violet rays. Possibly the belief that ultra-violet light has beneficial effects in the treatment of certain diseases of the skin may have influenced the trend of modern inventions. Thus it has led to the invention and development of the Finsen lamp. Here the immediate aim has been to enrich the light with ultra-violet rays by impregnating the carbons of the arc light with various metals. Experiments in this direction have demonstrated that there is in some cases a marked concomitant increase in luminosity as measured by the photometer and this aspect has attracted the attention of illuminating engineers and lamp manufacturers.

As has already been mentioned, exposure for any considerable length of time to light rich in ultra-violet rays causes "sunburn" and intense conjunctivitis. What is at present unknown is if still more prolonged exposure to much weaker doses of ultra-violet rays has any deleterious effects upon the eyes; and if so, which of the ultra-violet rays, those nearer the violet end of the spectrum or those more remote, have the worse effects. These weaker doses spread over a long period certainly had little tendency to produce the more severe acute results which immediately attract attention. There is good reason, however, to think that they are by no means innocuous. The problem can only be solved conclusively by experiments on animals and many of great value have been recorded, though they are not altogether confirmatory and much work yet remains to be accomplished.

We must first inquire what happens to ultra-violet rays when they pass into an eye. If we look at a spectrum in an absolutely dark room and screen off the luminous portion, we shall find that the ultra-violet part near the violet end is not completely invisible; it has a faint greyish luminosity. This may be due either to the possibility that these rays are capable of stimulating the percipient elements of the retina and so producing visual impressions or to the possibility that certain parts of the eye are capable of degrading the ultra-violet rays into rays of longer wave-length which are faintly luminous. This degradation of ultra-violet rays, which is a property possessed by various substances, is called "fluorescence." Since some of the structures of the eye undoubtedly fluoresce when exposed to these rays of short wave-length, the second alternative must be accepted as the more probable. The structure which shows this property most is the crystalline lens. If ultra-violet rays pass into the pupil, the lens is seen to fluoresce strongly. The same occurs if the eye is exposed to the emanations of radium. Bodies which fluoresce do so by absorbing the ultra-violet rays. Their absorption by the lens is strikingly shown by placing an excised lens on the skin and exposing both to strong ultra-violet rays. The skin becomes "sunburnt" everywhere except where the lens lies, the part thus protected remaining normal or much less affected. The retina also fluoresces, though much less. It has been shown experimentally that if light rich in ultra-violet rays is allowed to pass into the eye for a considerable time the lens loses its power of absorbing these rays. They then pass through almost unimpeded and are able to effect any deleterious consequence of which they are capable upon the delicate structures of the retina. Absorption means a utilisation of energy which must be associated with some chemical or other change. It is therefore not unlikely that prolonged exposure to ultra-violet rays may produce pathological changes in any structure of the eye which normally absorbs them. With the help of Mr. E. C. C. Baly, F.R.S., I have recently investigated the absorption of ultra-violet rays by the various parts of a freshly excised rabbit's eye. The cornea or other structure is placed before the slit of a spectroscope, all the constituent lenses and prisms of which are made of quartz. We found that the cornea, lens and vitreous humour all caused quite definite absorption. Considering the predominant fluorescence of the lens it was surprising to find how much absorption was occasioned by the cornea and vitreous humour. It is possible that the fluorescence of the lens has been overestimated owing to its high refractive index which would tend to exaggerate the effect of any possible escape of luminous waves. Similar results have since been published by Schanz and Stockhausen.

The evidence, however, goes beyond this point. There is definite experimental evidence of the actual changes which are produced. Thus Widmark caused slight opacity in the lens of the rabbit by prolonged action of concentrated light from an arc lamp. Exposure for two to four hours in a 1,200 candle-power lamp was necessary and he then only succeeded in four out of eleven cases. On the other hand it must be stated that Ogneff failed to obtain any change by the brief application of a 5,000—6,000 candle-power lamp. More recently Hess found microscopic changes in the anterior cells of the lens after exposure for some hours to the light from Uviol mercury vapour lamps. There was no actual opacity of the lens but this was probably because of the comparatively brief exposure. Birch-Hirschfeld has shown that changes also occur in the retina when exposed to ultra-violet rays. The nerve cells of the retina show early stages of those changes which are known to be followed by degeneration and atrophy if the stimulus is sufficiently prolonged. The experiments were performed on rabbits and it is striking that the effect was much greater if the lens of the eye had been previously removed. The crystalline lens, therefore, by its absorption of these rays acts as a protective mechanism to the perceptive structures. Hence, there is ample evidence to show that ultra-violet rays are deleterious to the eyes, not only in so far as the superficial structures are concerned but also the deep; further that prolonged action of weak doses is also be avoided.

All the more recent forms of electric lamp emit light rich in ultra-violet rays. This is specially true of arc lamps in which the carbons are impregnated with metals or have a metal core. It is true in less, but still marked degree, of the various metallic filament lamps—osram, tantalum, etc. It is also true of Nernst lamps. Schanz and Stockhausen have published some very interesting observations on the spectra obtained from these lamps photographed with a quartz train. The results show the effect of various kinds of globe upon the spectra. Ordinary clear glass globes cut off all the shorter wave-length ultra-violet rays—*i.e.* beyond 300 $\mu\mu$. Those from 400 $\mu\mu$ to 300 $\mu\mu$ pass unimpeded. They have succeeded in obtaining a glass, called "Euphos" glass, which cuts off all the ultra-violet rays without much loss of the luminous rays. Mercury vapour lamps emit light which is very deficient in luminous rays at the red end of the spectrum but rich in those towards and beyond the violet end. For some purposes, *e.g.* photography, therapeutics, etc., it is desirable to utilise the ultra-violet rays to the full. For this purpose another kind of glass has been invented, which resembles though it is not so efficient as quartz. It is called "Uviol" glass. The most powerful ultra-violet lamp is a mercury vapour lamp in which the tube is made of quartz and in which the mercury vapour is under pressure. By increasing the pressure the spectrum obtained, instead of consisting of isolated lines or groups of lines, becomes continuous. Great heat, however, is developed by these lamps, necessitating their being cooled by a water-jacket.

It will be seen, therefore, that the ordinary lamps of commerce when protected by glass globes emit ultra-violet rays of from 400 $\mu\mu$ to 300 $\mu\mu$ only in any considerable quantity. It is not yet known whether these rays are more or less deleterious to the eyes than those of shorter wave-length than 300 $\mu\mu$. It is certain that they are deleterious when concentrated and when the exposure is prolonged. It is to be hoped, therefore, that some such glass as the new "Euphos" may be employed for these lamps whenever they are used simply for the purpose of illumination. There is obviously less danger in the use of even naked arc lamps, since their heat and candle-power are so great that they are little adapted for indoor illumination.

ULTRAMICROSCOPY AND ULTRAMICROSCOPIC PARTICLES

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Introduction

It follows from a passage in Seneca, that the ancients knew that when writing is observed through a globe of water the characters appear of increased size. The action was attributed to the water and the effect of the glass was not suspected.

In 1285 Salvino d'Arnato degli Armatii, of Florence, discovered the art of glass working and of manufacturing spectacles. The lenses first constructed were of slight curvature, being intended only for spectacle glasses. By successive steps improvements were introduced; the curvature was made greater and greater, the lens at the same time becoming smaller. Thus, by gradual stages, lenses came to be used as magnifying-glasses and simple microscopes. Dr. Henri Van Heurch makes mention of the existence of an engraving, copied from a picture by Raphael, dating back to 1513-20, in the Palais Pitti at Florence, in which Pope Leo X. is represented looking at some miniatures with the assistance of a magnifying-glass.

The first microscope was called by the name of "*vitrum publicanum*." It consisted of a small cylindrical box, the magnifying lens set at one end; at the other, two pieces of glass between which was placed the object to be examined—usually a flea. There was also a "seed microscope," used for observing seeds.

Another form consisted of a magnifying-glass mounted on a foot, with a needle point at a short distance from the lens; the object was placed on this needle. The microscope of the savant Leeuwenhoeck was a development of this: there was a greater perfection in the lenses, and the needle carrying the object was of adjustable height. The magnifying power obtained was much higher, in some cases being as high as

270 diameters. About the year 1740 Wilson introduced further improvements. A mirror mounted on a foot was provided to reflect the light into the instrument. The object was placed between two glass slips, which were held between two small brass screws; a screw-thread enabled the object to be raised or lowered.

The invention of the compound microscope seems to have been accidental. While arranging some glasses for spectacles, Zacharias Janssen, an optician of Middelberg, noticed the greater power which could be obtained by using certain combinations of lenses. His instrument has since been gradually improved. Bonanimi, in 1691, added numerous appliances: he used three lenses—and also three tubes with which he could obtain different magnifications. A distinct advance was the use of a condensing lens to concentrate the light on the object.

A short time later Marshall, in England, used a series of object-glasses, and for his eyepiece used smoked glass. The purpose of this was to render the coloured edges of the object less distinct; the method appears to yield good results, especially with lower powers. It is a little strange that this method of producing achromatic images seems to have been neglected by later workers. In 1791 a Dutchman, Beeldsnyder, constructed an achromatic objective by using two convex exterior lenses of crown glass with a biconcave of flint glass between them.

Since this time, the great advances that have been made, as a result of the work of Le Chevalier, Ramsden, Abbe, and others, lie in the construction of lens systems in which the distortion due to spherical aberration and the colour effects due to chromatic aberration are eliminated; in addition, the introduction of immersion lenses by Amici, in 1855, must not be forgotten, since it marks an important step in the improvement of the microscope.

It was only during the latter half of the last century, largely owing to the work of Abbe, that the limit to the resolving power of the microscope was clearly recognised and understood: that there exists a limit beyond which it is impossible to push the resolving power of the microscope. If two objects approach within a certain limiting distance, it is vain to hope to be able to separate them with the microscope under ordinary conditions;

if an object is too small, we can no longer hope to make out its details, however much the magnifying power of the instrument be increased. The resolving power can be increased by using light of shorter wave-length, that is, by using light towards the blue end of the spectrum; but again a limit is soon reached. Moreover the experimental difficulties are increased.

In 1903 Siedentopf and Szigmondy invented the ultra-microscope, by the aid of which the existence of particles much smaller than those hitherto known has been demonstrated. The hypotheses of Faraday and others on the nature of coloured glasses, crystals and colloidal liquids are no longer hypotheses, and our knowledge of these and other objects has been greatly extended. The method has also been applied, with advantage, to biological work; it is probable that, in the near future, its application will be still further extended.

It will be the aim in this paper to give some account of the ultramicroscope, its development and applications and of the ultramicroscopic particles the existence of which has been revealed by its use. In the few years that have elapsed, a vast amount of work has been done on the subject and it is possible to consider only a few of the many directions in which the use of these methods has thrown some light on the problems of science.

The Resolving Power of the Microscope

At the outset, it may be useful to consider the more salient facts with reference to microscopic vision and the resolving power of microscopes. It is well known that a point source of light gives rise to an image consisting not of a single point but something more complex. If the aperture of the objective is circular, the image consists of a circle of light surrounded by diffraction rings, the diameters of the successive rings depending on that of the aperture; if it is rectangular, we have for the image a rectangular patch of light surrounded by a network of light and darkness. Consider a neighbouring point source. This will give rise to an image similar to that of the former and, if the sources are close together, the diffraction pattern of one may partially overlap that of the other; when this overlapping exceeds a certain amount, the two images can no longer be separately distinguished.

It can easily be shown that the smallest distance apart of two points, consistent with their images being recognised separately, is given by the relation—

$$d = \tfrac{1}{2}\lambda/\sin a,$$

where λ is the wave-length of the light used, in the medium employed, and a is the angle subtended at the point by half the diameter of the object-glass.

This can be written in the form—

$$d = \tfrac{1}{2}\lambda_0/\mu \sin a$$

where λ_0 is the wave-length in air, μ the refractive index of the medium for this wave-length, and a has the same meaning as before. This form of the expression will be more convenient for our purpose.

Thus the value of d, the limiting distance, depends on the wave-length of the light used and on the product of μ and $\sin a$, a product which is usually known as the numerical aperture. There are two ways in which d can be diminished and the resolving power of the microscope increased. In the first place, this can be achieved by using a large numerical aperture. If the refractive index μ is given, a is made large: in good objectives this may reach the value 73°, when $\mu \sin a$ is equal to $0{\cdot}95 \times \mu$. For air, the numerical aperture would be 0·95, the maximum possible value being unity (when a is 90°). The value of μ is increased by using objectives of immersion, where the object-glass is immersed in some liquid. If this be water, values of $\mu \sin a$ up to 1·25 can be obtained. Sometimes, especially in mineralogy, a-monobromonaphthalene, which has a refractive index of 1·66, is used; in this case an aperture approaching the value 1·66 is possible.

When a liquid of so high a refractive index is used and the lenses are of crown glass, the angle a cannot be increased beyond the angle corresponding to the critical angle for the two substances (67°). It is therefore more convenient to use flint glass.

It follows, from what has been said above, that with such an arrangement we cannot hope to see distinctly objects which are separated by a distance less than about $\lambda_0/3$, and this for the sodium lines (λ approximately 6×10^{-5} cm.) will be 2×10^{-5} cm. or 0·2 μ.[1]

[1] $\mu = 10^{-4}$ cm.; $\mu\mu = 10^{-7}$ cm.

The value of the expression $\frac{\lambda_o}{2\mu \sin a}$ can also be diminished by using light of short wave-length. Passing along the spectrum from the red end towards the violet, the wave-length diminishes, until at the extreme violet it is only about half as long as in the red. Continuing beyond the range of the visible spectrum in the same direction, the ultraviolet is reached, which, though not affecting the optical nerves, acts on a photographic plate and gives rise to fluorescence in certain substances. In virtue of its shorter wave-length, it can be seen that this will give a higher resolving power than can be obtained with light of the visible spectrum. As a source of light the electric arc between two metal electrodes is commonly used; electrodes of magnesium or cadmium are very useful, since the light emitted is fairly homogeneous. Since glass absorbs ultraviolet light, the lenses and other portions of the apparatus must be made of quartz; further, the methods of observation must depend either on photography or fluorescence; the latter will diminish the accuracy and both increase the experimental difficulties.

The use of ultraviolet light allows us to obtain the form of objects which are only about half as large as those which can be examined with the light of the visible spectrum.[1] It will therefore be possible to separate the images of two objects separated by a distance of 1×10^{-5} cm. or 0·1 μ.

The Ultramicroscope.—By using the ultramicroscope, however, much smaller objects can be seen, though as will appear later their form cannot readily be obtained. It has been shown that two point sources of light which are too close together cannot be seen separately with the microscope; and similarly, details of structure cannot be recognised, if an object is too small, when the microscope is used in the usual manner, and transmitted light is used. Now there are some ultramicroscopic objects which can neither be studied nor even seen under these conditions; by using the ultra-microscope, we are enabled to see them but not study them in any detail. We will now pass on to consider how these ultramicroscopic particles can be rendered visible.

Consider an object, self-luminous and sufficiently far re-

[1] Wave-length of sodium light is approximately 6×10^{-5} cm., of ultraviolet light $2 - 3 \times 10^{-5}$ cm.

moved from neighbouring objects. In an image of this formed by means of a lens system, there will be a small patch of diffracted light of sensible dimensions; and if the background be sufficiently dark compared with the intensity of this patch, the latter will show up. This will happen whatever the size of the particle, providing the intensity of the patch of diffracted light is great enough.

An analogy will make this clearer. As in the case of the microscope, so in that of the telescope, there is a limit to the resolving power. Here, of course, objects at different distances are observed; the resolving power depends on the angular distance of the object. If two points subtend less than a certain angle at the telescope, their images can no longer be separated; or if an object subtends too small an angle, though it may be visible, it cannot be studied. Now it is well known that there are some stars the forms and details of which cannot be observed even with very large telescopes, yet they may be visible to the naked eye. All that is known concerning them is their colour and brightness, which serve as a basis of classification.

Just as stars are only visible to the naked eye at night, so the small luminous particles considered above will be seen to the best advantage against a dark background; and the smaller the object, the more brightly illuminated must it be to be visible. Moreover, in the telescope there is more light concentrated into any image than there is with the naked eye, and so a greater number of stars is observed through the telescope than without it. In the same way, the microscope by concentrating the light will render visible a greater number of ultramicroscopic particles; for the greater concentration of the light will reveal some particles which could not be seen with the naked eye. Again, the smallest angular distance of two objects which can be separated by the eye is about one minute, and this will set a limit to the distance between two ultramicroscopic particles which can be seen separately. It can be readily understood that with the assistance of the microscope this minimum distance will be diminished, but even with the microscope there will be clusters of particles which cannot be resolved. It is obvious then that in the observation of ultramicroscopic particles the microscope will be of great assistance, not only because of the greater concentration of the light but on account of its greater resolving power enabling us to separate objects which with the naked eye would be irresolvable.

Though there are very few ultramicroscopic particles which are self-luminous,[1] yet they can often be made virtually self-luminous by illuminating them with an intense beam of light. When such a beam is incident upon a small opaque object, diffraction occurs; the shadow cast by such a body is not what would be expected from the theory of geometrical

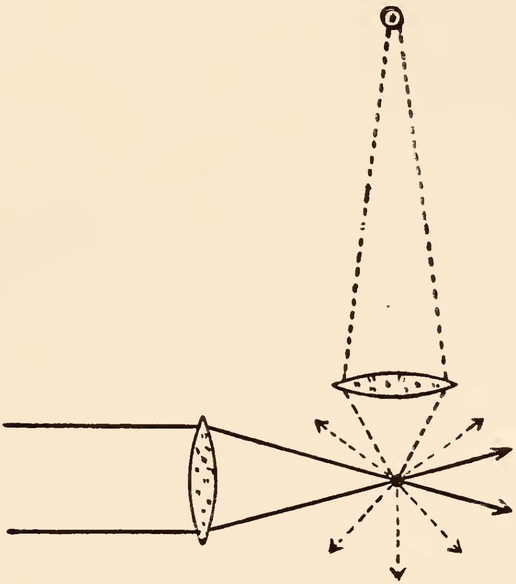

FIG. 1.—Diffraction of light by an ultramicroscopic particle; orthogonal arrangement of the axes of the illuminating (drawn full) and diffracted (dotted) rays.

optics; light is given out in all directions, the intensity of the light depending on the direction. The small object will act like a self-luminous particle, and it may become visible by reason of this diffracted light (see fig. 1).

There are many familiar examples of this action. When a beam of sunlight enters a dark room, the path of the beam is shown by the light diffracted by the small dust particles

[1] A striking example of very small self-luminous objects rendered visible under such circumstances is afforded in the spinthariscope of Crookes. The small particles shot out from the radium salt strike a screen of zinc sulphide and give rise to phosphorescence. The small patches of light stand out on a dark background.

present in the air. The drops forming the clouds produced when air saturated with water or other vapour expands can be rendered visible in the same manner by passing a beam of light through the cloud vessel.

Particles very small compared with the wave-length of the light used can give rise to diffracted light in this way. According to Lord Rayleigh, it is possible that the blue colour of the sky is due to diffraction by the molecules of the air.

If the intensity of the light diffracted by the particle become too small, the latter will no longer be visible. Since this intensity is proportional to that of the incident light, the importance of using a powerful beam, which can be concentrated on the particles, is obvious. The size of the diffracting particle is also important. Lord Rayleigh has shown that the amplitude of the diffracted light is proportional to the volume of the particle; therefore the intensity of the light diffracted by a particle of volume nv will be n^2 times as great as that diffracted by one of volume v. The smaller the particle, therefore, the greater must be the intensity of the incident light. Cotton and Mouton have found that certain particles are only visible when sunlight is used in the middle of a summer day.[1]

The importance of using a dark background has been pointed out above; and in order to achieve this, it is essential that none of the light of the illuminating beam should enter the microscope and that all stray light be excluded. It will be advantageous also to illuminate only those particles which are to be examined; any other particles which may be illuminated will scatter the light, thus reducing the contrast between the particles and the background.

Finally, if the particles are to be seen to the best advantage, too many must not be illuminated.

Necessary Conditions.—Thus there are two essential conditions, which must be fulfilled if the ultramicroscopic particles are to be seen to any advantage, or even at all: (1) a powerful beam of light must be concentrated on them, care being taken that the only light entering the microscope is the diffracted light; (2) the particles must not be too close together.

Apparatus of Siedentopf and Szigmondy.—The earliest form

[1] The intensity of bright sunlight is about ten times that of a powerful arc lamp.

of apparatus satisfying these conditions was invented by Siedentopf and Szigmondy in 1903.[1] The arrangement is shown diagrammatically in fig. 2.

The light from the source—sunlight focussed by means of a heliostat, or an arc-lamp—is focussed by a lens A on to a slit B. By means of C and D a reduced image is formed on the axis of the observing microscope E and in the preparation to be examined. The slit B is adjustable in both directions, and in some the dimensions can be reduced so that the length is about 0·2 mm., the width only 0·05 mm. The lens C and the feeble microscope D gives an image which is only about $\frac{1}{36}$ of the dimensions of the illuminating bundle; therefore the illuminating beam at the centre of the field of the microscope has a minimum width of about 0·003 mm. or 3 μ and a minimum

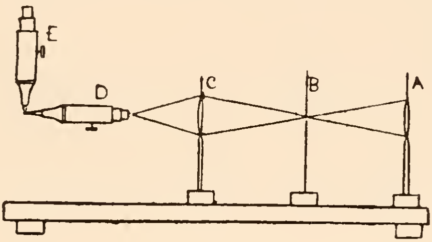

Fig. 2.

depth of 1·5 μ. The intensity of illumination is therefore very great and the only light entering the microscope is diffracted light. The background is therefore dark. For greater efficiency, it is obviously better to arrange that the depth of the illuminated field is equal to that of the microscopic field, which, of course, for microscopes of high magnifying power is very small.

It is often necessary to know the volume observed: this can readily be determined in the following manner. The precision slit is placed horizontally and the width of the microscopic field is read off on the scale in the eyepiece. The slit is then rotated through 90° and a second reading taken; this gives the depth of the layer illuminated. The illuminating bundle is, of course, spread out a little in the field of the

[1] This has usually been recognised; but in the *Phil. Mag.*, April 1909, there appears a letter from Prof. Ranan, of Calcutta, referring to an apparatus of Mr. G. Dubern which he claims is almost identical with Cotton and Mouton's modification of Siedentopf's apparatus. Dubern's papers appeared in the weekly journal *Indian Engineering*, April 7, 14, and 21, 1888.

microscope, but this can be allowed for. Knowing the width and depth of the field, the volume can be determined.

This apparatus of Siedentopf and Szigmondy has the advantage that objectives of immersion can be used, since the fulfilment of the necessary conditions is unaffected if a drop of liquid is run in between the object-glass and the upper surface of the preparation.

Another arrangement satisfying the above conditions, though by a different device, which is especially suitable for the examination of liquids, is that due to Cotton and Mouton. The principle of the method can be readily seen from fig. 3. A convergent beam of light is incident on a block of glass, F—a Fresnel rhomb serves admirably for the purpose. After reflection at the lower surface of the block, the light is brought

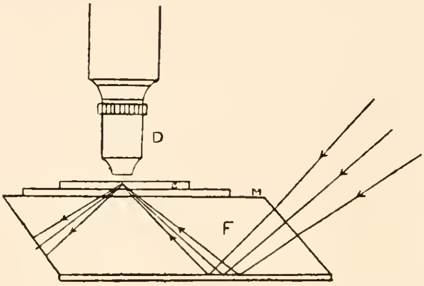

Fig. 3.

to a focus in the preparation C, and the angle of incidence is adjusted so that this light is then totally reflected at the upper surface of the coverslip. Thus the only light entering the observing microscope D will be light diffracted by any particles which break the homogeneity of the preparation.

Since, for greater efficiency, it is advantageous to be able to use all the light entering F, and to allow none of this to enter the microscope D, the angle of incidence on the upper surface of F must lie between the critical angle for a water-glass surface and that for a glass-air surface—that is between 61° and 41°; hence a cone of rays, the angle of which is about 20°, can be used. The inclination of the surface LM is usually such that normal incidence can be used, and in this way very little light is lost by reflection.

This form of apparatus is very easily set up; and a Nernst lamp serves quite well, as a source, for most purposes. It has the disadvantage that it cannot be used with objectives of immersion; for total reflection will no longer necessarily be produced at the upper surface of the coverslip if, instead of air, there is some other fluid.

In the two arrangements described above, the illuminating rays and those employed for observation have their axes at right angles. Though this condition is necessary when very small particles are to be observed, yet, for somewhat larger

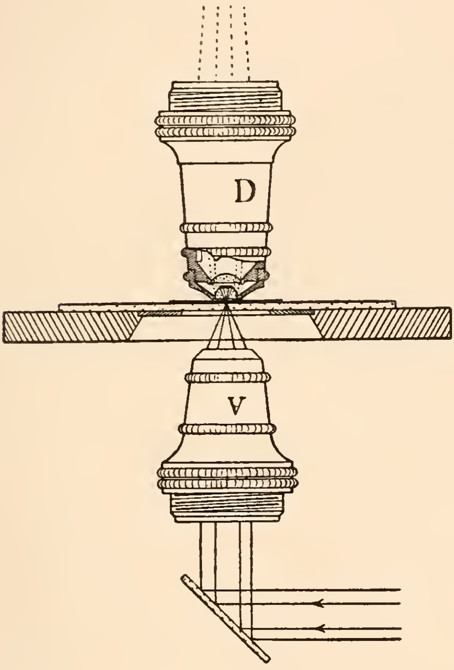

FIG. 4.

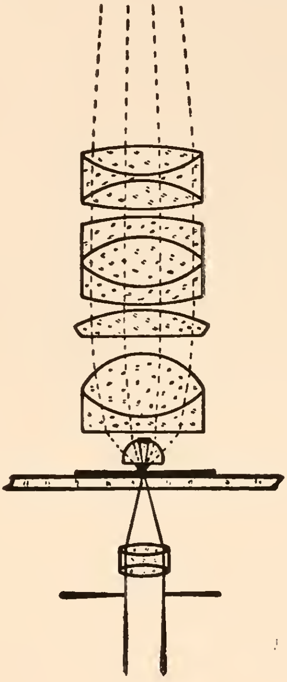

FIG. 4A.

objects, such as cells, fibres and bacteria, a co-axial arrangement can be used. "The arrangement can be made nearly identical with that employed in ordinary microscopic vision, so that a transition from ordinary to ultramicroscopic observation and *vice versa* is at all times easily accomplished."

Figs. 4, 4A, 4B and 5, reproduced with M. Zeiss and Co.'s permission, illustrate two methods now frequently used in the examination of fibres and bacteria. In these figures the illuminating rays are shown by continuous lines, the diffracted rays by dotted lines.

In fig. 4 the incident rays are reflected upwards into the condenser placed below the stage of the microscope; they are brought to a focus in the preparation and then enter the objective. A portion of the back of the first lens of this objective is made plane and blackened. This is chosen so that the whole of the illuminating beam is absorbed, and therefore the only light passing through the objective and reaching the observer's eye will be light which is diffracted in the preparation. From

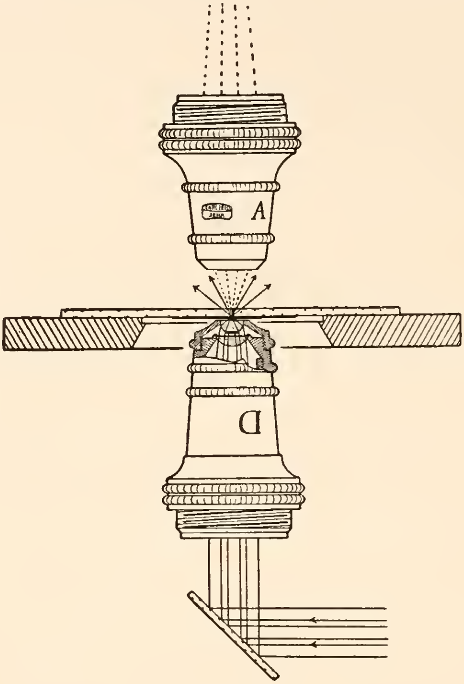

Fig. 4B.

the figure it can be seen that only a portion of the diffracted rays is used, since some are absorbed by the blackened surface of the lens. In Zeiss's objectives, all rays having an aperture larger than 0·2 are available for observation, so that the loss of light is small. It is obvious that it is immaterial whether the objective be a dry or immersion system (fig. 4A).

For observation with low magnifying powers, where the aperture is usually smaller, this method becomes impracticable. The arrangement of M. Zeiss and Co. shown in fig. 4B is more convenient. The illuminating system has the larger aperture, the observing system the smaller; dark ground illumination is produced when the front lens of the illuminating system blocks out the smaller aperture of the observing system from the cone of illuminating rays. The specimen is illuminated with light from apertures greater than 0·2, and diffracted rays are employed up to the same aperture. The functions of the two lens systems can readily be seen from the figure.

Fig. 5 shows Siedentopf's paraboloidal condenser. P is a plano-convex piece of glass, the curved portion of which represents a paraboloid of rotation. B is a central stop and blocks out rays which have an aperture from 0 to 1·1 (Zeiss). The focus of the paraboloid is situated on the upper side of

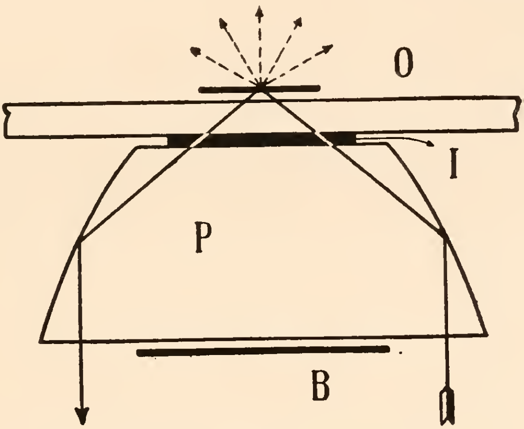

FIG. 5.—Path of rays in the paraboloidal condenser.

the object-glass O. I is the immersion liquid between the slide and the condenser. Dark ground illumination is produced by total reflection at the upper surface of the coverslip when air is above it. With this paraboloidal form better correction for spherical aberration can be obtained, and a greater brightness is produced. It is very convenient for observing, among other objects, very small bacteria, and for taking instantaneous microphotographs of the live bacteria.

Having now described the principles involved and some of the methods and apparatus employed in ultramicroscopy and dark ground illumination, we will pass on to consider some of the applications and the interesting results which have been obtained by its use.

Solids.—(1) *Coloured Glasses.*—One of the first applications Siedentopf and Szigmondy made of their apparatus was the examination of specimens of glass which had been coloured by gold. When molten glass to which a little chloride of gold has been added is allowed to cool, glass of various colours may be produced. If the cooling is very rapid a colourless specimen is obtained; but if the glass is cooled more slowly, and kept at a temperature near its melting-point for some time, coloured varieties are produced. Thus at first a rose-colour is shown; if the cooling proceeds still more slowly, it may be red (the well-known ruby glass), blue, or even brown. The coloured varieties can also be prepared by heating the colourless one.

Faraday, who had prepared specimens in this manner, found that they produced scattering in a beam of light; and he suggested that the coloration was due to the presence of very small particles, which were beyond the range of his microscope.

The German physicists mentioned above showed the truth of Faraday's hypothesis. On examining a piece of ruby glass with the ultramicroscope, a number of bright, circular patches of light are observed, clearly proving the presence of small particles in the glass. The colour of the patches changes from one glass to another: in ruby glass it is green, in blue glass yellow or brown. The size of the particles, which is determined by a method to be described later, differs for differently coloured glasses. In the case of the colourless specimens no particles are revealed by the ultramicroscope, but, on heating, they give rise to coloured varieties which are clearly heterogeneous.

The above authors have suggested that there are particles of gold present even in the colourless specimens, but, on account of their small size, the light diffracted by them is of feeble intensity. Now it was mentioned above that this intensity is proportional to the square of the volume of the obstacle, so that if n particles unite to form one, the intensity of the light diffracted by a single particle is increased in the ratio $n^2 : 1$. Siedentopf and Szigmondy are of opinion that, in the colourless glasses produced by rapid cooling, there are germs of gold present; and on heating, something analogous to distillation takes place, giving rise to larger visible particles, such as are found in ruby glass. We have a similar phenomenon in the formation of large drops during condensation of a vapour, where distillation proceeds from the smaller to the larger drops. The formation of larger crystals in a similar manner may also be noticed.

Size of the Particles.—The magnitude of the particles present in these coloured glasses has been determined in the following manner: The total mass of gold present in the specimen is known from the amount of gold solution added to the molten glass. The number of particles present in the field of the microscope is counted; and since the volume of the field is known (see above), the total number of particles, and hence the mass of each, can be determined. In this process it is assumed that all the particles present in the given volume are visible; that none are too small to be seen, and that no two are so close together as to be indistinguishable. Knowing the mass of a particle, and assuming that the density has the ordinary value and that the shape is some simple figure—for example, a sphere or a cube—the radius of the particle can be determined. The size of the particles depends on the glass examined, the smallest observed having a radius of $3 - 6 \times 10^{-7}$ cm., or $3 - 6$ $\mu\mu$. It is important to notice that no information as to the shape of the particles can be obtained from direct observation of the patch of diffracted light, which usually appears circular. The method of examining the shape of the particles will be discussed later.

(2) *Coloured Crystals.*—Some interesting results have been obtained by Siedentopf by the examination of coloured crystals. Some crystalline substances—*e.g.* fluor spar and rock-salt—occur in several differently coloured varieties. Recent work seems to indicate that the coloration is due to the presence of small particles of matter. Thus, in the case of fluorite, Cotton and Mouton show that the colouring-matter is formed of grains easily visible with the ultramicroscope, arranged in striæ.

The case of rock-salt is interesting. The blue or violet colours of the natural specimens can be produced artificially by heating colourless crystals of rock-salt in sodium vapour. On further heating these crystals become colourless again. Rock-salt can be coloured in many other ways; the change is produced by exposure to Röntgen rays, cathode rays, or to the action of the radiations from radium salts. As a result of a series of experiments on the subject, Siedentopf and Goldstein have come to the conclusion that the cause of the coloration is always the same—the existence of metallic sodium in the form of small particles scattered through the mass. The yellowish specimens are not resolved by the ultramicroscope, but the blue varieties show well-defined particles, according to Cotton and Mouton, of a brownish red colour. They are not arranged so regularly as they are in the glasses already considered; sometimes they form clusters or beads, at other times they are arranged in straight lines, which lie along the cleavage planes. The artificially coloured specimens act just like the natural ones, and can be modified in the same ways.

There is some evidence in favour of the hypothesis mentioned above, that the coloration is due to the presence of particles of sodium. Thus, they are prepared by heating the colourless crystals in sodium vapour; and further, the crystals lose their colour when heated to a temperature near the melting-point of sodium. It may, at first sight, appear strange that sodium should be present, as such, in moist air. Siedentopf supposes that each particle is surrounded by a protecting pellicle, but Cotton and Mouton think that this is unnecessary, and that the phenomenon is explained "by the extreme slowness with which diffusion of the gas occurs in these ultramicroscopic openings." This is supported by some experiments of R. W. Wood. When sodium vapour is condensed in a vacuum tube, films are obtained which present very striking colours. When the pressure rises the film changes, and with it the colour. In Wood's experiments the vacuum tubes communicate with the atmosphere by means of very long, fine capillary tubes. It was found that the rate at which the pressure increased, as shown by the change in colour of the film, was very slow, indicating the slow rate of diffusion in such fine tubes.

Liquids.—Having shown how the ultramicroscope has been successfully applied to the study of solids, such as glasses coloured by metals and coloured salts, we will pass on to the consideration of liquids. Here a new experimental difficulty is encountered. In the case of solids there are practically no particles present in the substance except those to be examined. It is very difficult, however, to free liquids from extraneous particles. Ordinarily distilled water usually contains an appreciable quantity of impurity, and if exposed to the air for any length of time it becomes contaminated, and the number of such particles may equal in number those to be observed.

Very great care is thus necessary in the preparation of pure water. Precipitation of suspended matter is often brought about by adding a small quantity of a colloidal liquid, such as ferric hydroxide; the colloid is then precipitated (see below), when the suspended matter is carried down with the colloidal particles. Biltz filters through unglazed porcelain, filtration through ordinary filter-paper, of course, not being permissible. Malfitano has recently discovered that a film of collodion serves very well for such purposes, and this is now commonly employed.

For a large amount of work, however, it is not necessary (even if it were possible, which is doubtful) to remove all traces of suspended matter. If the portion of the preparation illuminated is in the form of a prism, with base 100 μ square and depth 5 μ, its volume would be 5×10^{-8} c.cm. Now, recently distilled water contains about ten million particles in a cubic centimetre; therefore in the field of the microscope there would probably be only one or two visible. Such a degree of impurity will not affect the observations. Once such liquids have been prepared, care must be taken that they are not contaminated by the vessels into which they are placed.

Experiments on the Rate of Chemical Action.—Many years ago Tyndall performed some beautiful experiments on the scattering of light by small particles. He allowed air to bubble through nitrite of butyl, and then pass into an exhausted tube. In addition, a little air which had passed through hydrochloric acid was admitted into the chamber. A beam of light was passed through the tube; scattering occurred, the colour of the scattered light changing from blue to white. A chemical action takes place between the two compounds, a cloud being formed, the drops which compose it gradually increasing in size; this increase can be followed by the change in the colour of the diffracted light.

Biltz has performed some similar experiments on the formation of precipitates when optically pure aqueous solutions of certain substances are mixed. Here, however, the growth of the particles can be actually observed with the aid of the ultra-microscope. When dilute sulphuric acid or oxalic acid is added to a solution of sodium thiosulphate, after an interval, which may be several minutes, the liquid becomes turbid as a result of the precipitation of sulphur. An appreciable time elapses between the mixing of the solutions and the precipitation of the sulphur. Biltz examined these solutions and found that, though in some instances the ultramicroscope shows the commencement of the action before the liquid appears turbid, yet for a short time nothing seems to happen; and then, quite suddenly, the ultramicroscopic particles appear, increasing in number and brightness. After a time the rings, characteristic of large particles, appear.

It is an interesting question whether action begins immediately the solutions are mixed, or whether there is an interval during which there is no change. The probability is that the particles which are first formed are too small to be seen, even with the ultramicroscope, and it is only after they have grown sufficiently, that they become visible. This period of growth would correspond to the *temps mort* (latent period).

This view is supported by the experimental fact that the presence of small quantities of the substances facilitates the appearance of the particles. Thus, Cotton and Mouton discovered that the interaction of sodium carbonate and calcium nitrate appeared to be accelerated when the action took place in a vessel in which it had previously taken place, the vessel having been washed with distilled water in the meantime. Similar results have been obtained by other observers.

The particles in these solutions may remain in suspension for a comparatively long time. Such suspensions can be obtained in many liquids and may persist for several months or even years.

Colloidal Liquids.—An important class of such liquids is formed by colloidal solutions. Many substances, which in the usual sense of the word are insoluble in water—for example, metallic gold and platinum or arsenious sulphide—can, by appropriate methods, be brought into the form of a solution, which, however, differs in many respects from a true solution. These liquids diffuse much more slowly than solutions of electrolytes, for example, and they do not, or only very slowly, pass through certain membranes which are permeable to electrolytes. Colloidal solutions may contain liquids other than water as the "solvent," and Graham, who was one of the first to investigate the subject, divided them into hydrosols, alcoosols, etherosols, etc.

Hydrosols.—Among the earliest known of the first-mentioned class are metallic hydrosols. Faraday prepared gold solutions by reducing a solution of a gold salt. Another method of preparation now usually employed is that due to Bredig. An electric arc is formed in pure water or other liquid, between terminals composed of the metal in question. A shower of particles is given off from the cathode and these colour the solution. The colour of the solution varies from red to blue, and depends on the current used; the latter affecting the size of the particles produced. If the current is small comparatively large particles are produced, which rapidly fall to the bottom; with larger values of the current, smaller particles are obtained, which remain in suspension for a long time.

In a similar manner hydrosols of silver can be prepared, the colour in this case varying from reddish brown to olive-green. They are very stable indeed, often lasting several years without much change.

Heterogeneous Nature of Colloidal Liquids.—The appearance of these hydrosols under the ultramicroscope is very striking: there is seen a number of brightly illuminated particles, which differ from one another in colour; these are not at rest in the field but display very clearly the well-known Brownian movements. The particles are usually separate but sometimes clusters are observed, the constituent particles often retaining their individual motions.

Returning to the case of ordinary colloidal liquids, such as "solutions" of ferric hydroxide and arsenious sulphide, it is found that they too, on examination with the ultramicroscope, show a lack of homogeneity. Faraday and others noticed that such liquids scatter a beam of light, the scattered light being partially polarised; and the former suggested that it was probably due to the presence of small particles in the liquid. But it was left to Szigmondy actually to demonstrate the truth of this hypothesis. The particles, which break the homogeneity, are of different sizes; they vary from particles approaching a microscopic size, to some which are only just visible with the ultramicroscope, when sunlight is used; and very probably there are some still smaller. The smallest observed by Szigmondy have a radius of only 3—6 $\mu\mu$ or $3—6 \times 10^{-7}$ cm. Like metallic hydrosols, they exhibit the Brownian movements. The number and size of the particles are determined as in the case of solids (see above).

For the examination of liquids, the form of cell shown in fig. 6 is useful. On the slide A a circular plate of glass B is placed. This is surrounded by a ring of glass C, of such a thickness that when a coverslip D is placed on it, a layer of liquid 0·1 mm. in thickness is enclosed between B and D. The pieces of glass are ground very carefully.

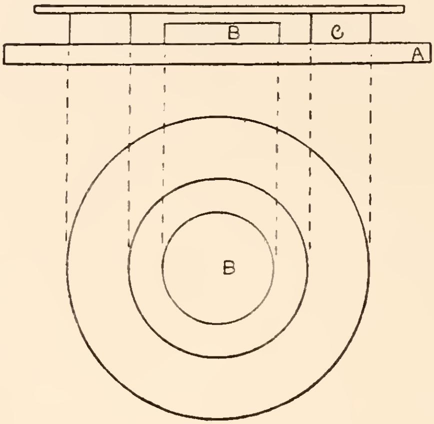

Fig. 6.

Explanation of the Properties of Colloidal Liquids.—This granular structure of colloidal liquids allows us to explain very readily their well-known properties, which may be briefly summarised:

(1) They diffuse very slowly—much more slowly than electrolytes in solution.

(2) They are unable to pass through certain membranes, such as parchment (or only pass through very slowly), which are permeable to electrolytes (crystalloids).

(3) The osmotic pressure is very low, and consequently the changes produced in the boiling-point and freezing-point of the solvent are small.

(4) Such solutions do not give the ordinary chemical reactions of the metals, etc., which they contain.

(5) Colloidal liquids frequently act as catalytic agents, greatly affecting the velocity of a chemical reaction.

The diffusion is connected with the movements of the particles, and when these are so large, we should expect them to be slower. It has been shown that some of these particles have a radius about forty times as large as that of a molecule, which for hydrogen, oxygen, and nitrogen is about 3×10^{-8} cm.; and it can be understood how a membrane, which is permeable to particles of molecular size, may be impermeable to these larger ones.

Brownian Movements

Any one observing hydrosols and other colloidal liquids with the ultramicroscope cannot help being struck by the fact that the small particles are not at rest but are constantly in motion. The movements are quite irregular and neighbouring particles appear to move quite independently; it is only when they come very close together, as they do in some flocculi, that the movement diminishes or ceases. The movements can often be seen with the microscope using ordinary illumination; it was in this manner that Brown first observed them as early as 1830. Later, they were described and studied in some detail by Gouy.

The introduction of ultramicroscopic methods, however, has greatly facilitated their study. This depends on the fact first observed by Gouy, that the movements increase in intensity, as the size of the particles diminishes. With very small particles, in addition to an oscillatory motion, there is observed also a translatory one.

The movements are shown very clearly by the particles in the hydrosols of gold and silver. The particles not only move, but show variations in brightness; at one instant a particle is quite bright and then it disappears, only to reappear again after a short interval. These scintillations are often very conspicuous.

Szigmondy gives a very interesting description of the Brownian movements, after first observing them. He says: "The small particles (of gold) no longer oscillate; they move and with a marvellous rapidity. He who has seen a swarm of flies dance in the sun's rays can picture the movements of the particles of the hydrosol of gold. They frisk, dance and leap, approach and retreat, so that one scarcely knows where one is in this tumult. For the smallest particles, the movements seem to be different from the ordinary Brownian movements (oscillatory); they have a translatory motion as well. After a series of irregular movements, they suddenly traverse the bright field almost as if they were alive, and disappear."

Brownian Movements in Smoke Fumes.—Some interesting experiments have been made by the Italian physicists, Puccianti and Vigezzi, on the Brownian movements exhibited by particles forming fumes or smoke. In some of their work, tobacco smoke, dried by passage over calcium chloride, enters a vessel such as has been described for the examination of liquids. Employing the apparatus of Siedentopf and Szigmondy and using only a small magnification, a large number of bright points is seen; and even after the motion of the gas has ceased, the particles show the ordinary Brownian movements. The movements are very rapid, and if the particles are not too close —if, for example, they are separated by a distance of about 20 $\mu\mu$ or 2×10^{-6} cm.—they show two movements, one oscillatory, the other translatory. The amplitude of the former is often comparatively large, being of the order of 10 μ.

Similar observations have been made with fumes of ammonium chloride; in this case the formation of the chloride from its constituents can be observed.

In addition to these Brownian movements, other movements can be impressed on the particles. Thus Puccianti and Vigezzi connected their vessel with a Helmholtz resonator, and were able to observe the effects of sound vibrations on these suspended particles. It was found that they were set into vibration, each bright spot becoming drawn out into a line of light; and it has been suggested that, in this way, some information might be directly obtained as to the magnitude of the movements in a sound-wave. It will be seen later that these movements are very similar to those of the particles of a colloidal solution, under the influence of an alternating electric field.[1] These experiments are important, because our knowledge of the kinetic theory of gases is much greater than in the case of liquids and solids. We will now pass on to consider the evidence as to the nature of the Brownian movements.

Explanation of the Brownian Movements.—At first it was thought that these movements were due to convection currents

[1] While speaking of these particles in gases, it may be mentioned that de Broglie (1908) has recently used the ultramicroscope in the examination of the large ions, which are present in the atmosphere.

in the liquid, but Gouy clearly showed that this could not be their cause. When every precaution is taken to avoid these currents, the movements are as pronounced as ever. Moreover, the motions of neighbouring particles are usually quite independent. Hence some other cause must be sought.

It has been mentioned above that the intensity of the movements increases as the particles diminish in size, and Gouy further showed that it also increased as the temperature rises. These facts remind us of the movements of the molecules of a gas on the ordinary kinetic theory of gases, which increase with temperature and are more intense, the smaller the mass of the molecule; and the above author suggested that the Brownian movements are due to collisions between the molecules of the liquid and the particles in suspension.

It is generally accepted that the molecules of a substance are in rapid motion. At intervals collisions occur between the different molecules, and as a result of these collisions their velocities are continually changing. If there are particles in suspension in the gas or liquid, it can be imagined that the molecules in the course of their excursions collide with them; and as a consequence the particle is set in motion. Now these particles in suspension are many times larger and heavier [1] than the molecules which collide with them, so that, though the molecules may be moving, on the average, with very great velocity (in hydrogen at 0°C, for example, the velocity is 1·84 kilomètres per sec.), the velocity acquired by the suspended particle may be small.

Again, although a particle may, in a given short interval of time, be struck by a large number of molecules, it cannot *a priori* be assumed that there will be any co-ordination between the individual effects, so that the resultant effect may be small. Further, we have seen that the particles make excursions of the order of 10 μ, distances which are large compared with the mean free path of the molecules of the gas or liquid.[2] Gouy assumed that there did exist some co-ordination between the collisions, so that a large effect might eventually be produced.

[1] A particle of gamboge in a colloidal solution of that substance has a mass 10^9 times as large as that of a molecule of hydrogen.

[2] In hydrogen at normal temperature and pressure, the mean free path is about 0·2 μ; and the distance between the molecules of water at ordinary temperatures is probably of the order 1 $\mu\mu$.

However, this was only an interesting hypothesis, with very little to support it.

A short time ago, M. Einstein worked out the problem of the motion of a particle due to collisions with the molecules, assuming that these collisions were not co-ordinated at all. He arrived at the conclusion that the mean path, which a particle can traverse in a second, is given in centimetres by the relation

$$l^2 = RT/3\pi N r k,$$

where r is the radius in cm. of the particle which is supposed to be spherical, T is the absolute temperature, N the number of molecules in one gram molecule (4×10^{23} approx.), k is the coefficient of viscosity ($1{\cdot}00 \times 10^{-2}$ for water at 20°), and R is the gas constant ($8{\cdot}4 \times 10^{7}$). Substituting these values for the letters, and considering particles of radius 1 μ or 10^{-4} cm, we find that during one second the particle can move over a mean distance of about 0·8 μ. It has been mentioned that the particles of the hydrosols of gold and silver may have a radius of only 3 to 6 $\mu\mu$; for such particles the mean path would be about 8 to 10 μ. The amplitude of the oscillations for some particles was seen to be about 10 μ, and thus the order of the values deduced by Einstein is the same as that obtained by experiment. Recently Chaudesaigues (November 1908), Henri (1908), and Seddig (1908) have made experiments to test Einstein's formula, and their results seem to support his work.

Perrin's Work.—It is only within the last year, however, that any direct experiments have been made with a view to throwing some light on the nature of the Brownian movements. In 1908 M. Jean Perrin experimented with colloidal solutions of gamboge, which show the Brownian movements very clearly.

The purpose of his experiments was to show that the cause of the Brownian movements lies in the molecular agitation of the liquid—Gouy's hypothesis—and in that alone; and what is perhaps more important, the experiment furnishes a new and more concise method of determining the number of molecules in a gram molecule of a substance.

If a stick of gamboge is placed in water a liquid is obtained containing grains in suspension, the grains being visible under the microscope with ordinary illumination. By employing a centrifugal force, as one does in the separation of blood corpuscles, Perrin obtained a liquid in which the particles were ultramicroscopic, and could no longer be seen with ordinary illumination. The particles do not show any scintillations, and, as will be seen later, are very probably spherical. After dilution a small quantity of the emulsion was placed in a microscopic preparation, the thickness of which was fixed and equal to about 0·12 mm.

Distribution of the Granules.—The distribution of the granules in this liquid was carefully studied, an interval being allowed in order that a steady state might be reached; and Perrin found that the weights of the particles caused them to concentrate in the lower horizontal layers; and as we ascend in the liquid the concentration of the granules diminishes.

In a freshly prepared solution the particles were observed to fall towards the bottom of the solution at a measurable rate. The concentration of the granules in these lower layers increased until, eventually, a steady state was arrived at, when the concentration varied with the level in the liquid in a very striking manner.

The number of particles present in a given volume—that of the field of the microscope—was determined at various levels, differing from one another by about 25 microns. This process of counting is extremely tedious, and Perrin found it necessary to make several thousands of observations. Owing to the movements of the particles the chance of any observation being correct is not very great; and it is only by taking a large number that an accurate value can be obtained.[1]

Below are given the concentrations, in arbitrary units, of the granules at heights differing from one another by 25 μ, the concentrations being given in the second line. In the third line are given the values these would have if the variation occurred in geometrical progression.

Height in μ	100	75	50	25	0
Number of particles	100	116	146	170	200
Number, if variation were in geometrical progression	100	119	142	169	201

It will be observed that the experimental values agree,

[1] During the process of counting the number present in the given volume may change, some particles entering or leaving; however, by taking a large number of observations this can be allowed for.

within the limits of experimental error, with the numbers representing the geometrical progression. Thus Perrin concludes that the distribution of the granules in the preparation, and probably in all colloidal solutions, is such that, as the height changes arithmetically, the concentration changes geometrically; in other words, the distribution is exponential. If a curve be drawn, in which the abscissæ represent heights in the liquid and the ordinates the logarithm of the concentration, a straight line is obtained.

Now it is known that in a gas in equilibrium under gravity a similar distribution obtains, the density varying exponentially with the height. We have a familiar example in the atmosphere, the density of which falls off according to the same law as the height increases. Here the concentration falls to half its value in a distance of 6 kilomètres; in the solution this occurs for a change of level of only 0·1 mm. The distribution in this case is readily explained by considering the partial pressure of the gaseous molecules.

Explanation of Distribution.—Perrin explains this law of distribution in a similar manner. The granules are supposed identical (probably not quite correct); they are in motion, and by their collisions against the walls which arrest them they exert an osmotic pressure proportional to their concentration. The osmotic pressure will thus vary with the height; and by considering that the granules in a given small volume are kept in suspension by the sum of the thrust of Archimedes, and the difference of osmotic pressure on the two faces, he deduces a relation giving the variation of concentration with the height. In this calculation Perrin assumes that the granules behave like the molecules in a perfect gas.

He finds the relation $2{\cdot}3 \log n/n_0 = \text{mgh}\ (1 - {}^1/\rho)/k$, where n_0 and n are the number of particles in unit volume at the standard height, and at another height h, respectively, m is the mass of a particle, ρ its density, g the acceleration of gravity, and k is the average osmotic pressure due to each particle. This would represent an exponential distribution such as is found experimentally.

So far there is a qualitative agreement between Perrin's hypothesis and the experimental results. It still remains to consider whether the quantitative agreement is equally good.

It can be seen from the above relation that if the concentrations n and n_0 for heights differing by h, and the mass and density, m and ρ, of the particle are known, the value of k, the average osmotic pressure exerted by each particle, can be calculated.

We have seen how the values of n and n_0 were determined; the method of finding m has still to be discussed.

Determination of the Mass of a Particle.—A body falling in vacuo has an acceleration of 32 feet per second, and in air bodies of high density usually fall with the same acceleration. Considering the case of small particles, however, certain differences arise. The viscous resistance of the air becomes important, and tends to prevent the increase of the velocity. A raindrop as it falls has its velocity increased; at first rapidly, because the weight exceeds the resisting force, and then more slowly, until a maximum value is reached, when the change of potential energy is equal to the work performed against viscous forces. It is this resistance, due to the viscosity of the air, which accounts for the comparatively small velocity of such a drop when it reaches the ground.

Sir George Stokes has shown that in the case of a sphere of radius a, moving with a small uniform velocity, V, through a fluid, the coefficient of viscosity of which is μ, the force resisting the motion is equal to $6\pi\ \mu\ a$ V. Now the weight of the sphere is equal to $4\pi\ a^3\ (\rho-\sigma)\ g/3$, where $d-\sigma$ is the excess of the density of the sphere over that of the fluid, and g is the acceleration due to gravity; and hence we have the relation

$$a^2 = 9\mu V/2g(\rho - \sigma).$$

The velocity is determined by observing the rate of fall in a capillary tube; in one of Perrin's experiments this was 0·97 mm. per day. Knowing V, and the values of μ, g and $\rho-\sigma$, we can calculate the radius a of the particle, and hence its mass m.

All the terms, except k, in the above expression being known, the value of the latter can be determined. From his experiments Perrin finds the value $k = 40 \times 10^{-15}$.[1]

The number of molecules N_0 *in a gram molecule of a substance.*—The average pressure exerted by each granule in unit volume

[1] Perrin gives two values. Duclaux has pointed out some errors in Perrin's first paper, in which the value $k = 36 \times 10^{-15}$ is given. Perrin, in his second paper, gives the value 40×10^{-15}.

was found to be 40×10^{-15} dynes. Consider a gram molecule of the substance occupying a volume of 22·4 litres at normal temperature and pressure. If there are N_0 molecules in a gram molecule, the concentration will be $N_0/2{\cdot}24 \times 10^4$, and therefore the pressure exerted should be equal to $N_0 \times 40 \times 10^{-15}/2{\cdot}24 \times 10^4$. According to Perrin's view this pressure must equal the atmospheric pressure, viz. $1{\cdot}013 \times 10^6$ dynes, and hence we have :—

$$\begin{aligned} N_0 &= \frac{\text{volume of 1 gm. molecule} \times \text{press. 1 atmosphere}}{\text{press. due to 1 molecule}} \\ &= \frac{2{\cdot}24 \times 10^4 \times 1{\cdot}013 \times 10^6}{4 \times 10^{-14}} \\ &= 5{\cdot}66 \times 10^{23}. \end{aligned}$$

Thus we arrive at the conclusion that the number of molecules in a gram molecule of a substance is of the order $5{\cdot}6 \times 10^{23}$.

This is the result arrived at by Perrin, making the above assumptions. How does it compare with values determined by entirely different methods? Without dwelling longer on the subject, it may be said that the value agrees very well indeed with these values, which vary from 4·3 to $9{\cdot}6 \times 10^{23}$.

This agreement between Perrin's value, and the value obtained by entirely different methods is very striking, and to quote Perrin's words: "It would therefore seem that the granules in suspension in a colloidal liquid function like the invisible molecules of a perfect gas with a molecular weight of about $3{\cdot}3 \times 10^9$."

Under such circumstances, the mean kinetic energy of a colloidal particle would be equal to that of a molecule, and therefore the Brownian movements could be explained as due solely to molecular agitation as was suggested by Gouy.

There has been some criticism of Perrin's work by Duclaux and others. Among the objections which have been raised are the following:

Duclaux has urged that the gamboge enters partly into a true solution, and does not form a colloidal liquid at all. Again, it has been objected that the method of Stokes cannot be applied to find the mass of these very small particles. Against this may be placed the fact that Perrin has since used three different methods, and has experimented with particles of very different sizes, with the same result.

However, as his full paper has not yet appeared, and until the work has been repeated, it is very difficult to criticise it. It is certainly of great interest, and is a distinct advance towards the solution of the problem of the Brownian movements. Moreover, it furnishes a concise method of finding N_0, the number of molecules in a gram molecule, a method the accuracy of which can, apparently, be pushed to any limit we please.

When the value of N_0 is known, some other important physical constants, such as the value of the atom of electricity—the charge of a corpuscle or of an ion—can be readily determined.[1]

Electrical Properties of Colloids

Towards the middle of the last century, Faraday discovered that when an electric current passes through solutions of different salts, the amount of an element deposited or liberated in a given time is proportional to the chemical equivalent of the element. Thus, when the same current passes through solutions of silver nitrate and copper sulphate, the amounts of silver and copper deposited, in the same time, are in the ratio 108 : 32·5. Later work, combined with this, has led to the idea that the current in such a solution is carried by the ions that are present. Thus in a solution of silver nitrate there are ions of silver with a positive electric charge, and ions of the radical NO_3 with a negative charge. Under the influence of the electric force, these move in opposite directions; the negative ions against the current, the positive with it.

In the case of colloidal liquids there are many differences. The conductivity of such liquids, when no electrolytes are present, is very small; and further, the colloid usually moves as a whole, either towards the cathode or the anode.[2] The former class, known as positive colloids, includes the mineral hydrosols of solutions of metallic hydroxides, ferric hydroxide, for example, and some dyes, such as night blue; the latter, known as negative colloids, includes many metallic hydrosols, such as those of platinum, gold, and silver, and arsenious sulphide. The movement of the colloid as a whole, towards one or other of the electrodes, can be very simply shown by employing Lodge's moving boundary method, or Whetham's modification of it; the surface of separation of the two liquids, in the latter case, moves when a current passes.

[1] Electrolysis shows that $Ne = 29 \times 10^{13}$ electrostatic units.

[2] Some colloids—a neutral solution of serum globulin, for example—do not move under the influence of an electric force; they appear to be neutral.

It becomes interesting to see whether these electrical properties are properties of the particles, which as the ultramicroscope has shown, break the homogeneity of the liquids. A very convenient arrangement for this investigation is shown in figs. 7a and 7b. C is the slide, on which are fastened two pieces of platinum foil, AA. BB are a couple of thin pieces of worked glass, and D is a coverslip. BB with AA form a shallow trough. To further define the cell, two pieces of glass are sometimes placed at right angles to BB. Platinum wires are sealed to AA, and serve to connect with the source of potential.

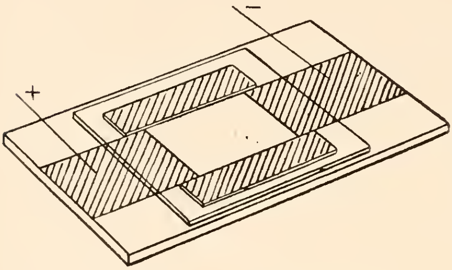

Fig. 7 a.

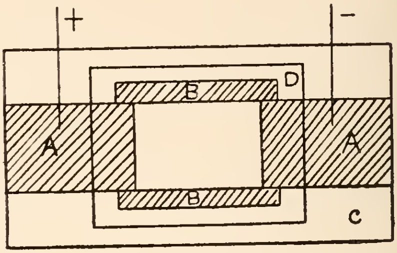

Fig. 7 b.

The preparations used should have a thickness of at least 0·1 mm. (see below).

If a little gold or silver hydrosol is placed in the cell and viewed with the ultramicroscope, the usual bright points of light are observed, which show the characteristic Brownian movements. On connecting with the source of potential, the particles are observed immediately to begin to move in parallel directions; there seems to be no lag between the application of the electromotive force and the commencement of the translatory movements. The motion is uniform, and the velocities can easily be determined, by observing the time taken by the particle to cross the field, the length of which is known. Thus, in an experiment of Cotton and Mouton, it was found that the time of passage was 20 seconds, the field having a length of 1·3 mm., and the potential gradient being 2 volts per mm. The velocity is proportional to the electric force,[1] and in this way the mobilities, or velocities under an electric force of 1 volt

[1] This usually obtains for particles moving against viscous forces, providing the velocity is not too great.

per cm., can be determined. In the above example, this velocity would be $3{\cdot}25 \times 10^{-3}$ mm. per second.

The Inverse Layers.—Using a thick preparation (about 0·1 mm.), and observing at some distance from the walls, all the particles of a given colloid move in the same direction; this direction is reversed when the field is reversed; for positive and negative liquids the movements are in opposite directions.

Leaving the central portion of the liquid, and observing parts near the walls, some differences are found. Here the granules move in a direction opposite to that taken by the rest of them, whatever the nature of the colloid. The thickness of this "inverse layer," as it is called, depends on the thickness of the preparation, and increases as that of the latter diminishes.

Alternating Electromotive Zones.—Returning to preparations showing the normal movements, it was mentioned that the direction of motion of the ultramicroscopic particles is reversed when the potential difference is reversed. It is interesting to observe the effect of reversing this potential at short intervals, which may be done by using an alternating electromotive force. The magnitude of the force in this case will usually be continually changing, and at intervals the force reverses sign. Under these conditions the spot of light seen in the microscope is drawn out into a band of light, slightly thickened at the ends, and somewhat resembling a dumb-bell.[1] This linear appearance is, of course, due to persistence of vision, the particle performing linear oscillations parallel to the electromotive force. If the preparation is rapidly moved at right angles to these lines, the lines are changed into curves.

This vibratory motion, which has its maximum amplitude in the central part of the liquid, decreases as we approach the walls, and may be entirely eliminated. Knowing the mobilities of the particles, the amplitude under a given alternating force of known frequency can be calculated, and it is found that the value thus obtained agrees very well with the observed values.

The Form of the Ultramicroscopic Particles.—As has been mentioned above, from the appearance of a spot of light (nearly always circular) seen in the microscope we cannot at once determine either the size or the form of the particle. The method of determining the size has been discussed above.

[1] Compare the movements of smoke particles under similar conditions.

Our knowledge of the form of the ultramicroscopic particles depends on the polarisation effects of them. The light which is diffracted by the granules is polarised to some extent, and on the amount of this polarisation and the direction of its plane depends the appearance of the particles. These two quantities are determined by the shape and position of the object. The theory of the methods and the experimental results obtained are somewhat complicated, but it may be stated that Siedentopf, Garnett, and others find that in the case of films coloured by gold the particles act as though they were spherical. Those occurring in crystals of rock-salt, in colloidal liquids, and large particles in general, appear to be in the form of needles. This explains the scintillations of the particles in a colloidal liquid. As a result of the Brownian movements the orientation of the particle is constantly changing, and the intensity of the light diffracted in a given direction is thus changed. This would give rise to scintillation.

Again, just as glass can be made doubly refracting to light by straining (Kerr effect), so colloidal liquids can be caused to become birefrigent and dichroic by exposure to a transverse magnetic field. When certain colloidal liquids are placed in a strong magnetic field and observed in a direction at right angles to this field they act as birefrigent substances, the velocity of light through it depending on the direction of the vibrations in the beam. This phenomenon can be more readily understood if the particles are elongated, and are orientated under the influence of the field.

We have confined ourselves chiefly to the study of solid substances and colloidal liquids, and it is perhaps in these physical and chemical branches that the ultramicroscope has been of most service. But there are many other directions in which it has been applied. Aggazzotti and others have employed it in the study of fibres, living cells, and bacteria; but the advance made here is, as yet, not so great, and reference must be made to the original papers for the details of the work.

The use of the ultramicroscope and of dark ground illumination is as yet in its infancy. In the time that has elapsed great strides have been made, and it may confidently be expected that in the near future it will be of further service, not only in the interests of pure science, but also in its applications, and so more directly to humanity.

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RECENT WORK ON THE DETERMINATION OF SEX

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At the Dublin meeting of the British Association the sections of Zoology and Botany devoted a morning to a joint discussion on the Determination of Sex. Some account of the opinions expressed has appeared in reports of that meeting.[1] As in all discussions on this subject, the speakers were divided into two groups holding opinions which at first sight appear irreconcilable. On the one hand there was the school which maintains that sex is a property of the germ-cells and that after fertilisation, if not before, the egg is irrevocably committed to one or the other sex; on the other there were representatives of the influential body of biologists who prefer the view that sex can be influenced by external conditions and that the sex of any individual is the result of a combination of forces, some tending in one direction, some in another. Not many years ago the latter was the prevalent opinion; it was supposed that the fertilised ovum was potentially bisexual, maleness being introduced by the spermatozoon and femaleness by the egg, and that the sex of the developing organism was determined by a variety of factors, the resultant of which decided to which side the balance should incline. This idea was supported by experiments on feeding the larvæ of insects, frogs, etc., in which it appeared that insufficient diet led to a higher proportion of males than when the creatures were abundantly fed. But critics have always pointed to the fact that these results might be explained by differential mortality or other circumstances not allowed for by the experimenters; as we shall see, there is such a mass of evidence accumulated on the other side that this idea is now largely abandoned by biologists.

But although the doctrine that sex may be influenced by

[1] *Nature*, October 22, 1908, vol. lxxviii. p. 647.

the environment of the embryo or larva is largely discredited, a considerable body of evidence has been brought forward to show that influences acting on the parents, particularly on the mother, before fertilisation, may affect the sex of the offspring; this idea is not open to the objections which appear fatal to the older view. Two of the most convincing pieces of work supporting this conclusion are those of Issakowitsch on the Daphniidæ [1] and von Malsen on *Dinophilus*.[2] Issakowitsch worked with the parthenogenetic females of *Simocephalus*, von Malsen with *Dinophilus apatris*, in which the eggs are fertilised; each found that differences of temperature caused differences in the proportion of males produced and both ascribed the difference to changes in the nutrition of the mother. Maupas [3] and Nussbaum [4] made somewhat similar statements about *Hydatina senta*, in which all females are from birth either male-producing or female-producing; but according to them the sex of the offspring of a parthenogenetic female is determined by the conditions of temperature or nutrition to which that female is subjected in the parental uterus. Punnett [5] denies that temperature or nutrition has any effect in the case of *Hydatina* and says that some stocks give rise to many arrhenotokous (male-producing) individuals, others to few or none. So it seems not impossible that in this case at least the evidence for the influence of environment may not be as good as it appears at first and that some such cause as differential mortality may bring about the results observed.[6]

The idea that various external circumstances may influence the proportion of the sexes does not rest only on experiments on invertebrates; there is a considerable mass of statistics pointing in the same direction in the higher vertebrates, including man. In these cases the number of young produced by one pair is relatively small and in most cases the evidence takes the form of figures drawn from a considerable population. The differences

[1] *Biol. Centralblatt*, xxv. 1905, p. 529; and *Arch. Mikr. Anat.* vol. lxix. 1906, p. 223.

[2] *Arch. Mikr. Anat.* lxix. 1906, p. 73.

[3] *Comptes Rendus*, cxi. 1890, pp. 310, 505.

[4] *Arch. Mikr. Anat.* xlix. p. 227.

[5] *Proc. Roy. Soc. B.* lxxviii. 1906, p. 223.

[6] See Whitney, *Journ. Exp. Zoo.* v. 1907, p. 1, for experiments on *Hydatina*, explaining Maupas' results.

due to altered environment or other circumstances usually amount to only a few per cent.; but if they are consistent in a large population they must be taken into account in any theory of sex-determination. There is a vast number of papers of this kind, suggesting that a great variety of external circumstances may affect the percentage of the sexes among the offspring born; among these we may choose a few of the more recent as examples of the kind of result obtained. Pearl[1] shows that of over 200,000 births in Buenos Ayres, the proportion of males is significantly greater when the parents are of different racial stocks than when they are of the same. The difference ranges from about one to about five per cent. but is always on the same side. Punnett[2] finds that in London the proportion of males is lowest in the poorest portion, highest in the wealthiest and intermediate in the intermediate portion. The males per 100 females were respectively 99·5, 102·2 and 100·7; but he points out that these differences are probably wholly explicable on the grounds of differential infant mortality, birth-rate and probably marriage-rate. Heape,[3] from statistics of over 17,000 greyhounds, concludes that whilst males are always considerably in excess (averaging 118·5 to 100 females), the proportion is noticeably higher in the season during which fewest pups are born. In a later note in the same volume (*loc. cit.* p. 201) Heape gives some curious figures with regard to canaries, showing that in one aviary (out of 200 birds hatched) the ratio of males was about 77 to 100 females, while in another (out of 68 birds) the males were in the ratio of 353 to 100 females. Evidence is given that these differences are not ascribable to mortality; Heape supposes that in both cases the proportion of the sexes is due to a selective action of conditions on the ova which are matured. He assumes that ova bear either maleness or femaleness and that some forms of environment favour the maturation of one kind, some of the other. The same explanation is applicable to other cases in which the proportion appears to be influenced by external circumstances.

It appears therefore that the idea that the proportion of the sexes may be influenced by conditions acting on the parents is not inconsistent with the hypothesis that the germ-cells bear

[1] *Biol. Bulletin*, xv. 1908, p. 194.
[2] *Proc. Camb. Phil. Soc.* xii. 1903, p. 262.
[3] *Ibid.* xiv. 1907, p. 121.

only one or the other sex, as long as the primary germ-cells do not all come to maturity. This is the case at least in the females of the higher vertebrates; and it is from them that the greatest amount of evidence in this direction has been obtained. Russo[1] has recently maintained that treatment with lecithin causes an increase in the number of female ova matured in the rabbit and believes that he can distinguish the male from the female eggs in the ovary. He admits however that the families in his tables are selected and it seems that the differences he observed between the two kinds of eggs were probably due to degenerative changes in some.

One of the first writers to maintain that the ova bear either maleness or femaleness was Beard,[2] who suggested that originally there had also been two kinds of spermatozoa but that one has disappeared in most animals, remaining in a functionless condition in such cases as *Paludina* and *Pygæra*, which give rise to the two kinds. His paper is somewhat speculative; but there is a steadily accumulating body of proof that the sex is irrevocably decided at least from the moment of fertilisation. This belief is supported by a number of different facts. It has long been known that in man "identical twins" are always of the same sex, *i.e.* that when twins are born so like one another that they are distinguished with difficulty, they are never of different sexes; in these cases the twins are produced by the division of one fertilised ovum and during fœtal life are enveloped in the same membrane.[3] Twins produced by the simultaneous development of two ova are not more like each other than other brothers and sisters, and are frequently of different sexes. A similar but perhaps even more conclusive case is provided by the parasitic Hymenopterous insects in which there is embryonic fission. Silvestri[4] has investigated two such insects, *Litomastix* and *Ageniaspis*. In each the flies lay their eggs in the eggs of other insects, and at the close of segmentation the embryonic cells become clustered into groups, each of which produces a separate embryo. In *Litomastix* the number of larvæ so produced may be about a

[1] *Atti Acad. Lincei*, xvi. 1907, p. 362.

[2] *Zool. Jahrbücher*, *Anat.* xvi. pp. 615 and 703, and other papers.

[3] See Galton, *Human Faculty*, 2nd ed. (J. M. Dent), p. 156.

[4] *Annali R. Scuola Agric. Portici*, vi. 1906, and *Bollettino R. Scuola Ag.* iii. 1908.

thousand; in *Ageniaspis* ten to twenty; but if only one egg be laid by the parasite in the egg of the host, all the flies which hatch are of the same sex. Similar cases of embryonic fission in parasitic Hymenoptera have been described by Marchal,[1] with the same results in respect of sex.

Another line of argument tending in the same direction is drawn from animals which have more than one kind of egg, in which eggs of one kind produce males, those of the other females. Some such cases occur among parthenogenetic species, *e.g.* the rotifer *Hydatina*, and *Phylloxera* among insects; but in other animals both kinds of eggs require fertilisation and the larger always yield females, the smaller males. This has been shown to be the case in *Dinophilus apatris* by von Malsen (*loc. cit.*), in the mite *Pediculopsis* by Reuter[2] and is suspected by Montgomery in a spider.[3] In these cases there can be no question of modifying the sex by external circumstances after the egg is fully formed; but it might perhaps be maintained that the very fact of one kind of egg being larger and having more yolk was the cause of its becoming a female.

Probably the most convincing proof that the sex is irrevocably determined from the beginning of development is obtained from the study of cases in which the same eggs may be either parthenogenetic or fertilised; the best-known example is the honey-bee. In this insect, as is well known, unfertilised eggs yield males and fertilised eggs females, either queens or workers according to the treatment to which the larva is subjected. This statement has several times been denied but the facts are overwhelmingly in favour of its truth in the hive-bee; and numerous other examples are now known among the Hymenoptera. As examples we may quote the work mentioned above by Silvestri on *Litomastix* and *Ageniaspis*, in which the developmental processes are precisely similar whether the egg be fertilised or not; but in the first case females are produced, in the second males. Similarly Wassiliew[4] found in the parasitic Hymenopteran *Telenomus* that all eggs laid by virgin females became males, whilst those of fertilised females yielded about 80 per cent. of females. It may be

[1] *Arch. Zoo. Exp. und Gen.* (4), ii. p. 257.
[2] *Festschrift für Palmén*, 1905–7, vol. i.
[3] *Journ. Exp. Zoo.* v. p. 429.
[4] *Zoo. Anzeig.* May 1904.

assumed that the remaining 20 per cent. received no spermatozoon. In instances of this kind it is perfectly clear that the sex is definitely determined at fertilisation; but it cannot be supposed that the egg bears irrevocably one sex or the other before the entrance of the spermatozoon. As will be seen below, it has been assumed by some writers on the subject that the egg before fertilisation bears maleness, and that the female element is introduced by the spermatozoon; but it is at least conceivable that the unmatured egg potentially bears both sexes and that the presence of the spermatozoon determines which sex-determinant shall become effective.

Before leaving this part of the subject it should be noticed that there are a number of Hymenoptera which are anomalous in this respect. In ants and wasps, workers sometimes lay eggs, said always to yield males,[1] so falling into line with the bee; but Reichenbach[2] states that the workers of a species of ant produced workers, except at the time of year when males are normally produced in the nests, when males appeared. It is of course possible that there was error of observation; but there is no doubt that in the sawflies some species produce males, a few mixed broods, some only females from unfertilised eggs. The case of the common *Nematus ribesii* is remarkable; males are usually yielded by virgin eggs but a small proportion of females (less than one per cent.) is generally obtained. These may possibly be introduced by accident, since the species is so abundant; but the writer[3] concluded on cytological grounds that there were two kinds of eggs: one requiring fertilisation and yielding females, the other developing into males without being fertilised. But more recent experiments (made with the help of Mr. A. C. Tunstall, and not yet sufficiently extensive) do not support this idea. Out of two broods—one of 58 eggs, the other of 102—53 and 67 male pupæ or adult larvæ were reared, which clearly indicates that the absence of females was not caused by their dying off; for among the eggs of fertilised females at least 50 per cent. commonly yield females. It was not possible in these experiments to hatch out the flies; but the size of the adult larvæ

[1] Field, *Biol. Bull.* ix. 1905, p. 355; Marchal, *Arch. Zoo. Exp. und Gen.* (3), iv. p. 1.

[2] *Biol. Centralblatt*, xxii. p. 461.

[3] Doncaster, *Q.J.M.S.* vol. li. 1907, p. 101.

or pupæ is an almost unfailing criterion of the sex. It seems probable therefore that *Nematus ribesii* must be placed in the same category with the bee. The gallflies (Cynipidæ) offer another anomalous instance; for in them there are two generations in the year, one of which consists of both sexes, of which all the eggs are fertilised and yield a parthenogenetic generation consisting wholly of females. The eggs of the latter yield both males and females but it is not known whether there are any differences in the maturation or development corresponding with the difference of sex.

One more point must be mentioned here. In bees some hives produce a large proportion of gynandromorphic individuals, which are irregular mixtures or mosaics of male and female characters. Von Siebold[1] described such a case and found that all the "Zwitterbienen" were in worker cells, the drones being all pure. The hive was hybrid from Italian stock crossed with black; the drones were of the Italian type, the workers mixed. Two possible explanations may be hazarded; first, that the egg which develops into a gynandromorph has begun to segment and that the male pronucleus conjugates with one of the segmenting nuclei; or secondly, that the male pronucleus conjugates with one (probably the second) of the polar nuclei, and that both the zygote nucleus so produced and the female pronucleus take part in the development. Of these the second is perhaps the more likely hypothesis.

It has been mentioned above that Beard was one of the first to suggest that the germ-cells bear the determinant for one or the other sex; it now remains to discuss the evidence which has since accumulated in favour of that hypothesis. It has received support on several very distinct grounds. We may take first the cytological results with which the names of several American investigators are chiefly associated, although much similar work has been done in Germany, France and elsewhere. To give an at all adequate account of the numerous papers on spermatogenesis and oogenesis, which have led to the hypothesis that the sex-determinant is a visible chromosome-like body, would occupy more space than is available; so we will take the work of E. B. Wilson as typical, although similar phenomena had been

[1] *Zeit. Wiss. Zoo.* xiv. p. 73.

already observed by Paulmier, McClung, Miss Stevens and others. Wilson,[1] working at a number of genera of Hemipterous insects, finds that in the unreduced germ-cells of the female there are always an even number of chromosomes, two of which ("idiochromosomes") are frequently distinguishable from the remainder by their size. In the males there is either an odd number, owing to the absence of one idiochromosome, or one idiochromosome has the size which it has in the female, while the other is vestigial. At the reducing division the number is halved; when both idiochromosomes are present they pair together and become separated into different daughter-nuclei; when in the male there is only one, it passes to one end of the spindle and the other is left without one. In this way it comes about that all the eggs appear alike as regards their chromosome-groups but in the male there are two kinds of spermatozoa, an idiochromosome being present in the one half but absent or vestigial in the other half. Wilson was therefore led at first to suggest that the spermatozoon determined the sex, sperms with the "accessory" chromosome giving rise to females, those without it to males. Later[2] he modified this hypothesis in favour of one which will allow the sex-determinants to be regarded as Mendelian characters, femaleness being dominant over maleness. The two idiochromosomes in the female are regarded as male-bearing and female-bearing respectively, so that some eggs after maturation bear maleness, others femaleness. The single idiochromosome in the male is male-bearing and there is supposed to be selective fertilisation; so that a male-bearing sperm can conjugate only with a female-bearing egg and a sperm bearing no sex-determinant (idiochromosome) with a male-bearing egg. If femaleness is dominant, all fertilised eggs having two idiochromosomes will become females, those having only one, males. It is interesting that breeding experiments with Lepidoptera, which will be mentioned below, led the present

[1] "Studies on Chromosomes," i. ii. iii. and iv., *Journ. Exp. Zoo.* 1905, 1906, 1909. Also several papers in *Science*, 1905-7, etc., especially 1909, vol. xxix. p. 53, a review of the whole subject. It should be mentioned that the accuracy of Wilson's observations has been questioned by several investigators—*e.g.* Foot and Strobell, *Amer. Journ. Anat.* vii. 1907, p. 279.

[2] "Studies on Chromosomes," iii., *Journ. Exp. Zoo.* iii. 1906. No. 1. For a still later suggestion of Wilson's see footnote near the end of this article.

writer[1] to formulate an almost exactly similar hypothesis at almost the same time. But it will be seen that later experiments with moths suggest that a slightly different explanation of the facts is more probable.

The essence of Wilson's hypothesis is that the sex-determinants behave as Mendelian characters, segregating from one another in gametogenesis (at the reduction division), that femaleness is dominant, and that there is selective fertilisation; so that a male-bearing egg is fertilised by a female-bearing spermatozoon. Suggestions closely similar to these were put forward by Castle[2] on quite different grounds in 1903. Castle collected a quantity of evidence from breeding experiments and from what is known with regard to parthenogenetic reproduction. He supposed that every individual arising from a fertilised egg is heterozygous (hybrid) in respect of sex and that segregation takes place at the second maturation division, so that half the gametes bear maleness, half femaleness. Male-bearing eggs conjugate with female-bearing spermatozoa and *vice versa*; but dominance is alternative, so that roughly half develop into each sex. In most parthenogenetic animals only one polar body is produced in eggs which will not be fertilised; in these cases femaleness is always supposed to dominate. Since with only one polar division no segregation takes place, the offspring are females. If in a parthenogenetic species two polar bodies are produced the offspring are commonly males, since the female determinant is supposed to be eliminated with the second polar nucleus. A further valuable suggestion was that the male and female determinants might be "coupled" with certain body characters, either invariably—so explaining sexual dimorphism—or frequently, by which is explained the general association of one variety with one sex, another with the other, in the offspring of certain crosses. Wilson has since[3] observed coupling of ordinary with idiochromosomes, which may be connected with this phenomenon.

Castle's suggestive paper has stimulated much work on the maturation of parthenogenetic species but his hypotheses do not always hold good. For example, it is now known that parthenogenetically produced males in the Aphides arise from

[1] Doncaster and Raynor, *Proc. Zoo. Soc.* 1906, i. p. 125.

[2] "The Heredity of Sex," *Bull. Mus. Comp. Zoo. Harvard*, xl. p. 189.

[3] *Science*, May 17, 1907, p. 779.

eggs which have only one maturation division;[1] some of his explanations of other exceptional cases, although ingenious, will not now bear critical examination. One of the most difficult is that of the hive-bee and those insects resembling it, in which all eggs have two polar divisions and when fertilised yield females, when parthenogenetic, males. Castle supposed that the female determinant is extruded with the second polar nucleus, leaving the egg male-bearing, and accepted the observations of Petrunkewitsch,[2] who maintains that the testis of the drone is derived from the fused polar nuclei of the unfertilised egg. Since the second polar nucleus by hypothesis contains the female determinant, the spermatozoa may be female-bearing and cause the fertilised egg to be female. But there is considerable doubt as to the accuracy of the observation and in any case it cannot be applied to some others; *e.g.* Silvestri (see above) finds that in *Litomastix* the polar nuclei are used up in forming the protecting membrane of the embryos, and yet the sex-phenomena are just as in the bee. An alternative speculation may be offered. It is possible that while the female determinant is extruded in the virgin egg with the second polar nucleus, the presence of a spermatozoon in the egg may cause the male determinant to be eliminated, leaving the egg-nucleus female-bearing.[3] This would fall into line with the explanation suggested above of gynandromorphic bees—that in them the sperm has conjugated with the second polar nucleus.

It now remains to describe work on the determination of sex which has led to similar conclusions to those suggested above but arrived at from a different starting-point. Castle suggested that as sex is inherited as a Mendelian unit, it might at times be "coupled" in the gamete with some other body-character. It has been found that something of this kind actually does take place in the case of certain varieties which are inherited differently by the male and female. As an illustration of this we will take some experiments made by the writer, accounts of

[1] *E.g.* Stevens, *Journ. Exp. Zoo.* ii. 1905, p. 313. But Morgan (*Proc. Soc. Exp. Biol. and Med.* vol. v. 1908, p. 56) finds in *Phylloxera* that the females have six chromosomes, the males five. And Stevens (*Journ. Exp. Zoo.* vi. 1909, p. 115) suggests that in *Aphis* also one complete chromosome is extruded in the maturation of male parthenogenetic eggs, but not in female eggs.

[2] *Zool. Jahrb.* xiv. 1901, and xvii. 1903.

[3] I find that this suggestion has also been made by Prof. T. H. Morgan.

which have already been published,[1] since the results in that case happen to be simpler than in some other instances which have been worked out.

In the common currant moth (*Abraxas grossulariata*) there is a rare and very distinct variety ("*lacticolor*") found in the wild state almost exclusively in the female. Crossing-experiments were made between this variety and the type-form; the results were as follows:

(1) *Lacticolor* ♀ × type ♂ gave type ♂, type ♀.
(2) Heterozygous (crossed) type ♀ × heterozygous type male gave type ♂, type ♀, *lacticolor* ♀.
(3) *Lacticolor* ♀ × heterozygous type ♂ gave {type ♂, *lacticolor* ♂. type ♀, *lacticolor* ♀.}
(4) Heterozygous type ♀ × *lacticolor* ♂ gave type ♂, *lacticolor* ♀.
(5) *Lacticolor* ♀ × *lacticolor* ♂ gave *lacticolor* ♂ and ♀.
(6) Wild type ♀ × *lacticolor* ♂ gave type ♂, *lacticolor* ♀.

These results at first may seem hopelessly confusing but there are several points of interest about them. Firstly, the *lacticolor* character behaves as a Mendelian recessive, disappearing in the first cross (No. 1) and reappearing after mating (2). Secondly, starting with a *lacticolor* female, it is possible to get males of that variety only in one way, viz. by pairing such a female with a heterozygous male, i.e. a male which is typical in appearance, but being of *lacticolor* parentage bears the recessive *lacticolor* character. *Lacticolor* males are also produced from mating *lacticolor* males and females together (No. 5); but from any other form of union all the *lacticolor* individuals which emerge are females. A third point of great importance is that converse crosses do not give similar results, the most unexpected case of this appearing in matings of types No. 1 and No. 6. In the first, a *lacticolor* female paired with a wild (pure) type male gives all the offspring of both sexes perfectly typical, a quite normal Mendelian result, since *lacticolor* is recessive to the type. But if a pure, wild female is mated with a *lacticolor* male, the male offspring are typical but the female are all *lacticolor*—exactly the same result in fact as when a first-cross female is used instead of a wild one.

[1] Doncaster and Raynor, *Proc. Zoo. Soc.* 1906, i. p. 125; and Doncaster, *Reports to Roy. Soc. Evolution Committee*, iv. 1908, p. 53.

In explaining these phenomena in the first paper this last result was not known; and it was suggested (in accordance with Castle's hypothesis) that the germ-cells bore one or the other sex, that fertilisation was selective, so that all individuals were heterozygous in respect of sex and that in the eggs the *lacticolor* character was coupled with the female sex-determinant. Later Bateson and Punnett[1] offered a modified hypothesis, which is more in accord with the facts as known at present. They suggest (1) that the sex-determinants behave as Mendelian units, femaleness being uniformly dominant over maleness; (2) that female individuals are heterozygous in respect of sex, being of the constitution ♀♂ and producing male-bearing and female-bearing eggs in equal numbers; males are homozygous in sex, of the constitution ♂♂, so that they produce only male-bearing spermatozoa; (3) that there is repulsion in oogenesis between the dominant determinant for femaleness and the dominant *grossulariata* (type) determinant, in consequence of which all male-bearing eggs bear the type, all female-bearing eggs the *lacticolor* character.

This suggestion completely accounts for the facts and has since been greatly supported by the discovery that *all* females with the type (*grossulariata*) character are heterozygous and produce *lacticolor* offspring when paired with a *lacticolor* male. This fact compels us to assume that the *lacticolor* determinant is present in all females of the species and is only prevented from appearing because typical males bear normally only the type (*grossulariata*) character, which dominates over *lacticolor*. If, then, the males are homozygous and the females heterozygous in respect of a character so intimately related with sex, it strongly supports the idea that the same may be the case with the sex-determinants themselves.

If this instance stood alone it might seem rash to found on it such a far-reaching theory of the nature of sex. But exactly similar cases have been found elsewhere. Miss Durham[2] has described almost identical phenomena in the case of canaries of the Cinnamon variety, which have pink eyes. A pink-eyed hen paired with a black-eyed cock gives offspring which are all black-eyed; but a black-eyed hen by a pink-eyed cock gives males which are all black-eyed, and pink-eyed females, together with

[1] *Science*, vol. xxvii. 1908, p. 785.
[2] *Reports to Roy. Soc. Evolution Committee*, iv. 1908, p. 57.

sometimes a small proportion of black-eyed females. This occurrence of exceptions suggests some disturbing factor not present in the moths. Bateson,[1] also, has discovered a similar case in fowls, and Correns from experiments on plants (*Bryonia*) has come to a similar conclusion,[2] except that he regards the male as heterozygous and the female homozygous.

Confirmatory evidence may be drawn from other observations. One of these is the effects of castration. In vertebrates castration of the male may prevent the appearance of the male secondary sexual characters but does not cause the appearance of characters proper to the female. Removal or atrophy of the ovary, however, may bring about the development of characters normal in the male. In the Crustacea the opposite result is found.[3] A female whose ovaries are destroyed by a parasite has its secondary sexual characters reduced; a male assumes more or less completely the characters of the female. And if the parasite dies and the host recovers, the ovary of the female may again become functional; but in the male under such circumstances eggs may be produced in the testis. Geoffrey Smith concludes from these observations and from others on the Cirripedes, that the female is homozygous in sex and the male heterozygous. There seems no *a priori* reason why this should not be true in the case of the Crustacea and flowering plants, while the converse is the case in moths and vertebrates.

One of the points of difficulty about the theory that one sex is homozygous and the other heterozygous in respect of the sex-determinants is that it appears inconsistent with Wilson's theory based on the study of "idiochromosomes." But phenomena such as he describes have hardly been observed outside most orders of Insects and possibly Arachnids, and are probably not of universal occurrence. And if all individuals of one sex are heterozygous, those of the other homozygous in sex, it may be imagined that in the homozygous sex two sex-determinants would not be necessary; one of them might become vestigial, as Wilson describes, if at the same time spermatozoa bearing such a vestigial determinant can only

[1] See note in *Science*, vol. xxvii. 1908, p. 785, referred to above. For full account of this case, and discussion of the whole subject, see Bateson, *Mendel's Principles of Heredity* (Camb. Univ. Press. 1909), chap. x.

[2] *Bestimmung und Vererbung des Geschlechtes* (Borntraeger), 1907.

[3] G. Smith, Naples Monograph, *Rhizocephala*, 1906.

conjugate with eggs of one kind. But it must be admitted that any suggestion of selective fertilisation interferes with the extreme simplicity of the theory outlined above.[1]

The hypothesis here described not only explains the cases which led up to it and such facts as the effects of castration but also accounts for the phenomena of sexual dimorphism and the inheritance of some structures by one sex only. But at present the more complex cases of sexual polymorphism, such, for example, as are known in the African butterflies of the genus *Papilio*, still remain obscure, although it is probable that when we have more extensive records of breeding experiments these also will be found to fall into line. And it should be explained that some forms of sex-limited inheritance are of quite a different nature—*e.g.* colour-blindness and the disease hæmophilia in man. In these diseases the abnormal condition is dominant in one sex (male) and recessive in the other and may appear in the female if both parents are tainted. Possibly a combination of some condition of this kind with sex-relations such as we find in *A. grossulariata* and the Cinnamon Canary may ultimately be found to account for the complex sexual polymorphism found in the African Papilios.

We have now sketched the principal lines of evidence which have been collected in recent years pointing to the conclusion that the sex-determinant is present in the germ-cell and is probably comparable in nature with a Mendelian unit. In a paper of this kind it is clearly out of place to attempt even to mention a tithe of the numerous hypotheses concerning sex which have been advanced even in very modern times. Many of them do not concern the point at issue, dealing as they do with possible factors which may influence the sex of a given individual; for we have seen that, whatever the true nature of

[1] Wilson has recently (*Science*, xxix. Jan. 1909, p. 53) put forward a fresh suggestion, viz. that the idiochromosomes do not bear the determinants for maleness or femaleness as such but that one idiochromosome in the fertilised egg causes it to develop into a male, two into a female, so that the difference is rather quantitative than qualitative. Castle (*Science* xxix. March 1909, p. 395) has taken up this idea, with the further suggestion that while some species are as Wilson suggests, in others the presence of one idiochromosome determines femaleness and absence of any idiochromosome at all brings about maleness. If this last condition should be found to exist in *Abraxas grossulariata*, it would then fall into agreement. In this connection it is of interest that cases such as Wilson describes have been observed in most of the chief orders of insects but not in the Lepidoptera.

sex may be, it is conceivable that the proportion of germ-cells bearing one or the other sex which come to maturity may possibly be influenced by external conditions. To do justice to what has been written on such subjects would require a book of considerable size, and in the present paper is impossible. The object of this account will have been fulfilled if it indicates the direction in which recent work is leading and if it makes it clear that the probabilities are overwhelmingly in favour of the idea that the determination of sex is not consequent on the accidental preponderance of one or other of two nicely balanced tendencies but is due to fixed and unalterable characters inherent in the germ-cells.

THE USE OF LIQUID AMMONIA AS A SOLVENT

By PERCY MAY, B.Sc.

Introduction

A knowledge of the conditions governing the course of a chemical action can be gained most rapidly when the interacting substances are in a liquid state. For instance, changes of concentration can be measured from time to time and hence the velocity and "order" of the action can be found; thermal changes or volume changes can also be easily measured.

In recent years much attention has been paid to the part played by the nature of the solvent in influencing the course and velocity of an action and the work done in this direction brings into prominence an interesting contrast between the two main branches of Chemistry—Organic and Inorganic. The number of organic solvents in common use is very great Alcohol, ether, chloroform, carbon tetrachloride, carbon disulphide, acetone, glacial acetic acid, benzene, light petroleum and many others are frequently employed and these show a great variety both in their chemical and physical properties. As an example of the influence of the solvent may be mentioned the interaction of triethylamine and ethyl iodide: in presence of acetone the action takes place ten times as rapidly as it does in benzene and about four hundred times as quickly as in heptane.

On the other hand, comparatively few substances other than water have been used as solvents for inorganic compounds. Systematic inorganic analysis has been founded on the behaviour of such solutions and, owing to the attention paid to them, our knowledge of both their chemical and physical properties greatly exceeds that which we possess of the corresponding properties of non-aqueous solutions.

During the last decade or two, however, a great deal of work has been done on other solvents of acids, salts, bases and simple inorganic compounds, more especially with regard to their physical properties, such investigations being intimately connected with the increased attention that is now being paid to the important part played by the solvent in determining ionisation, etc. (cf. Lowry, Science Progress, October 1908, p. 202). Most of the solvents used in such researches are gases at ordinary temperatures or very volatile liquids, amongst the most important being ammonia, sulphur dioxide and hydrogen cyanide.

The first-mentioned of these is of special interest owing to the fact that solutions in liquid ammonia show many interesting analogies with aqueous solutions. This is in accordance with the close parallelism that exists between many of the physical properties of ammonia and of water. Thus, the boiling-point of water is abnormally high, when compared with that of sulphuretted hydrogen and other non-associated bodies and is probably accounted for by water being polymerised in the liquid state. The boiling-point of ammonia ($-33{\cdot}5^\circ$ C.) is also somewhat high when compared with the boiling-points of substances such as phosphine, arsine, sulphuretted hydrogen and hydrogen chloride. Among the properties of water which are most noteworthy are its high specific heat, its high latent heat of vaporisation and of fusion, its high critical temperature and pressure, its power to unite with salts as water of crystallisation and its low boiling-point-elevation constant. All these properties are shown in a marked degree by ammonia also. Indeed, its specific heat and latent heat of fusion are greater than the corresponding constants for water, whilst its boiling-point-elevation constant is the lowest of any known liquid. Ammonia readily unites with salts as ammonia of crystallisation and although inferior to water as a solvent for salts, excels it in its power of dissolving carbon compounds. Lastly, ammonia is a fairly good ionising solvent but in comparison with water both its dielectric constant and its factor of molecular association are low. It has been noticed that the ionising power of a solvent seems to run parallel to the possession of a high dielectric constant and to the existence of polymerised molecules. The behaviour of ammonia seems to be against this view but on the other hand it seems probable that the ionising power and other exceptional properties of water and ammonia are connected with the presence of residual affinity in the molecules. In the case of the former, divalent oxygen is capable of acting as a tetrad and in that of the latter, triad nitrogen can become pentad.

Liquid ammonia is easily obtained commercially, as it is used as the "working substance" in refrigerators. The ammonia sold for this purpose is fairly free from impurity and can often be used as a solvent without any further purification being necessary. As is stated above, inorganic salts are, generally speaking, less soluble in ammonia than in water; but there are many exceptions—such, for example, as silver iodide, which is *far* more soluble in ammonia than in water. Many of the ordinary interactions of salts take place as in aqueous solutions, but owing to differences in the order of solubilities in liquid ammonia many actions take place in it which do not occur in water. Thus, for example, calcium chloride is precipitated by adding a solution of ammonium chloride to calcium nitrate in ammonia. There are numerous interactions which only take place in liquid ammonia, which are of especial interest owing to the fact that they give rise to new classes of compounds analogous to bases, acids and salts.

Solubilities in Liquid Ammonia

Liquid ammonia boils at $-33{\cdot}5^\circ$ C. and so it was not easy to work with it in the days before vacuum-jacketed vessels were in common use. A liquid which can be kept at ordinary temperatures was prepared by Divers (1)[1] in 1873 by passing dry ammonia over dry ammonium nitrate; in this way a solution of ammonium nitrate in anhydrous ammonia was obtained, of which the boiling-point was as high as $+25^\circ$ C. This liquid resembles pure ammonia in its solvent action on many substances and numerous solubility determinations were made with it; but more interest is attached to the work of Gore (2) on solubilities in pure liquid ammonia. He investigated the solubilities of many substances, both inorganic and organic, and found that amongst the metallic salts the nitrates are generally the most soluble, as is the case with aqueous solutions. The chlorides, bromides and iodides are also most of them soluble but there are many exceptions, one of the most striking being calcium chloride. Oxides, carbonates, sulphides and sulphates are generally insoluble, although many of the ammonium salts form exceptions by being soluble. Amongst the elements, iodine, sulphur and white phosphorus were found to be soluble, also the alkali metals which form blue solutions. Gore also found

[1] A list of references is given at the end of the article.

that metallic copper was somewhat soluble but Seely (5) showed that this was not the case if air and moisture were excluded.

More recently much work on solubilities in ammonia has been carried out by Franklin and Kraus in America (3). They confirmed most of Gore's work and have found that very many organic compounds are soluble in liquid ammonia. Most of the aliphatic halogen compounds, monohydroxy alcohols and ethers are miscible with ammonia; so also are the aldehydes and acids, except those of high molecular weight, such as palmitic and stearic acids. The sugars are easily soluble but dulcitol and mannitol are almost insoluble. The hydroxy-acids, amino-acids, amides and ureides are mostly readily soluble; amines and esters vary much amongst themselves, though most are soluble to some extent, whilst most of the dibasic acids are insoluble. The foregoing remarks apply to aliphatic substances only. The solubility of the aromatic compounds in many groups varies so much with the individual compounds that it is difficult to give a general statement. Phenols, alcohols, ethers, monobasic acids and esters in general are readily soluble; detailed information on this subject can be found in the publications of Franklin and Kraus.

Ammonium and its Metallic Derivatives

In order to amplify the ammonium radicle theory of Ampère and Berzelius, a series of investigations was begun by Weyl (4) in 1864. His work included many observations on complex ammonia compounds of mercury and other metals which are not of great general interest. By the action of dry ammonia under pressure on metallic potassium or sodium, Weyl obtained a copper-red metallic-looking liquid, which he thought was probably ammonium in which one of the hydrogen atoms had been substituted by an atom of the alkali metal, NH_3Na.

Weyl's work was severely criticised by Seely (5), who denied that a compound was formed when sodium dissolved in liquid ammonia. He regarded the blue liquid so formed as an ordinary solution, the solid becoming liquefied in precisely the same way as sugar becomes liquefied when dissolved in water. When the liquid is evaporated the solid is left in its original form with its properties unaltered. Recent work with this blue solution—to which reference will be made later—seems to confirm Seely's views; but some further work of his gave indications of the formation of a compound of sodium and ammonia, although this was not the view taken by Seely himself. He passed dry ammonia gas over sodium and noticed that the sodium lost its metallic appearance, became pasty and then liquefied, forming a mobile opaque liquid of a fine copper-red colour when viewed by reflected light. As the ammonia continued to liquefy, it mixed with this liquid, which, after a series of colour changes, finally appeared of an intense blue colour when viewed either by reflected or transmitted light. Seely sought to explain these colour changes on purely physical grounds; in order to throw more light on the nature of the solution of the alkali metals in ammonia, a fresh investigation was started by Joannis in 1889 (6). He mixed one equivalent of sodium with twenty equivalents of ammonia and found that the vapour pressure of the solution rapidly diminished as the ammonia was allowed to evaporate, until it became constant at a certain point corresponding to a liquid having the composition $Na + 5{\cdot}3\ NH_3$. The composition of the liquid was a function of the temperature and hence it could not be regarded as a true chemical compound. If more ammonia were caused to evaporate a solid separates having the same colour as the liquid—namely an intense copper-red. As more ammonia was removed the pressure remained constant; finally, when the mixture contained one equivalent of sodium and one equivalent of ammonia, not a trace of liquid was left. The solid thus obtained therefore had the composition $NaNH_3$ and must be regarded as sodium-ammonium—*i.e.* ammonium in which one hydrogen atom has been displaced by sodium.

Further removal of ammonia caused the substance to decompose, sodium being left as the final product. As the pressure remained constant during this "decomposition," it was thought by Joannis that we are here dealing with a case of the true dissociation of a compound. It is noteworthy that the vapour pressure of the saturated solution is the same as the dissociation pressure of the compound. For this reason it is extremely difficult to determine the exact point when all the liquid has vanished but the dissociation of the compound has not yet begun. This fact explains how it was that Seely was led to the opinion that no compound was formed at any stage of the process of solution. In favour of Seely's views it may be pointed out that the results obtained from measurements of the electrical conductivity and electrical potential of the blue solution point to its being a colloidal solution of the metal analogous to the "metallic fogs" formed by the electrolysis of fused salts. Now the blue solution is formed by adding an excess of ammonia to the "compound" and therefore it must be supposed that the latter dissociates when an excess of one of its products of dissociation is added. This is quite contrary to the law of mass-action; but as the absence of a compound in the blue solution has not been definitely proved, the existence of the compound $NaNH_3$ may be provisionally accepted. The foregoing statements apply to potassium as well as to sodium. If the compound has the formula $NaNH_3$ or KNH_3, the nitrogen would function as tetrad, which is unusual; and as ammonium is a univalent radicle, we should expect the sodium-ammonium to be also univalent and hence the free compound would probably be $(NaNH_3)_2$. By determining the diminution of the vapour pressure of ammonia containing some dissolved sodium, Joannis (7) was able to calculate the molecular weight according to the well-known method of Raoult. The results agree very well with the double formula both in the case of sodium and potassium.

More recently Moissan (8) has obtained other metal-ammoniums by the action of the dry gas on the metal. Those of lithium and calcium are reddish-brown solids spontaneously inflammable in air and less soluble in ammonia than the corresponding sodium and potassium compounds. Their composition corresponds to the formulæ $LiNH_3$ and $Ca(NH_3)_4$ respectively. Both lithium and calcium ammoniums gradually decompose at ordinary temperatures, liberating hydrogen and forming the amide: $LiNH_4 = LiNH_2 + H_2$, this decomposition being more rapid in the case of the calcium than of the lithium compound. The properties of the metallic amides will be considered later, together with those of the acid amides. A great deal of work has been done on the interactions of the metal ammoniums which lies outside the scope of this paper.

Attempts at the Isolation of Ammonium

When an aqueous solution of an ammonium salt is electrolysed, ammonia and hydrogen are evolved at the kathode. This is not surprising when regarded from the point of view of the valency of nitrogen, as free ammonium would contain tetrad nitrogen; diammonium, $H_4N - NH_4$, however, could be formed by the union of the two ammonium radicles in just the same way as cyanogen is formed by the union of two cyanide radicles. It has already been mentioned that the substituted alkali-ammoniums probably exist in this form, although they are extremely easily dissociated. Ammonium also, if it is capable of existence, would probably be easily dissociated and therefore it seems necessary to carry out any attempts that might be made to isolate it at as low a temperature as possible.

In order to ascertain whether ammonium can exist at a temperature of about $-100°$ C., Moissan (9) made use of the interaction of calcium-ammonium and ammonium chloride in the presence of liquid ammonia. Pure dry ammonia was led over a known weight of calcium contained in a U tube one of the arms of which was constricted so as to hold ammonium chloride in the constriction. On cooling the tube to $-40°$ C., the compound $Ca(NH_3)_4$ was formed and dissolved in the excess of ammonia. The tube was then cooled to $-80°$ C., when more ammonia liquefied and the ammonium chloride dissolved in it. The red-brown colour of the calcium-ammonium then disappeared rapidly, hydrogen being evolved and collected in a special apparatus. The volume of the hydrogen, which was found to be pure, showed that at a temperature of $-80°$ C. ammonium did not exist.

$$2NH_4Cl + Ca(NH_3)_4 = CaCl_2 + 4NH_3 + 2(NH_3 + H).$$

Exactly similar results were obtained on using lithium instead of calcium.

Moissan (10) also prepared the so-called "ammonium-amalgam" by the action of ammonium chloride in ammonia on sodium amalgam. This forms ammonium amalgam and sodium chloride, no gas being evolved. The product was washed with liquid ammonia; on cooling to $-80°$ C. a metallic mass was obtained which was washed with ether at $-80°$ C. This did not evolve any gas, even in a vacuum. On warming, the mass began to swell, till at 15° C. it occupied twenty-five to thirty times its original bulk. Several determinations of the evolved gases were made; all of these agreed closely with the theoretical volumes of two volumes of ammonia to one of hydrogen: $2NH_4 = 2NH_3 + H_2$. Nevertheless Moissan did not regard this as a true ammonium amalgam but from the similar behaviour of sodium hydride when saturated with ammonia, he inclined to the opinion that it was an ammoniacal mercury hydride.

Ruff (11) attempted to prepare ammonium by the electrolysis of potassium iodide in liquid ammonia but without success. At the negative pole he obtained a solution of potassium-ammonium as a metallic-looking liquid, as described by Joannis; on coming into contact with the ammonium iodide formed at the anode, this was decomposed but the ammonium which it was thus hoped to obtain was dissociated into hydrogen and ammonia. To prevent this dissociation, the experiment was performed under a pressure of sixty atmospheres but without success. This indicates that ammonium cannot exist at $-95°$ C. at a pressure of sixty atmospheres.

Now it has been already pointed out that the substituted alkali ammoniums are very easily dissociated; and by analogy with these, Ruff regards ordinary ammonium as hydrogen ammonium and draws the conclusion that the limits of the existence of ammonium probably are to be found in the neighbourhood of the critical point of hydrogen. As the critical temperature of hydrogen is as low as $-238°$ C., no suitable solvent is known which could be used for this purpose; hence the truth of this view cannot be tested.

Ammonium in which the hydrogen atoms have been substituted by organic radicles might be expected to be more stable; therefore Palmaer (12) attempted to prepare tetraethylammonium by electrolysing tetra-ethylammonium hydroxide dissolved in liquid ammonia. The hydroxide, which contained about 2·5 per cent. of chloride, was electrolysed between platinum electrodes, when blue striations were seen at the kathode; these were thought to be tetra-ethylammonium.

Ammono-Bases, Acids and Salts

The blue solution formed when sodium or potassium is dissolved in ammonia slowly changes on standing into a solution of sodium or potassium amide, the interaction taking place more swiftly in presence of a catalyst such as ferric oxide or platinum black: $2K + 2NH_3 = 2K . NH_2 + H_2$. The potassium amide remains dissolved in the ammonia; the solution exhibits basic properties quite analogous to those of an aqueous solution of potassium hydroxide. Anhydrous liquid ammonia has no basic properties whatever. A solution of potassium amide in ammonia possesses the power of reddening an ammonia solution of phenolphthalein and when added to ammonia solutions of metallic salts precipitates compounds analogous to the basic oxides. Thus, with silver nitrate it yields silver amide as a very explosive white precipitate:

$$AgNO_3 + KNH_2 = \text{ppt.}AgNH_2 + KNO_3.$$
$$AgNO_3 + KOH = \text{ppt.}AgOH + KNO_3.$$

Similar compounds are formed when a solution of the alkali amide is added to ammonia solutions of mercury iodide, lead nitrate and lead iodide but in these cases intermediate products, corresponding to basic salts, are first formed. These indications point to the conclusion that the $\overset{-}{N}H_2$ ion in ammonia is analogous to the $\overset{-}{O}H$ ion in water. We should then expect to find that the hydrogen ion had acid properties in ammonia, just as in water. Ordinary acids cannot be dissolved as such in liquid ammonia but they can be in the form of their ammonium salts and it is then found that these substances behave as acids, the $\overset{+}{N}H_4$ ion behaving in an analogous manner to the $\overset{+}{H}$ ion in water.

The following properties of ammonium salts indicate the acid nature of their solutions in liquid ammonia:—

(1) They discharge the colour of an ammonia solution of phenolphthalein to which a little alkali has been added.

(2) They dissolve metals such as sodium, potassium, magnesium and calcium, hydrogen being evolved.

$$2M + 2NH_4X = 2\,MX + 2NH_3 + H_2.$$

(3) Basic salts and metallic oxides which are insoluble in ammonia are soluble in solutions of ammonium salts:

$$CaO + 2NH_4NO_3\ \text{am}^1 = Ca(NO_3)_2\ \text{am} + H_2O + 2NH_3.$$

Substances like these, which behave as acids in ammonia solution, are termed ammono-acids by Franklin (13); the corresponding bases and salts are designated ammono-bases and ammono-salts. This nomenclature is convenient and saves the repetition of phrases and therefore will be used in the rest of this account.

[1] The symbol (am) means that the substance is in solution in ammonia just as aq means that it is in aqueous solution.

Besides the ammonium salts of the acids there are other classes of bodies which behave as ammono-acids. The amides and imides of the organic acids form a very large and well-known group of compounds and bear exactly the same relation to the organic acids as ammonia does to water.

$$CH_3.CO.NH_2 \qquad H.NH_2$$
$$CH_3.CO.OH \qquad H.OH.$$

This relationship is shown very clearly by their formation from the acid chlorides.

In liquid ammonia: $CH_3.CO.Cl + NH_3 = CH_3.CO.NH_2 + HCl$

In water: $CH_3.CO.Cl + H_2O = CH_3.CO.OH + HCl.$

Hence it is not surprising to find that these compounds behave as ammono-acids, their solutions in liquid ammonia being capable of conducting electricity, discharging the colour of alkaline phenolphthalein solutions and forming ammono-salts with ammono-bases.

Less well known are the amides, imides and nitrides of the non-metallic electro-negative elements; these are strictly comparable with the better-known organic compounds; and they also are related to the corresponding acids as ammonia is to water. The hydroxy compounds of the metallic elements are bases and those of the non-metallic elements form acids; in just the same way the amido compounds of the former are ammono-bases, while those of the latter are ammono-acids. Just as the halides and sulphides of the strongly electro-negative elements are completely decomposed by the action of water, so also are the same compounds completely decomposed in liquid ammonia, yielding compounds bearing the same relation to ammonia as the products of hydrolysis do to water.

$$\begin{cases} BCl_3 + 3NH_3 = B(NH_2)_3 + 3HCl \\ BCl_3 + 3H_2O = B(OH)_3 + 3HCl \end{cases}$$
$$\begin{cases} PCl_3 + 2NH_3 = P(NH).NH_2 + 3HCl \\ PCl_3 + 3H_2O = P(OH)_3 + 3HCl \end{cases}$$
$$\begin{cases} SiS_2 + 2NH_3 = Si(NH)_2 + 2H_2S \\ SiS_2 + 2H_2O = Si\,O_2 + 2H_2S \end{cases}$$

Franklin and Stafford (14) find that the ammono-bases already mentioned and the ammono-acids interact when dissolved in ammonia, giving rise to ammono-salts. For instance, acetamide and either one or two molecules of potassium amide interact in the same way that a dibasic acid and either one or two molecules of a base interact:

$$CH_3.CO.NH_2 + KNH_2 = CH_3.CO.NHK + NH_3$$

The ammonia solutions of these substances—if the term "ammono-salt" is justifiable—should conduct electricity. Measurements have only been made with one of them—mercury succinimide—which was found to give a good conducting solution. Besides the simple ammono-salts, there are others analogous to the aluminates, plumbates and zincates. For example, some of the ammonia derivatives of silver, copper, aluminium and lead dissolve in excess of potassium amide solution, just as many of the metallic hydroxides dissolve in excess of caustic potash. Ammono-basic salts also exist and are produced by the action of ammonia on the simple salts in precisely the same way that ordinary basic salts are produced by the hydrolytic action of water. The term "ammonolysis" is suggested by Franklin for this kind of action, which resembles hydrolysis in all its details. The formation of ammono-basic salts by ammonolysis is reversible, the amount of precipitate being increased by the addition of an ammono-base and diminished by adding an ammono-acid. Examples are:

$$HgCl_2 + 2NH_3 \rightleftarrows NH_2HgCl\text{ppt.} + NH_4Cl$$
$$2PbI_2 + 6NH_3 \rightleftarrows Pb(NH_2)_2PbNH_2I\text{ppt} + 3NH_4I$$

The NH_2 is analogous to OH in ordinary basic salts and the ammonium salt liberated is analogous to the acid liberated by ordinary hydrolysis.

$$\text{Cf. } Hg(NO_3)_2 + H_2O \rightleftarrows HO.HgNO_3 + HNO_3$$
$$BiCl_3 + 2H_2O \rightleftarrows Bi(OH)_2Cl + 2HCl.$$

Other Interactions in Liquid Ammonia

The interactions of sodium or potassium amide and solutions of metallic salts have already been mentioned but they are of sufficient importance to be considered again in a little more detail, as they throw much light upon the nature of many well-known compounds. Mercuri-ammonium compounds are very numerous, the most familiar being the "infusible white precipitate" which is looked upon by various authorities as mercuri-ammonium chloride:

$$Hg = NH_2Cl$$

as dimercury diammonium chloride, $N_2H_4Hg_2Cl_2$, as dimercury ammonium chloride plus ammonium chloride, NHg_2Cl, NH_4Cl, and as mercuric chloramide, NH_2HgCl. Franklin (13) obtained this compound by the action of liquid ammonia on $HgCl_2$, and he regards it as ammono-basic mercuric chloride,

$$NH_2 . Hg . Cl. \quad \text{Cf. basic mercuric chloride, } HO . Hg . Cl.$$

In accordance with this view, the dimercuri-ammonium salts are represented by the general formula $Hg = N = Hg - X$ where X is any monovalent acid radicle.

$$\text{Cf. } HO - Hg - O - Hg - X.$$

The fusible white precipitate is regarded by Franklin as a salt with ammonia of crystallisation, while the numerous mercury ammonia compounds containing oxygen are regarded as mixed hydro and ammono basic compounds. According to this view the well-known Nessler's precipitate is regarded as

$$NH_2 . Hg . O . Hg . I$$

These somewhat intricate details are given to show the new light that has been thrown upon the nature of well-known compounds by the study of interactions in liquid ammonia. In addition to these compounds Franklin, by the use of liquid ammonia solutions, prepared mercuric nitride, Hg_3N_2, lead imide, $PbNH$ and bismuth nitride, $Bi N$, as well as the previously mentioned silver amide, $AgNH_2$. These compounds were prepared by adding the solution of the metallic iodide in ammonia to a solution of potassium amide. Silver amide was obtained as a white precipitate but the others are of different shades of brown and all of them are extremely explosive. An excess of KNH_2 was used to prevent contamination with ammono-basic salts but this cannot be done in the case of lead imide, as it is soluble in excess of potassium amide.

Many "double decompositions" take place in liquid ammonia, just as in water:

$$\textit{e.g.} \quad Zn(NO_3)_2 \text{ am} + (NH_4)_2S \text{ am} = ZnS \text{ ppt} + 2\ NH_4NO_3 \text{ am},$$

but in many cases precipitations occur which have no counterpart in aqueous solutions:

$$\textit{e.g.} \quad Zn(NO_3)_2 \text{ am} + 2NH_4Cl \text{ am} = ZnCl_2 \text{ ppt} + 2NH_4NO_3 \text{ am}$$

There are many other cases in which insoluble chlorides are precipitated.

Reference has already been made to the interactions of ammono-acids and ammono-bases. Amongst the products that have been obtained in this way and whose composition has been confirmed by analysis are:

$$CH_3 . CO . NHK + (NH_3)n$$
$$C_6H_5 . CH_2 . CO . NHK + (NH_3)n$$
$$C_6H_5 . CO . NHK \text{ and } C_6H_5 . CO . NK_2$$
$$C_6H_5 . SO_2 . NHK \text{ and } C_6H_5 . SO_2 . NK_2.$$

The method of preparing these is obvious from the formulæ. In each case an ammonia solution of the acid amide is added to a solution of potassium amide, whereby some of them are precipitated whilst others are caused to crystallise by concentrating the solutions.

A general method of preparing cuprous salts by means of liquid ammonia has been suggested by Joannis (15). Cuprous oxide is added to an ammonia solution of the ammonium salt of the desired acid:

$$2NH_4X + Cu_2O = Cu_2X_2 + 2NH_3 + H_2O.$$

In this way he prepared cuprous formate and benzoate. Another example of synthetic work in liquid ammonia is that of Emil Fischer (16), on the conversion of alkyl derivatives of malonic ester into the corresponding amides. He found that the yield of amide obtained when using liquid ammonia was less than when using saturated alcoholic ammonia in the case of the simple ester. On the other hand the alkyl derivatives gave a better yield with liquid ammonia.

The great solubility of ammonium nitrate in liquid ammonia has been made the subject of a patent for the separation and purification of ammonium nitrate from mixtures containing sodium and potassium nitrates. Other patents that make use of the solvent action of liquid ammonia are for the purification of crude anthracene and anthraquinone and for the purification of cyanides. The former depends upon the insolubility of anthracene in liquid ammonia, most of the impurities being readily soluble; in this way it has an advantage over the use of sulphur dioxide, which dissolves actually more of the anthracene than of the impurities and hence makes the process costly and inefficient. The latter process mentioned above depends upon the different solubilities of various cyanides in liquid ammonia and can be used for the separation of sodium from potassium cyanide, the latter being more soluble.

The Electrical Conductivity of Ammonia Solutions

The earliest observations on the conductivity of liquid ammonia are those of Bleekrode (17), who found that liquid ammonia was a good conductor, the solution becoming blue and remaining so only as long as the current passed. Cady (18) in 1896–7 observed that liquid ammonia itself was a bad conductor and that the phenomena noticed by Bleekrode only became apparent when traces of sodium or potassium salts were present. According to the measurements of Cady the conductivity of liquid ammonia is as low as $7{\cdot}4 \times 10^{-6}$. He found that when a solution of potassium iodide was electrolysed hydrogen was evolved and that precipitates were formed at both electrodes, the one at the kathode being potassium amide, KNH_2, that at the anode being nitrogen iodide.

Silver nitrate and lead nitrate gave good conducting solutions, the metal being deposited at the kathode. Cady also investigated solutions of potassium bromide, potassium nitrate, ammonium chloride, ammonium bromide, sodium bromide, sodium iodide, mercuric iodide and mercuric cyanide. All of these gave good conducting solutions, the conductivity for concentrations of a hundredth normal being generally greater than that of the corresponding aqueous solutions. It was found that the solution of metallic sodium conducted very well and as there were no signs of decomposition while the current was flowing it was concluded that the solution acted as a metallic and not as an electrolytic conductor. As will be seen later, subsequent work has tended to confirm this view.

This work of Cady and also that of Goodwin and Thompson (19) indicated that liquid ammonia must possess a considerable ionising power; in order to obtain more information on this point Frenzel (20) determined the conductivity of silver nitrate solutions at different concentrations. He found the following values:

Concentration.	$\frac{N}{64}$	$\frac{N}{78{\cdot}9}$	$\frac{N}{94}$	$\frac{N}{110{\cdot}0}$
Conductivity in water at + 18°C . .	106·4	107·4	108·3	109·0
Conductivity in ammonia at − 16°C .	163	171	176	180

and from these he expected that liquid ammonia would have a high dielectric constant compared with that of water, as there was supposed to be a very close connection between the dielectric constant and the ionising power of a solvent. Goodwin and Thompson measured the dielectric constant of ammonia and found it to be only about a quarter that of water; they therefore considered that the high conductivity of ammonia solutions was due to the great mobility of the ions and not to the high ionising power of the solvent.

The problem which then arose was to find out for certain which of these two factors—mobility or ionising power—was the cause of the high conductivity of liquid ammonia solutions. Frenzel endeavoured to settle this question by direct measurements of the ionic velocity but owing to the numerous experimental difficulties the results were not of much value. More decisive results were obtained by Franklin and Kraus (21) from an extended series of conductivity measurements at different dilutions. They found that the conductivity of pure ammonia was as low as $0{\cdot}01 \times 10^{-6}$, a result which made possible the accurate measurements of the conductivity of very dilute solutions. A large number of inorganic salts and also some organic ammono-acids were used and measurements were made up to very high dilutions. These showed that the limits of the molecular conductivity were reached only at dilutions of from 25,000 to 50,000 litres per gram equivalent, though with water the limiting conductivity is usually met with at dilutions of 1,000 to 5,000 litres. The following dilutions were found to be necessary in order to produce a 50 per cent., 75 per cent. and 90 per cent. ionisation of the salt:

		Number of litres required to ionise one gram molecule.		
		50 per cent.	75 per cent.	90 per cent.
KI	In water	—	0·4	20
	„ ammonia	80	400	2000
KNO_3	In water	0·5	5	25
	„ ammonia	200	1200	5000
$NaNO_3$	In water	0·5	5	33
	„ ammonia	—	800	4000
$AgNO_3$	In water	0·6	5	40
	„ ammonia	125	350	1500

These numbers indicate that in moderately dilute solutions the salts are far less ionised in ammonia than in water. The value of the limiting molecular conductivity was generally found to be about three times as great as for the corresponding aqueous solution. If Λ_∞ represents the maximum molecular conductivity, $\Lambda_\infty = U_K + U_A$, where U_K and U_A are the velocities of the kation and anion respectively.

This therefore indicates that the ionic velocities in ammonia are far greater than in water and accounts for the fact that most salt solutions in liquid ammonia show a higher conductivity than the corresponding aqueous solutions, in spite of the fact that dissolved salts are far less ionised in ammonia than in water. This high mobility of the ions is in accordance with the low viscosity of ammonia solutions. The influence of the viscosity of the solvent on the mobility of the ions was investigated by measuring the conductivity of tetramethylammonium iodide in various solvents. It was found that the limiting value of the molecular conductivity was approximately proportional to the fluidity of the solvent, fluidity being the reciprocal of the viscosity determined by the ordinary physical methods (rate of flow of liquid through a narrow tube, etc.). Direct measurements of the ionic velocities in ammonia were made by Franklin and Cady (22), who found that these were from 2·4 to 2·8 times as great at $-33°$ C. as in water at $+18°$ C.

In liquid ammonia, Ostwald's dilution formula holds approximately for solutions of the binary salts. This seems to indicate that in ammonia solutions the conditions are more simple than in aqueous solutions.

Before leaving this branch of the subject, mention should be made of the more recent work of Franklin and Kraus (23). They found that the conductivity of pure liquid ammonia is not greater than $0·005 \times 10^{-6}$, a value much lower than that usually given in the literature of the subject. In favour of this low value they point out that the velocity of the interaction of sodium and liquid ammonia is far less than that of the analogous interaction of sodium and water and this in spite of the fact that in the former the sodium is in the dissolved state, which—other things being equal—would tend to make the action go more swiftly. In the case of the metallic salts special attention has been paid to the more concentrated solutions, the earlier work having dealt more with dilute solutions in order to find the values for the maximum conductivity.

The conductivity values afforded by ammonium and sodium iodide in concentrated solutions are very high, but the reverse is the case with potassium nitrate, ammonium chloride and silver iodide. In ammonia solution the anions appear to exert a greater influence on the ionisation than the kations, the iodides being the most highly ionised of the salts examined, the bromides and chlorides following in the order named. In all cases it was found that the ionisation was far less than in water—potassium nitrate, for example, in a third normal solution being one-fourth ionised, while in water, at the same concentration, it is three-quarters ionised. Even at a dilution of a thousandth normal in ammonia solution the salt is less than three-quarters ionised, but in water the ionisation is practically complete. The cyanides of the heavy metals and cyanacetamide are remarkable in showing a decrease in the molecular conductivity with increasing dilution in the more concentrated solutions. As the dilution increases the molecular conductivity passes through a minimum and then increases with the dilution in the normal manner.

The results obtained with the acid amides are of great interest and justify the use of the term "ammono-acids." The analogy between the acid amides and ammonia on the one hand and the oxygen acids and water on the other was found to extend to the conductivity of the solutions of the acid amides in ammonia. The amides of the weaker acids yielded solutions which were poor conductors, whilst the amides of the stronger acids were found to conduct very well. For example, urea—the amide of the weak carbonic acid—scarcely conducts at all, acetamide and benzamide form poorly conducting solutions, formamide and cyanacetamide conduct much better, whilst the amides of the strong sulphonic acids, as well as picramide (trinitro-aniline), nitramide and sulphamide are excellent conductors. Of the metallic salts of the acid amides (ammono-salts) mercury succinimide was the only one of which the conductivity was measured. It gives a good conducting solution.

Many of the nitro-compounds, both aliphatic and aromatic, give solutions which are excellent conductors, many of these solutions being strongly coloured.

Measurements of electric potential in liquid ammonia have been made by Wilsmore (24), Johnson and Wilsmore (25), and Cady (26); the effect of dissolved substances on the boiling-point of ammonia has been studied by Franklin and Kraus (27).

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THE DEGENERATION OF ARMOUR IN ANIMALS

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

In tracing out the history of many classes of animals it is impossible for the palæontologist not to be impressed with the fatal effect which an ever-increasing development of armour inevitably produces upon the vitality and persistence of a race. A long continuance of special and uniform conditions of environment would appear to favour a steady progression in the development and elaboration of the exoskeleton for defensive purposes. This tendency may be carried on to so great an extent as to sacrifice plasticity of structure, as well as all variation in the direction of plasticity, to the attainment of a more complete protection with the aid of armour. Whenever new and adverse conditions of existence arise, those races which have adopted such means of defence find themselves severely handicapped in their struggles to cope with their altered surroundings; they are unable to vary adequately and, as an inevitable natural consequence, eventually die out. It would indeed appear that only species which can revert, more or less, to the original flexibility of their ancestral forms or which have to some extent retained their flexibility, can give rise to suitable variations, capable of developing on fresh and successful lines of evolution. In short, this tendency may be briefly enunciated in the form that, in response to change of environment, reduction of armour is essential to the preservation of the race. Even in the history of human warfare the new conditions which arose from the invention of gunpowder exercised an analogous effect upon the reduction of armour, the burden of which had increased to so great an extent that when a mediæval knight was knocked off his horse in the shock of battle he often lay helpless and disabled by the very weight and complexity of his armour.

Among Vertebrates perhaps the most striking instance of the abandonment of armour in an ancient class is afforded by the

Amphibians, which in the late-Palæozoic era were dominantly represented by the armoured Labyrinthodonts or Stegocephalia. Yet at the present day armour is conspicuously absent from the naked skin of frogs, toads, newts and salamanders, occurring only in the form of small, flexible scales embedded in the integument of the worm-like Cæcilians; the horns of the horned toad (*Ceratophrys*) are almost the only instance of superficial, bony plates occurring in modern Amphibians. In the Palæozoic era, however, we find that side by side with armoured forms such as *Cricotus*, *Anthracosaurus* and *Seeleya* there existed naked forms, from which the modern Amphibians are doubtless descended. Thus in a single family—the *Dolichosomatidæ*—we find a naked form like *Dolichosoma* (from which the modern Apoda are supposed by Fritsch[1] to be derived), as well as an armoured form—*Ophiderpeton*—with both dorsal and ventral scutes.

Chelonians are perhaps the most typically armoured class of Vertebrates at the present time; yet some of the largest and most active forms, such as the leathery turtles (*Dermochelys* or *Sphargis*),[2] have largely discarded armour, for they possess no tessellated plastron and their carapace is thin and devoid of epidermal shields. But an earlier form of this group, viz. the Eocene *Psephophorus* (nearly 10 ft. in length), not only has also a completely tessellated carapace and plastron but exhibits marginal ossifications which are absent in the existing leathery turtle. In the highly specialised, carnivorous and therefore particularly active group of mud turtles (*Trionyx*, *Emyda*, etc.), the integument likewise develops no epidermal shields and is quite soft, whilst the plastron is always separate from the carapace.

Chelonians also furnish a striking instance of the inevitable dying-out of forms which have developed extensive, massive or complicated armour, as in the case of the great bizarre horned tortoises (*Miolania*), found in the Pleistocene of Australia, Lord Howe's Island and Patagonia, in which even the tail was enclosed in a thick, knobbed, bony sheath. The same tendency is exemplified, in a less degree, by the gigantic

[1] *Fauna der Gaskohle und der Kalksteine der Permformation Böhmens*, 1883-6.

[2] A recent specimen measured by Mr. Lucas (*Nature*, February 11, 1909) reached 6 ft. 10 in. from nose to tail along the curve, and 8 ft. 9 in. from tip to tip of the flippers across the shoulders.

Pliocene Indian tortoise of the Siwaliks (*Colossochelys Atlas*); by the giant tortoises of the Egyptian Eocene (*Testudo Ammon*), of the Miocene of France (*T. gigas*) or of the Pleistocene of Malta; and by the related either recently extinct or moribund species of giant tortoises of the Aldabra and Mascarene Islands, Madagascar, South America and the Galapagos Islands.

Among Edentate Mammals the armadillos of South America furnish a close parallel to the fate of these giant tortoises—the family of the unwieldy *Glyptodon*, 7 ft. in length, with its inflexible, solid carapace, ventral buckler (in some cases) and bony caudal sheath and of the related and still more bizarre *Dœdicurus*, reaching even 12 ft. in length, with its tail-sheath expanded at the tip so as to bear a cluster of bony processes, has—like *Miolania*—completely died out. The existing armadillos, however, are not only much smaller in size and possess no ventral armour whatever but their dorsal carapace is jointed and flexible and composed of three to thirteen movable zones of scutes, allowing of considerable freedom of action, although their flexibility varies somewhat in the different species. Consequently the present survivors have been able to battle successfully with the adverse conditions resulting from the union of the two Americas (which was brought about by the emergence of the Isthmus of Panama) and the consequent introduction of higher forms, especially of Carnivores. Some of the extinct armadillos (*Chlamydotherium*) were, however, still gigantic in bulk, reaching nearly the size of a rhinoceros and actually possessed a carapace with movable bands, as in the recent forms; want of sufficient suitable food, caused by a drier climate, may have greatly militated against the continuance of such bulky monsters. The modern armoured pangolins (*Manidæ*) of the Old World are similarly the smaller survivors of a race which was represented in the Pliocene of Samos by a form (*Palæomanis*) three times as large as the existing West African *Manis gigantea*.

Other well-known instances of the extinction of unwieldy armoured creatures are to be found in the horned dinosaurs (*Triceratops*, *Sterrholophus*) among reptiles and in the colossal *Dinoceras* (*Uintatherium*) and *Arsinoitherium* among Mammals, although in these cases the excessive armour was confined to the extraordinary horned processes of the skull. Even in recent years one species of rhinoceros has become extinct and the family seems to be moribund, a circumstance not entirely due to human agency.

Turning to the early forerunners of fishes, we find that the heavily armoured Ostracoderms, which flourished in the Silurian and Devon, did not survive the latter period; their part in the economy of nature was thenceforward represented by the more highly organised true fishes of the Ganoid type. In this group a marked progression is traceable in the abandonment of armour and the evolution of more and more flexible types, through the *Amioidea* to the modern *Teleostei*, which show such remarkable variability and adaptability to all conceivable circumstances.

Among the Mollusca several striking examples of the advantages resulting from a reduction of armour are to be found both among the Cephalopoda and the Gastropoda. The massive guard of the *Belemnitidæ*, so successful a group in the Mesozoic era, became reduced in size in course of time and finally disappeared; some of the descendants of the *Belemnitidæ* continued to retain the phragmocone, either well developed but modified into a spiral shell as in *Spirula* or else flattened out and accompanied by a very rudimentary guard as in *Sepia*. Other members of the group, on the other hand, retained and developed still further the horny pen or pro-ostracum of the belemnite, as in the Calamaries. In this group the calcareous secretion of shell-substance has been entirely abandoned and it is here that we find the Cephalopoda reaching their highest development, often attaining the gigantic length of 40–60 ft.,[1] as in *Architeuthis* and its allies, which probably form the basis of most of the sea-serpent stories.[2] Finally, the octopus, which in the adult stage has lost all trace of a shell (excepting, according to Owen, for two short, cartilaginous stylets embedded in the dorsal mantle), shows indications of a shell only during its development.

In Gastropoda there are several independent lines along which a reduction of the shell has taken place, accompanied by a greater predominance and a wider distribution of the race, by a frequent increase in size and usually by the adoption of carnivorous habits. Among the Euthyneura, the Pulmonata

[1] Verrill, *Amer. Journ. of Science and Arts*, 1875, ix. pp. 123, 177.

[2] Lee, H., *Sea Monsters Unmasked*, London, 1883; and Hoyle, W. E., *Proc. Roy. Phys. Soc. Edinburgh*, ix.

furnish in the slugs one of the most familiar instances of the reduction of the shell. Thus, according to Simroth,[1] *Vitrina* and *Hyalinia* of the Limacidæ family are to be considered ancestral types. On the one hand, *Vitrina* (fig. 1), in which the shell is already insufficient to cover the animal when retracted, seems to have given rise through the somewhat flattened *Daudebardia* (Helicophanta) *brevipes*, fig. 2 (with the mantle-lobes only partly reflected over the shell) to *Parmacella*, fig. 3 (in which the flattened shell still shows a subspiral apex open to the air), and thence onward to *Limax*, fig. 5 (with its completely flattened, internal, disc-like shell). On the other hand, the worm-eating *Testacella*, fig. 4 (still with an external shell), has diverged in a less advanced degree from *Daudebardia*. These forms are either wholly or occasionally carnivorous. Their relationships are based mainly on a comparison of the tooth-ribbons of the various genera. On the other hand, the slugs *Arion* (with no shell or only calcareous granules), *Geomalacus* (shell unguiform), *Oopelta* (no shell) and *Philomycus* (no shell) have been shown on similar grounds to display an affinity to *Helix*. The Helicidan slugs reach their highest differentiation in the naked *Peronia* and *Onchidium* (with dorsal eyes on a vertebrate plan) and some of these forms seem to be reverting to a marine mode of life.

A similar degeneration of the shell, coupled with more active habits and greater differentiation of form, is afforded by the Opisthobranch section of the Euthyneura. In the Cretaceous period, beds of limestone were often filled with the large, massive shells of *Actæonella* (fig. 6) but at the present day their near relatives—e.g. *Aplustrum*, *Atys*, *Philine* (figs. 7, 8, 9)—are so thin-walled as to be called bubble-shells (Bulloidea); they are carnivorous and show the tendency to a more active and pelagic life by being able to use their wide lateral mantle-lobes for swimming. In some cases the body-whorl of the shell becomes so large (*e.g.* in *Philine aperta*, fig. 9) that the shell is widely open and in this case completely enclosed by the mantle-lobes, which Garstang[2] has shown to be protectively coloured. Indeed, a gradual series can be constructed of living species of the Bulloidea, from *Actæon* to *Doridium*, clearly showing how the reflected epipodia gradually assume the supporting

[1] *Sitz.-Ber. Naturforsch. Ges. Leipzig*, 1886-7, pp. 40-48.

[2] *Conchologist*, ii. p. 49.

function of the shell, until the latter is completely enclosed and correspondingly degenerates to a membranous rudiment.

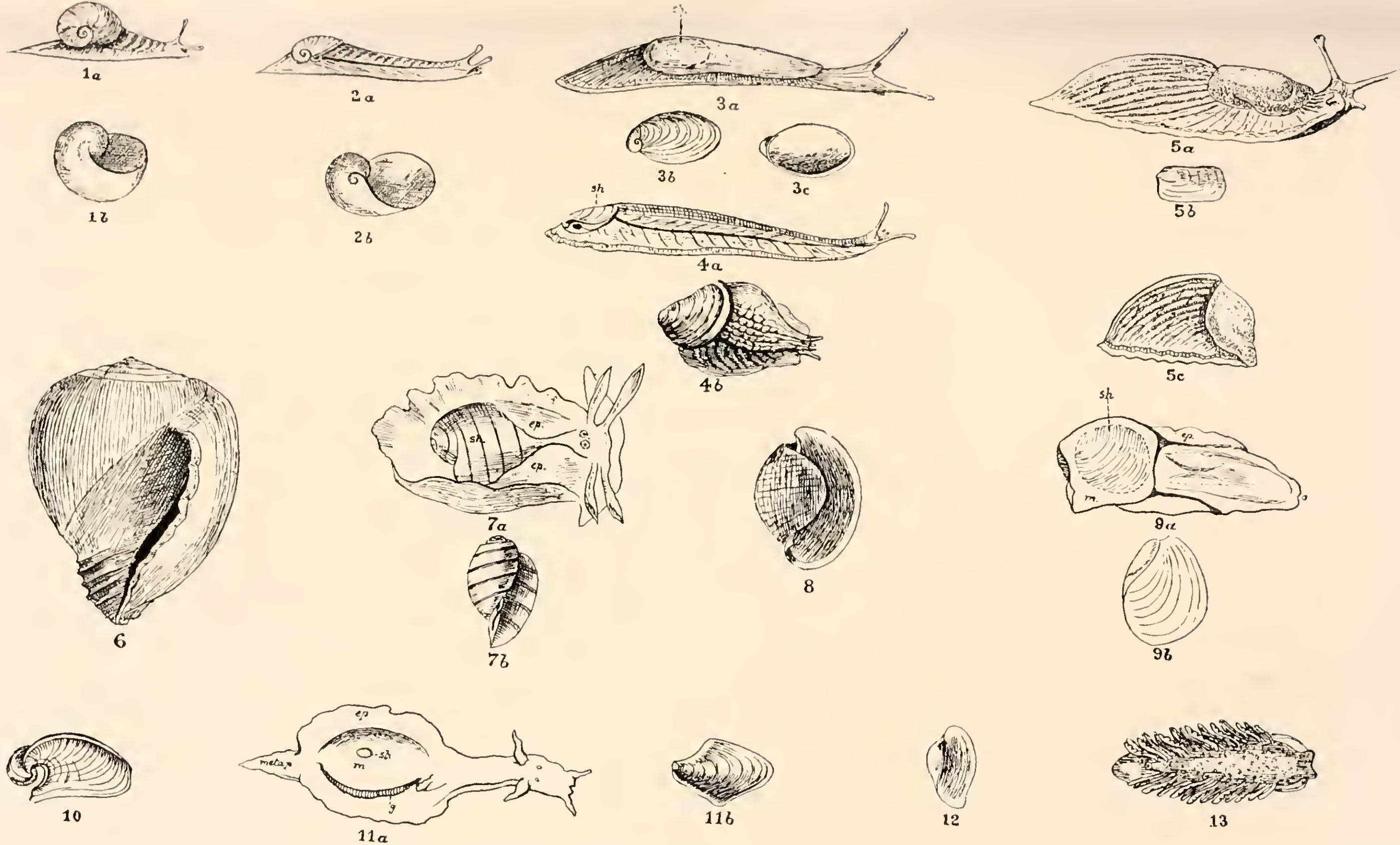
In the closely allied Aplysioidea, *Dolabella* (fig. 10) still has a calcareous shell with a curved apex, whilst in the large, active, carnivorous sea-hare (*Aplysia*, fig. 11), the nearly enclosed shell is flattened and membranous; furthermore, in *Notarchus* the shell is microscopic and the animal is ornamented with dendritic filaments, foreshadowing the *cerata* or dorsal papillæ of the Nudibranchs. According to Bergh,[1] the closely related Ascoglossa (*Lobiger*, fig. 12; *Alderia*, fig. 13; *Elysia*), in which the shell is usually altogether absent and *cerata* are more fully developed, form a transitional group between the Tectibranchiata (as exemplified by the Aplysioidea) and the Cladohepatic Nudibranchiata (*Æolis*, *Dendronotus*, fig. 14; *Phyllirrhoë*, *Tritonia*, etc.), whilst he considers the Pleurobranchoidea (*Umbrella*, *Pleurobranchus*), which have developed on parallel lines to the Aplysioidea, to lead on to the Holohepatic Nudibranchiata (*Doris*, *Polycera*, *Idalia*, *Phyllidia*, etc.).[2] Although both shell and mantle have completely disappeared in the adult Nudibranchs—the larval stage still possesses a nautiloid shell, as in *Aplysia*—yet the group is by no means degenerate in character, for it is so universally widespread and shows such a rich variety of adaptive forms, that it must, on the contrary, be regarded as a highly successful branch of molluscs. The disappearance of the mantle has enabled the animal to develop *cerata* to a wonderful extent, with the concomitant advantage, according to Prof. Herdman,[3] either of protective colouring (*Tritonia*, *Doto*, *Archidoris*, *Calma*) or, when brightly coloured, of acting as warning signals (*Æolis*, with stinging-cells); in his opinion the *cerata* are only of secondary importance so far as respiration is concerned.

Whilst the sea slugs or Nudibranchs mark the limit in one direction of the evolution of completely naked forms from primitive Tectibranchiata with massive shells, thereby attaining a greater activity in crawling (only a few, e.g. *Phyllirrhoë*, are

[1] Semper's *Reisen im Archipelago der Philippinen*, 1870–92.

[2] The Cladohepatic sea-slugs are distinguished by the possession of jaws and a branched liver, the branches often extending into the *cerata*. In the simpler Holohepatic division the liver is never branched and jaws are usually absent.

[3] *Quart. Journ. Microsc. Soc.*, N.S., xxxi. (1890) p. 41; and also Garstang, *Conchologist*, ii. p. 49, and Hecht, *Comptes Rendus*, cxv. p. 746.



1. *Vitrina Draparnaldi*, Cuv.; *a*, animal; *b*, shell. 2. *Daudebardia* (*Helicophanta*) *brevipes*, Fér.; *a*, animal; *b*, shell. 3. *Parmacella Valenciensii*, W. & B.; *a*, animal (*sh.*, position of internal shell); *b* and *c*, shell, external and internal aspects. 4. *Testacella haliotidea*, Fér.; *a*, animal extended (*sh.*, shell); *b*, animal partly retracted. 5. *Limax Sowerbyi*, Fér.; *a*, animal extended; *b*, internal shell; *c*, animal retracted. 6. *Actæonella gigantea*, Sow.; partly broken, showing the thick shell. 7. *Aplustrum aplustre*, L.; *a*, animal (*ep.*, epipodia; *sh.*, the partly concealed shell); *b*, shell. 8. *Atys naucum*, L. 9. *Philine aperta*, L.; *a*, animal (*ep.*, epipodium; *o*, mouth; *m.*, mantle; *sh.*, position of internal shell); *b*, shell. 10. *Dolabella scapula*, Martyn. 11. *Aplysia depilans*, Gmel.; *a*, animal (*ep.*, epipodium; *sh.*, internal shell seen through orifice of *m*, reflected mantle-skirt; *g*, ctenidium; *metap.*, metapodium); *b*, shell. 12. *Lobiger Philippii*, Krohn. 13. *Alderia modesta*, Loven.

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pelagic), the Pteropoda, on the other hand, mark the present limit of evolution from the same group in the direction of a free-swimming, pelagic existence. Here again, as in the sea slugs, a double origin for this group has been suggested: the Thecosomata, in which a shell is present in the adult, being derived from the more primitive group, the bubble shells or Bulloidea; the naked, carnivorous Pteropoda Gymnosomata (with only a larval shell) from the more advanced section, the sea-hares or Aplysioidea. It is, however, only in the family of *Limacinidæ* that the shell has retained its spiral form (e.g. *Spirialis* and *Limacina*, figs. 15 and 16); in the *Cymbulidæ* it is certainly a new structure altogether; in all probability this is also the case with the *Cavoliniidæ* (fig. 17), the third remaining family of the Thecosomata.

A somewhat analogous, although less well-established case of the degeneration of the shell in Gastropoda, concomitant with increased activity of the individual organisms and widespread distribution of the family, is to be found in the Streptoneura. Although it is somewhat hazardous to deduce affinities from fossil shells of which the animals are completely unknown, many authorities concur in regarding the thoroughly pelagic class of Heteropoda as derived from the dominant Palæozoic family of the *Bellerophontidæ*, in spite of the fact that intermediate forms are hardly known; but want of continuity is probably due more to lacunæ in the present state of our knowledge, which may at any time be filled up by some fortunate find (as in the case of *Archæopteryx*). Yet *Bellerophon* shows a progressive widening of the body whorl, which (as already seen in the Bulloidea) is associated with an increased development of the body and a corresponding degeneration of the shell. Some of the Silurian forms (*Salpingostoma*, *Tremanotus*) are much more expanded and possess wider mouths than the Ordovician representatives (*Cyrtolites ornatus*, *Bellerophon argo*, fig. 18, *B. acutus*) of the family; this tendency is still noticeable in the Carboniferous forms—e.g. *Bellerophon bicarenus* (fig. 19), *B. costatus*. In point of massiveness of shell, *Bellerophon* can compare with *Actæonella*, whilst the modern Heteropoda have as thin and delicate a shell as the equally pelagic Pteropoda. The small, globular *Bellerophina minuta* (fig. 20) of the Gault may perhaps be regarded as belonging to the Heteropoda; and the shell of the modern *Atlantidæ*, which still forms a complete protection to the animal, shows several points of resemblance with that of the Palæozoic *Bellerophontidæ*, not only in the general shape but also in possessing a well-marked keel as well as the characteristic notch (*Atlanta*, fig. 21); in the closely allied *Oxygurus* (fig. 22), as well as in the other shelled Heteropoda (in which the shell is only large enough to protect the visceral hump—*e.g.* in *Carinaria* and *Cardiopoda*), the notch is no longer present, although the keel is still prominent in *Carinaria* (fig. 23). In the Pterotracheacea (*Pterotrachea*, *Firuloides*) the shell is absent altogether, the visceral hump is reduced and the body has undergone still further elongation. Indeed, the group of the Heteropoda or Natantia shows its successful character, not only in a considerable degree of differentiation in their organs (*e.g.* elaborate tooth-ribbon, highly organised eyes, fin-like development of the mesopodium and tail and separation of the sexes) but by their abundance in numbers; for the shells of *Atlanta peronii* (fig. 21) and *Oxygurus Keraudrenii* (fig. 22) are sometimes found in very considerable quantities in the *Globigerina*-ooze.[1] There is also a tendency towards increase in size, for a *Carinaria* over two feet in length was captured in 1898 during the voyage of the *Valdivia*.

In the Pelecypoda it is obvious that since their bivalve shells act (apart from their protective rôle) essentially as plates of attachment for the powerful adductor muscles, it is impossible for the enclosed animal to dispense with them. Hence it is not surprising that, when the members of a family like the *Hippuritidæ* became so extraordinarily massive as to allow very little room for the organism and became grouped into masses rivalling coral-reefs, they died out completely under altered conditions, since they were utterly unable to dispense with their shells or even to reduce their heavy armour, as in the case of the different groups of Gastropoda which have just been discussed. The theory of Steinmann,[2] that no group ever becomes extinct and that the descendants of the *Hippuritidæ* are to be found in the Tunicata, is so violently opposed to our knowledge of the anatomy and vertebrate relationship of the latter group, that it may be merely mentioned to be dismissed.

In another class of animals—the Crinoidea—in which calcareous armour plays a still more pronounced rôle than in the

[1] Thomson, Sir Wyville, *Voyage of the "Challenger,"* i. p. 121.

[2] *Die geologischen Grundlagen der Abstammungslehre*, Leipzig, 1908.

14. *Dendronotus arborescens*, Müll. 15. *Spirialis bulimoides*, d'Orb. 16. *Limacina antarctica*, Hooker. 17. *Hyalea tridentata*, Gmel. 18. *Bellerophon Argo*, Billings; ***a***, end view; *b*, side view of shell. 19. *Bellerophon bicarenus*, Lév.; *n*. notch. 20. ***Bellerophina minuta***, Sow. 21. ***Atlanta Peronii***, Les.; ***a***, side view; *b*, end view of shell (*n*., notch; *k*., keel). 22. *Oxygurus Keraudrenii*, Rang.; *k*, keel; *operc.*, operculum; *metap.*, metapodium; *mesop.*, mesopodium; *prop.*, propodium; *ph*., pharynx; *t*, tentacles; *e*., eye. 23. *Carinaria cymbium*, L.; *k*., keel of shell; *metap.*, metapodium; *mesop.*, mesopodium; *s*., sucker; *ph.*, pharynx; *t*., tentacles; *e*., eye; *ct*., ctenidium. The figures are not drawn to scale.

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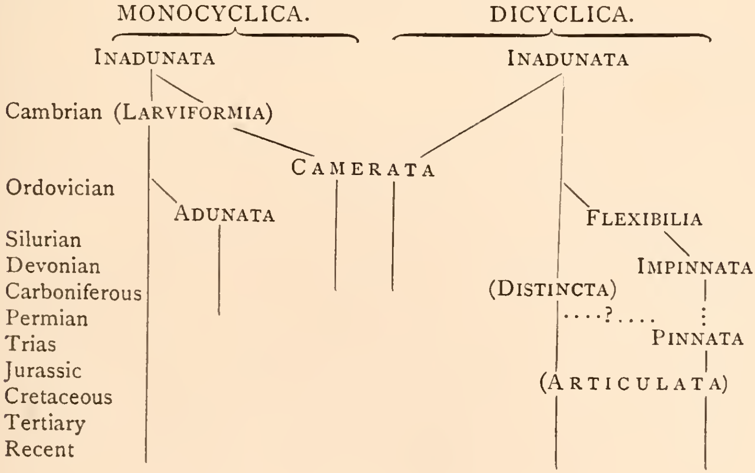
Mollusca, the tendency to extinction, owing to increased rigidity and excessive accumulation of armour, is even more marked; the efforts of the race towards attaining flexibility are more than once evidenced by forms which evolved in response to new and altered conditions of environment.

The accompanying genealogical diagram (after Bather[1]) shows graphically how, in his opinion, the two great sections of Crinoids, the Monocyclica and the Dicyclica, both produced forms in the Palæozoic era which displayed an increasing tendency towards rigidity of structure. Although exception has been taken to his separating into monocyclic and dicyclic groups the Inadunata (those forms in which the arms are quite free from the calyx, which are regarded by Wachsmuth and Springer as a single, homogeneous group) and to his treating the Camerata as having a double genetic origin from each of these groups, yet for the present purpose this difference of opinion among experts is immaterial. In fact, in discussing the evolution of Crinoids and their tendency to greater rigidity (Camerata) or flexibility (Flexibilia), the differences become even more greatly emphasized by taking in turn the monocyclic and dicyclic divisions.

Thus, in the Camerate section of the Monocyclica and of the Dicyclica the brachial plates of the arms and their first branches became rigidly incorporated in the calyx and the ambulacrals

[1] "Echinoderma," vol. iii. of Lankester's *Treatise on Zoology*, 1900, from which many of the following facts have been taken.



became similarly anchylosed in the solid, dome-like tegmen. In this manner there arose massive, plated forms like *Eucalyptocrinus* (with an anal tube so large that the arms lie in grooves of it), *Batocrinus*, and *Actinocrinus* among the Monocyclica and *Rhodocrinus* and *Reteocrinus* among the Dicyclica.

In the Silurian the monocyclic section gave rise to another offshoot—the Adunata—in which a somewhat similar rigidity was secured by the tegmen growing into a solid mass and the arms becoming biserial but the calyx was not affected to so great an extent, *e.g.* in such forms as *Platycrinus*, which attained its maximum development in the Carboniferous and *Hexacrinus* in the Devonian.

All these forms in which the armour was increased and its elements anchylosed together so as to secure greater rigidity of structure, failed to survive the great alterations of environment which took place at the close of the Palæozoic. In the Mesozoic era the only forms of the Monocyclic Crinoids which could persist were simpler and more generalised forms, such as *Plicatocrinus*, from which the Upper Jurassic free-swimming, stemless *Saccocoma* is considered by Jaekel[1] to have evolved. The adaptation of the latter to a pelagic existence is exemplified by the thin and flexible character of its calyx and by the manner in which the skeletal tissue in general has lost its dense nature, showing a loose and reticulated structure. The arms also are relatively slender, with only scattered pinnules and possessed the power of rolling themselves up in a marked degree. Even in *Hyocrinus*, which is the modern, living representative of the Monocyclic Crinoids, the plates of the calyx are comparatively thin and the basals are reduced to three in number.

In the Inadunate section of the Dicyclic Crinoids, the Cyathocrinoidea (*Cyathocrinus*, *Cupressocrinus*, etc.) were the most specialised; hence the last survivors of this group died out altogether in the Permian. On the other hand, the Distincta or Dendrocrinoidea possessed more generalised characteristics and were therefore able on occasions to give rise to more flexible forms—the Flexibilia—which in Palæozoic times possessed simple arms without pinnæ (e.g. *Ichthyocrinus*, *Taxocrinus*, *Dactylocrinus* among fixed forms and *Edriocrinus*, which was free-swimming in the adult condition). Again, in Mesozoic

[1] *Zeitsch. deutsch. geol. Ges.*, xliv. 1892.

times, this primitive stock budded off a fresh series of flexible forms with pinnate arms (*Apiocrinus*, *Eugeniacrinus*, *Bourgueticrinus*, and the existing *Holopus* among fixed forms; *Thaumatocrinus*, *Antedon*, *Atelecrinus* and *Actinometra* among free-swimming forms). In the shallow-water genus *Holopus* the stem is no longer calcified but is represented by a leathery outgrowth from the calyx. *Antedon* and *Actinometra* are world-wide in their distribution and testify also by their numbers to the success attained by the adoption of a free and unattached existence in the adult stage.

Although in the Distincta section of the Dicyclic Inadunata (*Dendrocrinus*, *Botryocrinus*, *Scaphiocrinus* and the Triassic *Encrinus*) there does not seem to have been any successful attempt at evolving free-swimming forms, yet in the later, Mesozoic, part of the main Dicyclic stock—the Articulata, fixed forms of which were represented by *Pentacrinus* and *Bathycrinus*, both with living species—there were several such attempts, e.g. *Marsupites* and *Uintacrinus*. In the latter especially the theca was flexible and comparatively large and the arms extremely long. Even in the living *Pentacrinus* (Isocrinus) the stem ceases to be permanently attached to the substratum in the adult stage, for the stem breaks across, the animal swims about freely with active movements of the arms, just as in *Antedon* and from time to time it attaches itself temporarily by means of a considerable remnant of its stem and its stem-cirrhi. Here we merely find a repetition of what has occurred again and again from the earliest times; either the end of the stem became smooth and rounded-off (the Ordovician *Calceocrinus*) or modified into an anchoring grapnel (the Devonian *Myrtillocrinus*) or else it is prehensile and able to curl itself round a *point d'appui* (*Acanthocrinus*).

In the light of modern research and consequently of more natural and exact methods of classification, entire classes of animals, which were formerly held to have become entirely extinct at the close of the Palæozoic era, have now been shown to have left descendants, which flourished in subsequent periods, even down to the present day. Thus the old classification of Palæocrinoidea has long since been proved to be inadequate and incorrect, since the Mesozoic Crinoids, as already indicated, were descended from and closely allied to Palæozoic families which had retained sufficient plasticity of organisation to vary adequately under changed conditions of environment. Similarly the term Palæoechinoidea is no longer held to denote a natural group, for the Cidaroidea, from which all the post-Triassic and living Echinoids can be derived, are clearly descended from the late-Palæozoic *Archæocidaridæ*; even in the Cretaceous period the reduction of the rows of interambulacral plates to two in number was not absolutely constant, for *Tetracidaris* has four rows of such plates near the mouth, decreasing to two at the apex.

In corals, again, the old designation of Rugosa or Tetracoralla, confined to Palæozoic forms, has proved to be equally artificial, some of its members being now classified with the Madreporaria, *e.g.* the *Cyathophyllidæ* (which are probably ancestral to both the *Astræidæ* and the *Fungiidæ*), and the *Cyathaxonidæ* (which are considered to have given rise to the *Turbinoliidæ*). The massive Zaphrentoidea, on the other hand, have recently been shown by Duerden[1] to be closely allied to the naked *Zoanthidæ* (many of which are epizoic on Hermit Crabs) and furnish another interesting instance of the abandonment of armour and the survival of more flexible forms.

[1] *Ann. Mag. Nat. Hist.* 7, ix. p. 381, 1902.

RECENT PROGRESS IN THE STUDY OF BRITISH CARBONIFEROUS PLANTS

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The British School of Fossil Botany, which in recent years has become probably the largest numerically in the world, continues to gain adherents, and, as the result of its labours, an ever varied series of fresh facts is being added to our knowledge of both the form and structure of the plants of the past. In the present paper it is proposed to enumerate the additions which have been contributed quite recently on the subject of *Carboniferous* plants. In the main the activities of the British School have lain in this direction, though much important work on both British and Foreign Mesozoic and Tertiary floras has also been published.

The studies under consideration naturally fall into two classes, for it is but rarely that a memoir is equally concerned both with the external morphology and the internal anatomy of the same plant. We will therefore first notice those dealing with petrified plant remains, in which the internal structure is preserved.

The "present position of Palæozoic Botany," as it stood some three years ago, is fully defined in the masterly article published by Dr. Scott (48)[1] in 1906 in the first number of the *Progressus Rei Botanicæ*. Some account will also be found of the quite recent advances in our knowledge, so far as the petrified fossils are concerned, in the second edition of the same author's *Studies in Fossil Botany* (52). Lady Isabel Browne (17) has also contributed a detailed, critical résumé, of great interest and importance on our present knowledge of the phylogeny and inter-relationships of the Pteridophyta. In the present instance no attempt will be made to summarise our knowledge on a similar scale, we will content ourselves with

[1] Full references to all the memoirs quoted will be found in the Bibliography on p. 146.

brief notices of those memoirs on the structure of British Carboniferous plants which, for the most part, have appeared since Scott's article was published. By arranging these memoirs systematically it will be possible to give some idea of recent contributions to our knowledge of each of the great groups of Carboniferous plants.

It will be apparent that the distribution of research has been very uneven. The Lycopods, the Pteridosperms and the Primofilices have received more attention than the Equisetales or the Cordaitales, or to some extent the Sphenophyllales. This may no doubt be accounted for, in part, by the chance which has furnished fresh material for inquiry, but it is also partly due to the fact that certain special advances have been made within the last decade in regard to the first three groups above mentioned, and thus an added stimulus has been given to research in these directions.

Equisetales

This group has for some years past been comparatively neglected. We still await a fuller knowledge of the structure of the chief Lower Carboniferous representative, *Archæocalamites*, derived from British material, and also of the interesting *Calamites pettycurensis* (48, p. 157) in which centripetal xylem is found to occur.

As regards the Upper Carboniferous members, the re-description by Hickling (22) of the rare Calamite cone, *Palæostachya vera*, is of great interest, especially as throwing light on the homologies of the corresponding organs of *Calamostachys*, and on the question of the general relationships of the Equisetales to the Sphenophyllales. Hickling finds that *Palæostachya* is more closely connected with *Calamostachys* than has hitherto been supposed, and that the axillary position of the sporangiophore in the former is derived from the condition of affairs met with in the latter genus.

The "relation of root to stem" in the genus *Calamites* has been discussed by Maslen (38). The roots prove to be mainly adventitious and not to be borne in the same manner as in the recent *Equisetum*. There is no evidence of any connection between the roots and the infra-nodal organs of Williamson. These infra-nodal organs were the subject of an earlier inquiry by Jeffrey (24). Miss Stopes (61) has described the formation of callus wood in wounded Calamite stems. In her specimens the wounds extended to the pith, and strands of new wood with inverted orientation had been formed to complete the broken xylem ring.

Further work is desired on this group in the direction of a correlation of the various types of pith casts, so well known as impressions, with structure specimens.

Sphenophyllales

The outstanding recent contribution to our knowledge of this group is the description by Scott (46) of the new cone *Sphenophyllum fertile.* Here both the dorsal and ventral lobes of the sporophylls are fertile, and "are divided in a palmate manner into several branches each of which constitutes a sporangiophore." This cone is regarded by Scott as a "specially modified rather than a primitive form of fructification."

A "suggestion of heterospory" in another cone, *Sphenophyllum Dawsoni*, has been recorded by Thoday (64) but otherwise this group has not been investigated recently. Further studies of the roots, the mode of branching, and the structure of the leaves remain for the future.

Lycopodiales

In quite recent times the lion's share of attention has been devoted to the Palæozoic Lycopods. Not only has a further seed-bearing Lycopod been described but the correlation of the internal structure of two species of *Lepidodendron* with their external features has been effected. The structure of the genus *Sigillaria* has been investigated in several cases, though the full account of the structure of *Bothrodendron*, discovered some years ago by Lomax, has not yet appeared. Speaking generally, our knowledge of nearly all the great Lycopod genera of Palæozoic times has been considerably extended, especially anatomically.

One of the most interesting of recent memoirs has been that by Miss Benson (13) on *Miadesmia*, the first herbaceous Carboniferous Lycopod, which has been made known from structure specimens. This plant bore seed-like organs (integumented megasporangia) with which may be compared *Lepidocarpon*, among previously described Carboniferous fossils, and *Isoetes*, among recent Lycopods.

From the Lower Carboniferous rocks, a new *Lepidodendron* (*L. pettycurense*) has been described by Kidston (34), the structure of which is interesting from the fact that the stem possesses not only a well-developed zone of secondary wood but a solid central mass of primary wood.

A new *Lepidophloios* (*L. Scottii*) in the Halonial condition, has been discussed by Mr. W. T. Gordon of Cambridge (20). Here the external surface of the stem was exposed and its characters were found to differ from the typical Calciferous Sandstone member of this genus, *L. scoticus*, well known from impressions.

From the Upper Carboniferous rocks, the correlation of the external features of two species of *Lepidodendron*, occurring very commonly as impressions, with their internal structure, has proved interesting and important. Both in the case of *L. obovatum* elucidated by Scott (51), and in *L. aculeatum* soon afterwards discussed by Seward (56), the type of internal structure proved to be that hitherto associated with the genus *Lepidophloios* alone. Thus we know now that the so-called *Lepidophloios* type of anatomy was common not only to the genus *Lepidophloios*, but also to some, though not to all, species of *Lepidodendron*.

The stems and branches of another species, *L. selaginoides*, have also been correlated from structure specimens by Weiss and Lomax (72) by means of actual continuity between the two organs.

Weiss (70) has contributed a special study of the parichnos in the Lepidodendraceæ, the function of which he regards as respiratory. Watson (66) has described a new species of *Lepidodendron* (*L. Hickii*) in the Halonial state, formerly confused with *L. Harcourtii*.

The latter author (68) has also published a memoir "On the Ulodendroid scar," which he regards as "the place of insertion of an ordinary branch, which was probably provided with some sort of branch-shedding mechanism," not, as has hitherto been supposed, the scar "produced by the pressure of the base of a sessile cone."

An important contribution to our knowledge of *Bothrodendron* has been made by Watson (67), who has attributed to this genus a heterosporous cone of small size, conforming to the Lepidostroboid type. This cone which belongs to *B. mundum*, has a large ligule and the sporangia have only a very narrow attachment to the sporophylls.

Miss Berridge (16) has described two new specimens of *Spencerites insignis*, which, "particularly in the absence of Dictyoxylon structure in the cortex, the more ovoid form of the sporangia" . . . "and the greater tangential width of the sporophyll-head," differ from those previously described. Mrs. D. H. Scott (54) has also figured and discussed the megaspore of *Lepidostrobus foliaceus*.

The structure of *Stigmaria* has received further attention at the hands of Weiss (71), who has described an interesting case of a Stigmarian axis in which centripetal primary wood occurs.

The stem of the genus *Sigillaria*, the structure of which was very imperfectly known until quite recently, at any rate so far as British examples were concerned, has now been worked out in the case of no less than three species, in all of which it has been possible fortunately to correlate the external features with the internal structure. These three species all belong to the "ribbed section" of the genus or Eusigillariæ. The Favularian type, *Sigillaria elegans*, was described by Kidston (32) in 1905, the structure of the cone-scars being illustrated and the general evolution of the genus discussed.

More recently the writer, in conjunction with Hugh Thomas (11) has illustrated the structure of *S. scutellata*, a Rhytidolepis type, on which species, with the addition of *S. mammillaris*, Kidston (35) has also issued a short note. In *S. scutellata* it was found that the leaf-trace, when traversing the leaf-base, possessed a double xylem strand; consequently the leaves, previously described by Scott (45) in 1904 as *Sigillariopsis sulcata*, are the leaves of a ribbed *Sigillaria*. Kidston also arrived at the same conclusion independently.

Miss Coward (18) has described the structure of a decorticated (*Syringodendron*) specimen of *Sigillaria*.

Thus we see that the study of Palæozoic Lycopods has recently yielded many interesting results. Yet much still remains to be accomplished, especially along the lines adopted in the above-mentioned memoirs. The correlation of the external features with the internal structure, a path of investigation which we have seen has already proved profitable, opens up a most promising field.

Additional species both of *Lepidodendron*, *Sigillaria* and *Bothrodendron* will no doubt be the subjects of further inquiry in this respect. The cone of *Sigillaria* remains still unknown from British Rocks in the petrified state.

The Pteridospermeæ

In an article in the second number of the new series of Science Progress (1906) I gave some account of recent work on the Pteridosperms, under the title "The Origin of Gymnosperms" (6). It is thus unnecessary to recapitulate these important discoveries here. During the last few years no further memoirs have been published in Britain bearing directly on the status of this group, nor on the attachment of the seeds. The impressions of seed-like bodies borne on a Sphenopterid frond, described by the present writer (8) under the name *Carpolithes Nathorsti*, have been shown by Nathorst (40, p. 10), on subsequent and chemical examination, to be of a doubtful nature, and more possibly of the nature of fern fructifications, or of the male organs of a Pteridosperm. At any rate, they do not appear to be seeds and must thus be dismissed from any discussion on the question of the seed-bearing habit of the Pteridosperms.

The announcement of the discovery of the seed and male organs of *Heterangium*, the most fern-like of all the Pteridosperms, has just been made by Miss Benson (15); her paper, now passing through the press, will be awaited with interest.

Several new seeds and stems belonging to the Pteridosperms have been described, notably the stem *Sutcliffia insignis*, elucidated by Scott (49) in 1906. Scott finds that *Sutcliffia* is the most primitive member of the Medulloseæ yet discovered, its simple monostelic structure adding "probability to the suggestion that the Medulloseæ, as well as the Lyginodendreæ, may have sprung from a type anatomically similar, apart from details, to *Heterangium*."

An important paper from the pen of Oliver (43) on *Physostoma elegans*, Williamson, a seed formerly known as *Lagenostoma physoides*, Williamson, has just appeared. This stem is ribbed, and possesses at the apex a whorl of ten tentacles, surrounding the pollen chamber. Both the ribs and tentacles are adorned with long tubular hairs, which give them a characteristic appearance. The seed is regarded as the most primitive which has yet been discovered and is referred provisionally to the Lyginodendreæ.

The structure of seeds belonging to the genus *Trigonocarpus* has also been elucidated by Scott and Maslen (53) and a note on the same genus and *Polylophospermum* has been published by Oliver (41). The latter (42) has also discussed the Palæozoic seed in its many aspects, in his address to the Botanical Section of the British Association at York.

Miss Benson (14) has recently figured the contents of the pollen chamber of a specimen of *Lagenostoma ovoides* in which germinating pollen grains showing antherozoids occur.

The Primofilices

In 1904 the present writer (5) suggested the use of the new term Primofilices to designate the ancient group of Palæozoic ferns, on the ground that at that period it is not possible to distinguish clearly the two more modern groups, the Lepto- and Eusporangiateæ. One family of the Primofilices, the Botryopterideæ, has been considerably elucidated in recent years at the hands of Scott (48, pp. 178–86; 52).

The same author (48, p. 184; 52, vol. i. p. 292) has described *Pteridotheca Williamsoni*, where sori of sporangia are borne on "the segments of a much divided leaf, apparently of Sphenopterid form." The sporangia and germinating spores of *Stauropteris oldhamia* have also been made known by Scott (47, 48, 50).

One of the most interesting advances in our knowledge of this group will be found in the paper by Miss Stopes (60) on *Tubicaulis sutcliffii*, the first British member of the genus, and the second example of *Tubicaulis* so far discovered. "It appears to be one of the simpler Botryopterideæ and to have no direct affinity to any living fern."

Mrs. Scott's (55) new fossil, *Bensonites fusiformis*, from the Lower Carboniferous of Burntisland, often associated with and occurring on the sporangia of *Stauropteris burntislandica*, appears to be of the nature of a gland, though possessing a vascular strand.

Other contributions to our knowledge of the Primofilices are Weiss's paper (69) on the tyloses occurring in the stem of *Rachiopteris corrugata*, and Watson's communication (65) on *Cyathotrachus altus*, an isolated sporangium from the Lower Coal Measures.

Kidston (36) has briefly described a new *Botryopteris* and a section of an interesting fern of the genus *Dineuron*, from the Lower Carboniferous of Pettycur (Burntisland). Gordon has in preparation memoirs on *Dineuron* and other remarkable genera from the same locality.

The Cordaitales

Scarcely any research has recently been attempted in Britain on this important group. The last memoir of special interest is that by Scott (44), "On the primary structure of certain Palæozoic stems with the Dadoxylon type of wood," published in 1902. Since then apparently only one other paper has appeared, founded on French Stephanian material, in which Miss Stopes (59) described the leaf structure of *Cordaites*. We await with interest the researches of Scott and Maslen on the genus *Poroxylon*, and of Miss Robertson on the seed *Cardiocarpon*. It is also much to be desired that further work on British specimens of this group should be undertaken, especially as regards the fructifications, if by any chance the material can be obtained.

The Origin of Plant Petrifactions

Within the last decade, the nature and origin of the calcareous nodules, known as coal balls, in which petrified plant-remains occur in the Lower Coal Measures of England, has been the subject of inquiry at the hands of Stocks (58) and more recently of Stopes and Watson (62, 63). Stocks studied the precipitation of calcium carbonate from solution in the presence of organic matter and concluded that the carbonate of the coal balls "first separated in the cells of waterlogged vegetable matter and then around it, forming a concretion which grew in the bacterial jelly, and hence acquired its rounded shape." The bacterial jelly was formed by the anaërobic decay of vegetable débris in stagnant sea-water brought about by bacteria.

Stopes and Watson find that "the coal balls were formed as concretions in the seams in which they are now found, and have not been derived from other sources." They instance several localities and seams in which they occur. In the formation of the coal balls "the sea-water played the principal part in the process of preserving and petrifying the plant-remains now found in coal balls, the calcium and magnesium sulphates being reduced from the sea-water by organic carbon liberated by the decay of the plants." Some of these conclusions are opposed to those held in other quarters (37).

Carboniferous Impressions

Turning now to the studies of Carboniferous Impressions and Fossil Floras, it may be well to take for review a somewhat longer period extending back to the year 1900. We still await eagerly the appearance of Dr. Kidston's Monograph of the Carboniferous Flora of Britain. The need for a systematic and standard work of this nature has been urgent for many years past. In this respect we have been much behind other countries. The fine series of Fossil Floras from the various coalfields of France, published in the *Études Gîtes Minéraux de la France* and other large quartos, has no parallel in Britain.

Special studies of certain genera or species, preserved as impressions, have been remarkably few in recent years, apart from the fructifications of the Pteridospermeæ, such as *Rhabdocarpus*, *Crossotheca* and *Lagenostoma* referred to in my previous review in Science Progress (6). Kidston (26) has contributed a full account of "Carboniferous Lycopods and Sphenophylls," in which a new genus from the Mountain Limestone of Westmorland, *Archæosigillaria*, is described, and a full account of *Omphalophloios* is also included.

Bernard Smith (57) has also discussed an interesting Lepidodendroid stem from the Middle Coal Measures of South Staffordshire, in which the leaf-cushions are distant from one another and separated by broad bands of striated bark.

Illustrations of the flora of the Carboniferous period have been published by Kidston (28), consisting of twenty-eight plates of Carboniferous plant impressions with explanations; quite recently the present author (10) has prepared a little volume containing sixty photographs of typical Upper Carboniferous plants.

Fossil Floras

Much progress has been made in the working out of the fossil floras collected from various British coalfields on the lines laid down by Kidston (25) in his memorable paper "On the various divisions of British Carboniferous rocks as determined by their Fossil Flora," published in 1894. The same author has proposed a new terminology of the various Palæobotanical Zones in a more recent contribution (33), which has not, however, met with universal acceptance. The following comparison indicates the changes proposed:

Kidston's Classification of the British Upper Carboniferous Rocks

1894	1905
Upper Coal Measures	Radstockian
Transition Series	Staffordian
Middle Coal Measures	Westphalian
Lower Coal Measures	Lanarkian

The detailed examination of the floras of the British coal basins has not only greatly increased our knowledge of many genera and species but possesses a special interest as a study of the course of evolution of plant life during Carboniferous times. It has also proved extremely valuable from the point of view of the mining engineer and colliery proprietor (1).

Special attention has been devoted by Kidston (33) to the floras of the unproductive *red measures*, bordering the productive measures of several of the Midland coalfields, especially the North Staffordshire, Birmingham, and Denbighshire coal basins. These have proved to belong either to the Upper Coal Measures or to the Transition Series or to both. Similarly, the red shales of the Ardwick Series (2) and part of the Bradford Series (19, 33) of Manchester have been shown to represent the Transition Series.

The fossil floras of part of the Cumberland coalfield, in which red rocks, known as the Whitehaven Sandstone, overlie a dark-coloured shale series containing coals, have also been worked out (3). The floras of both lithological divisions proved to belong to the same horizon, the Middle Coal Measures.

In regard to the floras of the larger coalfields of the Midlands and the North of England, descriptions have been given by Stopes and Watson (63) of the impressions and petrifactions of the South Lancashire coalfield, and by Horwood (23) of the Leicestershire coalfield. Investigations are in progress by Moysey of the flora of the southern portion of the South Derbyshire coalfield, especially in comparison with that of Yorkshire. Attention may be called to the method successfully employed by him (39) for splitting the fossiliferous ironstone nodules so abundant in this coalfield.

The fossil floras of the Mendip Series of coalfields, in the south and west of England and Wales, have received further attention. It has been shown that a great barren coalfield exists in Devon and North Cornwall, composed of Middle Coal Measures overlying Lower Carboniferous and Devonian rocks (4, 7). The fossil plants obtained from the recent borings through the Waldershare and Fredville Series of the concealed coalfield of Kent have been examined, and have proved to indicate that these rocks belong to the Transition Series (9). The fossil flora of the Forest of Dean coalfield has been the subject of study and the results will appear shortly. D. G. Lillie, of Cambridge, has made further collections from the Bristol coalfield, and has some interesting records to add to those published by Kidston, more than twenty years ago. His paper may be expected to appear before long.

As regards the Scotch coal basins, a few species have been added by Kidston (31) to the Lower Coal Measure flora of Ayrshire, and the same author has also published a systematic list of the Carboniferous plants of the Clyde basin (27). Bailey and Tait (12) have recorded a coal measure flora at Port Seton, East Lothian.

Kidston (29) has published an interesting and important memoir on the fossil floras of the Canonbie coalfield, Dumfriesshire, near the Scottish borderland. The whole of this region was formerly mapped by the Geological Survey as Calciferous Sandstone. Kidston finds, however, that nearly all the palæobotanical horizons of the Lower and Upper Carboniferous are represented, including the Lower, Middle and Upper Coal Measures. Several new and interesting plants are described from this region.

Little or no progress appears to have been made with regard to the fossil floras of the Irish coalfields. A short note by Kidston (30) on the Lower Carboniferous plants derived from the Arigna Mines, Co. Roscommon, is apparently the sole contribution to Irish Palæobotany to be recorded. There remains an almost untouched field for research among the Irish rocks.

The above notices, necessarily brief and in many cases quite inadequate as indications either of the value, importance or scope of many of the memoirs with which they are concerned, may, however, serve to indicate the varied activities during the last few years of those who are pursuing the study of Carboniferous plants in this country alone. It is hoped that this summary may be of use to others who may wish to keep abreast of the progress that has been made in recent years in this department of Fossil Botany.

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THE ORGANISMS OF THE SOIL

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To Liebig and the early investigators of the soil, the processes of decomposition which obviously take place in it were the results of purely chemical action. But the more the soil was investigated, the more this explanation became untenable. There was discovered in it a teeming race of animals, as well as of plants, of an order different from those which live upon the outer surface; a race of minute organisms distinguished in essential characters from the larger forms which had been thought to be the only tenants of the globe. In these dwarfs the living substance of those which had their being in Archæan times is alive to-day. Brought into existence to destroy, to break up the rocks of the primitive earth, to prey upon everything that came within their reach, many of them, when the earth became peopled with the higher animals and overgrown with the plants for which their activities had prepared the way, turned upon these usurpers and sought their annihilation. These microscopic beings of the underground world are the bacteria, moulds, fungi, blue-green algæ, myxomycetes and the host of dreaded germs which plague us, our cattle and our crops.

The main work of these organisms, however, is not to cause disease in the higher animals and plants. The soil is not primarily a medium on which to grow trees and herbs but is the domain in which bacteria and other lowly forms of life exert their activity; the higher plants exist by virtue of these, just as animals live by virtue of the herbage.

The lower organisms which live in the soil and belong to the vegetable kingdom are usually divided into the bacteria and true fungi, moulds, yeasts and so on. The following estimates are given by Ramann of the relative proportions in the various types of soil.[1]

These numbers may seem enormous but it must be remembered that the bacteria and spores are very small: a bacterium is about one-thousandth of a millimetre in diameter and the "saturation" point in soil is only reached when there are six hundred million bacteria in a cubic centimetre.

The manner of estimating such germs is by growing cultures in nutrient gelatine and agar-agar and then counting the developing colonies of bacteria; the results are approximate only and there are doubtless many that do not germinate in the particular medium employed. Others again are too small to be recognised under the highest powers of the microscope. Another factor which increases the difficulties of estimation is the enormous powers of multiplication which these organisms possess: a bacterium divides into two every thirty-five minutes; one bacterium, therefore, at the end of twelve hours will have four million descendants, so that the numbers in the soil vary from moment to moment.

Whilst actual numbers cannot be given definitely, the proportions are more or less correct. It has been found that bacteria are more abundant in the first foot of soil.[2] Thus Adametz found in one gramme at the surface 38,000 bacteria, whilst at a depth of ten inches there were 460,000. In this particular sample there were only forty to fifty fungus germs, of which six species were true moulds and four were ferments, including the yeasts of wine and beer. At three feet down the numbers decrease rapidly in proportion to the aeration of the soil. Fraenckel, however, found that even in the soil beneath

[1] E. Ramann, *Bodenkunde*, Berlin, 1905, p. 120.

Type of Soil	Bacteria in 1 grm. of dry substance	Fungi in 1 grm. of dry substance
1. Pines with beech undergrowth	35,000,000	60,000
2. Pines in boggy ground	1,647,000	343,000
3. Beech-leaf mould	31,000,000	560,000
4. Old leaf mould below No. 3.	264,000	800,000
5. Leaf mould in oak coppice	40,000,000	3,430,000
6. Pine-needle mould	50,000,000	Uncountable
7. Loamy soil	4,860,000	4,000—277,000*
8. Sandy soil	2,500,000	66,000—566,000*
9. Soil below humus	247,000	35,000—350,000*

* From one cubic centimetre.

[2] Adametz, *Inaug. Diss.* Leipzig, 1876.

the pavements of Berlin there were still considerable numbers at a depth of eight to ten feet.[1]

Bacteria predominate in cultivated lands, whilst moulds are found in open meadow and in fresh soil. It is a function of the moulds to keep the surface layer open; they send their hyphæ between the grains of sand and particles of clay, push them aside and make channels for the entrance of air. They may be called Nature's tillers. In cultivated ground man ploughs and harrows the land so that an artificial tilth is produced far in excess of that in natural soil and crops grown on it are enabled to thrive without hindrance; whereas if the same seed were planted in natural soil, just sufficiently aerated to support the indigenous flora, the germinating plants would be stifled.

The work of the moulds in another direction can be seen on pine-needle litter or fresh leaf litter, where the leaves are bound together by a web made of the tender filaments of growing moulds and decomposition goes on rapidly. Rostrup[2] called this particular form of mould *Clodosporium humifaciens* but there are doubtless many kinds at work, all active in breaking down the cellulose of plants into humus. It is this parasitic and saprophytic action of the lower organisms that has overshadowed the importance of their other less obvious activities. Many species are certainly specially fitted to promote fermentation, putrefaction, decay in all its forms in vegetable and animal tissues but some have other work to do. Kunze has shown that the higher plants have roots that are incapable of breaking down the mineral substances which they absorb and Kunze attributes the assimilation of these to the work of bacteria and moulds.[3] Nikitinsky,[4] Czapek and Kohn[5] have shown that cultures of the moulds *Aspergillus niger* and *Penicillium glaucum*, when fed with ammonium chloride, set free hydrochloric acid, which alone or in the presence of nitrates is capable of dissolving most of the known mineral substances. It is not impossible, therefore, as has been assumed, that the precipitation of gold in the hot water of the Steamboat Springs of Nevada is brought about by the

[1] Fraenckel, *Zeitschr. f. Hygiene*, vol. ii. p. 521.

[2] E. Ramann, *loc. cit.* p. 119.

[3] G. Kunze, *Jahrbüch, wiss. Bot.* vol. xlii. 1906, p. 357.

[4] J. Nikitinsky, *Jahrbüch, wiss. Bot.* vol. xl. 1904, p. 1.

[5] F. Czapek and E. Kohn, *Hofmeist. Beiträge z. Chem. Phys.* vol. vii. 1906, p. 302; F. Czapek, *Progressus rei botanicæ*, Jena, 1907, p. 436.

action of plants much in the way in which Cohn has shown, calcium carbonate is deposited in the Sprudelstein of Carlsbad.[1]

Bacteria have been studied principally from the standpoint of disease in man and animals but recently the attention of agriculturists has been directed to the nitrifying organisms. The first step in the fixation of atmospheric nitrogen is accomplished by certain flagellate cells called *Nitrosomonas*, belonging probably to the animal kingdom; these are succeeded by minute rod-like bacteria called *Nitrobacteria*, which oxidise the product of the former into nitrates. These latter live principally, or perhaps more properly should be described as having been detected living, in the root nodules of clover, peas and similar leguminosæ, and have been called *Rhizobium leguminosum*; they are occasionally found on the roots of forest trees and it is now recognised that bacteria with similar functions live free in the soil. Another nitrifying organism is the *Azotobacter chroococcus*, which lives on the leaves of trees and causes leaf mould to be so rich in nitrogenous compounds.[2] Some of the bacteria and some of the ferments also have the power of undoing the work of these nitrifying bacteria; they denitrify and liberate nitrogen from nitrates. The fact is familiar to gardeners in the case of fresh stable manure, where the action is brought about by denitrifying bacteria, whereas from rotted manure the deleterious organisms are absent.[3]

Regarding the action of the blue-green algæ in forming soil Fritsch has accumulated a large amount of information.[4] In a large tank at Nalande in Ceylon the first forms to secure a foothold on the bare rock were found to be red-coloured gelatinous species of the genera *Glœocapsa* and *Aphanocapsa*; then, when a resting-place is secured, an adhesive species, *Phormidium laminosum*, grows upon it, covering large portions of the rock surface with huge thin papery films. Tangled filaments of *Scytonema* develop out of the *Phormidium* and tufts of *Tolypothrix* succeed these. Treub, who visited Krakatoa after

[1] F. Cohn, *Neues Jahrb.* 1864, p. 580; see also W. H. Weed, The Formation of Hot-Spring Deposits, *Internat. Congr. Geol.*, *Compte Rendu*, 5th Sess. 1893, p. 360.

[2] A. D. Hall, Recent Developments in Agricultural Science, *Addresses and Papers*, *Brit. and S. Afr. Assoc. Adv. Sci. Johannesburg*, 1905, vol. i. p. 103.

[3] R. Burri and A. Stutzer, *Centr. f. Bakt.* (2), vol. i. 1895, p. 442.

[4] F. E. Fritsch, The Rôle of Algal Growth in the Colonisation of New Ground, *Geogr. Journ.* vol. xxx. 1907, p. 531.

the eruption of 1883, found the ground covered with a thin, gelatinous, hygroscopic layer of blue-green algæ of which the genera *Tolypothrix*, *Anabæna*, *Symploca*, and *Lyngbya* were the first to appear on the bare rock. These growing on the volcanic ash and pumice, of which the whole island was composed, gradually formed a soil on which higher plants could grow.[1]

Welwitsch describes a similar growth of algæ on the "Black Rocks" of Pungo Andongo, in Angola. These black rocks owe their colour to the abundant growth of a sub-aerial alga, *Scytonema myochrous* var. *chorographicum*, which generates and multiplies so rapidly during the rainy season that the upper portions of the mountains are covered with it in a very short while. Soon after the hot season has set in, at the end of May, the black plantlets begin to be discoloured by the intense heat. They gradually become dry and brittle, until they peel off entirely by-and-bye, after which the rocks lose their sombre aspect and reappear in their natural grey-brown colour.[2] Bohlin has described four algæ in the Azores living on the volcanic rocks in a similar way.[3]

The blue-green algæ are, however, semi-aquatic and they can only live in moist places; when, however, they are joined with a fungus symbiotically to form a lichen, the weft of the mycelium of the fungus protects them sufficiently from desiccation and the blue-green algæ are rendered practically independent of moisture. Welwitsch describes how in the sandy valley of Cuanza River, in Angola, a blue-green alga, *Porphyrosiphon notarisii*, extends over wide meadows. By reason of its hygroscopic nature, it absorbs the atmospheric moisture during the dewy nights, affording by this means a refreshing protection to the roots of the larger plants during the glowing heat of the day. Boodle has described a more

[1] M. Treub, Notice sur la nouvelle Flore de Krakatoa, *Ann. Jard. bot. Buitenzorg*, vol. vii. 1888, p. 213; see also Penzig, *loc. cit.* vol. xviii. 1902, p. 92.

[2] F. Welwitsch, *Journ. of Travel and Nat. Hist.* vol. i. 1868; see also Apontamentos Phyto-Geographicos solve a flora da provincia de Angola, etc., *Annaes do Conselho Ultramarino*, Parte não off. Ser. i. Dez. 1858, p. 533; also E. Tenzl, Bericht über einige der wichtigsten Ergebnisse der Bereisung der Portugiesischen Kolonie von Angola in den Jahren 1850–1860 durch Dr. F. Welwitsch, Vienna, 1864.

[3] F. Bohlin, Étude sur la Flore algologique d'eau douce des Açores, *Bib. K. Svenska vet. Ak. Handl.* vol. xxvii. Afd. iii. No. 1, p. 12.

vigorous growth in the deserts of Australia, where the dried algæ form a crust resembling elastic bitumen on the surface.[1]

In all these cases the algæ and lichens do not merely cling to the rock surface; they definitely eat and digest the rocks on which they grow, as may be seen by the corrosion of the surface, and also by the presence of the substances in the rock in the cells of the plants in the form of crystals of oxalate of lime and so forth.

The investigation of the microscopic animals of the soil is practically untouched. Müller found *Difflugia*, a large fresh-water rhizopod, in bog humus[2] and I have mentioned the case of *Nitrosomonas*. The intestines of earthworms swarm with gregarines, which seem to play the same part in them as bacteria do in the case of plants. In tan pits the *Fuligo varians* (Æthalium septicum), commonly known as "flowers of tan," spreads out in colonies a foot or more in diameter; the germs of this organism must exist in the natural bark and in the soil of forests.

It is a legitimate question to ask, "What do the bacteria and other organisms in the soil do when all decomposition possible has been accomplished?" We have seen what teeming myriads live in the soil. We know that if the soil is treated with weak solutions of carbolic acid and mercury chloride, which kill bacteria, the soil is rendered sterile. We shall now proceed to show that bacteria are known to act directly on inorganic substances and the inference seems to follow naturally that a large part of the activities of the micro-organisms in the soil is concerned with the breaking down of rock substances.

The absorption of carbonate of lime by the lower organisms is well known. In plants the minute coccoliths and rhabdoliths, the blue-green algæ, *Chroococcus* and *Glœocapsa*, the larger red or calcareous seaweeds, are examples, whilst among animals all the protozoa and some sponges absorb and secrete carbonate of lime as one of the functions of the activities of their cells. The action is perfectly simple: by the oxidation of the carbon in their protoplasm carbon dioxide is produced, which acts on calcium carbonate and forms a soluble compound. The for-

[1] L. A. Boodle, *Bull. Miscellaneous Inf.* Kew, No. 5, 1907, p. 145; see also W. T. Thiselton-Dyer, Australian Caoutchouc, *Journ. Bot.* New Ser. vol. i. 1872, p. 103.

[2] P. E. Müller, *Natürliche Humusformen*, Berlin, p. 27.

mation of oolite grains is another instance: the collection of the carbonate of lime is supposed to be brought about by the thallus of an alga, which encrusts the central grain, depositing calcium carbonate in concentric layers as it grows. Certainly the encrusting red algæ act in the same way but the living organism has never been observed on the oolite grains, though nodules of carbonate of lime in fresh-water lakes are usually covered with blue-green algæ, *Glœocapsa*, etc. The same process is believed to give rise to the pisolites which separate in pea-like granules as deposits from hot springs. The blue-green algæ can live in hot water but the actual organisms on the pisolites have not been seen, though Cohn asserts their presence.

On the other hand, the destruction of oolite grains and shells generally is accomplished by boring algæ, such as Hyella,[1] which send their microscopic filaments through and through the hard calcite similarly to the mycelia of a fungus penetrating rotten wood; Lind,[2] in fact, found that fungi were actually capable of sending their hyphæ through marble. Boring sponges like Cliona do the same.[3]

The separation of silica cannot be so easily explained. There are countless plants and animals which absorb and secrete silica, and the lower forms are usually closely allied to the lime-secreting genera. Among the silicious plants there are the hosts of the diatoms and among animals the radiolaria and sponges.

In the case of iron again, there are differences of opinion. The bog-iron which forms at the bottom of lakes and under the soil in marshy places, where it is known as moor-bed-stone, ortstein or oude klip, is thought to be the result of chemical deposition. Organic acids certainly dissolve the iron and when the solution is oxygenated, carbon dioxide is given off and the iron is deposited either as a carbonate or as a hydrate. This is what happens in the laboratory; in Nature, however, the precipitation goes on in the bottoms of lakes and in soil which is not properly aerated. Ehrenberg attributed the deposition to a diatom which he called *Gallionella ferruginea*.[4]

[1] E. Bornet et Flahault, Note sur deux nouveaux genres d'algues perforantes, *Journ. Bot.* vol. ii. 1888, p. 161; see also J. E. Duerden, Boring Algæ, *Bull. Ann. Mus. Nat. Hist.* New York, vol. xvi. 1902, p. 323.

[2] K. Lind, *Jahrbüch. wiss. Bot.* Bd. xxxii. 1898, p. 603.

[3] Topsent, *Arch. Zool. Exper.* (3), vol. viii. 1900, p. 226.

[4] C. G. Ehrenberg, *Nukrogeologie*, Leipzig, 1854.

When we come to the sulphates the evidence is clearer. There are definite bacteria which feed on sulphur and separate it both in the form of oily globules of the element and as sulphuretted hydrogen. The effect of feeding the bacteria of the soil with gypsum (hydrated sulphate of lime) is most marked. Pichard states the fact in the following way: the nitrification in the soil by bacteria is stimulated by—

Magnesium carbonate	12·5	times	proportionately[1]
Calcium carbonate .	13·3	,,	,,
Potassium sulphate	35·8	,,	,,
Sodium sulphate .	47·9	,,	,,
Gypsum . . .	100·0	,,	,,

In the case of the carbonates the action is probably simply due to the neutralisation of acids which act deleteriously on bacteria; but the action of sulphates is certainly direct and is due to their forming food-stuff for the organisms.

Though no direct evidence is as yet available as to the action of sulphur bacteria in the soil, there are the researches of Zelinsky and Brussilovsky on the bacteria in the Black Sea, which leave very little doubt that the reaction on land is a similar one to that in the sea. The surface waters of the Black Sea contain free oxygen and support an abundance of organic life; but the deeper and denser waters are charged with sulphuretted hydrogen and the only organisms present are the bacteria. The amount of sulphuretted hydrogen increases with depth. At 100 fathoms there are 33 cubic centimetres in 100 litres; at 200 fathoms, 222 c.c.; and at 1185 fathoms, 655 c.c. Several species of bacteria have been observed but only one, the *Bacterium hydrosulphuricum ponticum*, has been studied in detail. This bacterium possesses the power of liberating sulphuretted hydrogen, not only from organic matter containing sulphur but also directly from sulphates and sulphites. All authors are agreed that the sulphates of the sea-water are acted upon but there is some divergence of views as to whether the changes are due solely to bacteria or whether they are in part purely chemical. Changes of an opposite kind take place in the zone where water containing sulphuretted hydrogen comes into contact with that containing

[1] E. W. Hilgard, *Soils in Humid and Arid Regions*, New York, 1906, p. 147.

oxygen. This zone occurs at a depth of about 200 fathoms. According to Yegunov and Vinogradski, there is at this depth a race of sulphur bacteria which derive the energy necessary for their existence from the sulphur of the sulphuretted hydrogen. The sulphuretted hydrogen is separated in their cells in the form of soft, oily globules and the oxidation of this sulphur gives them the necessary vital energy in precisely the same manner as the oxidation of carbon in other organisms supplies it. I must express my indebtedness to the presidential address to the Geological Society by Dr. Teall for the above facts, especially as it has led to the train of reasoning adopted in the present article.[1]

The organic substance of plants and animals, the protoplasm, consists essentially of carbon, hydrogen, oxygen, nitrogen, sulphur and phosphorus. We have dealt with the separation of all these by micro-organisms except the last. We know of no phosphorus-eating bacteria but phosphorus exists plentifully in the soil, being derived from the mineral apatite, a calcium phosphate, which is an accessory mineral in all igneous rocks. The natural phosphates of lime and magnesia are readily soluble in all weak acids and are available directly to the plant roots; but if there is any hydrated oxide of iron present in the soil, there is formed a phosphate of iron which is totally insoluble. Basic slag, for instance, which contains large quantities of phosphorus and iron, lies unaltered on fields with damp soil showing an acid reaction. As previously stated, Kunze has proved that in many of the higher plants the roots do not secrete organic acids, the only secretion being water containing carbon dioxide. Prianischnikoff found that on growing peas, lupines, mustard and buckwheat, in sand containing aluminium phosphate, which, like phosphate of iron, is insoluble in carbonic acid, there was a vigorous absorption of the phosphates by the plants and the conclusion he arrived at was that the breaking down of the insoluble substance had been accomplished by bacteria.[2]

In South Africa the central parts of the country are dry; along the river courses there are magnificent stretches of alluvium, which, however, can only be occasionally watered by floods or artificial irrigation. The soil is usually rich in

[1] J. J. H. Teall, *Quart. Journ. Geol. Soc.* vol. lviii. p. lxvii.

[2] D. Prianischnikoff, *Bericht. deutsch. bot. Ges.* vol. xxii. 1904, p. 184.

phosphates but on taking samples of the soil after a spell of drought they are found to be in the insoluble form. When, however, the fields are watered and brought under cultivation, then on analysis the soil shows a high percentage of soluble phosphates. There may be other means of explaining this phenomenon but the one which suggests itself to any one acquainted with bacteria in the soil and their life history is that the solubility of the phosphates is brought about by the action of bacteria. When the soil becomes dry and parched the bacteria encyst or retire into minute horny capsules and their activities cease; when water is supplied to them, the capsules absorb water and burst, freeing the rested organisms, which straightway start propagating at the very rapid rate observed in such organisms.

This explanation is founded on analogy; but then similar analogies have been proved to be actual facts in case of carbon, nitrogen and sulphur and it is likely to prove so, judging from Prianischnikoff's experiments, in the case of the last essential constituent of protoplasm. If it be so, then it is one of the most beautiful examples of the manner in which Nature preserves her most precious assets against the proper time.

We come then to regard the organisms of the soil as the inhabitants of the globe persisting from a period when it was still impossible for the higher plants and animals to live upon it. We can imagine the earth to have been in such a state as Treub found the island of Krakatoa in 1886, three years after the great eruption, when the primitive rocks were teeming with microscopic life. In Krakatoa the whole island had been reduced to a mass of glowing ash; but still, after a short interval, the surface became slimy with micro-organisms busily breaking down the silicates and forming a soil which the higher plants, later on, would take advantage of. In the early history of the earth the soil became similarly formed but æons of time had to pass before the higher plants became developed and were able to take advantage of the habitat prepared for them. Heat, which kills most living beings, was no hindrance, for the blue-green algæ live in nearly boiling water to-day in the hot springs;[1] food, as we understand it, these organisms did not require, as they obtained their supplies

[1] As far as I can discover the highest recorded temperature is 85° C.: A. Engler and K. Prantl, *Die Nat. Pflanzenfam*, 1. Teil, Leipzig, 1900, p. 63.

directly from the rocks. We can assert that plants originally formed, collecting their carbon, hydrogen, oxygen, nitrogen and phosphorus from the inorganic substances around them and then, with their oily secretions forming emulsions with water, creating, as Bütschli has suggested, the primitive animals which were to devour them. It is to the soil that we should look as the seat of the origin of organic life, not the sea.

REVIEWS

Essays on Evolution, 1889–1907. By EDWARD BAGNALL POULTON, D.Sc., M.A., LL.D., F.R.S., etc., Hope Professor of Zoology in the University of Oxford. [Pp. xlviii + 479.] (Clarendon Press, Oxford, 1908. Price 12*s*.)

THE first essay in this interesting volume asks whether the period of time supposed to have elapsed since the earth cooled is long enough to allow of the whole process of organic evolution at a rate consistent with Darwinism. The physicists deal out the time, biologists have their views as to the rate of evolution; do they agree? Prof. Poulton argues that the rate of progressive change was much quicker in the higher grades of the animal kingdom, and he points out that recent discoveries—such as that of radium—have made the physicists willing to grant the biologists all the time they want. "Natural selection will never be stifled in the Procrustean bed of insufficient geological time."

The second essay seeks to answer the old conundrum, "What is a species?" and the solution offered is, "A synganic and synepigonic group of individuals," that is to say, a group of individuals which can freely interbreed and are descended from common ancestors.

In the third essay Poulton contrasts in a most lucid way the Darwin-Wallace theory of the natural selection of individual variations with the Lamarck-Spencer theory of the inheritance of functional and environmental modifications; in the fourth essay—also a model of popular exposition—he states the pangenetic and the germinal continuity theories of heredity.

The fifth essay is of a kind which is particularly welcome; it applies an expert's knowledge of entomology to the question of the transmission of acquired characters, and the conclusion is noteworthy: "When we bring together the evidence supplied by the study of insects it is seen that it nowhere supports the assumption upon which Lamarckian evolution is founded, the assumption that acquired characters are transmissible by heredity."

The next essay is a very interesting historical study dealing with James Cowles Prichard's remarkable anticipations of modern views on evolution. In the 1826 edition of his *Researches into the Physical History of Mankind*, Prichard recognised the operation, though not the importance, of natural selection, and pronounced against the transmission of acquired characters. Though he was not quite consistent even at first, and certainly did not stick to his guns in subsequent editions, he was "one of the most remarkable and clear-sighted of the predecessors of Darwin and Wallace." In his hitherto unpublished Huxley Lecture Poulton makes an interesting suggestion to explain why Huxley was unable to feel much confidence in the theory of natural selection. It is the experience of the student of living nature, he says, that inspires confidence in this theory, and Huxley was not in the strict sense a naturalist. To use his son's words, "It was the engineering side of nature, the unity of plan of animal construction, worked out in infinitely varying detail, which engrossed him."

The rest of the book is devoted to a discussion of mimicry and allied phenomena

—which the author has analysed and for which he has invented an elaborate and precise terminology. It is not too much to say that the marvels of "mimicry" in the wide sense were never more vividly expounded, and that the essays afford a valuable body of evidence in support of the theory of natural selection. Prof. Poulton considers other interpretations of mimetic phenomena, that they are due to environmental influences, that they are in part due to the operation of sexual selection, and that they are due to "internal developmental causes, compelling different species to pass through similar phases." We are not sure that he does justice to this last view, "the theory of internal causes"—the bathmist position, but that is probably because it has never been clearly stated by any of its advocates. In any case the author contrasts this and other rival interpretations with the selectionist interpretation, and one cannot help sharing his satisfaction with his master-key. Some of the cases to which he applies the selectionist interpretation are extraordinarily intricate and subtle, but these cannot be briefly cited. We read of the Sudanese Locustid *Myrmecophana*, on whose body the slender-waisted form of an ant is represented in black pigment, the rest of the body being inconspicuous against an appropriate background; of a Central American Membracid which has an enormous prothoracic shield shaped like an ant, and concealing the rest of the body; of insects that lose their shadows, like students of the Black Art; of a South American moth like a leaf attacked by a fungus which has "skeletonised" certain parts and is still at work upon others; but these are relatively simple cases compared with some of the best instances of Müllerian resemblance. In true or Batesian mimicry the mimetic animal resembles an object which positively repels its enemies or positively attracts its prey, as when a harmless snake resembles a poisonous one; in Müllerian mimicry a similar kind of warning coloration is shared by a number of unrelated species. "Each species which falls into a group with common warning or synaposematic colours contributes to diminish the destruction of the other members." It is interesting to notice that the researches on mimicry pursued during the last twenty years, in which Poulton has been one of the foremost workers, tend to increase the number of illustrations of Müllerian mimicry and to reduce the number of Batesian cases.

Though most of the contents of this volume have been published before, the individual essays have been revised, and it is very convenient to have them brought together and thoroughly indexed. They are prefaced by an introduction, written with considerable heat, in which the author protests against the style of recent writings by enthusiastic investigators of Mutation and Mendelism. The writings in question are injurious, according to Poulton, because of their dogmatism, their assumptions, their appropriation under the name of Mendel of results which the present generation owes to Weismann, their exaggeration of the value of certain kinds of results and methods, and their contemptuous depreciation of other lines of investigation which have proved themselves valid. Probably Prof. Poulton does well to be angry, and it is a relief from the monotony of the all-round toleration which enervates the philosophic temperament. It is certain, however, that Bateson and Poulton are working at different corners of the same great problem, that their inquiries are complementary, not antithetic, and that their respective researches in the breeding-pen and in the meadow, in the under-world of the gametes and in the open-air struggle for existence, are being conducted in the scientific mood characteristic of Darwin who inspired them both, who turned unembarrassed from genetic to œcological inquiry and back again, as the needs of the problem demanded.

J. A. Thomson.

The Principles of Pathology. Vol. I., General Pathology. By J. George Adami, M.A., M.D., LL.D., F.R.S. [Pp. xvi + 948.] (London: Henry Frowde and Hodder & Stoughton, 1909. 30*s*. net.)

A congeries of facts, however extensive, does not constitute a science. It is indeed hardly an exaggeration to say that a fact in natural history only becomes of interest when it can be correlated with other facts: it is certain that it has no status in a philosophical science until a place has been found first in the co-ordinated scheme of the branch of knowledge to which it belongs. All natural sciences pass through the phase in which the collection of data is the chief consideration: from time to time certain groups of these data are extracted by a master-mind, or extract themselves, and their interrelation becomes demonstrated. In this way the data are made subservient to general principles, and it is on the basis of these general principles that future work is planned. Pathology has long laboured under the disadvantage of being, at first in fact and afterwards in theory, a chaotic mass of data. At one time physiology was a mere appendage of anatomy, and the only data as regards function were the facts of structure. Pathology has not yet altogether emerged from the parallel stage of development. That morbid anatomy, morbid histology, and systematic parasitology constitute the bricks and straw of pathology is axiomatic: the realisation that bricks and straw are in themselves singularly unlovely and useless has long been delayed. There are about a dozen text-books of pathology originally written in English which may be called well known; with the notable exception of the *General or Experimental Pathology* of Lazarus-Barlow hardly one of these is more than a dictionary of facts. Hence it has come about that the rare and the curious, of which we know nothing, is often held to be of more moment than the commonplace, of which we may know much; the data of pathology are treated, in short, on the pre-Darwinian method which prevailed—and alas! still prevails—among collectors of beetles and the like. If it is one's business in life to introduce the student to this arid literary desert, one cannot but feel rather ashamed of our text-books. His study of physiology has taught him that his business is to find out what things are: he finds that in pathology he is apt to be put off with a statement of what things are called. Now this lamentable state of affairs has not been due to any failure in the accumulation of data. The data have been with us long enough: it is meditative co-ordination that has been lacking. This is no place to inquire at length into the reasons for this. There can, however, be little doubt that the abundant applications of the art of pathology to the daily procedures of practical medicine have to some extent delayed the advance of the true academic science, in the first place by occupying the time of the available staff, in the second place by obscuring the relative importance of the phenomena, and in the third place by introducing a type of mind which approaches pathology through medicine instead of the reverse.

Some such notions as these have evidently been the principles guiding Prof. Adami in writing the remarkable book which is before us. He tells us that "what is needed in a text-book of pathology is not the mere record and description of phenomena, but the attempt to analyse these phenomena in an orderly manner." The attempt is a brave one and a successful one, and the book forms a landmark in the history of English pathology. There is no doubt that it will go far in helping to bring about the realisation of the author's ideal which he has expressed elsewhere: "Our pathology is not to be a mere *catalogue raisonné* of names of morbid states, with precise descriptions of what those names indicate, but is to be a science

—or, in other words, an endeavour to bring into order and relationship the phenomena of disease, and, recognising the relationship between phenomena, to deduce the laws which underlie and determine the individual cases of diseases" (*Inflammation*, p. 8).

It is fortunate for pathology that there are those who, like Prof. Adami, are willing to forgo to some extent the exacting pleasures of the laboratory for the more meditative life. It is impossible, and is becoming more hopeless every day, for any one engaged in active observation to keep abreast of the data which are being accumulated in any but one or two comparatively small fields. The result is that workers lose their sense of proportion and a separation arises, which, while hardly amounting to a divorce, is distinctly obstructive of progress. The bacteriologist, for example, is apt to forget that the rationale of his studies is the effects of bacteria on tissues and of tissues on bacteria; the morbid histologist in his study of inflammation may in a blaze of aniline dyes lose sight of just the same fact from the opposite end. The meditative and scholarly pathologist, on the other hand, can see both ends at once, and to his good offices as an intermediary body every one is deeply indebted.

The book commences with a general survey of cellular structure and function, and includes an admirable survey of inheritance. Section II. considers the causes of disease. The space devoted to monsters seems disproportionately great: it is, however, a subject on which the author has himself thrown important light. Bacteria, protozoa, and metazoa are only dealt with in a general way as parasites, and the action of chemical poisons arising both without and within the body is considered in due order. Section III., on the morbid and reactive processes, occupies more than half the book. We have all known Adami on Inflammation; the chapters here are familiar but none the less delightful. Immunity is dispatched in some eighty pages, and we confess to some disappointment. The term in which all the data of immunity must ultimately be expressed is the resistance of the living animal to the direct test. Prof. Adami has dealt with the subject in the conventional way, and the real wood is a good deal obscured by the trees of the curious properties of blood serum *in vitro*—which may after all have very little to do with the question. Hypertrophy, regeneration and kindred subjects come next, to be followed by a portentous treatment of tumours extending to no less than 155 pages, apart from teratomata and cysts. How Prof. Adami can reconcile his elaborate discussion of the classification of tumours with his confession of faith which we have already quoted, we do not know: still less how he can justify the embryological scheme which he adopts. The desire to classify tumours has seized many distinguished men from time to time: we know enough about tumours now to abandon this way of saying something about nothing, and the really essential data as regards tumours have no more relation to the embryogeny of the tissues in which they arise than to the size of the laboratory in which they are examined. Hidden under this stamp-collecting arrangement, we find an abundant growth of philosophic thought, directed, as it should be, towards the discovery of the nature, rather than the names, of benign and malignant growths. The concluding hundred pages are among the best in the book, and deal with regenerative tissue changes.

The outstanding feature throughout is the philosophical nature of the discourse. There is a clear understanding between the author and his reader that their aim is to discover the principles which underlie the facts which they review. The well-known facility of expression which Prof. Adami enjoys is displayed at its best, and the book in consequence makes excellent reading. References to literature are carefully selected, and seem to be admirably adapted to the needs of the more advanced student : through the memoirs mentioned the rest ot the literature can generally be found pretty easily. We note with pleasure that at last due credit is given to English-speaking workers. The illustrations originally prepared were destroyed by fire just as the book was finished—a catastrophe which would have broken most people's hearts. We feel sure, despite the author's suggestion to the contrary, that they were better than their substitutes. From the practical point of view the book is too heavy (it weighs more than seven pounds) and too expensive. The discredit of the weight belongs to the publishers : it seems a pity that the high reputation of the Oxford University Press in these matters should be endangered by their taking American printing under their wing. The price might have been reduced by the omission of many of the unnecessary coloured plates : the student who does not know from practical experience what hæmin crystals look like is hardly fit to read the book at all.

A. E. Boycott.

The Causation of Sex. By E. Rumley Dawson, L.R.C.P., M.R.C.S. [Pp. 196.] (H. K. Lewis. 6*s.* net.)

We are told on the title-page and in the preface of this book that the hypothesis advocated is a new theory of sex, based on clinical materials, as the result of prolonged and careful study. But the comparative biologist will not be encouraged by finding on the first page of the introduction that "the whole question of the causation of sex in mankind had been hedged around, encumbered, and obscured with observations *ad nauseam* on the eggs of the invertebrata ; on worms and tadpoles ; on sponges and plants ; on bees and waterfleas ; and lastly on hens' eggs, to which nothing more dissimilar could be found than the human egg or ovum." The same kind of thing reappears elsewhere, *e.g.* p. 159. "In another class of vertebrata—the reptilia—if we claim the causation of sex in woman is the same as in snakes, we must, to be impartial, revise our ideas of respiration in woman, owing to the undeveloped condition of one of the lungs seen in various snakes." Such arguments do not inspire our confidence.

But with the hypothesis itself we have no quarrel ; it can be tested only by careful experiment, and if proved to be true will be a very important contribution to the subject. Put shortly, it is, first, that the sex is already determined in the unfertilised ovum, a statement with which we are in complete agreement, and secondly that the right ovary in the human species and probably the mammalia produces only male-bearing eggs, the left only female. This second part is difficult to prove, for, as the author says, after ovariotomy a small portion of ovarian tissue may remain, and may grow so as to produce functional ova. Almost the only safe method of testing it is to find whether a corpus luteum in the right ovary is always associated with a male fœtus, and he produces a large number of cases in support of his contention.

But the author is mistaken when he supposes that his theory is quite new ; we understand that in parts of Ireland the same belief has long been held, with the addition that if the woman lies on the right side at the time of conception a male will be born, if on the left a female. Mr. Dawson makes a similar suggestion, that the preponderance of male births is partly due to the habit of lying on the right side, so making the fertilisation of an ovum from the right ovary more certain than from the left.

A further important point is that ovulation takes place alternately from right and left ovaries, so that after a child has been born, if the number of menstruations between its birth and the next conception is known, the sex of the succeeding child can be foretold. In forecasting the sex of children by this method he claims to have had 97 per cent. of successes. So also the sex can be determined at will, by allowing conception to take place only when one or other ovary ovulates.

The author has collected a quantity of evidence in support of his theory, much of which is apparently quite sound; but cases like the following leave one somewhat sceptical. "*Lloyd's Weekly News*, October 26, 1902, quoting the *New York Herald*, says that . . . Signora M. Giannetta, of Nocera, near Naples, had fifty-nine boys and three girls. She had triplets on eleven occasions, quadruplets on three occasions, sextuplets once, besides eleven children in single births" (p. 144).

The thick sprinkling of italics and black type is irritating, as are at times the dogmatic statements, *e.g.* "by no other theory can the similarity of such twins be explained" (p. 133).

The chapter on greater resemblance to one parent shows an entire lack of knowledge of heredity: we are told that though one spermatozoon conjugates with the egg-nucleus, the greater or less resemblance of the child to the father depends on the number of additional spermatozoa which enter the ovum.

We hope that the book will lead to more careful collecting and recording of cases which shall show whether the ova from one ovary do actually always give rise to children of the same sex.

L. DONCASTER.

The Development of the Chick: an Introduction to Embryology. By FRANK R. LILLIE. [Pp. xii+472.] (Henry Holt & Co., New York, and George Bell & Sons, London, 1908. Price 16*s*. net.)

THE aim of this volume is to provide the elementary student with a plain account of the development of the chick, which, for obvious reasons, has always been a favourite type among teachers of embryology. But although this work is intended primarily for the beginner, as an introduction to the subject, it contains much that is of interest to the advanced student and the original investigator. The account given refers almost exclusively to *Gallus*, but it has been found necessary to fill in certain gaps in the ontogenetic history by descriptions of other birds. Mammalian embryology is not discussed at all, and no attempt is made to deal with the comparative side of embryological science, but the author is careful to point out that this omission does not imply any want of appreciation of this branch of the subject, but is due to the conviction that the elementary student is not in a position to understand the problems involved. It is, however, open to question whether in a book intended for the beginner it might not have been better to illustrate the principles of embryology by references to types belonging to more than one class of vertebrate in preference to giving an exhaustive account of the developmental history of one.

There is a short introduction dealing with the cell theory, the recapitulation theory, the physiology of development, embryonic primordia, the general characters of the germ cells, and the polarity and organisation of the ovum. In discussing the recapitulation theory the author does well to point out that von Baer repudiated the principles involved in the law so often associated subsequently with his name. Meckel appears to have been the first to advance the doctrine that the higher organisms pass through the definitive stages of the lower, but the theory was not fully elaborated until after the publication of *The Origin of Species*, when Fritz Müller and Ernst Haeckel stated it in what is essentially its present form.

The account given of the general nature of the germ-cells is too compressed to be properly intelligible to a beginner, and it is difficult to see what object is gained by its inclusion. The words "ovogenesis," "ovogonia," and "ovocyte" are objectionable.

The developmental processes prior to laying and during the first three days of incubation are treated by stages, and form the subject-matter of Part I. The later developmental history, on the other hand, is dealt with according to the organs concerned, and is included in Part II. The second chapter, dealing with the development before laying, is particularly interesting, since it embodies accounts of the original researches of Dr. Mary Blount and J. T. Patterson, which were undertaken especially for this work. Miss Blount has worked out in detail the cleavage processes in the egg of the pigeon, and has found that there is an accessory cleavage in the marginal zone or periblast, which surrounds the segmental area. The nuclei in the periblast are shown to arise from supernumerary sperm nuclei, which accumulate and multiply in this zone, but afterwards degenerate. This and other facts have been brought to light by Miss Blount's investigation. The process of gastrulation and the formation of the primitive streak in the pigeon have been fully worked out by Patterson, who interprets the first stage in the formation of the entoderm as an ingrowth of the free margin of the blastoderm, as supposed by Haeckel and others. In support of this view, Patterson states that the antero-posterior diameter of the blastoderm is shorter than the transverse diameter during the process in question, whereas previously the blastoderm was approximately circular. Moreover, experiments showed that injuries to the margin made just prior to gastrulation appear later in an anterior position in the entoderm. Patterson says also that the thickening of the margin is brought about not so much by a multiplication of cells *in situ* as by an immigration of cells from the sides. These facts are of interest, but it is doubtful whether the beginner will be able to appreciate their significance.

The results of other recent investigations are mentioned in their proper places. Thus, Eycleshymer's observations on the temperatures of the hen and egg during incubation, and Locy's researches on the fifth and sixth aortic arches, are duly referred to, and there is a fairly complete bibliography at the end of the volume. The work has been written directly from the material in nearly every part, and the result is that, for an elementary text-book, it is of an original character throughout. It is admirably illustrated, and the majority of the figures are new, having been drawn or photographed from original preparations. Attention may be called also to the chronological tables of development, which should prove very useful to the student. In conclusion, we have no hesitation in congratulating both author and publishers.

FRANCIS H. A. MARSHALL.

Recent Advances in Organic Chemistry. By A. W. STEWART, D.Sc. With an Introduction by J. Norman Collie, Ph.D., LL.D., F.R.S. [Pp. xv+296.] (London: Longmans, Green & Co. Price 7*s*. 6*d*. net.)

"SPEAKING exclusively of observational and experimental sciences, it is obvious that progress can be accomplished only at the cost of destroying or modifying current theories."

"Without any disrespect, it may be stated that the majority of scientific investigators are not possessed of strikingly original minds."

"The last twenty years of organic chemistry, however, have been rather barren in many directions. The reason for this is most probably to be sought in the recent and sudden rise of physical chemistry, which has drawn away from the organic field many chemists who would doubtless have carried the older branch much further forward than has been possible without their assistance."

These are hard sayings to place before the average organic chemist in the first few pages of a book intended to describe to him recent advances in the science he particularly affects. Both Collie and Stewart appear to be distressed at the rapidity with which new compounds are being prepared and described, and their *dossiers* neatly tucked away in "Beilstein," the "Abstracts" published by the various Chemical Societies, or in other equally convenient depositories of information. Collie goes so far as to say that "organic chemistry has become a vast rubbish-heap of puzzling and bewildering compounds," while Stewart adds that "if 70 per cent. of them had never been synthesised we should not feel the lack of them to any appreciable extent." Both mentors are agreed that this is not progress, and the three quotations with which this notice opens are some of Stewart's reflections on the situation. With the first two of these organic chemists sympathise to a certain extent, but they can scarcely be expected to admit that their branch of science has become somewhat of a "back number" in recent years because here and there an eminent chemist has elected to become what a chemical Kipling might call "a kind of a giddy harumfrodite"—chemist and physicist too; for after all, it may be said with equal justice that physical chemistry has proved very barren in many directions in the last twenty years.

Turning to the subject-matter proper of the book, there are thirteen chapters devoted to the consideration of those branches of organic chemistry in which chemists have been particularly active in recent years, including the Grignard reaction, so-called asymmetric syntheses, the terpenes, synthetic alkaloids, the polypeptides, chemical action of light, and so on, and the exposition given of the work accomplished in each section is usually good and readable. It is not to be expected that every one will agree with Stewart's selection of subjects to be discussed, since such selection is essentially a matter of personal taste, and it is therefore only as an individual opinion that we suggest that a chapter on the correlation of chemical constitution with physiological action might usefully have been included, since that is a field of work in which only a beginning has been made as yet, and in which the interest of chemists might with advantage be stimulated.

There are evidences throughout the book that it has been compiled in a rather hurried manner. Thus, on p. 46, "polyketides" are defined as "substances which are obtained by polymerisation of keten, and subsequent addition of other atoms," whilst "any substance which can be produced from a polyketide by addition, subtraction, or substitution" is regarded as a "polyketide derivative." A little consideration will show that these two definitions do not distinguish a "polyketide" from a "polyketide derivative." On p. 160 it is stated that non-volatile alkaloids are extracted from plant tissues by treating the latter "first with alkali and then with acids, which dissolve the alkaloids." It ought to have occurred to Dr. Stewart that the alkali is unnecessary if an acid is to be employed subsequently, and further inquiry would have shown him that a chemist experienced in alkaloidal work rarely resorts to the use of an acid-extracting medium unless the use of a neutral low-boiling organic solvent is for some reason precluded.

The author has a curious habit of indulging in rather loosely-worded statements and of using nouns as adjectives, of which one or two examples may be given. Thus (p. 83), "When the saps and tissues of plants belonging to the coniferæ or the citrus species are distilled, the distillates are found to contain a mixture of substances, which are classed under the general head of ethereal oils." This statement really implies that "Stockholm tar" and acetic acid are ethereal oils, which is probably not quite what the author intended. Again, on p. 159, there is this statement: "The monocotyledon species of plants seems to be the richest in members whose tissues produce these substances (alkaloids)." This, to say the least, is a very unhappy use of the word "species." These points and others like them should be put right in issuing a revised edition. The book can be recommended to students and to teachers as giving a good summary of recent work in the subjects with which it deals.

T. A. H.

Outlines of Physical Chemistry. By GEORGE SENTER, Ph.D., B.Sc. [Pp. xvii + 369.] (London: Methuen & Co. 3*s.* 6*d.*)

THIS volume is a welcome addition to the available text-books of physical chemistry. Although it professes to be of an introductory character and the knowledge of mathematics required by the reader is of an elementary description, the author has contrived, within these limits, not only to give an excellent outline of the subject, but also to fill in much interesting detail. His familiarity with the student's point of view enables him to expound with commendable clearness the leading principles of this attractive branch of science.

The employment of physical methods in solving chemical problems crops up primarily in the determination of atomic weights, and the author has done well to deal with this fundamental problem in the first chapter. It is a problem which the average student usually fails to grasp, and it is essential that his attention should be turned once more in this direction before he proceeds to consider matters belonging more especially to the province of physical chemistry.

Dr. Senter has followed up his introductory chapter with others on the properties of gases, liquids, and solutions. This arrangement is much preferable to the one favoured in some quarters, by which the reader is first introduced to the conceptions of velocity and equilibrium. There is no doubt something to be said for the latter method of beginning the study of physical chemistry, but it is infinitely more difficult for the elementary student.

The author's arrangement of the material in the later portions of his book is open to some criticism. It is difficult to see why a chapter on "Thermochemistry" should be placed between one on "Dilute Solutions" and another on "Equilibrium in Homogeneous Systems." Again, the discussion of electrolytic solutions, instead of following immediately the chapter on dilute solutions, is postponed until "Thermochemistry," "Equilibrium in Homogeneous Systems," "Heterogeneous Equilibrium" and "Velocity of Reaction" have been expounded. Surely, on historical as well as logical grounds, the electrolytic behaviour of acids, bases, and salts ought to be discussed in close conjunction with their abnormal osmotic behaviour.

The general excellence of the book, however, is scarcely touched by these criticisms, for in regard to the best way of expounding any given scientific facts there are bound to be different opinions.

One very gratifying feature of Dr. Senter's volume may finally be mentioned, and that is the addition of a section on "Practical Illustrations" at the end of each chapter. This should certainly be of great service to the student, perhaps also to the lecturer on physical chemistry.

J. C. PHILIP.

A Compendium of Food-Microscopy, with Sections on Drugs, Water, and Tobacco. Compiled with Additions and Revision from the late Dr. A. H. Hassall's works on Food. By EDWY GODWIN CLAYTON. [Pp. xxxix+406, with 282 illustrations.] (London: Ballière, Tindal & Cox 1909. Price 10*s*. 6*d*. net.)

THE valuable aid that the microscope offers in the detection of adulteration in foods and drugs is now so well recognised that the appearance of a new work dealing exclusively with this subject will be welcomed by analysts. The welcome should be the more cordial as, although the title is new, the volume is practically a revised and augmented edition of Hassall's *Food: Its Adulterations and the Methods for their Detection*. Such a revision has long been a desideratum, and in its preparation the author has had the inestimable advantage of having discussed with the late Dr. Hassall the plan upon which it was to be based.

On comparing it with Hassall's work it will be seen that the general arrangement of the matter has been comparatively little altered. Nearly all the original illustrations remain, but, being printed on a more suitable paper, are distinctly improved: to these have been added new illustrations dealing more particularly with animal parasites. The text has also undergone considerable revision, but here, too, the original has been as far as possible retained. The addition of a bibliography, that has been brought well up to date, distinctly adds to the utility of a work which must form an indispensable addition to the analyst's library.

Notwithstanding these advantages a certain feeling of regret must be expressed that the treatment of the various substances has not been brought more nearly abreast of the times. Comparatively little account has been taken of the progress made during the past twenty years in the knowledge of the anatomy of foods and drugs and the methods adopted for examining these and their powders. Instances of this are to be found in the defective descriptions of the various flours, of pepper, cayenne, cardamoms, etc. Improved methods of clearing the tissues have facilitated the observation of minute details, and have resulted in the production of more accurate illustrations and in the detection of more refined adulterations. These methods and their results find, unfortunately, no place in the work, the author professing to regard them as unnecessarily elaborate. Hence no attempt has been made to advance the microscopical examination beyond the stage to which Hassall carried it. The adulterations cited and illustrated are, with few exceptions, those described by Hassall, many of which the author himself admits are extremely unlikely to be met with, while others of everyday occurrence, such as powdered almond-shells, olive-stones, exhausted drugs and spices, etc., are either passed over or receive, at most, the briefest mention. An outline of the methods suitable for the examination of nut foods and the means by which their identification and purity may be established would have been very acceptable. The mere statement that the aleurone grains of the Brazil nut are large is surely insufficient, and the illustration affords but little help. This is the more to be regretted as, during the last few years, increased attention has been paid to the characters of the aleurone grains as a means of identifying such substances, and many have been accurately described and figured. Bombay mace is mentioned as a sophistication of Banda mace, but no hint is given of the means by which it may be detected. Pepper, that most important of powdered spices, is similarly insufficiently treated.

It is sincerely to be hoped that a new edition of so useful a work may soon be necessary, and that the author will take the opportunity of bringing the treatment of the material abreast of modern requirements.

H. G. GREENISH.

Die Termiten oder Weissen Ameisen. By K. ESCHERICH. [Pp. 198, with 51 text figures and 1 coloured plate.] (Klinkhardt, Leipzig.)

THE author has made some observations, in Abyssinia, on the nest of *Termes bellicosus*, and, finding that the literature of the subject is scattered through numerous and often inaccessible periodicals, has compiled an excellent summary of the life-history of termites, as far as this is known. The book provides an admirable introduction to the subject, and should prevent much haphazard destruction and hasty generalisation by indicating what details should be noted, either for confirmation of old theories or for extending our present knowledge, when the inexperienced investigator attacks a termite nest. We may join in the author's hope that it will arouse greater interest in the study of these insects, though it may be doubted whether any real progress will be made until some method is discovered whereby they can be observed under natural conditions. The coloured plate—which shows the king and queen in the royal cell—illustrates this point: considering the disturbance created in opening a termite hill, it can scarcely be expected (although the author believes it is so) that the condition of affairs in a royal cell removed from the nest is normal. The author figures the soldiers guarding the king and queen, but it is extremely doubtful whether soldiers inhabit a royal cell under normal conditions.

The first chapter describes the various forms of workers, soldiers, and winged insects, their development (after Grassi) and functions, and the probable causes of their differentiation from larvæ which are assumed to be identical in every respect: on the last point, the theory of parasitic castration is favoured. The second chapter deals, *inter alia*, with the formation of new colonies: this section is the least convincing, since the methods described have been deduced from observations on species which form colonies consisting of a few individuals, and will not hold good in the case of the mound-building species.

Chapter III. describes the various forms of nests, and is the longest in the book. On this head there is abundance of information available, more or less correct, and the author has selected his examples and illustrations judiciously. With slight alteration, Holmgren's classification of the types of termite nests is followed, and they are divided, first, according to the arrangement of the chambers, into "non-concentric" and "concentric" nests; while the second group is subdivided, according to the material used, into pure earth nests, mixed nests consisting of earth and wood, and pure wood (*holzcarton*) nests. This classification is open to considerable objection, for the concentric arrangement of supposed concentric nests is by no means always recognisable, and some of the layers required on Holmgren's theory are certainly not present in the large mound nests. It would seem preferable to attach more importance to the "comb" than to the outer covering of the nest, and to divide the nests in the first instance into those which do not contain combs (e.g. *Calotermes*) and those which do. The latter group could then be subdivided into (*a*) nests with a single comb contained in a cavity of earth or wood, (*b*) nests containing numerous combs in similar cavities, and (*c*) nests of a single comb, the outer layer of which forms a continuous closed surface. At present there seems to be too great a tendency to consider the combs as a "fungus garden" only, regardless of the facts that the fungus culture is only a secondary feature, and that the combs are really homologous with the hanging nests of other species. The author suggests that the (in general) black colour of *holzcarton* nests may be due to the presence of fungi, but investigations in progress do not tend to confirm this view. Many species of "white ants" which inhabit such nests are themselves black, and a black trail marks their usual paths.

The chapter on the food of Termites would have been improved by the inclusion of illustrations of the fungi, and better figures of the "fungus gardens." Those of the latter have been photographed from dried specimens, and show nothing which would distinguish them from the combs of species which do not cultivate fungi. Fig. 36, "Pilzgarten einer *Termes* spec. (Ceylon)," is evidently a fragment of the comb of *Termes obscuriceps*. Döflein's observation that the "fungus garden" is inhabited chiefly by larvæ can scarcely be adduced as confirmatory evidence that the larvæ alone feed on the cultivated fungi, and it seems to imply that there are other workers and soldiers living elsewhere. It would be interesting to know where the author imagines they can live, in a nest in which every chamber is filled by a "fungus garden." The disparity in numbers is no doubt due to the escape of winged insects. And the observation that a mycelial sphere just fills a termite's mouth is about as true as a parallel statement *re* boys' mouths and apples would be.

The remaining chapters include an account of guest termites and other insects known to inhabit termite nests, and the usual description of the damage caused by termites. Among the methods of extermination, the author omits the most efficient and practical—*i.e.* the injection, into the nest, of the gases formed by burning together sulphur and white arsenic. There is a much-needed synopsis of genera, and a bibliography extending to seven pages. In the latter we do not find any reference to recent researches in Madagascar, and it is evident that the author is acquainted with reviews only of some of the works he quotes. A reference to the originals might have modified his statements on several points.

Any reader who has studied termites in the East will find himself in constant opposition to the author's generalisations. But this is not the author's fault. The subject is so wide, and the habits of termites so varied, that it is impossible to write a general account which will not leave the majority of species exceptions.

T. PETCH.

The Stone Implements of South Africa. By J. P. JOHNSON. (London: Longmans, Green & Co., 1908.)

THE rapid advance in our knowledge of the prehistoric sites of South Africa has enabled Mr. J. P. Johnson already to publish a second and enlarged edition of his useful little book on *The Stone Implements of South Africa*. In his first edition he divided his material into three groups—"primitive," "palæolithic," and "advanced," and claimed that these three groups represented a definite sequence of periods. In the present volume he has adopted a more elaborate classification into "Eolithic, Strepyic, Palæolithic or Acheulic, Solutric or Neolithic." It may be regretted, in passing, that he should have introduced such monstrous forms as the second (which should in any case be "Strépyic"), fourth and fifth of these terms: why not simply "Strépy," "Acheul," and "Solutré"? The additional material at his disposal has enabled him to increase the number of his illustrations, which now include a number of Bushman paintings and chippings, mostly scenes from the hunt and the dance. Of the additional information, that furnished by Mr. Lamplough and Mr. Henry Balfour on the subject of the Zambesi Valley implements is extremely interesting, but it may be mentioned that, since the publication of the book, it is rumoured that an entirely different opinion as to the age of these implements has been formed by another competent observer who has studied them on the spot.

The book is further enlarged by an appendix in which the author defines at length the terms "Eolithic," "Palæolithic," and "Neolithic," as representing a sequence of periods in Europe.

On the whole, the second edition of Mr. Johnson's book—now grown to eighty-five pages—will be welcomed by those who are interested in the comparatively new study of the archæology of savage Africa; and, while it cannot be said that the sequence he claims is yet absolutely clear, the fresh *data* which he has obtained are in themselves ample excuse for a second edition. A general map showing localities of finds, and an index, would be valuable additions to any future issue.

T. Athol Joyce.

Text-Book of Petrology, containing a Summary of the Modern Theories of Petrogenesis, a Description of the Rock-forming Minerals, and a Synopsis of the Chief Types of the Igneous Rocks and their Distribution, as illustrated by the British Isles. 5th Edition. By F. H. Hatch, Ph.D. [Pp. xvi + 404.] (London: Sonnenschein, 1909. Price 7*s.* 6*d.* net.)

Dr. Hatch has produced a useful introduction to the study of the igneous rocks. In the first part he describes their structure, texture and genesis, and endeavours to place the student *au courant* with the recent work that is beginning to throw some light on the process of their crystallisation. The second part is devoted to the rock-forming minerals, and is of somewhat more elementary character than the rest of the book. Nowhere are the symbols of the faces supplied, although every student is now presumed to be acquainted with the Miller notation. It is gratifying to see that the author retains the familiar form *kyanite*, instead of following Dana in adopting the spelling *cyanite*. The latter, due to Werner, has, it is true, priority, but it is apt to be given a pronunciation which is indistinguishable from that of syenite. There is really no satisfactory ground for representing the Greek κ by c in English, except in old-established words which have come to us by way of Latin or French. It not only tends to obscure the derivation and assimilate the sound of words of different origin, but more often than not it introduces an unnecessary divergence between the nomenclature in use in this country and that employed by the majority of Continental workers.

In Part III. we enter on the substantive portion of the book, the classification and description of the igneous rocks. It is to be regretted that the author retains the customary primary division into *plutonic*, *hypabyssal*, and *volcanic* rocks instead of following the examples of Prof. Bonney and Prof. Weinschenk in grouping together rocks of similar chemical composition which have consolidated under different conditions. He carries out his classification so rigidly that he will not even allow the peridotites a place among the plutonic rocks, on the ground that they only occur in dykes; a contention that can hardly be maintained in view of the massive development of dunite and serpentine in some localities.

He next separates each of his main divisions into a number of families according to their mineral composition. It would have rendered the classification easier to grasp and remember if the limits of the families had been the same in each division. As it is, there are six families of plutonic rocks, fourteen of hypabyssal and seven of volcanic rocks. The families are divided into subfamilies, and these again into types, which are briefly but clearly described, though subdivision is carried somewhat too far. It is only right that every rock-name which has been employed by a writer of authority should be explained, but only a selection need be recommended for adoption by the student.

In a few cases the author has given a new connotation to well-known terms. For instance, he uses the expressions *nepheline basalt* and *leucite basalt* for rocks containing those minerals but no felspar, and does not make the presence of olivine a necessary part of the definition as required by present usage, according to which such rocks when free from olivine are known as *nephelinite* and *leucitite*. The present method of employing these different names is illogical, but it is accepted and understood by petrologists of all nationalities, and it seems a pity to introduce a new ambiguity into a department of science where so many already exist. A better mode of simplifying the nomenclature, which would not be open to the same objection, would be to call rocks containing the felspathoid and olivine, but no felspars, *olivine nephelinite* or *olivine leucitite*, as the case might be.

Dr. Hatch adopts a suggestion of Lindgren and accepts the principles of the American classification to the extent of calling a rock a syenite if the alkali felspar bears to the soda-lime felspar a ratio of more than two to one ; a monzonite if less than two to one and more than one to two, and a diorite if less than one to two. The irregular distribution of minerals in igneous rocks would make these distinctions very unsatisfactory and inconvenient.

The author concludes with abstracts of most of the classical descriptions of igneous rocks in the United Kingdom, accompanied by reproductions of the original maps by which they were illustrated. This portion of the work should prove of special value to the student. The representations of rock slides, many of which are supplied by Mr. Rastall, are very effective. Some of the older figures that still remain will no doubt be replaced in another edition. This should certainly be the case with the drawing on p. 105 of a hornblende crystal representing an interpretation now everywhere abandoned.

The table for the determination of the chief minerals of the igneous rocks, which forms an appendix, will be appreciated by the elementary student. Arfvedsonite and riebeckite should not, however, have been included among minerals with "strong double refraction."

The use of the index, which is due to Mr. Pringle, is facilitated by the adoption of different type for place-names, rocks, and other matters.

J. W. Evans.

The Commercial Products of India, being an Abridgment of the "Dictionary of the Economic Products of India." By Sir George Watt, C.I.E., M.B., LL.D., etc. Published under the authority of his Majesty's Secretary of State for India in Council. [Pp. viii + 1189.] (London : John Murray, 1908. Price 16*s*. net.)

The standard work of reference on the economic products of India has been, for many years, the Dictionary referred to in the sub-title with its continuation the *Agricultural Ledger*. Both were produced by the Reporter on Economic Products to the Government of India, an appointment held until quite recently by Sir George Watt.

The Dictionary, in eight large volumes, published between 1885 and 1894, has for some time been out of print. Moreover, during the period of close upon a quarter of a century which has elapsed since the first volume appeared, a considerable amount of knowledge has been accumulated regarding Indian products; a revised edition was thus definitely wanted. The scope of the work has been reduced by restricting attention to the products of present or prospective importance in Indian commerce or industry.

The arrangement of the contents is alphabetical; vegetable products are placed generally under the scientific name of the plant whence they are derived, whilst animal and mineral products appear more commonly under their ordinary designations. The convenience of the reader unacquainted with botanical nomenclature is studied, and by reference to the index or to collective articles, such as Oils, he can usually find with ease the account of products in which he is interested. This cross-referencing has not been carried out with absolute uniformity, and we have not been able to ascertain how any one quite ignorant of the subject could find, for example, which were the chief tanning substances produced in India.

The general treatment is very full, and many of the more important articles are practically monographs. The account of cotton (pp. 569-624) affords a good example. A general introduction is followed by a discussion of the history of cotton from very early times, not only in India but also in other countries. This is very interesting, but in a commercial book of reference might well, perhaps, have been abridged. Some fourteen pages are next devoted to the botany of the cottons, in which the author summarises the views recently expounded in detail in his *Wild and Cultivated Cotton Plants of the World*. Adulteration and deterioration and improvement of stock are followed by a very good account of cultivation—area, seasons, yields, etc.—in the chief Indian districts. This part has been very systematically compiled, and it is possible to work through it and ascertain for example the relative importance of each district, times of sowing and picking, and other important facts. Short sections on soils, manures, diseases and pests, ginning and baling, cotton seed and the fibre follow, and the article concludes with a lengthy section on Indian trade and manufactures, whilst copious references to literature are given throughout.

It is thus evident that the treatment is comprehensive and by no means confined purely to Indian interests. The work might perhaps have been improved in some ways had the scope been more restricted. Thus, wishing to ascertain the chief commercial varieties of Indian cottons and their relative values, we were unable to find any concise statement to this effect, and had to obtain the information elsewhere. Yet this is precisely the book in which it should have been given.

Similar criticisms might be advanced regarding some of the other articles. They do not, however, detract seriously from the great service which Sir George Watt and his coadjutors have rendered in bringing together this mass of information regarding the products of the Indian Empire. As we have already indicated, we are of opinion that its scope might in some instances have been still further restricted; but, on the other hand, this wider range will tend to enhance the value of the book to planters and others interested in commercial products in other parts of the world than India.

W. G. FREEMAN.

Recent Publications of Messrs. Zeiss & Co.

MESSRS. ZEISS & CO. have recently issued a number of prospectuses describing the apparatus necessary for the projection of various optical phenomena. Among these are three describing (1) a collective-lens system of great light-gathering power (Mikro 233), (2) apparatus for the projection of experiments on polarised light (Mikro 235), and (3) apparatus for the projection of spectrum experiments (Mikro 205).

(1) It is customary when illuminating an object for microscopic vision to use a condenser below the stage of the microscope. This is really a microscope objective of focal length 1–2 cm. provided with a large numerical aperture. When daylight is used a reduced image of a cloud or patch of light in the sky is projected on to the object, the image produced being larger than the area of the object. In this case uniform illumination is readily obtained.

The use of an artificial source of light, however, is attended by certain difficulties. The condenser projects an image of the source on to the object, but this image is usually too small, and the intensity of the illumination is too unequal to result in uniform illumination of the field of view.

These difficulties have been overcome by the use of a collective-lens system of great light-gathering power. It consists of three lenses, but for the highest magnifications one lens is sufficient, being used as a simple collector. This lens is adjusted so as to project a magnified image of the source on to the iris-diaphragm of the condenser of the microscope. The full aperture of the collector acts as a uniformly illuminated surface obviating the difficulty arising from irregularity of illumination. For high-power dry and immersion systems the field of view of the microscope is very small ; this single lens is sufficient for producing illumination of the whole object. For lower-power work, in which the field of view is larger, the other two lenses of the system are also used. "The diameters of the lenses are chosen of a sufficiently large size, so that in most cases the images of the aperture of the collector, projected by the microscope condenser, illuminate a larger area than can be included by the microscope objective in the magnification concerned." This collecting system is of great use in projection experiments and in photo-micrography.

(2) Perhaps the most important feature of the apparatus for the projection of experiments on polarised light is the small size of the Nicol prisms ; this is made possible by an ingenious arrangement of lenses. In the projection of the phenomena, which doubly refracting specimens show between crossed or parallel Nicols, certain difficulties arise unless large prisms are available : this is due to the necessity of illuminating the specimen in its entirety. The illumination can be conveniently produced by placing in front of the specimen to be examined a collecting lens at the focus of which is a small but powerful source of light. The polarising Nicol is then inserted between the lens and the object. It can easily be seen that the aperture of the Nicol will limit the illumination in the object plane. This limitation is avoided by introducing two more collective lenses between the first lens and the object. They are arranged so as to have a common focus near which is placed the polarising Nicol. With this arrangement only a small Nicol is necessary for transmitting all the rays. The extent of the illumination in the object is, under these circumstances, almost independent of the aperture of the Nicol, depending only on the aperture of the lenses. Using this arrangement, it is easy to project experiments illustrating the polarisation of refracted light, double refraction by a calcite prism, production of axial images—interference phenomena—and many others. The small size of the Nicols allows the apparatus to be placed on the market at a comparatively low price.

(3) In Mikro 205, there is described a useful method of projecting spectrum experiments, including a neat device for showing complementary colours and their combination.

H. Thirkill.

THE SEWAGE DISPOSAL PROBLEM

By F. N. KAY MENZIES, M.D., D.P.H.

The sewage problem has arisen with the growth of civilisation. The enormous increase in our urban population during the last century has resulted in the development of many problems concerned with the public health, such as housing, water supply, milk supply, etc., and prominently among these we may include the question of the disposal of filth.

Prior to this period (which for all practical purposes we may put at a hundred years ago) every man disposed of filth as best he could. Cesspools were common, and where they were not, fæcal matter and domestic refuse were allowed to accumulate in the neighbourhood of dwellings. To some extent these accumulations were, no doubt, periodically removed and placed on the land; but it is easy to see that as the population grew and tended to aggregate in towns, the conditions would become very objectionable, more especially in the thickly populated areas of some of our large towns. It is interesting at this point to note that the first efforts at sewage disposal were mechanical—that is to say, the decomposing matter was transported from the vicinity of human dwellings to what was considered a safe distance, and when thus transported it was left to such purification as the elements might afford. Thus manual labour and animal haulage came first, to be followed by "water-carriage," which may be traced back to a remote period of antiquity, when it seems to have attained a high degree of efficiency. At Knosos, in Crete, an elaborate system of drainage with traps and lavatories, etc., has been unearthed. In the cities of Greece, Dr. Caton tells us, the water supply was liberal and the sewer conduits large, while the aqueducts and cloacæ of ancient Rome are well known to all of us. It is curious that with the fall of the Roman Empire came the complete decay of all sanitary enlightenment; the renaissance in this case did not occur until some fifty years ago, and it began in this country. Referring to this period, Winslow and

Phelps, of the Massachusetts Institute of Technology, have said: "The wonderful administrative work of the British sanitarians effected the greatest sanitary progress that has ever been known." The name of Sir Edwin Chadwick will ever be associated with the sanitary reforms which were initiated during this period. The midden, pit, pail, and privy system was superseded by water-carriage, which has been gradually extended since, and is now firmly and finally established. "Water-carriage is with us, for better or worse, the only practicable method in a country like this for the removal of filth from the centres of population."

It is also worth while recalling the fact that about the year 1810 the water-closet came into use in this country. It soon grew into great favour, but few, if any, realised the difficulties which would arise from its more general use. At first the contents were discharged into cesspools, then these cesspools had to be provided with overflows. The overflows were made to discharge into street drains, and, as many of these street drains were open along the streets, they were, of course, most unsuitable for such a purpose. Putrefaction took place, and the streets soon became unbearable. In consequence, underground sewers were constructed, and, to save expense, they were connected with the nearest watercourse. Up to a certain point the streams and rivers could deal with the filth discharged into them; but later on, as the quantity increased, the rivers became polluted, and complaints came from all sides of the nuisances arising therefrom. From these few facts one can see how the sewage problem, which originally was confined to individual dwellings, developed into a serious question involving the health of towns and the pollution of rivers. We are still grappling with it to-day.

In parts of the country with a river running through a narrow but thickly populated valley, the pollution of the river soon became excessive, and the problem of dealing with it still more serious, especially where, as so often happened, the water supply of towns lower down the river was derived therefrom. In addition to sewage, a good deal of trade refuse soon found its way into the river where there were manufacturing towns, and the extraordinary growth of industries in this country during the last fifty years served to complicate this problem still further. Remarkable accounts are given in various reports, official and otherwise, of the conditions which prevailed about the middle of the last century in some places. The general impression appeared to be that rivers were the proper receptacles for sewage and refuse of any kind. During summer-time many of those rivers became "a boiling, stinking mass"—"children amused themselves by setting alight the gas which floated on the surface of the water, and blue flames arose to a height of six feet." "Carcases of animals floated down the rivers, or lay stranded on the banks." "One brook in Manchester was found to contain nineteen dead dogs." "The refuse from tanneries, woollen mills, dye works, paper works, and large quantities of putrescible sewage found their way into the rivers"—and so on. It seems incredible now that those conditions could have prevailed for so long without action being taken. Complaints were, of course, made here and there, and some inquiries held; but matters went from bad to worse, until at last, in 1865, the Government were compelled to appoint a Royal Commission to inquire into the pollution of rivers, and from that day to this the subject has been continually before us.

As already mentioned, the main object of each individual was, in the first instance, to get rid of the filth and refuse from the immediate neighbourhood of his own habitation. Then followed the period when the filth was transferred by water-carriage to street drains. To the foul condition of the street drains was attributed the prevalence and spread of "fever" and other diseases, and an Act was actually passed in 1847 giving power to local authorities to discharge their sewage direct into the river or sea. In 1854 the highest public health authority in England expressed it as their opinion that it was better to admit sewage to the nearest watercourse than to allow it to accumulate near dwellings, and it was not until 1857 that experts recommended the removal of suspended matter or deodorisation of the sewage before discharge into streams. In 1858 the pollution of rivers in England was legally prohibited. In 1861 an Act was passed requiring the sewage to be purified before being discharged into streams. In 1865, as already mentioned, the Royal Commission was appointed and produced some valuable Reports. In 1868 it was dissolved and another Commission appointed, called the Rivers Pollution Prevention Commission. In 1869 a Commission was appointed to deal with the question of the disposal of London sewage. The Public Health Act of 1872 gave power for the formation of Boards for the prevention of river pollution, and in 1876 was passed the Rivers Pollution Prevention Act. Although the two Commissions of 1865 and 1868 had clearly pointed out that the foul and filthy condition of rivers need not continue, very little was done. The Act of 1876 was rendered almost inoperative owing to the great parliamentary influence wielded by the manufacturers. From 1888 onwards Joint Committees for the various rivers have been formed—*e.g.* the Mersey and Irwell, the West Riding River Board, etc. These Boards consist of representatives of the sanitary authorities bordering upon the specific rivers. They were entrusted with the powers of a sanitary authority so far as was necessary for administering the Act of 1876. In certain cases they have applied for and obtained special powers from Parliament. There can be no doubt that they have done much valuable work, especially when due consideration is given to the fact that their efforts have been consistently opposed by interested manufacturers.

All the Royal Commissions dealing with the sewage problem have recommended the establishment of a special authority to deal with all questions connected with river pollution, and there can be no doubt that this would be the most satisfactory solution of the many difficulties which arise where the interests of the manufacturers and the community are opposed. In 1898 another Royal Commission dealing with sewage disposal was appointed, and is still at work. It has issued in the interval several large, most interesting, and valuable Reports. We shall have something to say in regard to these Reports later on.

Composition of Sewage

It is quite impossible to lay down any exact standard of the chemical and bacterial quality of sewage. This must necessarily depend upon the size of the community, the presence or absence of trade effluents and waste products, the addition of rain- or storm-waters, etc., etc. Furthermore, the sewage itself is constantly undergoing changes due to fermentation. The chief chemical characteristic of sewage is its enormous amount of contained organic matter in suspension or solution. In addition there are various inorganic substances. Roughly speaking, we may classify the constituents of sewage thus:

(*a*) Excretory substances—*i.e.* solid excreta and urine.
(*b*) Household waste—washings, etc.
(*c*) Manufacturing waste products of all kinds.
(*d*) Rain- and storm-water.
(*e*) Grit, sand, gravel, etc.

Bacterially, sewage should be an ideal nidus for organic life, owing to the large amount of organic matter present. There is but one reason against this, and that is the struggle for existence, which must be exceptionally keen. The source of the organisms is principally the dejecta, but the air and extraneous substances also contribute. Various figures have been given by observers as to the number of organisms present per c.c. of sewage—*e.g.* Laws and Andrewes found that London crude sewage contained from 2,781,690 to 11,216,666 micro-organisms per c.c. Not only are these numbers incredibly large, but in addition there is an extensive representation of species, including saprophytes and parasites, pathogenic and non-pathogenic. "Just as the superficial layers of the earth contain economic organisms whose rôle it is to complete the cycle of nature, removing the dead remains of animals and plants and assimilating them in such a way as to add to the fertility of the soil, and so recommence the cycle of life," so also in sewage we have all the required organisms normally present, whose business it is to render soluble the solid matters and to split up organic compounds into their simple elements and so produce effluents free from putrescible matter.

For all practical purposes these organisms are divisible into two main groups: first, the "breakers down," the liquefying or anaerobic organisms, as they are called; they are most active in the absence of air and light; and second, the "builders up," or aerobic organisms, whose activity is greatest when the conditions permit of an abundance of light and air. No strict line of demarcation can be drawn as to where one group ends and the other begins. It is a complete co-operation shared in by a variety of organisms roughly classified into the above two groups.

The chief valuable ingredients of sewage are the phosphates, salts of potash, and the different forms of combined nitrogen. The money value of these constituents has been calculated by various authorities, but such theoretical calculations are probably very far from representing the real values available for agricultural purposes. Bearing in mind the fact that the strength of sewage depends upon various factors above mentioned—*e.g.* the presence of rain- and storm-waters, etc.—it may be roughly calculated that "100 tons of sewage are worth 17*s.*, or in other words 2*d.* per ton. The value of the whole of the excretal refuse of one person is put at 6*s.* 8*d.* to 7*s.* per annum. Further it may be stated that 855 tons of sewage of average composition contain one ton of solid matter in solution or suspension, estimated to be worth £7 5*s.* 4*d.*" From these data one can calculate very roughly the theoretical yearly value of the sewage of any town. Many authorities—*e.g.* Sir William Crookes—consider that the present methods of sewage disposal in this country are wasteful and thriftless. The sewage of our towns is of course an invaluable source of fixed nitrogen. Sir William Crookes in 1898 calculated that we lost fixed nitrogen to the value of £16,000,000 per annum by disposing of our sewage into drains and watercourses; and Baron Liebig, for similar reasons, predicted some time ago that "nothing would more certainly consummate the ruin of England than a scarcity of fertilisers, which sooner or later must mean a scarcity of food." The late Dr. Vivian Poore demonstrated the enormous capacity possessed by the living humus of surface soil for dealing with the excremental matter, and showed the exceptionally heavy crops of vegetables yielded by the soil of his own garden when thus treated. These and many other facts bring home to us some conception of the gigantic loss of our potential food supplies sustained by our water-carriage system for the removal of sewage. But against this loss must be pitted the vast saving of life and improvement in the health of the people which we owe to the water-carriage system. Furthermore the loss of fixed nitrogen entailed by this system is not necessarily to be looked upon as continuous. Chemistry has, we believe, already discovered a method of recovering the loss; but it would be premature to say that it is economically practicable.

The Disposal of Sewage

The history of sewage disposal up to a recent date is a long category of failures, coupled with an enormous outlay of public money. But all these failures have been instructive, and it would appear that at last we have fairly grasped the true principles underlying the treatment of sewage under varying conditions, and we may look forward much more hopefully to complete success in future.

The chief methods of treatment which have survived the severe tests of prolonged experience may be grouped as follows:

(1) Discharge into the sea or tidal estuary with or without previous partial treatment.

(2) Precipitation by means of chemicals, etc.

(3) Biological purification:

(*a*) Land treatment, with or without previous partial treatment.

(*b*) Artificial bacterial methods.

Besides these there are certain special processes which need not be specifically referred to as they are so very little used.

(1) *Discharge into the Sea or Tidal Estuary.*—Many authorities still think that where practicable this is the simplest, cheapest, and most expeditious method for the disposal of sewage. Necessarily it must be limited to certain favourably situated towns. Where the method is in actual use the sewage, generally in a crude condition, is discharged directly into the sea at ebb-tide in order to carry it out to a great distance from the shore, and to diffuse it well into the sea before the tide begins to flow again. In some cases the sewage is partially or completely treated by precipitation before discharge into the sea. Great care has to be exercised in deciding the proper time for discharge. For this purpose careful observations have to be made of surface tides and currents, rise and fall of tides, configuration of coastline, etc. Storage tanks are sometimes required in which the sewage is accumulated until the favourable moment for discharge into the sea. There is nothing necessarily objectionable in this method of disposal if the conditions are carefully studied and the town is favourably situated. Sea-water certainly delays oxidation of organic matter, and therefore, if the sewage is washed up on the foreshore, one may find accumulations of offensive matter. Furthermore, it has been shown that nuisances may arise in respect of bathing grounds, or actual injury to health or population, or spread of disease by shell-fish, etc. But broadly speaking the method is unobjectionable in cases where the volume of sea-water is large in proportion to the volume of

sewage, and still more so where the sewage has been treated beforehand by precipitation or by some other method.

(2) *Precipitation.*—If sewage is allowed to settle in tanks a portion of the suspended matter will subside, and a more or less clarified liquid can be decanted from the top. When certain chemical substances are added to the sewage before it enters the settling tanks, a still greater degree of precipitation will result. For this purpose very many chemicals have been tried—*e.g.* lime as milk of lime, sulphate of alumina, iron salts, etc. The most commonly used is probably lime, either alone or in combination with sulphate of alumina or iron. These chemical substances rapidly subside in the settling tanks, and in doing so carry down with them most of the suspended matters of the sewage, forming "sludge" at the bottom of the tank. The quantity of lime used is generally about twelve grains per gallon of sewage, or, if lime and sulphate of alumina are used together, about five grains of each. The chemicals not only produce more or less complete deposition of suspended matters of the sewage, but also remove to some extent the sewage odour. The organic matter in solution is only partially reduced. The clarified sewage is then in some cases discharged direct into a quickly running stream or river, and so long as the volume of water is sufficiently large, and the river below that point is not used to provide drinking water, this may be allowable. But difficulties do arise in many ways—*e.g.* during periods of prolonged drought—and therefore it has been found advisable in most cases to follow up precipitation by further treatment on filter beds or on land.

The sludge coming from the settling tanks contains some ninety per cent. or more of water, and its disposal is one of the most difficult problems in connection with this method of sewage treatment. The general opinion appears to be that it is practically worthless, and must be regarded as an inevitable source of trouble and expense. It has been stated that "a town of 100,000 inhabitants will produce thirty tons of pressed sludge daily," and from this fact alone one can easily realise the seriousness of the question. Various plans have been adopted at different places for dealing with it—*e.g.* it may be conducted into a well, from there pumped out in a semi-liquid condition, and distributed over a specially prepared piece of land on the sewage works. Then it is dried by simple exposure to the air in pits and furrows. Another method is to dry the sludge in hydraulic presses by which the moisture is reduced to about fifty per cent., and a solid cake is formed which has some slight manurial value and may be sold or given away to farmers according to the demand. Recently, Dr. Grossmann, of Manchester, has suggested a method of dealing with sewage sludge by non-destructive distillation. He points out that especially in some towns—*e.g.* Bradford—the sludge contains a large amount of fatty matter, grease and soap, arising from household waste, etc., and estimates that the sludge cake should contain at least five per cent. of the fatty acids, which ought easily to repay recovery if a practicable process could be found. Furthermore, the removal of these fatty matters would render the remainder of the sludge more suitable for manurial purposes. He has been experimenting during the last twelve months at Oldham, where the sewage is a fair example of an ordinary town sewage. The sludge contains about ninety per cent. of water, which is reduced to fifty per cent. by passing it through presses. The sludge cake is dried and mixed with a suitable amount of sulphuric acid. Then the mixture passes through a retort consisting of a cylinder, in which revolves a hollow shaft bearing perforated paddles. The sludge is thoroughly agitated whilst steam is made to permeate the mass. The steam carries with it the fatty acids which are condensed in a tower into which water is injected. The condensed fat collects on the surface of a tank. The residue from the retort varies in the amount of nitrogen and organic matter which it contains owing to the constant variations in the composition of the sewage sludge. But it is important to notice that the composition of this residue shows that it contains some forty per cent. of partly decomposed organic matter, and thus resembles humus. Dr. Grossmann puts the intrinsic value of this manure at 25*s.* per ton, but for the purpose of his own calculations assumes that it is not more than 10*s.* per ton, and it is found that about 7½ cwt. of this manure are obtained from every ton of pressed sludge containing fifty per cent. water; therefore this at 10*s.* per ton represents a value of 3*s.* 9*d.* The intrinsic value of the crude grease is put down at £12 per ton, but it is only calculated at £7 per ton. The yield of crude grease is 1·4 cwt. per ton of pressed sludge (fifty per cent. water), or in other words 9*s.* 9½*d.* per £7 ton. Therefore the products obtained are of the total value of 13*s.* 6½*d.* from every ton of sludge containing fifty per cent. water, or £1 7*s.* 1*d.* per ton of dry sludge. The working charges are put at 6*s.* 11½*d.* per ton of sludge (fifty per cent. water), or 13*s.* 11*d.* per ton of dry sludge. The difference leaves a margin of 13*s.* 2*d.* per ton of dry sludge, which in the case of a town of 100,000 inhabitants should result in a profit of at least £2,500 per annum. However much opinions may differ as to the practicability of Dr. Grossmann's scheme, all will agree that it is a step in the right direction, and the further results of his experiments on a large scale will be watched with very great interest and with every wish for their complete success.

The method of sewage treatment by precipitation is thus by no means obsolete. The Royal Commission in its Fifth Report (1908) stated that: "In the case of some sewages which contain trade wastes, it is almost essential to subject the sewage to some form of chemical treatment before attempting to oxidise the organic matter contained in it, and in the case of domestic sewages, chemical precipitation materially aids the deposition of the suspended solids, and facilitates subsequent filtration." Brewery waste might be indicated as an instance of the class of trade waste where chemical precipitation is found to serve a useful purpose.

(3) *Biological Purification.*—All biological methods of purification are said to have two features in common, viz.:

(*a*) The destruction of the injurious and putrescible substances in the sewage. There is a destruction of sewage as sewage and a building up of new substances in its place.

(*b*) The production of this desired effect, not by adding anything to the sewage, but by the action of the organisms normally present in the sewage or the medium (land or artificial filter) upon which the sewage is treated.

"All biological processes depend upon the presence of bacteria in some shape or form." In addition to this, it is claimed that the bacterial treatment of sewage is under our control, can be regulated at our will, and, lastly, that "the processes of decomposition and nitrification ultimately destroy the pabulum upon which the organisms in question depend for their existence, and hence lead to their death when they have fulfilled their function."

There is no essential difference between the treatment of sewage by land filtration or artificial filters. In each case the purification, so far as it is not mechanical, is chiefly effected by the agency of micro-organisms.

Land Treatment

It is a well-established fact that where the soil is suitable and the area of land sufficient, the organic matters in sewage can be thoroughly oxidised by land treatment.

There are two methods of land treatment in use, viz.:

(*a*) Intermittent downward filtration.

(*b*) Broad irrigation.

By the former method "the sewage is concentrated at short intervals on an area of specially chosen porous ground as small as will absorb and cleanse it, not excluding vegetation, but making the product a secondary consideration." The purification is partly due to the soil acting as a mechanical filter, separating out and retaining the suspended matters in the sewage, but much more to the destruction by organisms of the organic matters in the sewage. These organisms are found in the upper layers of all soils, and particularly of rich, loamy soils. Sandy soils are not so efficient until they have been in use for a time, while clay, peat, and the more retentive soils are not really suitable media for this purpose. The selected land is divided up into plots; no plot receives sewage for more than six hours in the day, therefore it has eighteen hours' rest daily. It is necessary to underdrain the plots, say at a depth of about four feet, and the sewage should be screened, filtered, or precipitated before application to the land, and distributed over each plot intermittently by branching carriers or some other means. It is usual to allow one acre for each thousand of population; but if the sewage is clarified beforehand by precipitation or other means, one acre may do for each five thousand of population. Under favourable conditions the effluent is found to be very free from organic matters and organisms.

(*b*) *Broad Irrigation* is "the distribution of sewage over a large surface of ordinary agricultural land, having in view a maximum growth of vegetation (consistently with due purification) for the amount of sewage supplied." The sewage should, if possible, reach the land by gravitation (to avoid the expense of pumping). The great difficulty, as a rule, is the cost of the land. The extent acquired varies with its nature and other circumstances, but on an average about one acre to every hundred persons is sufficient. The most suitable land is, of course, a friable loam, and the least suitable heavy clay or peat. Where it is not porous the land should be underdrained. The sewage is distributed over the land by channels, the ridge and furrow system being the best. The crops grown are Italian rye-grass, mangolds, cabbage, etc. These sewage farms may, with careful management, just pay their way; but for many reasons, which cannot be set forth here, one cannot expect them to yield any large profit. They are not unhealthy, and should not produce any nuisance. No facts have ever been brought forward to prove that diseases are spread or produced by these farms. The great difficulty, as a rule, is that land suitable for these farms in the immediate neighbourhood of towns is much too expensive to be put to the purpose of sewage purification. Hence our next method—artificial bacterial filters.

Bacterial Treatment of Sewage

This system depends upon the presence of certain organisms, which may be classified thus:

(*a*) Anaerobes—organisms which work in the absence of air—(oxygen).

(*b*) Aerobes—working in the presence of air—(oxygen).

(*c*) Facultative aerobes—capable of working either in the presence or absence of air—(oxygen).

Ever since cesspools were employed for the reception of sewage, it has been noticed that the material which is periodically emptied out, or overflows from them, contains very little suspended matter. Solid matter is, in fact, generally only found at the bottom of the cesspools. What happens, then, to all the solid material which enters with the sewage? Its disappearance is undoubtedly due to the presence of myriads of micro-organisms, which are constantly feeding upon it, and ultimately convert it into products which become dissolved in the liquid part of the sewage. The anaerobic organisms are probably mostly concerned in causing liquefaction of the solid material contained in the sewage.

The first stage of sewage purification has been compared to gastric digestion, whereby the organic matter is split up and liquefied, and just as in digestion time is an essential factor, so here these anaerobic organisms must be allowed a reasonable interval of time to carry out their work.

The second stage of purification is performed by the aerobes, and therefore this part of the process should be carried on in an abundance of air. The organic matters having been broken up, they are next converted into their ultimate products, and eventually we have nothing left but a solution rich in nitrates and other salts; the gases escape into the air. These natural agencies have only within recent years been scientifically applied to the purification of sewage. The Royal Commission in 1901 issued a Report in which, referring to this method, it was said: "It is practicable to produce by artificial processes alone, as opposed to land treatment, from sewage or mixtures of sewage and trade refuse, effluents which will not putrefy, which would be classed as good according to chemical standards, and which might be discharged into a stream without fear of creating a nuisance." One of the great advantages of this system is that there is no "sludge" difficulty. The residue is very small in bulk, and hence we are saved the trouble and cost of dealing with sludge. But more important even than that is the fact that we get an excellent effluent in every sense of the word.

It is worth remembering that this system of bacterial treatment really owes its origin to the fact that Pasteur in 1862 showed that putrefaction is the work of micro-organisms. His work was considerably advanced in this direction by Muntz, Mueller, and others, who about 1872 proved that nitrification also is accomplished by the agency of micro-organisms. In 1881 Mouras, of Vesoul, published an account of an hermetically sealed, inodorous, and automatically discharging cesspool, in which sewage was anaerobically broken down by "the mysterious agents of fermentation." He surmised that the agents in question were the "anaerobes of M. Pasteur," and it would appear that this is the first record we have of the treatment of sewage by allowing Nature to fulfil her functions by means of bacteria.

The next step in the new bacterial treatment of sewage came from the Massachusetts Board of Health in 1888. They published reports of work carried out by Jordan and others, which went to prove that intermittent downward filtration, through prepared artificial filters of suitable material, will achieve all that is done by the land treatment, and this on a much smaller area; and, furthermore, that this purification was the work of contained bacteria. In 1892 Scott-Moncrieff suggested a system whereby the sewage was purified by a series of stages. His filter-beds were filled with coke, flint, or gravel, and the crude sewage passed into the bottom of the bed, rose through it, and escaped at the top. The solid matter was kept back at the bottom while it underwent solution by means of the anaerobic organisms. The effluent escaping from the top was found to contain very little suspended matter. Then it was led along a series of channels, where it was freely exposed to the air and thereby oxygenated, and hence nitrification ensues. More recently Mr. Scott-Moncrieff has introduced other improvements into his method.

From 1891 to 1895 Mr. Dibdin carried out a number of experiments with the metropolitan sewage which led him to advocate what is sometimes called the "Sutton system of biological purification." Stated very shortly, this system consists in first removing from the sewage, by means of fine metal strainers, the large particles of matter, then exposing it to downward filtration through beds of coarse material. These beds are about four feet deep, and made of burnt ballast of such a size that pieces will pass through a two-inch ring and be rejected by a half-inch ring. The first bed deals with the larger and coarser particles of sewage without fear of clogging. Then the liquefied sewage passes on to a second bed of similar size, but finer filtering material, $\frac{1}{4}$-inch to $\frac{1}{16}$-inch ring. The sewage is allowed to fill these beds almost to the top surface, and then to rest for a period of two hours (hence the name "contact beds"). Then the beds are slowly emptied and allowed to remain empty for a period of several hours. Each bed is charged twice daily, and by this intermittent application of the sewage a certain amount of aeration of the bed is ensured.

The septic tank method was devised by Mr. Cameron, of Exeter, about this time also. This method provides a well-defined line of demarcation between the stages of liquefaction and subsequent purification. The process is carried out under strictly anaerobic conditions by constructing a large underground vault (septic tank) of concrete, cemented on exposed surfaces, and holding as many thousands of gallons as are required according to the population. The sewage travels slowly through the tank, so that every particle is supposed to take about twenty-four hours to get through. This period of time is sufficient for such a complete sedimentation and liquefaction of solids that the tank effluent should contain only a very few grains of suspended matter per gallon. The second stage of purification is effected by passing this effluent through an aerator—*i.e.* a long trough, over the edges of which the liquid falls in thin films, thereby coming in contact with a certain amount of air, and from there on to a series of coke breeze filter beds, 4½ feet deep. These beds are filled, emptied, and rested like Mr. Dibdin's.

The advantages claimed for this method were (1) that it solved the sludge difficulty, inasmuch as practically all the organic solid matter was digested in the tank; (2) that it destroyed any pathogenic organisms which might be present in the sewage; and (3) that the sewage which had passed through a septic tank was more easily oxidised than sewage from which the solids had been allowed to settle, either with or without the aid of chemicals, in tanks which were frequently cleaned out.

The Royal Commission, in its Report of 1908, concluded in regard to the above: (1) that it is now clearly established that all the organic solids are *not* digested by septic tanks, the actual amount of digestion varying with the character of the sewage, size of tank relatively to volume treated, frequency of cleansing, etc. With a domestic sewage, and tanks worked at a twenty-four hours' rate, the digestion is only about twenty-five per cent. (2) The liquor issuing from septic tanks is bacteriologically almost as impure as the sewage entering the tanks; and (3) domestic sewage which has passed through a septic tank is *not* more easily oxidised in its passage through filters than domestic sewage which has been subjected to chemical precipitation or simple sedimentation.

Aerobic filter-beds should be constructed of fine grain material. It does not appear to matter very much what the nature of the material is so long as it is hard and durable—*e.g.* coal, coke, cinders, clinker, refuse pottery, flint, gravel, burnt ballast, etc., have all been used and proved more or less satisfactory. Local circumstances should determine to a large extent in each case the selected material. The water capacity of all filter-beds diminishes to some extent with use owing to the retention of sediment; but after a time a condition of equilibrium is reached. The eight hours' cycle of treatment is recommended by the Local Government Board for contact beds—*i.e.* one hour to fill, two hours to rest full (contact), one hour to empty, and four hours to rest empty. If this cycle is maintained, it is found that such contact beds can be worked at the rate of 1,000,000 gallons per acre per diem.

Percolating Filters.—We have seen that in the so-called contact filter beds the pores of the beds are filled with sewage and allowed to remain full for a certain period of time. During this time (one to two hours) the biological processes and the exchange of gases with the surrounding atmosphere are stopped, and this must be regarded as a disadvantage inseparable from the method. In the case of land filtration the sewage passes through the soil very rapidly (according to the nature of the soil), and there is very little interference during the whole time with the natural interchange of gases between the soil and the atmosphere. In 1893 Corbett, of Salford, developed the artificial bacterial method in another direction. He attempted to apply the principle laid down by Sir E. Frankland that in intermittent filtration the liquid should always be allowed to flow freely away. The chief difficulty was to obtain a uniform distribution of the sewage over the entire surface of the filter. After many experiments Corbett adopted for this purpose fixed spray jets, from which the sewage was distributed under pressure in the form of a fountain. He also had a layer of half pipes underneath the filter beds in order to increase aeration and facilitate drainage. The more thorough the contact between the sewage and the filtering material the better the purification. Such conditions are best fulfilled by the adoption of such a method as Corbett's. The important point is that the sewage should fall upon the filter bed in single drops if possible. Then it passes through the filter bed, dropping from one piece of coke to another until it reaches the bottom, where it is collected by channels or drains, and so reaches the exit pipe. Many different mechanical devices have been invented within recent years for the purpose of securing uniform distribution of the sewage over the surface of the filter bed—*e.g.* Mather & Platt's sprinkler, the Accrington sprinkler, the Fiddian distributor, etc. For a description of these and other forms, the reader is referred to the various technical books. There is no doubt that by means of sprinklers and percolating filters the absorption processes are much more favoured than in contact beds. Aeration is more thorough, and thereby the vital activity of micro-organisms and the higher forms of animal life is encouraged. On the other hand, there is some risk of unpleasant smell arising from the beds, and in the warmer months of the year flies swarm upon them and may become a serious source of trouble and nuisance in the neighbourhood, especially if there happen to be dwellings near the works.

General Conclusions in regard to Filter Beds

The Royal Commission, in its Fifth Report (1908), formulated some interesting conclusions in regard to filter beds as a result of prolonged investigations.

"Within ordinary limits the depth of a *contact bed* makes practically no difference to its efficiency per cube yard, and it is generally inadvisable to construct contact beds of greater depth than 6 feet or less than 2 ft. 6 in."

"For practical purposes, and assuming good distribution, the same purification will be obtained from a given quantity of coarse material, whether it is arranged in the form of a deep or a shallow percolating filter, if the volume of sewage liquor treated per cube yard be the same in each case."

"In the case of percolating filters of fine material, if the liquid to be purified were free from suspended and colloidal solids, and if thorough aeration could be maintained, the same statement made in regard to coarse filters might also hold good of fine bacterial filters. But in practice these conditions can scarcely be maintained with large rates of flow, and therefore probably the greatest efficiency can be got out of a given quantity of fine material when arranged as a *shallow* filter."

"Taking into account the gradual loss of capacity of contact beds, a cubic yard of material arranged in the form of a percolating filter will generally treat about twice as much tank liquor as a cubic yard of material in a contact bed."

It is generally desirable to remove from the sewage, by a preliminary process, a considerable proportion of the grit and suspended matter before attempting to purify it on land or by filters. This is done by screens and detritus tanks or sedimentation tanks of various designs.

Animal Life in Bacterial Filters

Dr. Barwise has recently drawn attention to the fact that bacterial beds are not correctly so called. He contends that they are, in the words of Sir James Crichton Browne, much more like "entomological menageries." He has not attempted to classify the inhabitants found there, but believes that in small-grain filters, at any rate, the destruction of organic matter is mainly effected by small river worms, and of these there are something like a thousand species. Amongst others, he has recognised *Naididæ*, *Tubificidæ*, and *Polychætæ*, etc., in enormous numbers. Then, again, in the large-grain filters there are large worms, leeches, and countless numbers of larvæ of gnats and flies. In addition to the above, there are water crustacea—*e.g.* Cyclops, Cyprides, freshwater shrimps, centipedes, water-spiders, etc., etc. There is at present very little information available as to the life history, food habits, and the various parts which these various animals play in connection with sewage purification; but it is obvious that, their presence having been proved, their function opens up a most interesting field of study to the zoologist, and a clear determination on this point may prove of vast importance in the extremely difficult and many-sided problem of sewage disposal.

The Travis Hydrolytic System of Sewage Tanks

This system of sewage treatment has been introduced by Dr. Owen Travis, and is to be seen in actual use at Hampton (Middlesex) and Norwich. The main object is the separation of the suspended solids and colloidal matters from the sewage before it reaches the filtration area or is discharged into the sea or tidal river. The "Hampton Doctrine," as it is called, insists that the purification of sewage is primarily and essentially physical, and only incidentally and consecutively vital—that is to say, that the physical separation of the impurities from the sewage constitutes the all-important part of the purification process, and that the bacterial and other life operations are subservient to this initial effect. It will be seen from this statement that the Hampton doctrine is entirely opposed to the generally accepted conception of sewage purification—viz. that the operation is essentially one of bacterial liquefaction and oxidation.

It has long been noticed that filter beds begin to show signs of loss of capacity after they have been in use for a time. This period may be a few months to a few years, depending upon various factors—*e.g.* the disintegration of the filtering material, the consolidation of the filtering material, the deposition of colloidal matter, the growth of organisms, the amount of suspended matter in the liquid passed on to the bed, etc. Leaving out of account several of the above factors, it is sufficient for our purpose to say that from the "Hampton" point of view the amount of suspended matter in the liquid passing on to the filter bed and the deposited colloidal matter form the most important factors, and the essential principles associated with the Travis system of tanks are (1) the necessity for eliminating the more readily removable suspended solids and colloidal matters from the sewage, in order to exclude the main proportion of the liquid from a prolonged tank operation, and so prevent it becoming impregnated with the gaseous and other products of putrefaction; (2) the desirability of more perfectly removing these impurities by the interposition of self-cleansing surfaces which will attract the fine suspended solids, and upon which a proportion of the colloidal matters will be deposited, and to effect this separation in a self-cleansing sedimentation chamber; (3) the importance of providing adequate means for the removal of sludge and scum, in order to minimise the carrying forward of deposited matters, and so as to maintain continuously the working capacities of the tank.

The plant consists of sedimentation chambers, reduction chambers, and hydrolysing chambers. For a full description of this system it is necessary to consult the more technical books and journals (e.g. *Journal of the Royal Sanitary Institute*, August 1909). It is claimed that by this system the sewage, after leaving the tanks, is in a much better condition for application to land or artificial filters, and certainly the results so far obtained seem to support Dr. Travis's contentions. But it is scarcely necessary to add that as yet, at all events, many of our best-known experts do not appear to be disposed to accept the Hampton doctrine.

Dibdin's Slate Beds

Dibdin has now modified the system of coarse contact beds of coke or clinker (Sutton system) by the introduction of slate beds. This new system has been tried at Devizes, where the sewage is exceptionally foul and strong owing to the bacon factories, slaughter-houses, etc., which exist in this town. The slate beds are constructed of refuse from slate quarries, 65 ft. by 45 ft. and 4 ft. deep; the slates are laid in horizontal layers $2\frac{1}{2}$ inches apart, and there is a 6-inch space between the last layer and the floor of the tank.

The crude, unscreened sewage is allowed to fill the beds and to remain for two hours. The beds are then emptied, given two hours' rest, and filled again. Enough air remains in the beds to allow of aerobic action going on while the sewage is at rest therein. It emerges in a fresh condition properly liquefied, and is then found to be more suitable for subsequent treatment on land or secondary filter beds. The Royal Commission inspected these beds at Devizes, and came to the tentative conclusion that they must be regarded as preliminary settling or septic tanks rather than as contact beds. The slate beds alone will give a purification of twenty-five to thirty per cent., which is practically very much the same figure as is ordinarily attained by septic tank treatment. The results obtained at Devizes are, on the whole, very favourable; but further experience is required before this method of treatment can be definitely pronounced upon.

Finally, one may say that from the foregoing statement it is clear that in sewage we are dealing with a highly complex liquid, the nature of which must vary within very wide limits depending upon many factors. The problem of purification therefore is exceedingly difficult in some instances, and it may be safely said that each particular case requires special study if a successful result is to be attained.

CHEMISTRY OF THE CELL NUCLEUS

By W. D. HALLIBURTON, M.D., F.R.S.

The cell-theory put in the briefest possible way teaches that the unit of structure, both in animals and plants, is a little lump of living material or protoplasm; the differences between the animal and the vegetable units are of subsidiary importance, though it was the greater prominence of a firm membrane on the exterior of the vegetable unit that led botanists, who were earliest in the field, to adopt the term "cell." Every cell originates by the subdivision of another cell, and the life history of every living organism is but the story of how a single cell, the fertilised ovule or the ovum, multiplies, and how its descendants become differentiated into the various tissues and organs of the adult.

The early histologists soon noticed that each one of these little living bricks which build up the body walls contains in its interior a structure of more solid consistency than the surrounding protoplasmic jelly in which it is embedded, and the term "nucleus" was adopted as its name. The nucleus is often difficult to see in cells which are examined in a fresh unaltered condition, but treatment with a dilute acid or with a dye will readily bring it into view, for the majority of stains which are employed for microscopic purposes colour the nucleus more deeply than the rest of the cell.

As the microscope became a more perfect optical instrument, and as microscopic technique improved, and the list of appropriate dyes grew longer, the structure of the nucleus became clearer; its investing membrane, its network of fibrils, and nucleoli floating freely in the sap which occupied the interstices of the network were recognised. Microscopists introduced a number of names for the materials of which the component parts of the nucleus were composed, although at that time there was no knowledge of the chemistry of these fantastically labelled substances. One of these terms has outlived the rest, namely the word "chromatin" or chromoplasm;

this is still a convenient name to use for the material of which the fibrillar network is mainly composed, for at least it serves to remind the student that it is this substance which bestows upon the nucleus its remarkable affinity for the dyes employed to throw it into view.

An important part which the nucleus plays in the life history of a cell was discovered by those early observers who had the good fortune to be the first to watch the division of a cell into two daughter cells from start to finish; they saw that the division of the cell was preceded by the division of the nucleus, and for many years the nucleus was regarded as the initiator of the process of multiplication. Much labour was expended in tracing the microscopic patterns (skeins, stars, rosettes, etc.) into which the primary threads or chromosomes are thrown, and it was found that each of these chromosomes divides longitudinally into two sister threads which gradually disentangle themselves from one another and form the basis of the two new nuclei. Exceptions later were found to this rule, and heterotype mitosis is characteristic of a certain stage in the development of the sexual elements, as well as of the cells which make up the growths that are called malignant.

Within comparatively recent years it has been found moreover that the nucleus is after all but the second in command, and that the primary impulse to cell cleavage is given by the division into two of a tiny particle called the centrosome, which the earlier workers had missed. The two centrosomes so formed retire to opposite poles of the nucleus, and the attractive force they exercise is seen in the radiating lines of protoplasmic threads and granules in their neighbourhood. These remind one very forcibly of the lines which one sees radiating from the poles of a magnet when one places it on a flat surface powdered over with iron filings. Whether the attractive force of the centrosomes is magnetic or something akin to magnetic is but a speculation, but the force is undoubted, for finally the sister threads of the nucleus accumulate around the centrosomes, and last of all the cell protoplasm follows suit and two new independent cells are formed.

That the chromosomes are of vital importance to the organism is clearly shown by several facts. For instance, they are constant in number not only in the cells of the body, but in all individuals of any particular species of animal or plant, though differing in number in different species. The equal halving of each chromosome which occurs during mitosis maintains this numerical constancy except in a certain period of the life history of each individual, and this period occurs in the formation of the reproductive cells; here accompanying the atypical or heterotype mitosis just mentioned, half of the number of chromosomes is thrown out, and the act of fertilisation consists in a fusion of the male and female germ cells; each parental nucleus provides half the normal number of chromosomes, and thus the fertilised egg-cell starts with the full complement once more. Many biologists regard the chromosomes as the actual bearers of the characters which an organism inherits from its parents, or at any rate this view is adopted as a working hypothesis and places the transmission of characters from parent to offspring upon a material foundation.

We thus see that observations on the nucleus form not only one of the most fascinating of microscopic studies, but that they open up all the vexed problems of the meaning and mechanism of heredity. It is even possible that the abnormal course mitosis pursues in cancerous and other tumours may explain, at any rate in part, their malignancy, and every step in the knowledge of cancer is important to the practical physician as well as to the unfortunate man or woman who happens to fall a victim to this fell disease.

I do not intend, however, to follow the subject further along any of these interesting lines, for the main purport of this essay is the more prosaic, though perhaps not less useful one of setting down certain advances which have been made in the chemical knowledge of the structures we have been mentioning. Many gaps still exist in our knowledge, but they are less numerous now than they were twenty or even ten years ago, so prolific and fruitful has the work of chemical physiologists and pathologists been during the last decade. I am not alone in believing that chemistry will play an important share in elucidating the secrets of that mysterious and fatal ailment cancer, to which allusion has already been made.

But before I can take up the question of nuclear chemistry, there is another function of the nucleus, not second in value to those already enumerated, which must be mentioned. This is the part it plays in nutrition. The expression "resting nucleus" is still to be found in the pages of many of our text-books, though it is gradually being replaced by the more appropriate term "non-dividing nucleus." For during life there is no such thing as rest, and although the nucleus may not be engaged in participating in the changes that lead to cell division, it is doing something equally important. However great may be the hiatuses in our knowledge of its chemistry, this much at least is certain, namely that the nucleus contains a store of nutritive material of a highly complex nature, and the compounds contained therein consist of carbon, hydrogen, nitrogen, oxygen, sulphur, phosphorus, iron and probably others in addition. This little barn or storage-house is not one which keeps its doors locked, but there is functional commerce between the nucleus and the rest of the cell. If any part of a cell is separated from the rest, and so cut off from communication with the nucleus, that part rapidly degenerates and dies. A nerve fibre is essentially just a long branch or tail of a nerve cell, and if nerve fibres are severed, those portions of them which are thus cut off from the nutritive control of the nucleus rapidly lose their power to conduct nerve impulses, and the microscope reveals in them those changes which are called after their discoverer Wallerian degeneration. Wallerian degeneration has been the subject of extended study because of its value as an experimental method in tracking the course which nerve fibres take, but after all it is but one example of a universal truth.

Staining reagents such as methylene blue, for which the nucleus has a special affinity, also enable us to detect those fragments of nuclear origin which have wandered out into the cell body and its branches to satisfy the nutritional needs and energy requirements of those parts; these are particularly well marked in nerve cells, and are known as Nissl's granules. In the embryo, before the nerve cells have sprouted out any branches, the nucleus is rich in this readily stainable material; but later on, when the branches have grown, its quantity is lessened in the nucleus, for it has largely migrated into the surrounding protoplasm.

I shall not be anticipating too much what I hope to develop more fully in later pages, if I state at this point that the stainable substance is a kind of protein or albuminous material. Chemists at the present day divide the proteins into two main groups, each of which has many subdivisions. The first of the two main classes is designated that of the simple proteins. The word "simple" is somewhat of a misnomer, for anything in connection with a protein is so complex that hitherto proteins have baffled all attempts at artificial synthesis. But at any rate such proteins as are included in this class (albumin, gluten, casein, gelatin, etc.) are simple as compared with those in the second group, which are called the compound or conjugated proteins. In these the protein molecule is united to another group, also of a complex nature. Hæmoglobin is an instance of a conjugated protein, where the protein proper is united to an iron-containing radicle termed hæmatin. The nucleus of cells consists of or contains large quantities of another conjugated protein, which has been called *nuclein* by chemists, and there is no doubt that nuclein is identical with the chromatin or chromoplasm of the histologist. Nuclein is a compound of protein with an organic acid rich in phosphorus, which is named *nucleic acid*. In the nucleus itself the protein component of the compound is small in quantity, and in the nucleus which we term the head of a spermatozoon, it is stated to be entirely composed of nucleic acid without protein admixture; this may be so in mammals, but in fishes there is a combination in the nucleus of the spermatozoon between nucleic acid and a simple kind of protein termed protamine.

There seems in fact to be a whole chain of nucleins containing a diminishing quantity of nucleic acid and an increasing quantity of protein; and those which contain the smaller amounts of nucleic acid are spoken of usually as *nucleo-proteins*. Most nuclei contain so much nucleic acid that they yield on analysis 10 to 12 per cent. of phosphorus; whereas in the nucleo-proteins of cell protoplasm the phosphorus percentage is in the neighbourhood of 2 or 1 per cent. or even less.

These facts and others to be described later have been made out by two sets of methods, those of micro-chemistry and those of macro-chemistry. In the former the reagents are applied to the microscopic object, and the change, colour reaction or what not, is watched through the microscope: it is quite easy, for instance, to demonstrate in this way that the nucleus gives the colour tests which are characteristic of the proteins. Of recent years methods have been devised for the micro-chemical detection of phosphorus and of iron, and the presence of both these elements in the nucleus can thus be shown. The existence of iron in the nucleus in organic combination is not devoid of practical interest, for there is no doubt that the normal supply of iron for the making good of effete blood-pigment in the adult, and the supply of iron wanted by the embryo for making hæmoglobin (of which there is none in the original ovum), is derived from the iron-containing nucleo-proteins, or as Bunge terms them *hæmatogens*. It is perhaps hardly necessary to mention in passing that Bunge's hæmatogen is a very different thing from certain patent medicines sold under the same name.

Macallum of Toronto is the most notable living exponent of the art of micro-chemistry, and has followed with infinite patience the distribution of various elements or compounds *in situ* in the different tissues. Among methods he has recently devised, for instance, is one for the detection of potassium by means of cobalt nitrite, and another for the detection of chlorides by means of silver nitrate. Both potassium and chlorine, however, curiously enough, are absent from the nucleus of all cells, or at any rate cannot be detected by such means. In a sense, too, every staining reaction is a chemical reaction. Many dyes, used either for staining fabrics or histological specimens, are taken up by the process known as adsorption; this is something midway between chemical combination and mere mechanical admixture. But in other cases the facts are not explicable by the adsorption hypothesis, and no doubt in these, or in some of them, a true chemical compound is formed between the dye and the material which is dyed. In the future, when our knowledge of the chemistry of dyes, as well as of the organic substances in living tissues, is more complete, it will be possible to state the nature of the compounds which are formed. But even at the present day we are not absolutely ignorant on such points, and perhaps it is in relation to the aniline dyes that our knowledge, such as it is, is greatest. Some of these stains are neutral in reaction, some acid, and some basic; and it is the custom to employ mixtures of these to stain microscopic objects. If, for instance, one applies a mixture of a basic dye (such as methylene blue) and an acid dye (such as eosin) to a film of blood, a selective action is exerted by the constituents of the colourless corpuscles; some parts will take up one stain, others another, and one obtains not only a pretty preparation, but, what is more important, facts which enable us to classify the corpuscles. Parts which have an affinity for the acid stain will be coloured red, and are called *oxyphile* or *acidophile*; parts which have an affinity for the basic stain will become blue, and are called *basophile*; in the case of the blood corpuscles, granules with an affinity for methylene blue are comparatively rare, although, as we have seen, the Nissl granules of nerve cells are intensely basophile. Most of the granular contents of the protoplasm of the white blood corpuscles or leucocytes are either neutrophile or oxyphile. But in all cases, whether we are dealing with blood, or nerve cells, or any other kind of cells, the nucleus is always basophile. The chemical explanation of this is not accepted by all observers; still, it is difficult to believe that the acidity of the nucleus (rich as it is in nucleic acid) is entirely unconnected with its affinity for basic substances.

The methods of micro-chemistry are, after all, not so decisive as those of macro-chemistry. It is an axiom with all practical chemists that they can obtain better results and eliminate errors of analysis when they work with large quantities of material, and the investigation of the nucleus has proved no exception to this rule. In order to obtain nuclei in abundance, it is necessary first to take large quantities of cells, adopt means to dissolve away the cell protoplasm, and to leave the nuclei unaffected; these can be collected and then the test-tube and the flask come into requisition in the usual laboratory processes. Luckily such methods have been found, and accidental changes in the nuclei produced by the reagents added to obtain them have been minimised though not yet absolutely excluded.

The earliest to attack the problem was Sir T. Lauder Brunton, who as a young man spent some months working in Kühne's laboratory at Heidelberg. He obtained the nuclei of the red corpuscles of birds and snakes; these were washed free from serum by salt solution, then shaken with water and ether; by this means the envelopes of the corpuscles were ruptured, the contents escaped, the hæmoglobin passed into solution, and the nuclei were found floating at the junction of the two fluids. Some subsequent methods of washing to free them from adhering impurities were then adopted, and finally the reactions and composition of the resulting material were examined. Brunton finally arrived at the conclusion that the substance of which the nuclei are composed was mucin. This was incorrect, but the mistake was pardonable in those early days of physiological chemistry. At that period the truth of the proverb "All that is sticky is not mucin" was not recognised; and since that time the viscous material in the bile which used to be regarded as mucin has been shown to be a nucleo-protein, in certain animals at any rate. Mucin and nuclein or nucleo-protein are not only often alike in physical consistency, but both of them contain a carbohydrate complex, both are soluble in dilute alkali and precipitable therefrom by acetic acid. In spite of these similarities the two substances are not identical, and one difference, the presence of phosphorus in the nucleic compound, was discovered by Plosz, who later on repeated Brunton's experiments.

In the same year in which Plosz performed this work (1871), Miescher, Professor of Physiology at Basle, published a memorable paper on the nuclei of pus corpuscles. He made the fortunate observation that on subjecting cells to the action of artificial gastric juice, the protoplasm was dissolved and the nuclei were unaffected, and to the material, rich in phosphorus, which he separated out from the nuclei he gave the name nuclein. Plosz, knowing of this work, surmised correctly that the mucin of Brunton was of a similar nature.

Miescher was rash enough at the time to ascribe an empirical formula to his nuclein, namely $C_{29}H_{49}N_9P_3O_{22}$, which he calculated from the percentage composition. He subsequently put forward a revised formula, and numerous others have been suggested by later workers. For it was found that nucleins of different origin presented great divergencies on elementary analysis. It was therefore supposed that there are different kinds of nuclein, differing not only in the quantity or kind of protein in their molecules, but also in the nature of their nucleic acid component.

Miescher devoted the rest of his life to the investigation of nuclein and nucleic acid, and it is impossible to speak too highly of the importance of this pioneer work. An enthusiastic fisherman, he combined pleasure with business in the capture of Rhine salmon during his holidays, and the roes of these fishes served him as the material from which to prepare nuclein. He it was, too, who discovered the simplest members of the protein family, the protamines with which nucleic acid is combined in the heads of the spermatozoa in the fish tribe.

One sometimes hears, especially on the lips of those who oppose the progress of science, a distinction drawn between utilitarian and purely scientific experiments. But as Professor Starling puts it in a recent pamphlet:

> "All researches are utilitarian, *i.e.* are for the benefit of man. We are, however, accustomed to restrict this term to those in which we can see the immediate benefit, and in which, therefore, the advance can only be a small one of detail."

He takes as his example the purely academic observations of Galvani, Oersted, and Faraday, and shows how they have rendered possible the whole of the electrical industries of the present day. He might have taken the work of Miescher as another instance. I do not suppose Miescher saw how his work on salmon roe would illuminate our knowledge of nutrition and malnutrition, and at least one example of the immediate application of his work I propose to treat more fully later in this paper, namely, the relationship of nuclear metabolism to the elucidation of our knowledge of gout and allied disorders.

I must pass over the work of many observers who confirmed and corrected details in the researches of Miescher, and making a long jump pass to those who definitely set themselves to the investigation of nucleic acid itself. In this new epoch, Kossel's name must be specially mentioned, for it is to his labours and those of his colleagues that we owe the isolation and identification of its cleavage products.

On breaking up nucleic acid it was found that, in addition to a yield of phosphoric acid, many crystalline bases were obtainable, so also was a carbohydrate.

The bases were at one time termed nuclein bases, because of their origin, at another xanthine bases, because xanthine was one of them and the others are related to it; another term was that of alloxuric bases, but finally the term purine bases was definitely adopted, for they are derivatives of a ringed nucleus called purine by Emil Fischer.

These bases are four in number; their names and formulæ are as follow:

Purine $C_5H_4N_4$

Purine bases:
- Hypoxanthine (monoxypurine) $C_5H_4N_4O$
- Xanthine (dioxypurine) $C_5H_4N_4O_2$
- Adenine (amino-purine) $C_5H_3N_4(NH_2)$
- Guanine (amino-oxypurine) $C_5H_3N_4O(NH_2)$

and at first it was supposed there were four different nucleic acids, each of which yielded a different base.

Later other bases were separated out and named cytosine, thymine and uracil. Their composition and constitution finally were unravelled, and they were shown to be derivatives of another ringed nucleus named pyrimidine; hence they are called the pyrimidine bases.

The result of this work was to show that the cleavage products of the nucleic acids may be tabulated into:

1. Phosphoric acid.
2. A carbohydrate.
3. Purine bases.
4. Pyrimidine bases.

A large amount of information was in this way placed at the disposal of physiologico-chemical workers, but a glance at the text-books of that date (six to ten years ago) will show that no clear conception of the construction of nucleic acid was then possible. Each worker or writer interpreted the mass of facts in a different way, and the net result was chaos. The difference observed in the composition and cleavage products of nucleic acids obtained from various sources was still interpreted as due to differences in the proportion in which the cleavage products were combined. It need hardly be said that if such a view were correct, and each nucleus had its own specific nucleic acid, the complexity of the subject would have been well-nigh insuperable. Happily, however, this is not really the case. As the years go by, and better methods for the separation and purification of nucleic acid are introduced, the difficulties are being cleared up, and nucleic acids previously supposed to be different are now to be regarded as identical. For the work which has been instrumental in reducing confusion to order we have to thank Schmiedeberg and Steudel in Germany, Ivar Bang in Sweden, and Walter Jones and Levene in America.

Schmiedeberg from careful analyses has been able to give an empirical formula for the acid which is probably nearer the truth than Miescher's; Levene and Mandel have advanced views as to its constitution which, though useful as a basis for renewed work, should be regarded more safely as provisional than final. Schmiedeberg also in his work drew attention to a distinction between what he regards as the hydrated and anhydrous forms of the acid, and to the power which the latter possesses of gelatinising. The recognition of a gelatinous or colloidal condition sometimes presented by nucleic acid and its salts is of importance, whether the explanation that the absence or presence of water in the molecule is the correct one or not. Jones pointed out that in the case of the sodium salt the gelatinous and non-gelatinous varieties are easily convertible one into another, and this reversible action is believed by him to be a simple explanation, if it occurs *in vivo* of the physiological localisation and migration of nucleic acid.

Bang's work was previous to that of both Schmiedeberg and Jones. He prepared from the pancreas a nucleic acid which is much simpler in composition than the majority of those previously isolated. It yields on decomposition only three substances, namely, phosphoric acid, a sugar of the pentose group, and one purine base, namely guanine. For this reason he bestowed upon it the name of guanylic acid. This has been confirmed by Steudel, Levene and others, and guanylic acid has been found in other organs, for instance the liver. Both liver and pancreas, however, contain in addition what may be termed ordinary nucleic acid, which yields on cleavage additional products.

The discovery of guanylic acid at first seemed to complicate the subject, but really it helped to simplify it, for there is no doubt that the older workers had investigated mixtures of nucleic acid proper and guanylic acid, and thus obtained divergent analytical results.

We need not, however, follow out in detail the curious comedy of errors which led to previous confusion; this has been admirably done in an interesting historical review of the subject in Walter Jones's article.[1] We will be content with mentioning a number of propositions which, if not absolutely certain, are not at any rate far from the truth.

1. All "ordinary nucleic acids" (that is, nucleic acid as opposed to guanylic acid) yield the same two purine bases (adenine and guanine) in the same proportion. Xanthine and hypoxanthine, when present, are due to the secondary action of enzymes or other agencies that produce removal of the NH_2 or amino-group and oxidation.

2. All yield the same two pyrimidine bases, namely, cytosine and thymine.

3. All contain a hexose carbohydrate, the identity of which is uncertain, though it is probably not dextrose. This has always been Kossel's view from the fact that lævulinic acid is such an abundant cleavage product. Previous statements about the presence of a pentose are no doubt due to admixture with guanylic acid.

There is therefore no insurmountable difficulty in accepting the hypothesis that the nucleic acids of different mammalian organs are identical substances. One must naturally be cautious in making the generalisation too sweeping at present, especially as it has been shown that those derived from plants and from fish spawn yield uracil, another pyrimidine base; uracil derived from mammalian nucleic acid is a secondary product from cytosine.

At the recent Congress of Applied Chemistry held in London (June 1909) we had the advantage of hearing the latest views on the nucleic acid problem from Dr. Steudel. He has carefully estimated the various elements, and also the cleavage products of nucleic acid, and proposes still another empirical formula for it, which differs somewhat from that advocated by Schmeideberg last year. The following equation will indicate what it is, as well as the way it splits up on decomposition with water and oxygen:

$$\underset{\text{[Nucleic acid]}}{C_{43}H_{57}N_{15}O_{30}P_4} + \underset{\text{[Water]}}{8H_2O} + \underset{\text{[Oxygen]}}{O_2} = \underset{\text{[Guanine]}}{C_5H_5N_5O} + \underset{\text{[Adenine]}}{C_5H_5N_5}$$

$$+ \underset{\text{[Thymine]}}{C_5H_6N_2O_2} + \underset{\text{[Cytosine]}}{C_4H_5N_3O} + \underset{\text{[Sugar]}}{4C_6H_{12}O_6} + \underset{\text{[Meta-phosphoric acid]}}{4HPO_3}$$

He was also able to put forward tentatively his view regarding

[1] *Journal of Biological Chemistry*, 1908.

the constitution of the nucleic acid molecule, which may be represented in the following way:

```
OH—P—Sugar—Guanine
   |
OH—P—Sugar—Adenine
   |
OH—P—Sugar—Cytosine
   |
OH—P—Sugar—Thymine
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That is, we have a chain of four atoms of phosphorus, each of which is united on the one side to a hydroxyl (HO) group, and on the other to a sugar (hexose) molecule. Each hexose group is united further to a base, and thus the four bases enumerated above are linked into the molecule.

It is impossible to prophesy whether such a formula will stand the tests of criticism and renewed experiment; but at any rate it appears that if we have not reached the last chapter of this interesting series of researches, we must be somewhat near the penultimate one.

Let me now add a few sentences relating to a practical application to which this increase of knowledge has led and is leading. The harmfulness of excess of uric acid in the body is now a matter of common knowledge, and although some enthusiasts would try to make us believe that uric acid is the source of every human ill, it cannot be denied that it is an important factor in many ailments. Every piece of knowledge that leads to an elucidation of its origin in the body is therefore a step in the direction of the ultimate cure of gout and allied disorders.

In birds, where uric acid is the chief final stage in nitrogenous katabolism, and where it is discharged as a urate in their semi-solid urine, a process of synthesis which occurs in the liver is the mechanism of its formation. But this synthetic building together of lactic acid and ammonia does not take place in the mammal; in the mammal it has now been proved beyond question that uric acid is formed by oxidation from the purine bases, and so ultimately originates from the breakdown of nucleic acid.

If we contrast together the formulæ of two of these bases with that of uric acid, the relationship is evident:

Hypoxanthine	$C_5H_4N_4O$
Xanthine	$C_5H_4N_4O_2$
Uric acid	$C_5H_4N_4O_3$

Hypoxanthine is mon-oxypurine; xanthine is di-oxypurine; and uric acid is tri-oxypurine. Any conditions that lead to the increased katabolism of the cell nuclei, or any foods such as sweetbreads or meat which are rich in nuclei or in purine bases, will therefore lead to an increased formation of the poisonous acid, and if the kidneys are unequal to the increased strain of excreting it, the result is its accumulation in the body and the production of the "uric acid diathesis."

The question is not only of interest in itself, but the mechanism of nuclear katabolism also illustrates a general truth concerning the importance of the tissue enzymes. These may be studied in extracts of different organs and tissues, and their distribution varies a good deal; but, speaking generally, those which have to deal with uric acid formation are most abundant in the liver and spleen.

The first of these to act is called *nuclease*; this liberates from nucleic acid its two purine bases adenine and guanine, each of which contains an amino- (NH_2) group. The next to come into play are called de-amidising enzymes, because they remove the NH_2 radicle; one of them termed *adenase* converts adenine into hypoxanthine, another termed *guanase* converts guanine into xanthine. Finally *oxidases* step in, which transform hypoxanthine into xanthine, and xanthine into uric acid. But even that does not bring the list to a conclusion, for in certain organs (*e.g.* the liver) there is a capacity to destroy the uric acid after it is formed, and so we are normally protected from a too great accumulation of this substance. What exactly happens to the uric acid is uncertain, although it is clear that the products of its breakdown are less harmful than uric acid itself. The enzyme responsible for uric acid destruction is called the *uricolytic* enzyme. The uric acid which ultimately escapes in the urine is the undestroyed residue.

The question may next be asked, does our present knowledge of the chemistry of the nucleus assist us in any way in understanding its behaviour during cell division. Here we enter much more uncertain ground, and it may be frankly confessed that at present we have no data for more than guessing what are the chemical transformations which accompany mitosis. There have, however, been theories put forward by those who work at another branch of chemistry, namely physical chemistry, which it may be interesting to briefly allude to, and so bring this article to a conclusion.

In 1876 Bütschli suggested that cell division is brought about by an increase of surface tension subsequent to nuclear division in the equatorial region of the egg-cell. But, as Brailsford Robertson has recently pointed out in an article on the "Chemical Mechanics of Cell Division,"[1] this would result in a streaming of material towards the equator and not away from it, which is what really takes place. He adduces evidence to show that surface-tension is lowered at the equator, and not raised as Bütschli supposed.

In our account so far of the chemistry of the nucleus, we have considered mainly the proteins and protein-like substances, because it is in connection with these that our knowledge is greatest. It must not, however, be forgotten that there is in all cells, and doubtless in their nuclei too, another important group of substances of a labile nature which participate in the metabolic cycle. The group is a heterogeneous one from the chemical standpoint, and Overton's term *lipoid* is now very generally applied to it. The lipoids are so called on account of their solubilities being like those of fats, and they include substances free from both nitrogen and phosphorus (*e.g.* cholesterin), nitrogenous galactosides free from phosphorus, and certain highly complex nitrogenous and phosphorised fats which were originally termed *phosphatides* by Thudichum. Lecithin is the best known example of these, and yields on decomposition phosphoric acid, fatty acid, and a nitrogenous base known as choline.

Brailsford Robertson has shown that if a thread moistened with a base is laid across a drop of oil containing a trace of fatty acid, the drop undergoes division along the diameter on which the thread lies. If the thread is moistened with soap, the same result follows; and therefore the explanation advanced for the division of the drop in the first experiment is that soap formation occurs—that is, the base on the thread and the fatty acid in the oil combine together. The soap lowers the surface tension in the equator of the drop, and this brings about the streaming movements away from the equator which culminate in the division of the drop into two; and he finally suggests

[1] Issued as a Bulletin from J. Loeb's Laboratory (University of California publications, 1909).

that a similar equatorial lowering of surface tension sets up the streaming movements which lead to the division of the protoplasmic material in a cell. This is explicable on the hypothesis that choline or soaps of choline are liberated in the cell by the cleavage of lecithin or similar phosphatides in the region of the nucleus.

I have thought it right to mention that there are theories of this nature in the air, but whether they are feasible is one of the many problems for the future which we all hope will be cleared up when bio-chemistry and other branches of biological study are founded more and more firmly on the bed-rock of experiment.

ISOMERIC CHANGE

PART II. THE MECHANISM OF ISOMERIC CHANGE[1]

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It has long been recognised that many isomeric changes can only proceed under specific conditions and that a definite machinery is necessary to effect the rearrangement of the various atoms in the molecule. By the work of Butlerow, between 1867 and 1877, it was, for instance, clearly proved that isomeric change amongst the alcohols depends on the formation of an intermediate olefine, that isomeric change amongst the olefines depends on the formation of an intermediate alcohol (or alkyl sulphate), and finally that these changes proceed reversibly only in contact with sulphuric acid of suitable strength.

In the present article it is proposed to describe the evidence which justifies the view that a similar mechanism is necessary in all cases of isomeric change, and to discuss the action of the catalyst and the mechanism of the action in a number of typical changes, including the racemisation or inversion of optically-active compounds.

A. The Laws controlling Chemical Change

Before referring specifically to isomeric change it will be desirable to prepare the way by a brief discussion of the mechanism of chemical change in general. In this matter the experiments of Dixon and of Baker on the catalytic action of moisture in gaseous interactions are of supreme importance, as the conditions are of the very simplest and on this account are admirably adapted for the investigation of the fundamental laws. It is, perhaps, an unconscious tribute to the influence of the German school of physical chemistry, or perhaps of the

[1] The previous article, "Isomeric Change, Part I. Historical Development of the Theory," appeared in *Science Progress*, 1909, **3**. 616–37.

Zeitschrift für physikalische Chemie as a vehicle for the concentrated inculcation of certain aspects of the subject, that even English writers have formed the habit of discussing at length the more superficial aspects of chemical change and of catalysis, but have to all intents and purposes disregarded the profound consequences that follow from the far-reaching observations made in their own country. It is scarcely too much to urge that the observations of Dixon and of Baker should be placed in the very forefront of any discussion of chemical change and that a casual reference to moisture as an "interesting catalyst," of which "the mode of action is quite unknown," does not form an adequate description of the most important experiments that have yet been made in this branch of chemistry.

In the absence of any satisfactory alternative explanation of the influence of moisture in promoting chemical change, the electrolytic theory put forward by Armstrong in 1885 undoubtedly occupies the field; it has received confirmation amounting almost to positive proof from the observation recorded by Baker[1] that the exceptionally pure water produced by the slow combination of hydrogen and oxygen, not quite perfectly dried, by prolonged heating in a glass vessel, is not a sufficiently active catalyst to initiate an explosion even when present in considerable quantities.

The essential feature of the electrolytic theory is the formation of a circuit of not less than three components, of which one at least must be an electrolyte. The typical battery consists of two metals and a solution, but it is important to recognise that the electrolyte may be discharged by electrodes of other kinds, since otherwise the theory would be restricted to a very narrow range of chemical changes. In an accumulator, for instance, sulphuric acid is electrolysed in such a way that the "sulphate" is discharged at a lead electrode, but the hydrogen by means of lead peroxide, a metal grid being provided as a backing for the peroxide mainly in order to secure mechanical strength and to diminish the electrical resistance of the circuit. In a gas-battery the electrolyte is discharged by means of hydrogen at one pole and oxygen at the other, platinum being employed in order to provide a metallic circuit of low resistance between the oxygen and hydrogen, which

[1] *Trans. Chem. Soc.*, 1902, **81**, 403.

constitute the electrodes in the chemical, if not perhaps in the electrical, sense. When the interaction takes place at a high temperature it appears that this solid circuit may be dispensed with, although in practice it is found that its place is frequently supplied by the (glass) walls of the containing vessel, which exert a remarkable influence in promoting action between gases.

The recent experiments of Dixon,[1] which have shown that an explosive wave can be propagated through a gaseous mixture with a velocity equal to that of sound, have an important bearing on this point, since it is clear that under these conditions the influence of surface-contact has been eliminated. It is not difficult to form a picture of what happens in such a case. The amount of water present is usually small, but here and there a circuit may be formed of which the essential elements are, for instance,

$$\begin{matrix} H_2 \\ H_2 \end{matrix} \Bigg| \begin{matrix} OH_2 \\ OH_2 \end{matrix} \Bigg| \; O_2 \text{ or } H_2 \; \Bigg| \; OH \; \Bigg| \; Cl_2,$$

the water being assumed to be sufficiently impure to conduct the current. At ordinary temperatures the circuit is unable to discharge, owing to the high electrical resistance of the gaseous "electrodes"; at a red heat this resistance is so far reduced that electrolysis takes place and the circuit assumes the form—

$$\begin{matrix} H_2O \\ H_2O \end{matrix} \Bigg| \begin{matrix} H_2O \\ H_2O \end{matrix} \text{ or } H_2O \; \Bigg| \begin{matrix} HCl \\ HCl \end{matrix}$$

When the gas is detonated the pressure wave has the effect of momentarily raising the temperature of the gas as it passes through; as a consequence each of the electrolytic circuits is discharged, and the catalyst is released for further action. If the electrolysis be accompanied by the liberation of a sufficient quantity of heat, the catalyst finds itself in contact with other gaseous molecules under conditions favourable to immediate interaction, so that in a very short time the wave of detonation is followed by the complete combustion of the gas. If, however, the energy liberated by the electrolysis be small, or be wasted in raising the temperature of a diluting gas, the catalyst is only able to form a circuit with molecules which are too "cold" to discharge, and the explosion dies out.

[1] *Phil. Trans.* 1903, **200**, 315-52.

Such being the conditions that govern chemical change in gases, it is important to consider how they may be applied to liquids, and especially to those organic liquids in which isomeric change is most frequently observed. Two types of change may be broadly distinguished. In the first type all the constituents which take part in the interaction are conductors, so that the action proceeds rapidly, and is often almost instantaneous; in this category must be included—in addition to the action of (aqueous) acids on (impure) metals—the large range of cases in which electrolytes interact, as in the precipitation of silver chloride from aqueous solutions of silver nitrate and of potassium chloride. In the second type the constituents include materials which do not conduct; the passage of the current is then hindered, and the progress of the chemical change is retarded, in many cases to such an extent that the action appears to be absolutely stopped, in spite of the fact that all the materials which are usually necessary for interaction are present. In this category may be included many of the interactions of organic chemistry, such as the bromination, nitration, and oxidation of carbon compounds. In most of these changes the carbon compound and the brominating or oxidising agent are non-electrolytes, which can only enter into an electric circuit as depolarisers to discharge the products of electrolysis. The action is then dependent on the presence of an impurity or catalyst to provide the electrolytic element of the circuit, and even when this is forthcoming, there still remains the difficulty of conveying the current between the non-conducting electrodes. In bromination it is probable that a trace of (moist) hydrogen bromide usually serves to start the action, the carbon compound acting as a depolariser for electrolytic bromine and the bromine as depolariser for electrolytic hydrogen. Thus in the bromination of ethylene

$$\begin{array}{c|c|ccc|c} CH_2 & BrH & Br & & CH_2Br & HBr \\ \| & & | & \text{becomes} & | & \\ CH_2 & BrH & Br & & CH_2Br & HBr \end{array}$$

In the interaction of chlorine and a saturated hydrocarbon, such as methane, the action would be

$$\begin{array}{c|c|ccc|c} H & BrH & Br & & HBr & HBr \\ | & & | & \text{giving} & & \\ CH_3 & BrH & Br & & CH_3Br & HBr \end{array}$$

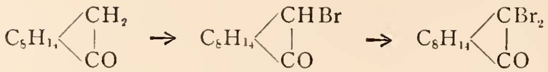
In each of these cases the action is held back, not only by the necessity of a catalyst, but by the much greater difficulty of discharging the electrolyte through electrodes which are almost entirely devoid of conducting properties.

Most of the isomeric changes that have been observed amongst organic compounds are actions of the same type as the bromination of ethylene or the nitration of benzene—these are considered in section B as "Isomeric Changes of the First Group"; but many examples are also known of compounds which can act as electrolytes and undergo isomeric change with the readiness characteristic of compounds of this class—these are considered in section C as "Isomeric Changes of the Second Group." Changes of both types involving the racemisation of optically active compounds are discussed in section D.

B. Isomeric Changes of the First Group

The isomeric changes observed in typical non-electrolytes, such as the olefines, proceed as a rule quite slowly, and only after the deliberate addition of some well-defined catalytic agent. They are therefore devoid of the qualities which confer such an elusive character on the more facile isomeric changes of section C; as a result, the history of their investigation has been pleasantly free from speculative theories and bewildering nomenclature. The changes that have been most fully studied are those involving the transference of halogens, and of nitro, sulphonic and hydroxyl groups; the compounds containing sulphonic groups are, of course, electrolytes, but as electrolysis results only in the separation of a hydrogen atom and not of the sulphonic group, the transference of this group takes place under conditions precisely similar to those which govern isomeric change in non-electrolytes. It is shown below that all these changes can be accounted for by means of an electrolytic theory, the catalyst acting as the electrolyte, and the organic compound as depolariser in the electric circuit.

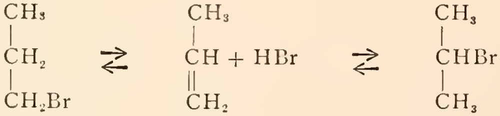
(*a*) *Transference of a Halogen Atom.*—An excellent example of the conditions governing the transference of a halogen atom is found in the behaviour of camphor on bromination. At ordinary temperatures and pressures (*e g.* on heating camphor in a flask on a water-bath and running in one or two molecular proportions of bromine) bromination takes place in the *a*-position, first one and then the other *a*-hydrogen atom being displaced.



Camphor — { *a* Bromocamphor / *a'*-Bromocamphor } — *aa'*-Dibromocamphor

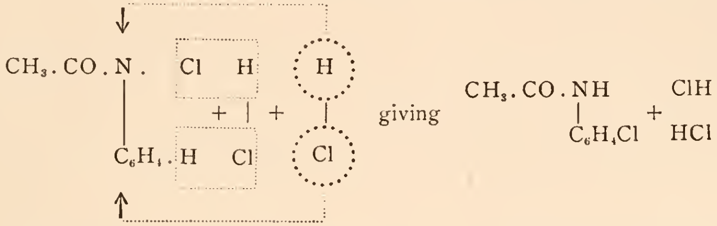
Bromination proceeds with the utmost readiness, but it is noteworthy that whereas the first bromine atom can only be removed by drastic methods of reduction, the second can be removed very easily by mild reducing agents such as alcoholic potash. If the bromination be carried out in a sealed tube at temperatures above 100°, bromine enters the nucleus, the products being an *aβ*-dibromocamphor, $C_8H_{13}Br\left\langle \begin{array}{l} CHBr \\ | \\ CO \end{array} \right.$, and sometimes an *aa'β*-tribromocamphor, $C_8H_{13}Br\left\langle \begin{array}{l} CBr_2 \\ | \\ CO \end{array} \right.$. It is also possible to start from the *aa'*-dibromocamphor and convert it into the *aβ*-isomeride by heating it in a sealed tube with concentrated hydrobromic acid. There can be little doubt that this change depends on a process of simultaneous bromination and reduction, the transference of the halogen being due to the tendency to form the tribromo-derivative coupled with the reducing action of the hydrogen bromide on the easily displaced *a'*-halogen atom. It is therefore not necessary in this case to postulate the existence of any special mechanism for effecting isomeric change, beyond that which is involved in the processes of bromination and reduction.

The conversion of propyl into isopropyl bromide in presence of aluminium bromide might be explained in a similar manner as depending on simultaneous bromination and reduction, but it appears more probable that in this case there is a reversible removal of hydrogen bromide, an olefine being formed at an intermediate stage as in the case of Butlerow's isomeric alcohols:



Propyl bromide — Propylene — Isopropyl bromide

A typical and very important case amongst aromatic compounds is the transference of a halogen atom from the side chain into the ring in the conversion of phenylchloroacetamide, $C_6H_5.NCl.CO.CH_3$, into *p*-chloroacetanilide, $Cl.C_6H_4.NH.CO.CH_3$. In presence of alkalis the former compound is stable, but it undergoes change very readily in presence of hydrochloric (but not of any other[1]) acid. This isomeric change takes place very readily and smoothly, and may be regarded as a process of self-chlorination, effected through the electrolysis of a hydrochloric acid circuit joining the $>NCl$ to the $-C_6H_5$ group. The discharge of two atoms of electrolytic hydrogen at the $>NCl$ group would suffice to reduce it to $>NH$, whilst the two atoms of electrolytic chlorine set free simultaneously at the $-C_6H_5$ group would convert it into $-C_6H_4.Cl$.



It is, however, of interest to note that the original chlorination of the amino group of the acetanilides $R.NH.CO.CH_3 + Cl_2 \rightleftarrows HCl + R.NCl.CO.CH_3$ has been shown by Orton and Jones[2] to be a reversible process in which the extent of the interaction may be ascertained accurately by passing a current of air through the liquid and determining the amount of chlorine carried forward by a known volume of air. As this action is reversible it is evident that the addition of hydrogen chloride as a catalyst to the chloro-amine would be followed immediately by the liberation of chlorine, so that if the original anilide were capable of being chlorinated in the benzene ring a secondary chlorination might ensue as represented by the equation

$$C_6H_5.NH.CO.CH_3 + Cl_2 \rightarrow Cl.C_6H_4.NH.CO.CH_3 + HCl$$

---

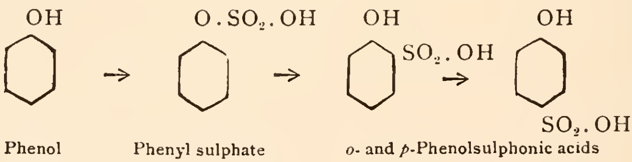
[1] "Hydrochloric acid is the only catalyst. Hydrofluoric, sulphuric acids, etc., have no effect; hydrochloric acid can always be detected when a change begins in the presence of these acids. Chlorine and bromine are without effect until hydrochloric acid is formed."—Orton, *Brit. Assoc. Report*, Winnipeg, 1909.

[2] *Proc. Chem. Soc.* 1909, 25, 196.

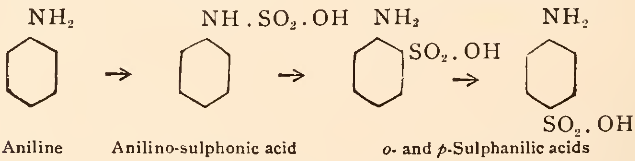
This change being accompanied by the reproduction of a molecule of hydrogen chloride the cycle of operations could be repeated until a steady state ensued. As the secondary chlorination of the benzene ring is not accompanied by any reverse action the conversion of the chloro-amide into a stable ring-substituted compound would in this case be complete. The transference of the halogen into the ring may therefore, as an alternative hypothesis, be regarded as an example of simultaneous chlorination and reduction, comparable with the *β*-bromination of camphor, and involving no special mechanism peculiar to the interconversion of isomers.

It is of interest to notice that owing to the affinity of hydrogen chloride for water the extent to which the amino group of the anilides is acted on by chlorine varies very greatly with the proportion of water in the acetic acid that is used as a solvent: the effects are, indeed, so pronounced that as a rule no interaction at all takes place in glacial acetic acid, although in some cases the nucleus is directly chlorinated. The chlorination of the amino group can however be brought about, either by the addition of water, of which 50 per cent. is sufficient to ensure practically complete interaction, or by adding a molecular proportion of sodium acetate to remove the hydrogen chloride, when about 90 per cent. of the chlorine reacts.

(*b*) *Transference of a Sulphonic Group.*—The introduction of a sulphonic group into the benzene nucleus is probably dependent in many cases on the sulphonation of a $-OH$ or $-NH_2$ group and the subsequent wandering of the $-SO_2.OH$ into the *o*- or *p*-position, by a process precisely similar to that by which a halogen atom enters the ring system of the anilines and anilides.



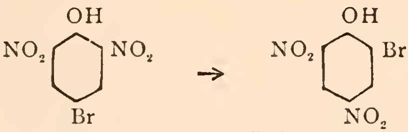
Phenol — Phenyl sulphate — *o*- and *p*-Phenolsulphonic acids



Aniline — Anilino-sulphonic acid — *o*- and *p*-Sulphanilic acids

The sulphonation of naphthalene compounds has been fully studied by Armstrong,[1] and of benzene compounds by Moody.[2] In the former series, isomeric changes were brought about under somewhat drastic conditions and the experimental evidence is of such a character as to lead to the view that, in certain cases at least, the formation of a second series of acids "involves the formation and subsequent partial hydrolysis of a higher sulphonic acid than that which is eventually separated." Some of the naphthalene acids and the majority of the benzene sulphonic acids were found, however, to undergo change under much simpler conditions—for example *o*-phenetol sulphonic acid was converted completely into the *p*-acid by heating during three hours at 100° in a current of dry air, although no sulphonating agent was added to bring about isomeric change. Even in this case, however, the acid, which darkened in colour, was found at the end of the heating to contain a small amount of sulphuric acid; there can therefore be little doubt that the catalytic action of this acid was an essential factor in the transference of the sulphonic group.

(*c*) *Transference of a Nitro Group.*—It has been seen in the preceding paragraphs that the transference of a halogen atom is effected only in presence of the halogen or halogen-acid and that a sulphonating agent is probably present in all cases in which a sulphonic group is transferred; in a similar way the transference of a methyl group when methylaniline, $C_6H_5.NH.CH_3$, is converted into toluidine $CH_3.C_6H_4.NH_2$ is effected in presence of a methylating agent and may be attributed to reversible methylation. The nitro group differs from these radicals in that it can (apparently) be made to wander by means of agents which have no nitrating properties and do not even contain any nitrogen. One of the most striking illustrations of this is to be found in the action of bromine on dinitro-*p*-bromophenol, whereby the radicals NO_2 and Br are caused to interchange.[3]



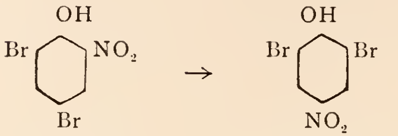
Dinitro-*p*-bromophenol Dinitro-*o*-bromophenol

[1] "Isomeric Change in the Naphthalene Series," *Proc. Chem. Soc.* 1887, **3**, 143 *et seq.*

[2] "Studies of Isomeric Change," *Proc. Chem. Soc.*, 1892, **8**, 90 *et seq.*

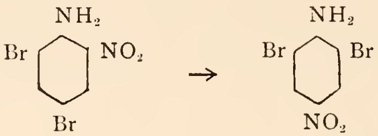
[3] Armstrong, *Trans. Chem. Soc.* 1875, 520.

A similar change takes place in the case of dibromo-*o*-nitrophenol



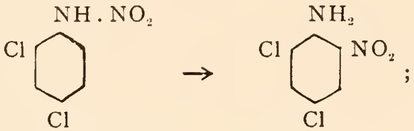
Dibromo-*o*-nitrophenol Dibromo-*p*-nitrophenol

and apparently also in the dibromo-*o*- and *p*-nitranilines when acted on by concentrated hydrochloric or sulphuric acid.[1]



The interchange does not occur, however, in the corresponding chlorine compounds nor when the hydrogen of the phenol is displaced by an alkyl group.

Somewhat similar conditions attend the conversion of the nitroamines into nitranilines, *e.g.*



Dichlorophenyl-nitroamine Dichloro-nitraniline

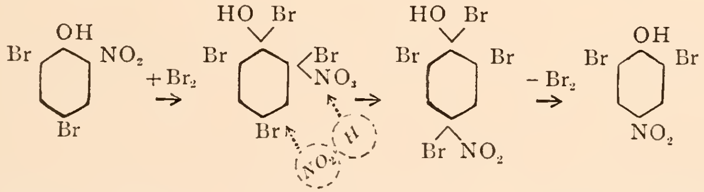
Orton and Reed have shown[2] that the colourless nitroamine can be converted into the yellow nitraniline, not only by the action of nitric acid, but also by that of hydrogen chloride, hydrogen bromide, chlorine and bromine, when these reagents are applied in the gaseous form to crystals of the nitroamine enclosed in a sealed tube with phosphoric anhydride; iodine, sulphur dioxide, carbon dioxide and methyl iodide had no action on the nitroamine. The mechanism of this action is in part elucidated by the discovery that a colourless compound of the nitroamine with a molecular proportion of hydrogen chloride can be precipitated by passing the dry gas into a solution of the amine in light petroleum; this compound is resolved by alkalis into the nitroamine and

[1] Orton and Pearson, *Trans. Chem. Soc.* 1908, **93**, 725.

[2] *British Assoc. Report*, Leicester, 1907.

hydrogen chloride, but when kept in a dry atmosphere slowly changes into the nitroaniline. The formation of this additive compound is evidently an essential stage in the process—a view that receives strong support from the fact that isomeric change is not brought about by hydrogen chloride at pressures below $\frac{1}{30}$ atmosphere nor by *aqueous* hydrogen chloride at concentrations below N/200. It will be noticed that the additive compound contains the group $-N\begin{matrix}HCl\\HNO_2\end{matrix}$; it is therefore not improbable that it may be partially resolved into the chloroamine and nitrous acid, and that—whatever the nature of the added catalyst may be—the isomeric change really depends on the presence of a trace of nitrous acid. The fact that nitrous acid is always formed during the transformation has recently been demonstrated by Orton[1] and affords important confirmation of the view that this acid is the only directly-efficient agent in bringing about the transference of a nitro group. The actual transference could be effected by electrolysing the acid in such a way as to discharge two atoms of electrolytic hydrogen at the $-NH.NO_2$ group, thus reducing it to $-NH_2 + HNO_2$, at the same time delivering two equivalents of $-NO_2$ at the adjacent $\geqslant CH$ group and so converting it into $\geqslant C.NO_2 + HNO_2$.

In the case of dibromo-*o*-nitrophenol and the related compounds the formation of an additive product is probably again the first stage in the action. From such a compound a trace of nitrous acid might easily be split off, and if this were electrolysed in such a way as to discharge its H—and—NO_2 groups at the "2" and at the "4" positions in the benzene ring an isomeric compound would result from which dibromo-*p*-nitrophenol could be produced by merely removing a molecule of bromine—



Dibromo-*o*-nitrophenol Bromine additive compounds Dibromo-*p*-nitrophenol

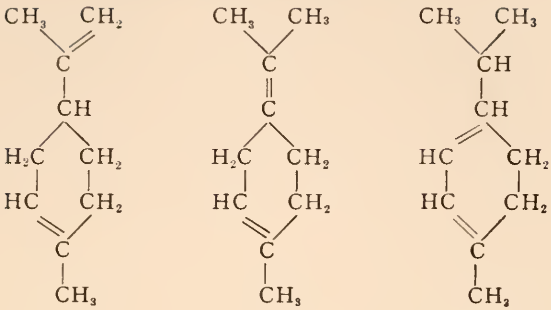
The ease and smoothness with which this and many other

[1] *British Assoc. Report*, Winnipeg, 1909.

isomeric changes take place is probably to be attributed to the greater readiness with which an electrolyte can discharge at two points in the same molecule as compared with interactions in which the products of electrolysis have to be delivered to different molecules; the larger number of units that have to be brought into the electrolytic circuit in the latter case provides an ample explanation, from the kinetic standpoint, of the lessened efficiency of the various agents employed. A chloroamine is thus a better agent for chlorinating the ring-system of its own molecule than it would be for acting upon some other substance, and similar considerations apply even more emphatically in the case of the nitro group.

(*d*) *Transference of Hydroxyl Groups and of Double Linkages.*—The interconversion of alcohols and olefines has been so fully dealt with in a previous article[1] that only a brief reference need be made to it at this point. The mechanism of the change depends on the dissociation of the alcohol (or its sulphate) into olefine and water (or sulphuric acid). These operations may be formulated electrolytically, the addition to the olefine of water or of sulphuric acid depending on the discharge at the double bond of the radicles H and OH or H and SO_4H, whilst the dissociation of the alcohol or its sulphate depends on an electrolysis in the reverse direction. Attention may, however, be directed to the dominant importance of this machinery in determining the plastic character of the members of the terpene group of compounds. These substances possess the faculty of undergoing isomeric change to such an extraordinary degree that in some cases, and notably in that of pinene, it is the exception rather than the rule to be able to prepare derivatives which possess the same structure as the parent substance. In nearly every case these changes can be explained by the addition and removal of water, sulphuric acid or hydrogen chloride, sometimes in connection with the ethenoid linkages of the molecule, and sometimes in connection with the ring-systems. Examples might be multiplied to almost any extent, but it will suffice to mention the conversion of limonene and of terpinolene into terpinene as illustrating the migration of a double bond under the influence of an acid catalyst by a series of changes precisely similar to those studied by Butlerow in the isodibutylenes:

[1] Lowry, "Isomeric Change, Part I.," *Science Progress*, 1909, iii. 616–37.



Limonene Terpinolene Terpinene

whilst the technical preparation of camphor from turpentine may be quoted as illustrating the changes in ring-system that may be effected by similar agents. The extraordinary fertility of the process when applied to camphor itself has been discussed by Armstrong and Lowry,[1] and need not now be referred to in detail.

One other change depending on dissociation possesses great historical interest—namely, the conversion of ammonium cyanate into urea. There can be little doubt that in this case the ammonium salt dissociates into ammonia and cyanic acid, and that the change into urea depends on the recombination of the ammonia with the cyanic acid functioning, not as an acid, but as an isocyanate—

$$\underset{\text{Ammonium Cyanate}}{NH_4.CNO} \rightleftarrows \begin{matrix} HN:C:O \\ H.NH_2 \end{matrix} \rightleftarrows \underset{\text{Urea}}{NH_2.CO.NH_2}$$

C. Isomeric Changes of the Second Group

(*a*) *Transference of a Mobile Hydrogen Atom.*—In striking contrast to the isomeric changes of the first group, which involve the interchange with a hydrogen atom of a halogen or of a sulphonic, nitro- or hydroxyl group, is the behaviour of compounds such as nitrocamphor which contain a "mobile" hydrogen atom and undergo change with extraordinary facility. So far as the mere formulation of these compounds is concerned there is no great contrast between their isomerism and that of the olefines, since in each case isomeric change involves only the transference of a single hydrogen atom and the rearrangement of the bonds in the molecule. The wide difference between

[1] "The Optical Inversion of Camphor and the Mechanism of Sulphonation," *Trans. Chem. Soc.* 1902, **81**, 1469.

the two cases appears to be associated with, though not necessarily dependent on, the fact that whilst the hydrogen of the olefines is inert and paraffinoid, the compounds now under consideration are characterised by the presence of hydrogen atoms which are displaceable by metals and possess more or less pronounced acidic qualities. Many of these compounds, it is true, are so feebly acidic that they only yield salts by the direct action of metallic sodium, but even this weak acidity appears to be sufficient to bring them into the group of electrolytes and to confer on the displaceable hydrogen atom a degree of mobility which has no parallel in the case of atoms which cannot be thus displaced.

Four chief types of compounds come into consideration in this group of changes,[1] namely, those in which there is a transference of hydrogen—

(i) Between carbon and oxygen: in carbonyl-compounds, of which the example most frequently quoted (though by no means the most fully investigated) is ethyl acetoacetate:

Ketonic form — Enolic (unsaturated alcohol) form

and in compounds such as nitrocamphor—

Normal form — Pseudo form

(ii) Between carbon and nitrogen as in prussic acid—

$$H.C\equiv N \qquad\qquad =C=NH$$

(iii) Between nitrogen and oxygen, as in isatin—

or in acetoxime—

(iv) Between oxygen and oxygen, as in nitrosophenol (quinone monoxime)—

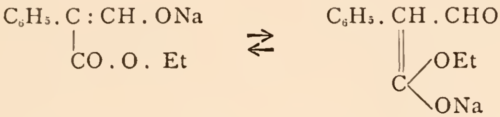
$$HO.C_6H_4.NO \rightleftarrows O=C_6H_4=NOH$$

[1] An exhaustive classification into a score of groups has been made by Laar

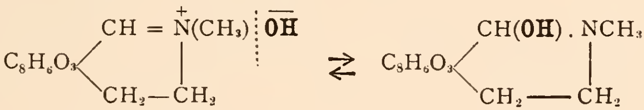
In all these cases isomeric change takes place so readily that in spite of repeated and persistent efforts it has often been found possible to isolate only one member of each pair of isomerides, although the existence of the second can be clearly demonstrated by a variety of methods.

(*b*) *Other Mobile Radicles.*—That this readiness to undergo isomeric change is associated with the electrolytic properties of these compounds and does not depend merely on the lightness of the hydrogen atom is shown by the fact that a similar mobility characterises not only the metallic radicle in isodynamic salts such as those of benzamide—

and ethyl phenylformylacetate—



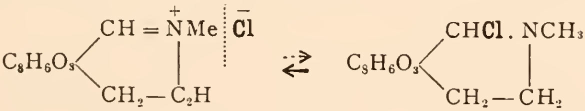
but also the inert radicles —OH, —Cl and —CN when these are present in one of the isomerides in an "electrolytic" or "ionic" form, *e.g.* in organic bases such as cotarnine—



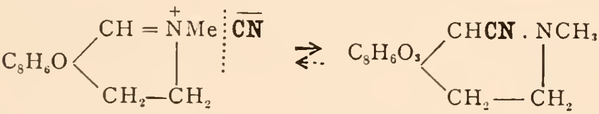
Basic form (predominates in aqueous solutions)

Carbinol form (predominates in ether and chloroform)

or in salts, such as its chloride (stable in the electrolytic form)—



and its cyanide (stable in the carbinol form)—



In all these cases one of the isomerides is an electrolyte, and there can be little doubt that the facility with which the radicle is transferred is closely associated with this fact.

(*c*) *Influence of Catalysts.*—The contrast between the two groups of isomeric changes is seen in a very striking form in the influence of catalytic agents. In the first group it is usually necessary to employ somewhat drastic methods of treatment in order to effect a change, and even powerful catalytic agents, such as the mineral acids and the halogens, have often to be applied at high temperatures and for prolonged periods before the transformation can be brought about. In the second group of compounds the problem is entirely opposite in character, the changes taking place so easily that it is a matter of the utmost difficulty to arrest them or to obtain proof that they do not proceed spontaneously in the absence of any catalytic agent whatever. Such evidence has, indeed, only been forthcoming in a limited number of instances, but there is no reason to doubt that similar methods of investigation would lead to analogous results in other cases.

The difficulty of investigation increases with the velocity of isomeric change and its sensitiveness to the catalytic action of minute traces of impurity. Relatively speaking, this velocity is low in the case of the ketonic compounds, since many of these can be recrystallised without undergoing isomeric change, provided that care is taken to crystallise rapidly and at a low temperature. The isodynamic sugars change more rapidly, at least in aqueous solutions, and in the nitro-compounds the velocity of change is still higher. These velocities are, in all probability, related somewhat closely to the acidity of the hydrogen atom, which is extremely slight in the ketones, but very pronounced in certain of the nitro-compounds, such as nitroform, $CH(NO_2)_3 \rightleftarrows C(NO_2)_2 : NO_2H$, benzoylnitromethane, $C_6H_5 . CO . CH_2 . NO_2 \rightleftarrows C_6H_5 . CO . CH : NO_2H$, and nitrocamphor.

Catalytic Action of Bases.—All these compounds are peculiarly sensitive to the catalytic action of bases, a fact that is perhaps to be attributed to the greatly increased tendency to ionisation which is observed when a weak acid is converted into a metallic salt. A climax of sensitiveness is reached in the case of nitrocamphor, which responds distinctly to the addition to its solution in benzene of piperidine to a concentration of N/10,000,000, *i.e.* roughly one part in one hundred million or one decigramme per ton of solvent. The sugars are much less sensitive, and in the case of a typical diketone, benzoylcamphor, an equal velocity is only attained by increasing the concentration of the catalyst about a thousand-fold.[1]

Neutral Salts have no marked catalytic action on neutral or feebly-acid compounds, such as the sugars, the simple nitro-paraffins, and nitrocamphane. Stronger acids are, however, capable of displacing the mineral acid from a salt, and when this is the case a neutral salt may exert a catalytic action comparable with that of the free bases. Quantitative experiments with nitrocamphor have shown that sodium chloride is a powerful catalyst, although about 800 times less efficient than sodium ethoxide. This action of neutral salts is of the utmost importance in attempting to check the progress of isomeric change by purifying the materials, since the use of glass vessels inevitably results in serious contamination, and satisfactory conditions can only be obtained when contact with glass is avoided by using apparatus made of silica.[2]

Acids exert a marked catalytic action on the isomeric changes of the sugars and of nitrocamphor, but this is much less powerful than when bases are used; in aqueous solutions of glucose hydrochloric acid was found to be about 100 times less efficient than caustic soda; in the case of nitrocamphor dissolved in benzene trichloracetic acid was about 1,000 times less efficient than piperidine.

Water also acts as an efficient catalyst, 1 per cent. of water added to an alcoholic solution of nitrocamphor producing the same acceleration as sodium ethoxide at a concentration of N/10,000.

(*d*) *Arrest of Isomeric Change.*—In the case of compounds which do not change rapidly and are not specially sensitive to the action of catalysts, there is no difficulty in holding back isomeric change even in presence of a solvent. This fact is illustrated by practically the whole of the compounds discussed in connection with the first group of isomeric changes; it is seen in a particularly striking form in the case of compounds which only change in presence of an acid catalyst, and may be preserved indefinitely in alkaline solutions. In the case of compounds of the second group, which are specially sensitive to alkaline catalysts, opposite conditions prevail; the presence

[1] Forster, *Trans. Chem. Soc.* 1901, **79**, 999.

[2] Silica tubes and discs for polarimetric work can be obtained of excellent quality and at a low price from the Silica Syndicate.

of a minute trace of an acid may in some instances prove almost as efficient in arresting change as the ideal condition of absolute purification; usually, however, the presence of the acid is itself sufficient to cause an interconversion of the isomerides, and this simple method of demonstrating the necessity of a catalyst is therefore not capable of general application.

Glucose.—In the case of the sugars, which are very sensitive to the action of alkalis and only slightly sensitive to the action of acids, it is possible by quantitative experiments to determine the part played by these agents in promoting isomeric change. The method of investigation depends on the fact that neutral salts, even at very high concentrations, have no marked catalytic action on the isomeric change of glucose; it should therefore be possible by careful neutralisation to get rid of any free acid or alkali present in the solution, and so to check, or even actually to arrest, the change. In order to account for the velocity of change observed in a 5 per cent. solution of glucose, it would be necessary to assume the presence of acid or alkali equivalent to N/100 HCl or N/10,000 KOH, the latter being a possible figure, the former obviously out of the question. After adding to an aqueous solution of glucose a quantity of hydrochloric acid sufficient to give a concentration of N/10,000, three points on the curve which showed the velocity of isomeric change were found to be absolutely normal, whilst two later readings indicated a very slight retardation. The quantity of acid used was a hundred times too small to produce any marked acceleration, but was amply sufficient to "hold up" the change, if this had indeed been due to alkaline impurities. Actually, even when ten times this concentration of acid was used, no substantial retardation could be effected, and the only two readings which gave any indication whatever of a decreased velocity showed nothing more than that 5 per cent. of the observed minimum velocity might have been due to an alkaline impurity present at a concentration of about N/200,000.[1]

There can therefore be little doubt that in this case the water is able to bring about an isomeric change without the help of acid or basic impurities; it is, however, probable that, as in Baker's experiments, the water used must be sufficiently impure to possess conducting properties. As the solutions used in the experiments had a conductivity (mainly due to the glucose) one

[1] Lowry, *Trans. Chem. Soc.* 1903, **83**, 1320.

thousand times greater than the lowest value recorded for purified water, it would evidently be a matter of extreme difficulty to arrest the change by diminishing the electrical conductivity of the solution, and no experiments in this direction have yet been attempted.

Nitrocamphor.—The first observations of arrested isomeric change in the case of nitrocamphor[1] were due to the happy

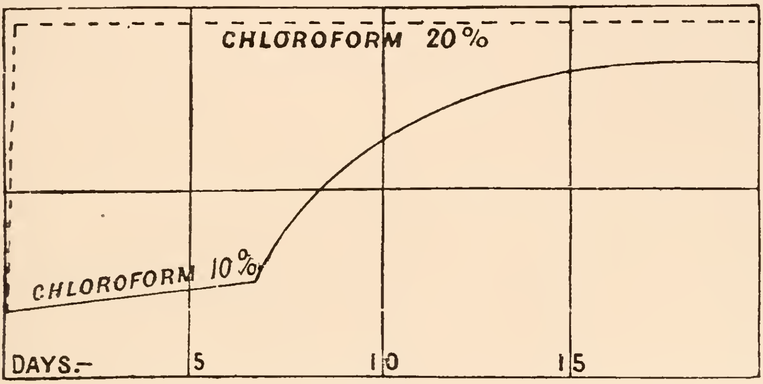


FIG. 1.

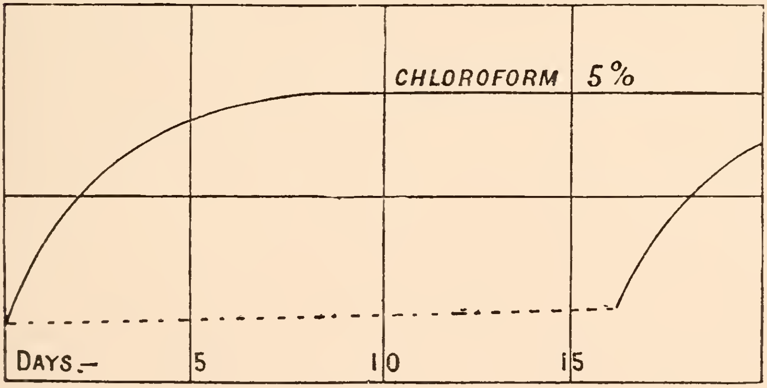


FIG. 2.

Change in rotatory power of nitrocamphor in solution in chloroform.

accident of using chloroform as a solvent. The character of the observations made is well shown in fig. 1. In the first case a 10 per cent. solution was kept in a polarimeter tube during seven days; a very gradual decrease in the rotatory power of the solution indicated that isomeric change was perhaps

[1] Lowry, *Trans. Chem. Soc.* 1899, **75**, 220.

proceeding, but with extreme slowness; at the end of a week some accidental disturbance set the change going, and at the close of a second week a condition of equilibrium was attained. In the second case (fig. 2) a larger quantity of a 5 per cent. solution was prepared in a graduated flask, and a portion only was transferred to a polarimeter tube for observation; at the end of a week a steady condition had been attained, and a second week was then allowed to elapse before the remainder of the liquid in the flask was examined; the surprising observation was then made that the rotatory power of this portion of the solution was substantially identical with the value recorded a fortnight previously, but when it had been transferred to the tube a change of rotatory power immediately ensued exactly as in the case of the portion first examined.

It is of interest to note that shortly afterwards a similar observation was made by Forster,[1] who stated that a solution of enolic benzoylcamphor *in chloroform* "could be preserved in darkness during twelve hours, exposed to bright sunlight during two hours, and even sown with a crystal of the ketonic isomeride without undergoing change."

From these observations it was concluded that a catalyst of some sort was necessary for isomeric change to take place, even though the change was one which involved only the transference of a single mobile hydrogen atom. It was not, however, until several years later that an explanation was forthcoming which accounted for the peculiar behaviour of these chloroform solutions, and rendered it possible to make observations of a similar character in other solvents.

This further discovery was made in the course of a series of quantitative experiments on the "Influence of Impurities on the Mutarotation of Nitrocamphor,"[2] when it was found that the solutions in chloroform again showed an altogether abnormal behaviour. Thus the basic solutions, to which small quantities of piperidine had been added, appeared at first to be *more* active than the corresponding solutions in benzene, but were found to be far *less* active after diluting with a further quantity of the solvent; and finally at a dilution of N/1,000,000 it was found that although isomeric change started in the ordinary way it proceeded more and more slowly, and finally ceased altogether

[1] *Trans. Chem. Soc.* 1901, **79**, 999.
[2] Lowry and Magson, *Trans. Chem. Soc.* 1908, **93**, 107.

after the liquid had changed to only about one-half of the usual extent. It was therefore evident that the chloroform either contained or produced some substance which was capable of destroying the catalytic action of a basic impurity.

The acid chloroform solutions were also anomalous in that the acids, which at higher concentrations produced an acceleration, were found at a lower concentration to produce an actual arrest of isomeric change, the rotatory power in one instance (a millinormal solution of trichloracetic acid) remaining absolutely constant during twenty-four days. These solutions were evidently not pure, as they soon acquired a pungent and unpleasant odour, which proved to be due to carbonyl chloride formed by oxidation from the chloroform

$$CHCl_3 + O = COCl_2 + HCl$$

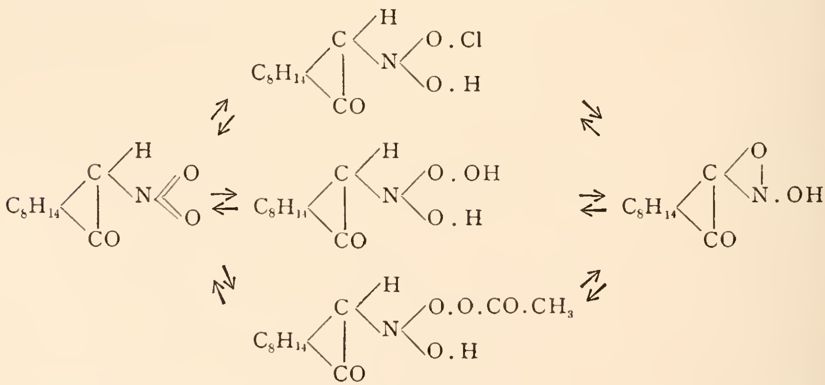
In view of the marked catalytic action of neutral salts, and the entirely different behaviour of acid solutions in chloroform and in benzene, it was evident that the arrest of isomeric change in the chloroform solutions could not be due merely to neutralisation of basic impurities by an acid. But carbonyl chloride, which is usually present in the chloroform and possesses the property of converting amines into ureas, *e.g.*

$$COCl_2 + 2NH_3 = CO(NH)_2 + 2HCl$$
$$COCl_2 + 2NC_5H_{11} = CO(NC_5H_{10}) + 2HCl,$$

would be able to transform these bases into compounds from which the nitrocamphor would be unable to recover them, so that their catalytic action would be permanently destroyed. Direct experiments soon proved that this was the case, as by the addition of either carbonyl chloride or acetyl chloride, the isomeric change of nitrocamphor could be checked, and to all intents and purposes arrested altogether, not only in chloroform but in benzene, in carbon disulphide and in ether. No retardation could be produced, however, by acetyl chloride in presence of acetic acid and acetic anhydride. Phenyl isocyanate, which, like carbonyl chloride, possesses the property of converting organic bases into ureas, was found to be of no value in retarding the change in benzene, probably because it was itself eliminated as a urethane by interacting with the hydroxylic form of the nitrocamphor. Finally, as carbonyl chloride has no special action on mineral bases, it was found that an efficient arrest of the change could only be brought about in silica vessels, the alkali of a glass vessel retaining at least a part of its activity in presence of an acid chloride.

(*c*) *Mechanism of the Transference of a Mobile Hydrogen Atom.*—It has been shown above that solutions of nitrocamphor in chloroform, benzene, carbon disulphide and ether do not undergo isomeric change unless a (basic) catalyst is also present. In acetic acid, however, the change proceeds rapidly without the addition of any such agent, and indeed under conditions such that a nitrogenous base, if present, would be rendered inactive; similar conditions probably prevail also in alcohol and in water, although it is not possible in these cases to employ an anti-catalyst to eliminate the impurities.[1] It can, however, be proved by experiment that the catalytic action of water is much greater than that of any basic impurities it may contain, and whatever action is observed must therefore be attributed to the water itself. It appears then that the isomeric change of nitrocamphor can be brought about not only by alkalis such as NaOH and by bases such as NH_3, but also by acids, by water, by alcohol, and by acetic acid, though *not by ether* or by hydrocarbon solvents. All the active compounds are characterised by the presence of a hydrogen atom which is displaceable by metals, and it is perhaps to this that they owe their activity.

It may further be noticed that in the case of each catalyst the isomeric change can be accounted for by the production and decomposition of an intermediate additive compound, *e.g.*



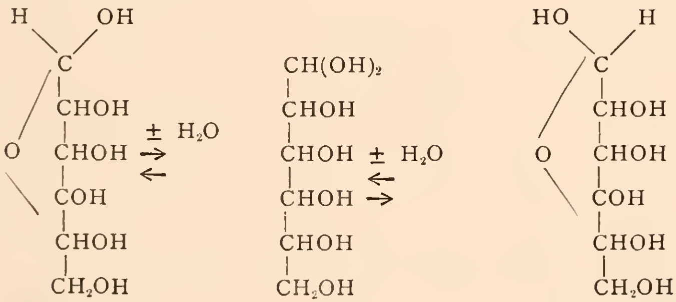
Normal nitrocamphor — Additive compounds — Pseudo-nitrocamphor

[1] Mere neutralisation would be ineffective, and substances such as carbonyl chloride and acetyl chloride are at once decomposed by water and alcohol.

whilst in the case of ether the absence of any catalytic action may be accounted for by the fact that even if an additive compound were formed it could not act as an intermediate stage in the interconversion of the two isomerides. There can therefore be little doubt that these catalysts act in the manner suggested, their addition to the nitrocamphor molecule being effected as in previous cases by an electrolytic process.

It is conceivable that in this operation the nitro-compound might act as electrolyte and the catalyst as depolariser, but this does not appear to be probable in view of the fact that it is the catalyst and not the nitro-compound that is split up in the formation of the additive compound. A more likely explanation of the extreme facility of the isomeric change in substances of this type is that the separation of the hydrogen in an ionic form and its addition to and removal from the molecule as H . Cl, H . OH, H . OC_2H_5 or H . O . CO . CH_3, although not directly related to one another as cause and effect, are cognate properties which are dependent on similar circumstances, and are affected in the same way by the presence or absence of negative groups.

The view that these facile isomeric changes are due to the addition and removal of water, or some similar substance, receives strong support from the behaviour of sugars, such as glucose and galactose, which undergo isomeric change very readily in aqueous solutions, rather less readily in alcohol, and are very susceptible to the influence of alkaline catalysts. There is the strongest possible evidence[1] that the conversion of *α*- into *β*-glucose takes place through the formation of an intermediate hydrate,



α-Glucose β-Glucose

[1] See *Trans. Chem. Soc.* 1903, **83**, 1314-16 ; 1904, **85**, 1565.

and indications are not wanting[1] that the increase in the velocity of change which is produced by adding water to the alcoholic solvent is accompanied by, and is probably due to, an increase in the proportion of this hydrate.

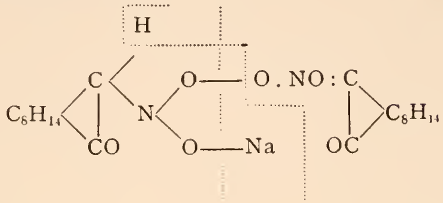
The action of alkaline catalysts requires separate consideration. The hydroxylic isomeride of nitrocamphor is a fairly strong acid, and if the alkali were efficient only in the free state its catalytic action would be immediately destroyed by neutralisation. In discussing the way in which an alkaline catalyst acts it is therefore desirable to work out a scheme in which the active material is the sodium or ammonium salt of *pseudo*-nitrocamphor rather than sodium ethoxide or free caustic soda.

In the case of compounds such as benzamide and cyanuric acid which form two series of salts, the acceleration of isomeric change by alkalis might be attributed to the increase of "ionisation" which results from the neutralisation of a weak acid, the idea being that the separation of the kathion in an ionic form would leave the anion free to undergo an almost instantaneous rearrangement of linkages, *e.g.*

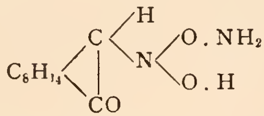
This explanation is difficult to apply to the case of nitrocamphor in view of the fact that there is no evidence whatever that *normal* nitrocamphor is capable either of being "ionised" or of giving rise to a series of "ionised" salts. There is, however, no difficulty in devising a satisfactory scheme based upon the formation of an additive compound of one molecule of nitrocamphor with one molecule of its sodium salt; such a compound could dissociate in two ways so as to liberate the nitrocamphor either in the normal or in the pseudo modification, and the conditions of its formation and dissociation would appear to be entirely favourable to a very rapid interaction. It is hardly necessary to discuss at this point the exact structure of the intermediate additive compound, but one method of formulation is given below for purposes of illus-

[1] See *Trans. Chem. Soc.* 1904, **85**, p. 1569.

tration, the dotted lines indicating the two ways in which the compound may dissociate.



The catalytic action of ammonia may be of the same nature as that of the alkalis, depending on the formation of an additive compound of nitrocamphor with its ammonium salt. But in view of its weak basicity it is possible that free ammonia is the active agent, as in the conversion of ammonium cyanate into urea, and that it acts by forming a compound



of the same type as those referred to above in discussing the catalytic action of water, alcohol, and acetic acid.

D. Optical Inversion

The racemisation or inversion of optically active compounds forms a special case of isomeric change which differs from those exemplified above only in two respects. Firstly, the product of isomeric change is an optical isomeride, the structure of which is precisely similar to that of the original material, and differs from it only in the order in which the groups are arranged around the asymmetric atom which is usually present in such compounds. Secondly, as the isomerides differ from one another only in the same way as an object and its image in a mirror, or a right-handed and a left-handed screw, they must be equally stable, and when a condition of equilibrium is attained must be present in equal quantities, giving rise to an optically inactive or "racemic" mixture. The necessity of dealing specially with this class of isomeric changes arises from the widespread—but completely unjustified—belief that the rearrangement of the groups can be accomplished by merely shaking up the molecule without bringing into action any mechanism whatever. How false this impression is will be realised on referring to the cases discussed below, which are seen to be limited in every direction by special conditions, all of which point to the necessity of some organised system for effecting the interchange of the various radicles.

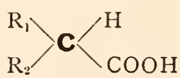
When several asymmetric atoms are present, these may be inverted one at a time: in this case the product is a "stereoisomeride" of the original material, in which the same groups are present, and are linked together in the same order, but differently arranged in space. Since groups which are close together in one modification may be widely separated in the other, the condition of equal stability no longer applies, and when equilibrium is attained the two forms may be present in widely different proportions.

Finally, attention may be directed to the step-by-step inversions discovered by Walden in the case of malic acid,

HO.**C**H.COOH
 |
CH$_2$.COOH

, which is converted on displacing $-$OH by $-$Br and again by $-$OH not into a racemic mixture, but into the optical isomeride; in this case equilibrium between isomerides is obviously not established, the inversion of the groups evidently taking place under conditions of bromination or hydrolysis which do not permit of any reverse action.

(*a*) *Optical Inversion of Acids and of Sugars.*—In order to produce optical inversion it is only necessary to bring the compound into equilibrium with a trace of an optically inactive isomeride, since this will revert equally readily to the *dextro* and *lævo* forms of the original material and so sooner or later reduce it to a racemic mixture. In the case of acids containing the group

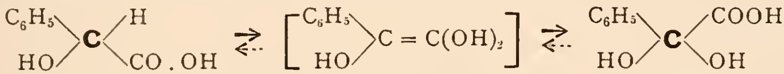


this can be effected by the simple transference of a hydrogen atom from carbon to oxygen, as set out in the following formulæ:

d- or *l*-Acid Inactive isomeride

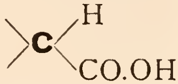
As inversion does not take place if the *a*-hydrogen atom is displaced by methyl it is clear that the mobility of this atom is an essential feature in the process of change, and there can be little doubt that the mechanism suggested above is that by which the inversion is actually accomplished.

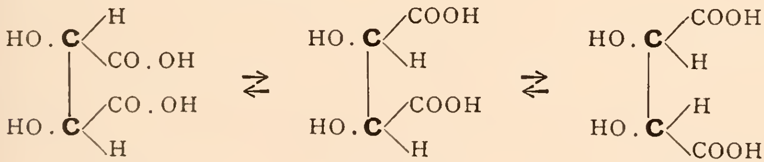
The transference of a mobile hydrogen atom is usually promoted to a greater extent by alkaline catalysts than by acids, but as the sodium salts are transformed less readily than the acids from which they are derived optical inversion is more frequently effected by the help of acids than of alkaline agents. *Mandelic acid* can, however, be racemised by heating it either with water[1] or with dilute alkali[2]



d-Acid — Inactive enolic acid — *l*-Acid

It may further be pointed out that the same series of changes would account for the interchange of H and COOH in the conversion of *maleic* into *fumaric* acid, although this explanation differs from that commonly given of this isomeric change.

Tartaric Acid, which contains two  groups, can undergo optical inversion in two stages, the *d*-acid giving rise first to the inactive (internally compensated) *meso*-acid and then to the (externally compensated) *racemic* or *d* + *l* acid. These are both optically inactive, but are of unequal stability, the production of the meso-acid from the racemic acid in presence of hydrochloric acid at 140° being nearly twice as rapid as the converse process.[3] The final equilibrium (*meso*-acid 66 per cent., *d* + *l* acid 34 per cent.) is, therefore, represented by the scheme



d-Tartaric Acid 17 per cent. — *meso*-Tartaric Acid 66 per cent. — *l*-Tartaric Acid 17 per cent.

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[1] Lewkowitsch, *Ber.* 1883, **16**, 2721.
[2] Holleman, *Rec. Trav. Chim.* 1898, **17**, 323.
[3] *Ibid.* 66.

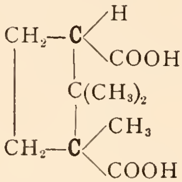
but the change proceeds so slowly that after heating for forty-two hours at 155° the proportions were still only

d-Acid	*meso*-Acid	*l*-Acid
74 per cent.	18 per cent.	3 per cent.

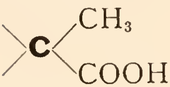
In the case of the salts, which undergo isomeric change when heated with an excess of alkali, the meso-compound is less stable than the racemic, the proportions for equilibrium being

d-Tartrate	*meso*-Tartrate	*l*-Tartrate
38 per cent.	24 per cent.	38 per cent.

Camphoric Acid,

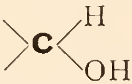


which also contains two asymmetric carbon atoms, each linked to a carboxyl group, ≡C.COOH, differs from tartaric acid in that only one of them is inverted when the acid is heated with water or with a mixture of acetic and hydrochloric acids, *d*-camphoric acid being partially converted into *l*-isocamphoric acid, but giving no trace of *l*-camphoric acid. This difference is undoubtedly due to the fact that only one of the asymmetric atoms is linked both to hydrogen and to carboxyl, the group



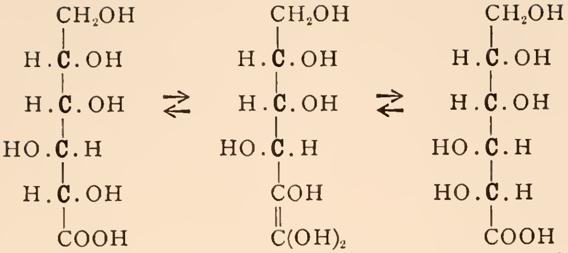
being incapable both of isomeric change and of optical inversion.

Gluconic Acid and the related acids of the sugar group which contain *four* asymmetric carbon atoms linked to hydrogen and hydroxyl, one terminal atom being also linked to carboxyl, afford similar, but even more conclusive evidence of the necessity of a mechanism for bringing about optical inversion, since when the acids are heated with pyridine or with quinoline at 130-150°[1] all the



groups retain their original asymmetry with the exception of the terminal atom which carries the $-COOH$ group and which alone becomes inverted.

[1] Fischer, *Ber.* 1894, **27**, 3193.



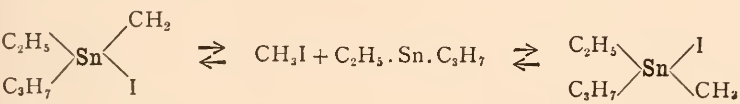
d-Gluconic acid — Intermediate enolic acid — *d*-Mannonic acid

The equilibrium between the *sugars*, glucose, mannose, and fructose which is set up by contact with alkali[1] may be attributed, like the interconversion of gluconic and mannonic acids, to the migration of a hydrogen atom between carbon and oxygen, since each sugar may be regarded as a ketonic form of the "enolic" compound

$$CH_2OH . CHOH . CHOH . CHOH . COH : CHOH.$$

The inversion of an asymmetric carbon atom which accompanies the interconversion of α- and β-glucose has already been explained (p. 235) as due to a hydrolysis of the lactone ring which converts the asymmetric group $>C\begin{matrix}/H\\ \backslash OH\end{matrix}$ into $-CH(OH)_2$; the change appears therefore at first sight to be a widely different process from that which converts glucose into mannose. It should be noted, however, that each may be explained by the addition and removal of water, the enormous contrast between the velocities of the two changes being attributed to the great readiness of the lactonic sugar to hydrolyse, coupled with a great reluctance on the part of the resulting hydrate to yield up a molecule of water in such a way as to produce the "enolic" form of the sugar which forms the bridge between glucose and mannose.

(*b*) *Optical Inversion of Asymmetric Tin and Nitrogen.*—Pope and Peachey have recorded an extremely facile racemisation in the case of the methylethylpropyl stannic iodides.[2] This probably depends on the dissociation of the stannic compound with formation of an inactive derivative of divalent tin.

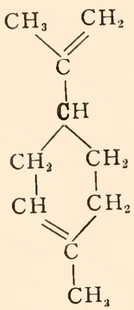


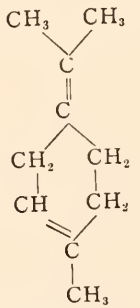
[1] Lobry de Bruyn, *Rec. Trav. Chim.* 1895, **14**, 201.

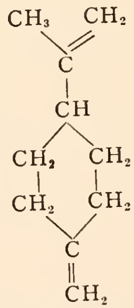
[2] *Proc. Chem. Soc.* 1900, **16**, 116.

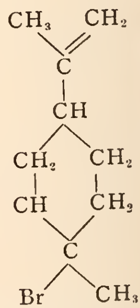
The tetra-substituted ammonium salts, such as $NR_1R_2R_3R_4I$, are much more stable, but no optically-active trialkylammonium salt has yet been prepared, a fact that is without doubt to be attributed to the readiness with which it would racemise by dissociating into the base and acid, *e.g.* $NR_1R_2R_3HI \rightleftarrows NR_1R_2R_3 + HI$. In this respect carbon presents a striking contrast to tin and nitrogen, and there can be little doubt that the stability of the asymmetric carbon compounds in resisting racemisation is largely due to the constant and permanent quadrivalency of the element.

(*c*) *Optical Inversion of Camphor and the Terpenes.*—In the preceding sections, optical inversions have been considered which depend either on dissociation or on the facile isomeric changes of the second group, the latter being handicapped, however, by the fact that the inversion involves two successive changes, one of which may be exceedingly slow. Excellent examples of inversions depending on isomeric changes of the first group are found amongst the members of the terpene group and in the case of camphor. It is, for instance, noteworthy that whilst natural limonene is optically active the limonene recovered from its derivatives or prepared from other compounds by various chemical methods is almost always optically inactive. The facility with which this hydrocarbon racemises is, however, easily understood when it is noticed that it contains only a single asymmetric carbon atom (formula I.), and that this loses its asymmetry if the double bond in the side chain is moved towards the ring as in terpinolene (formula II.) or if the double bond in the ring is moved towards the methyl group (formula III., compare phellandrene) or is eliminated by the addition of hydrogen chloride, or any other agent of the type HX (formula IV.).


Limonene (active)
I.


Terpinolene (inactive)
II.


Inactive Hydrocarbon
III.


Inactive Hydrobromide
IV.

It is, therefore, scarcely possible to carry through a chemical operation with limonene or the related compounds without at some stage producing a symmetrical compound and so destroying the optical activity. The remarkable point is, indeed, not the inactivity of the artificially prepared limonene, but the fact that the natural material is produced in an optically active form.

In striking contrast with the optical instability of limonene and the mono-cyclic terpenes is the extreme stability of camphor and the related members of the dicyclic group of terpenes. Pinene, for instance, gives an inactive product when converted into a *mono*cyclic *di*hydrochloride (limonene or dipentene dihydrochloride, $C_{10}H_{18}Cl_2$) but retains its activity when converted into a *di*cyclic *mono*hydrochloride (pinene hydrochloride or *iso*bornyl chloride $C_{10}H_{17}Cl$) in spite of the fact that the latter operation involves a rearrangement of one of the ring-systems; or, if these operations be pushed to a further stage by removing the hydrogen chloride and so reproducing a hydrocarbon of the formula $C_{10}H_{16}$, the active pinene is found to give an inactive monocyclic limonene, but an active dicyclic camphene.

Camphor itself possesses the quality of optical stability in an extraordinary degree, and retains its activity in a very remarkable way when submitted to the action of the most drastic agents. Indeed, the only action in which racemic compounds are produced to any extent is during the preparation of a π- sulphonic acid from camphor itself,[1] even the analogous treatment of α-bromo- and α-chloro-camphor failing to produce any but optically-active derivatives.

If it be admitted that merely shaking the molecule can never produce a rearrangement of the groups, the optical stability of camphor follows at once from the fact that its activity is due to the presence of three dissimilar chains joining the two asymmetric carbon atoms, and can therefore only be destroyed by breaking one of these chains and converting the compound into a monocyclic derivative. That this actually takes place during the "Optical Inversion of Camphor"[2] is evidenced (1) by the fact that a monocyclic terpene, carvenone, is actually produced from camphor under very similar conditions,[3] and (2) by the difficulty of accounting otherwise for the ready sulphonation

[1] Kipping and Pope, *Trans. Chem. Soc.* 1897, **71**, 958.

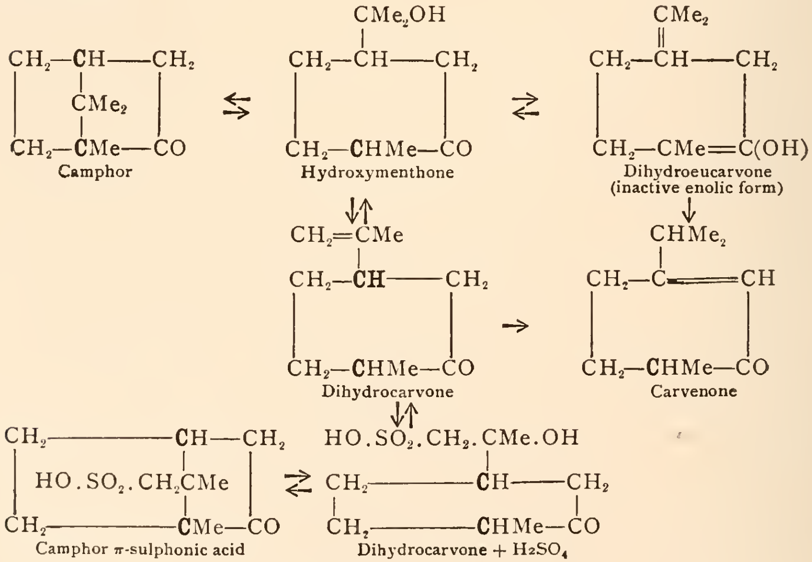
[2] As discussed by Armstrong and Lowry, *Trans. Chem. Soc.* 1902, **81**, 1469.

[3] Bredt, *Annalen*, 1901, **314**, 369.

of the methyl group by the action of chlorosulphonic acid. Both of these observations point to the opening of the $-C(CH_3)_2-$ chain and the production of a terpene-like derivative of cyclohexane. It has been suggested[1] that the first product is a hydroxymenthone (or its chloride or sulphate) formed by the addition to the camphor molecule of a molecule of water (or HCl or H_2SO_4), and that this may part with the elements of a molecule of water in such a way as—

(1) To reproduce a molecule of camphor, or
(2) Give rise to a molecule of dihydrocarvone and so provide an opportunity of sulphonating the methyl group, or
(3) Give rise to a molecule of eucarvone, the enolic form of which would be optically inactive and so provide a mechanism for racemising the whole of the compounds in equilibrium with it, whilst
(4) Finally the wandering from the side-chain into the ring of the double bond of the dihydrocarvone or dihydro-eucarvone would lead to the production of carvenone as recorded by Bredt.

These operations are set out in the following scheme, which bears all the evidences of being absolutely correct in its main outlines, although some of the details may perhaps be open to criticism.

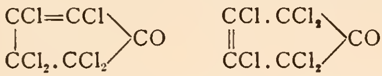


[1] Armstrong and Lowry, *loc. cit.*

E. Velocity of Isomeric Change

Although, as has been shown above, every isomeric change takes place in a complex molecular circuit, it is nevertheless a fact that almost all the changes that have been studied obey the "unimolecular law," which expresses in mathematical form the fact that the rate of change at any particular moment is proportional to the amount of material which awaits transformation. The progress of the action can therefore be expressed by curves and formulæ of the simplest logarithmic type. It is necessary to point out, however, that this simplicity of mathematical form implies nothing more than that the catalyst, whether present in minute traces or in such large excess as to play the part of solvent, remains constant in quantity and in efficiency during the progress of the action, and that if the transformation takes place in successive stages these differ so widely in velocity that the course of the action is controlled almost entirely by the slowest of the processes.

The unimolecular law has been established for the interconversion of the two hexachloroketocyclopentenes[1]



for the conversion of phenylchloroacetamide into *p*-chloroacetanilide,[2] for glucose and a large number of other sugars which undergo isomeric change—accompanied by a change of rotatory power—in freshly prepared aqueous solutions,[3] for π-bromonitrocamphor,[4] and for nitrocamphor itself[5] in several different solvents and under a score of different conditions. It is indeed altogether exceptional to find a case of isomeric change which does not obey the unimolecular law, and it is to the exceptions rather than to the multitude of regularities that interest specially attaches. Two such exceptions may be referred to briefly.

(*a*) *Phenylchloroacetamide.*—Orton has shown[6] that the conversion of this compound into *p*-chloroacetanilide,

$$C_6H_5 . NCl . CO . CH_3 \rightarrow Cl . C_6H_4 . NH . CO . CH_3$$

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[1] Kuster, *Zeit. phys. Chem.* 1895, **18**, 171.

[2] Blanksma, *Rec. Trav. Chim.* 1903, **22**, 290.

[3] For a list of these sugars, see *Trans. Chem. Soc.* 1899, **75**, 214. See, for example, Trey, *Zeit. phys. Chem.* 1895, **18**, 198; Osaka, *ibid.* 1900, **35**, 66.

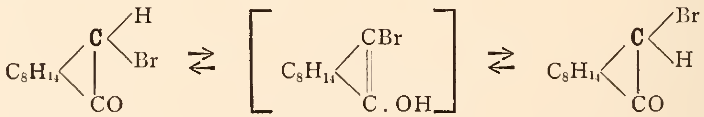
[4] Lowry, *Trans. Chem. Soc.* 1899, **75**, 227.

[5] Lowry and Magson, *Trans. Chem. Soc.* 1908, **93**, 109.

[6] *British Assoc. Report*, Winnipeg, 1909.

in glacial acetic acid proceeds according to a dimolecular law when an excess of hydrochloric acid is present. But if the concentration of the acid is relatively small the reaction is unimolecular, as Blanksma has stated. The exceptional behaviour of this compound is perhaps an indication that the chlorine comes right away from the chloroamide, and then slowly rechlorinates the compound in the *para-* position. The latter process, involving the interaction of *two* kinds of molecule, the concentration of each being proportioned to that of the unchanged chloroamide, might well work out as a dimolecular reaction, especially if the chlorination of the anilide were so much slower than the hydrolysis of the chloroamide as to become the dominant factor in determining the velocity of the action.

(*b*) *Camphor carboxamide*, $C_8H_{14}\langle \begin{matrix} CH.CO.NH_2 \\ | \\ CO \end{matrix}$, and *camphor carboxypiperidide*, $C_8H_{14}\langle \begin{matrix} CH.CO.NC_5H_{10} \\ | \\ CO \end{matrix}$.—These two compounds, which are related somewhat closely to ethylacetoacetate, $\begin{matrix} CH_2.CO.OC_2H_5 \\ | \\ CO.CH_3 \end{matrix}$, differ from it in that they are capable of changing, not only into the "enolic" form, but also by the "inversion" of the active α-carbon atom into stereoisomerides, possessing the same general structure as the original compounds, but differing in the relative positions of the $-H$ and $-CO.NH_2$ radicles. In this respect they resemble α-bromocamphor, the mechanism for inversion being supplied in each case by the production of an "enolic" isomeride. In the case of α-bromocamphor the inversion takes place only on the addition of an alkali,[1] and even then only a minute quantity of the enolic form appears to be produced.

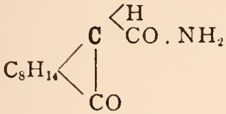


α-Bromocamphor Enolic form α′-Bromocamphor

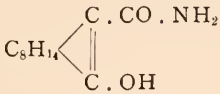
The amide and piperidide of camphor carboxylic acid differ from α-bromocamphor in that inversion takes place without the

[1] Kipping, *Proc. Chem. Soc.* 1905, **21**, 125.

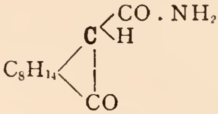
deliberate addition of alkali, and a considerable proportion of the enolic form appears to persist when equilibrium has been attained.



α-Amide



Enolic amide



α′-Amide

The isomeric change which takes place in a freshly prepared solution of the amide or piperidide is readily followed by observing its rotatory power from time to time; but when the results are plotted out in the form of curves these are found not to obey the unimolecular law, but to be inflected in character. This exceptional behaviour is almost certainly due to the conversion of the α-amide or piperidide into an enolic form *of similar rotatory power*, followed by a further change into an α′-compound of *widely different rotatory power*; in the first stage the rotation would alter but little, and towards the end of the action the rate of change must again be very slow, so that under these conditions an inflected curve must inevitably result.

THE LEUCOCYTOZOA:

PROTOZOAL PARASITES OF THE COLOURLESS CORPUSCLES OF THE BLOOD OF VERTEBRATES

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I. Introduction

The *Leucocytozoa* are a group of blood parasites that are of peculiar interest on account of their habitat. It is well known that the chief blood elements in vertebrates are red corpuscles and colourless ones called leucocytes. One function of the leucocytes is that of removing from the blood various injurious micro-organisms that may be present. The leucocytes are enabled to perform this function by means of substances termed opsonins which are developed in the blood plasma. The leucocytes are migratory and can become amœboid, when, by the protrusion of pseudopodia, they surround and engulf protozoal and bacterial parasites and destroy the organisms thus enclosed. From consideration of the above statement, one would hardly imagine that the leucocytes, the destroyers of intrusive organisms, would themselves be invaded by parasites. Nevertheless, such is the case, and these parasites, termed *Leucocytozoa*, both invade and live at the expense of the leucocytes of the blood.

The *Leucocytozoa* are microscopic, parasitic, unicellular animals belonging to the same group as the malarial parasites, that is, to the *Hæmosporidia*—minute, blood-inhabiting, spore-producing organisms. The *Hæmosporidia* are a group of the *Sporozoa*, a large and diverse class of parasites belonging to the great phylum *Protozoa*, the lowest division of the animal kingdom.

II. Historical

Many *Leucocytozoa* have been recorded from mammals. They are known in other vertebrates, but seem to be restricted at present to birds and Amphibia. Like all *Sporozoa*, they occur more commonly in animals inhabiting the tropical zone.

The earliest known of these *Leucocytozoa* was that recorded by Danilewsky in 1890 from the blood of an owl. He regarded his "leucocytozaire," however, as a stage in the development of a *Polymitus* of malaria occurring in the same bird, though he was careful to state that he was far from regarding all *Leucocytozoa* as stages in the development of some *Polymitus*. It is now known that the *Leucocytozoa* and the *Polymitus* of malaria have no connection.

Berestneff, Sacharoff, Ziemann, and Laveran also investigated *Leucocytozoa* in birds, their subjects including various owls, crows and hawks. The parasites resemble closely those described by Danilewsky. Schaudinn (1904) also wrote a paper on a *Leucocytozoön* from the little owl, and considered it to be a stage in the life-history of a Spirochæte. It is now generally recognised that Schaudinn was dealing with a mixed infection, and that Spirochætes and *Leucocytozoa* are distinct organisms, each with its own individual life-cycle.

Recently Sambon (1907) has published a short note on a *Leucocytozoön* occurring in the blood of grouse. Similar parasites have been found in other game birds (cf. Sambon, 1908–9).

Among the amphibian forms, the best known *Leucocytozoön* is *L. ranarum* (Carini, 1907), a large form from *Leptodactylus ocellatus*, a South American frog.

So far as I am aware, no *Leucocytozoa*, apart from the above, have been recorded as occurring in vertebrates lower than the *Mammalia*, but there are several mammalian *Leucocytozoa* which may now be considered.

Leucocytozoön canis, common in the pariah dogs of India, was described by Bentley and by James in 1905. Later, Christophers (1906–7) worked on the same parasite, and described its stages in the tick that carried the disease from dog to dog. He thus linked a definite vertebrate parasite with an invertebrate host, the latter seemingly being unaffected by the parasite. Patton (1906–7) has described more mammalian *Leucocytozoa* than any other investigator of the group. He found a parasite in the leucocytes of the Indian palm squirrel (*Funambulus pennantii*), and named it *L. funambuli*; in the blood of dogs (*L. canis*); in the blood of the bazaar-cat of Madras (*L. felis domesticæ*); and in that of the Indian hare *Lepus nigricollis* (*L. leporis*). Malay dogs also contain *Leucocytozoa* as reported by Gerrard (1906), while dogs in Tonkin have recently been found to be infected.

Several varieties of rats harbour these parasites. Balfour (1905–6) described one (*L. muris*) from the leucocytes of the Norway rat (*Mus decumanus*), occurring at Khartoum. Adie (1906) in rats in the Punjab found the organism that he named *L. ratti*. Cleland (1906) also recorded a similar parasite from West Australian rats. Early in 1909 a remarkable paper appeared by Miller describing a parasitic organism said to be pathogenic in rats. He called this parasite *Hepatozoön perniciosum*. Examination of his text and plates has led most workers on the group to the conclusion that his parasite was really a virulent form of *L. muris*. This will be dealt with in a subsequent section of this paper.

Last year (1908) the writer described a *Leucocytozoön* from the blood of white mice (*Mus musculi*) obtained in London, the parasite being the first recorded both from the mouse and in this country. This parasite was named *L. musculi*.

To the best of my belief, the above list includes all the true mammalian *Leucocytozoa* known.

The *Leucocytozoa* of mammals are very similar, in general morphology, to the vermicular parasites belonging to the genus *Hæmogregarina*, found in the red blood corpuscles of certain mammals, *e.g.* in the jerboa (*Hæmogregarina balfouri*), in the Indian gerbil, and in certain marsupials of Australia. On this account, Mesnil and others consider that the *Leucocytozoa* of mammals should be placed within the genus *Hæmogregarina*. Surely the difference of habitat is sufficiently striking to separate the genera of strict mammalian Hæmogregarines and Leucocytozoa. Further, the species of *Hæmogregarina* are very numerous, occurring especially in reptiles, amphibia, and fishes. We will return to this matter of nomenclature in a subsequent section.

III. General Description

The general shape of the *Leucocytozoa* may be described as somewhat vermiform (figs. 1, 2, 3). There may be no very marked difference between the ends of the body, which are usually rounded, e.g. *L. musculi*, or the parasites may be slightly attenuated and curled at one end, as in *L. funambuli* (fig. 3, A). In some cases, e.g. *L. canis*, *L. musculi*, the parasites are rather broad compared with their length, and may be accurately described as bean-shaped or reniform (figs. 1, A–D; 3, B, C). The average size of the intra-corpuscular (cytozoic) forms is from 8 μ to 10·5 μ long and 4·5 μ to 5·5 μ broad.

Sometimes it is necessary for the *Leucocytozoa* to leave the leucocyte or host cell and come out into the plasma, and so free forms are found. The parasites that are free in the plasma

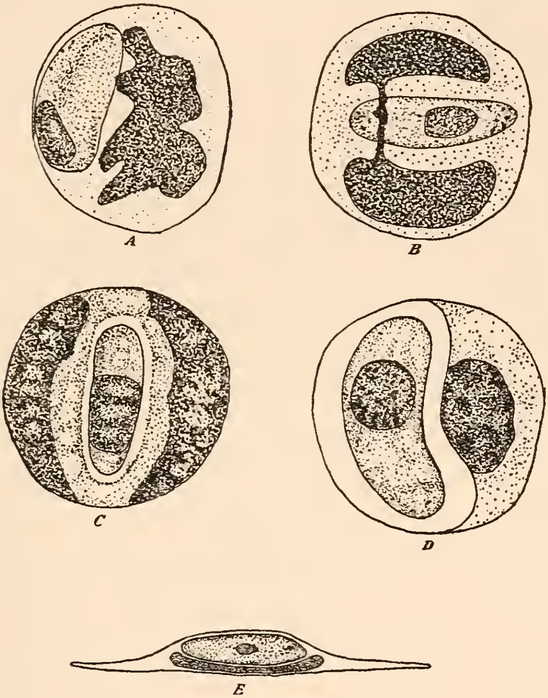

FIG. 1.—Various *Leucocytozoa*.

A, *L. canis* (after James), in polymorphonuclear leucocyte, parasite lying to the left. B, *L. funambuli* (after Patton), showing the splitting of the nucleus of the host-cell by the parasite. C, *L. muris* (after Balfour), showing vermiform parasite which has divided the nucleus. D, *L. musculi* (after Porter), surrounded by a clear cytocyst, lying in a mononuclear leucocyte. E, *Leucocytozoön* (*Hæmamœba*) *ziemanni* (after Laveran). The host cell is drawn out at either end, the nucleus of the host-cell is flattened, while the parasite appears as a vermiform cell in the middle of its host.

may be somewhat narrower than those within the cells, and have been well described as vermicules. This is especially the case in *L. funambuli* (fig. 3, A). The leucocyte may endeavour to protect itself from the action of the parasite by forming a capsule or protecting layer around the invader, such a capsule being termed a cytocyst (figs 1, D; 3, C). If a cytocyst be present it is highly refractile and stains with difficulty. The internal contents or cytoplasm of the *Leucocytozoön* are usually granular; the granules take up stains very easily, and thereby the nucleus or controlling apparatus of the cell, which lies beneath these granules, is frequently obscured. The nucleus often lies nearer one end of the parasite than the other (fig. 1, A, B). Contractile threads or myonemes are present in the outer layers of the protoplasm of the body of *L. musculi* (fig. 3, B), but these have not been recorded in other forms. The myonemes are the agents of locomotion whereby the parasite is enabled to change its position and to progress backwards or forwards in the plasma of the blood.

The parasites living within the leucocytes (cytozoic forms) vary more in appearance than do the free organisms, because they naturally adopt the form most suitable to a limited space. Further, twisting of the parasite on itself occurs, resulting in a thin-edge or tailed appearance. Wenyon (1906) has described a U-shaped form of the parasite discovered by Gerrard in the polymorphonuclear leucocytes of Malay dogs. Judging from the observations of other workers, this is a somewhat rare form. Polymorphonuclear leucocytes (fig. 1, A) naturally afford greater opportunities for irregularity in shape than do mononuclear forms (fig. 1, B, C, D).

IV. Movements

The movements of *Leucocytozoa* have been most fully described in the case of *L. musculi*. The movements of the small forms (merozoites or young daughter-forms) are very slight. On the surface of the body of *L. musculi* myonemes are present, though these have not been described in other forms. By the contraction of the myonemes, the two ends of the parasite are approximated, then, by a sudden wave of relaxation, travelling from the posterior towards the anterior end, the organism is straightened, and at the same time is propelled forwards. As the organism moves forwards, it also rolls from side to side and partially twists on itself. The path of the parasite is never straight and is frequently very restricted. The movements of these small forms are much more active when the parasite happens to be in the neighbourhood of a leucocyte, and they are terminated as such when the parasite penetrates the blood cell. Intra-corpuscular existence involves movement in a much denser medium and avoidance of still denser portions of the cell contents such as the nucleus. Consequently the movement of the invader is greatly altered.

When a young *L. musculi* has penetrated a leucocyte, it remains at first near the periphery (fig. 2, A), and so directs its

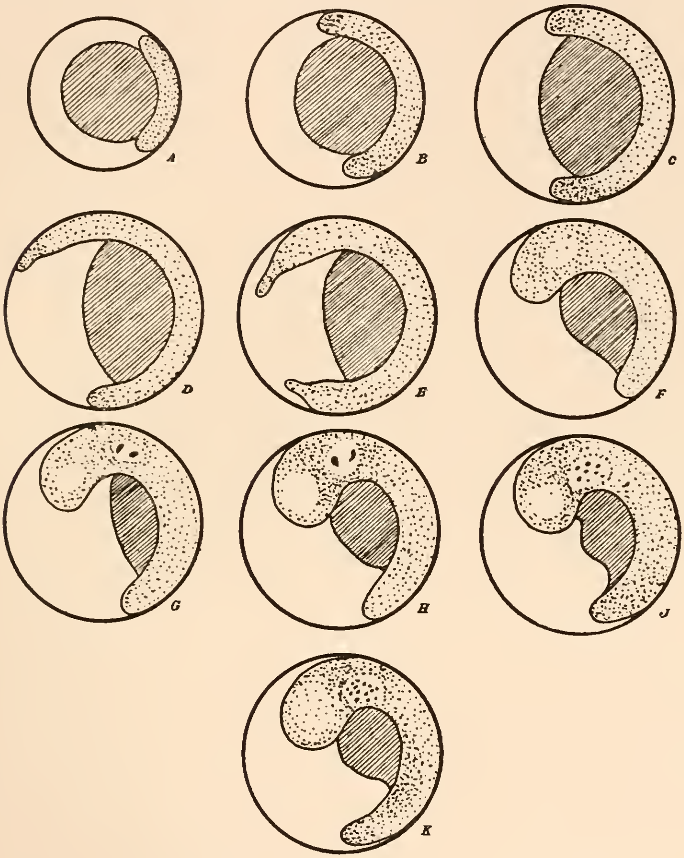

FIG. 2.—Varying forms of a single living *Leucocytozoön musculi*.

Diagrams of a young intra-corpuscular parasite observed in the living condition for seven hours. In these diagrams the cytoplasm of the parasite is represented by small dots, more closely aggregated where the protoplasm stained more deeply (*intra-vitam*). Vacuoles are left clear. The chromatin of the parasite is represented black in G–K, and the nucleus of the leucocyte is shown by shaded lines. The outline of the leucocyte is indicated by a circular area. (After Porter.)

movements that it partly surrounds the nucleus (fig. 2, B, C). Osmotic diffusion from the cytoplasm of the host to the parasite occurs, producing vigorous movements within the leucocyte, accompanied by fairly rapid growth of the parasite. The movement of the latter is more noticeable at one end. This appears to advance steadily by an outflow of the cytoplasm, which outflow is fairly easily seen as the protoplasm is richly granular and stains easily *intra-vitam* with methylene blue. The parasite now lies nearer the nucleus of the leucocyte, and the nucleoplasm appears in a state of agitation. The parasite grows rapidly during this period (fig. 2, A–D).

The gliding movement of the parasite continues, and the nucleus of the leucocyte becomes altered in shape, until at times it appears as a lens-shaped mass, lying within the horns of a crescent formed by the parasite (fig. 2, D, E).

The further forward motion of the *Leucocytozoön* results frequently in one end becoming much larger than the other, and a comma-like appearance ensues (fig. 2, F–K). The posterior end appears somewhat "tail"-like and filamentous under these circumstances, but this is due to the turning of the *Leucocytozoön* on its side, thereby exposing its edge only (fig. 2, J).

When the young parasite has assumed the "comma" form, a vesicle often appears, usually towards the broader end of the organism (fig. 2, F). The *Leucocytozoön* remains much the same in shape for some time, but changes occur in the vesicle, in which small chromatin masses begin to differentiate (fig. 2, G–K). Gradually, however, concentration of the protoplasm occurs, and the "comma" shape gives place to a somewhat bowed, reniform body, in which the vesicular nucleus lies somewhat nearer one end than the other. When the parasite has reached this stage, the host-cell commences to react upon it, and a cytocyst or capsule is produced (fig. 1, D) within which the *Leucocytozoön* is enclosed. The organism may continue motionless for some long time, probably days, but its extrusion either with (fig. 3, C) or without (fig. 3, A, B) its cytocyst, usually occurs.

Extrusion of the parasite is brought about by internal pressure. The *Leucocytozoön* moves forward with a slow, gliding movement, which continues until extrusion is completed. There is slight resistance at the periphery of the leucocyte, but the parasite presses outwards, carrying a small column of the protoplasm of the host-cell at its sides. When it has passed out from the leucocyte, the point of exit closes and is invisible, but the parasite often remains in the vicinity of its host-cell, in a practically quiescent condition, for some time.

V. Comparative Morphology

In most species of *Leucocytozoa* the outer layer, or ectoplasm, of the body is clear, smooth, refractile and difficult to stain. Differentiation of the ectoplasm is rarely seen, but *L. musculi* has myonemes passing down the entire length of its body (fig. 3, B), and these have a bead-like appearance in life. A refractile cytocyst is also recorded in some (fig. 3, C); probably it is secreted by the leucocyte around the invader. In such cases a clear space may be present between the ectoplasm of the parasite and the cytocyst (fig. 1, D).

The endoplasm is always richly granular and stains deeply.

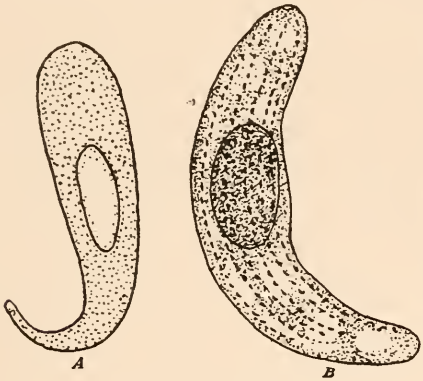

Fig. 3.—Vermicular forms of *Leucocytozoa*.

A, *L. funambuli* (after Patton). Fresh preparation. Parasite attenuate and curled up at one end. Nucleus appears as a clear, central spot. B, *L. musculi* (after Porter). Free parasite showing myonemes. Protoplasm vacuolated. Nucleus almost central. C, *L. musculi* (after Porter), showing cytocyst around the free parasite. Extra-nuclear chromatin granules (chromidia) are seen in a group in the cytoplasm above nucleus. D, *L. musculi* (after Porter). Free vermicule with pointed end, from the gut of the louse, *Hæmatopinus spinulosus*.

In many cases the granules are evenly distributed throughout the endoplasm, but in *L. musculi* slight concentration of the granules occurs beneath the myonemes. A clearer area suggestive of a vacuole is also present in some specimens of *L. musculi* (fig. 3, B, C).

The nucleus is either of the vesicular type, as in *L. musculi* (figs. 1, D; 3, B), or is fairly compact in character as in *L. canis* (fig. 1, A). A nuclear membrane is present, and is denser in forms of the parasite with vesicular nuclei. The part of the nucleus that stains more deeply is termed chromatin. The nuclear chromatin may be present in the form of grains as in *L. musculi* (figs. 1, D; 3, C), or, as in *L. muris* (fig. 1, C), it may extend across the breadth of the nucleus in the form of strands, or the chromatin may form a fairly compact mass, such as may be seen in *L. canis* (fig. 1, A). The general outline of the nucleus is oval or round, its breadth is sometimes that of the parasite (fig. 1, C), or may be slightly less (figs. 1, A, B, D; 3, B, C). In no case is it easy to stain the nucleus, for the outer layer of ectoplasm is difficult of penetration by stains.

Chromidia, or extra-nuclear grains of chromatin, are found in some Leucocytozoa. For instance, preparations of *L. musculi*, stained with Giemsa's stain (alkaline methylene blue and eosin), show areas or caps of a red (chromatin) colour at either end. Isolated chromatic granules also are present (fig. 3, C), occurring in various positions in the body of the organism. *L. funambuli* also exhibits this feature well, scattered groups of chromatin granules being found near the ends of the parasite (fig. 1, B). Chromidia are migratory in nature, and pass out from the nucleus into the general protoplasm, where their presence is often associated with great metabolic activity of the organism.

The exact structure of the *Leucocytozoa* of birds is still a matter of some controversy. The most recent account is that of Wenyon (1908), who agrees that the host-cell may, in some cases, be drawn out into a long, spindle-shaped body (fig. 1, E). In size the gregariniform parasites may be 40 μ to 60 μ long by 5 μ to 8 μ broad. The *Leucocytozoa* of birds are thus larger than those of mammals. Of these avian *Leucocytozoa*, those forms which have markedly granular protoplasm, and are somewhat larger, have been considered female mother forms, while the smaller parasites with few protoplasmic granules have been considered to be male mother cells. The female (gamete) forms, arising from the female mother cells, are usually round, while the male gametes are small, thin, and flagella-like. Undoubted fertilisation has, however, been described very rarely—at any rate from observation of the living organisms.

The best known of the amphibian *Leucocytozoa* is *L. ranarum*, described by Carini in 1907, from the blood of *Leptodactylus ocellatus*, a South American frog. The parasite is large, being from 30 μ to 40 μ long and from 10 μ to 15 μ broad. No trace of differentiation of gametes was seen by Carini in this parasite.

VI. Multiplication and Reproduction

(*a*) *Asexual Multiplication or Schizogony.*—The multiplication of *Leucocytozoa* by the asexual method, or schizogony, is fairly well known. The parasites encyst in different tissues, where nuclear fragmentation occurs, resulting in the production of a number of small, daughter forms, known as merozoites. The schizogony of *L. canis* (fig. 4, A) and *L. musculi* (fig. 4, C) takes

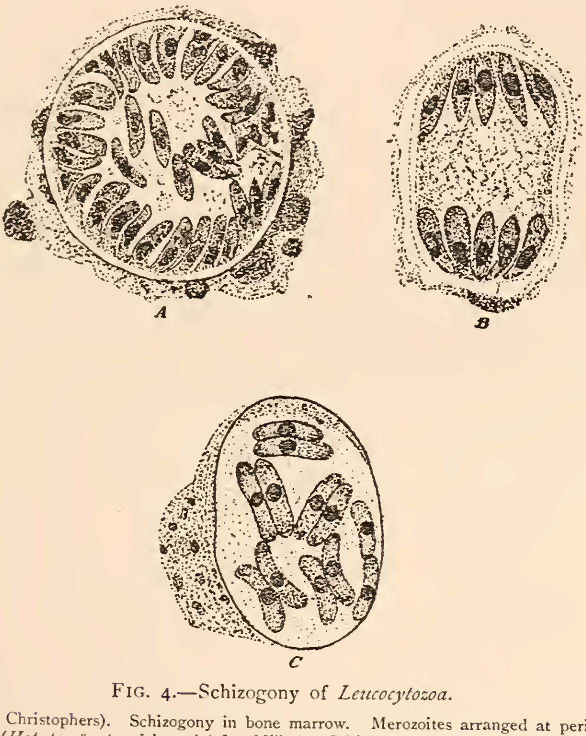

Fig. 4.—Schizogony of *Leucocytozoa.*

A, *L. canis* (after Christophers). Schizogony in bone marrow. Merozoites arranged at periphery of cyst. B, *L. muris* (*Hepatozoön perniciosum*) (after Miller). Schizogony in liver. Merozoites at the poles of the cyst. Much residual protoplasm. C, *L. musculi* (after Porter). Schizogony in bone marrow. Merozoites in groups of two.

place in the bone marrow, that of *L. funambuli* and *L. leporis* in the lung, while the liver is the seat of multiplication of *L. muris* (fig. 4, B). The encysted schizonts vary in size: those of *L. canis* are very large, while those of *L. funambuli* are very small, and are apt to be overlooked in the lung preparations.

The schizonts, or sporulating cells, are oval, refractile bodies, usually with a somewhat thick wall. The protoplasm of all Leucocytozoön schizonts is evenly distributed at first, and eacn schizont has a single nucleus. This nucleus divides into a number of fragments, around each of which protoplasm collects, producing a small daughter form. In *L. canis* at least thirty such daughter forms or merozoites are produced (fig. 4, A), arranged about the periphery of the cyst wall. In *L. muris* the nuclei of the dividing schizont migrate to the poles of the cyst, and the total number of merozoites is about sixteen, arranged in two groups, one at each end of the cyst (fig. 4, B). The merozoites of *L. musculi* (fig. 4, C) are also few in number, some ten to twelve, usually arranged in couples within the cyst. A small amount of the protoplasm of the parent schizont may be left over in merozoite formation, and remains behind in the cyst (fig. 4, A–C). The merozoites are small and usually falciform or vermiform. They become free within the cyst, whose walls ultimately break down, so setting free the minute parasites into the blood-stream, where they infect other leucocytes. The remains of the cell in which encystment of the schizont occurs are found outside the cyst. Schizogony of the *Leucocytozoa* has never been observed in the peripheral blood.

(*b*) *Sexual Reproduction.*—Possible sexual methods of reproduction in *Leucocytozoa* have been rarely observed, and in the cases that have been described there is a certain amount of doubt, owing to the possible occurrence of natural parasites of the invertebrate host, within which the sexual reproduction of the parasite is supposed to occur. The earliest record of sexual reproduction is that made by Christophers (1907), who described a developmental cycle for *L. canis* in the dog tick (*Rhipicephalus sanguineus*). Christophers removed ticks from dogs heavily infected with *Leucocytozoön canis*. The ticks were immediately dissected, and free vermicules were found in their guts. Each vermicule penetrates the gut epithelium of the tick, and there divides into two, then four, and sometimes eight smaller individuals. These secondary vermicules conjugate in pairs, and, as a result of fertilisation, oöcysts are formed. These are about 14 μ in diameter, are oval in shape, and within each, twelve to fourteen small, sausage-shaped, sexually produced parasites, the sporozoites, are differentiated. The possible existence of a natural parasite of the dog tick (*e.g.* a Coccidium) needs very careful consideration in connection with this supposed sexual cycle of *L. canis*.

The late Surgeon W. W. Miller (1908) recently described a sexual cycle of the rat parasite that he called *Hepatozoön perniciosum*, which is probably *L. muris.* The cycle takes place in the mite *Lelaps echidninus*, and as this is migratory, infection of other rats is easily brought about. Miller describes schizogony of the usual type as occurring in the liver of the rat, and by this means numerous merozoites are set free into the blood-stream. These penetrate the leucocytes, and finally encyst. Mites, when feeding on the rats, swallow the encysted *Leucocytozoa*, which are liberated in the stomach of the mite as free vermicules. These vermicules were described by Miller as "similar," and "two similar vermicules become associated and conjugate." One, the female or macrogamete, grows larger and partly surrounds the other, the male or microgamete. After the association the nuclei fuse to form a zygote, which gives rise to a sluggishly motile body, that, in the course of its wanderings, penetrates the stomach wall of the mite, encysts there, and becomes the oöcyst. The nucleus of the oöcyst divides into many parts, which travel to the periphery, and the surface of the growing oöcyst becomes raised into rounded projections. Each projection contains several nuclei, becomes detached, and forms a sporoblast. Finally the sporoblasts acquire a cyst wall, and, within the sporocyst so produced, sixteen minute parasites, the sporozoites, are formed. As each oöcyst contains fifty to a hundred sporocysts, the number of sporozoites is very great. The sporozoites serve to infect other rats. The possibility of the infection of the mite by a parasite peculiar or natural to itself, stages of which might be mistaken for stages of sporogony of the *Leucocytozoön*, must not be overlooked.

Up to the present there is no definite evidence that undoubted sexual differentiation occurs either in the Hæmoflagellates or in the *Leucocytozoa* of the mammalian blood. Some workers, influenced perhaps by what occurs in the malarial parasite, have been led to declare that male and female forms exist. They argue that shorter, thin forms, with sometimes a slightly denser nucleus, are male; while larger, broader forms, with a somewhat less chromatic nucleus and granular protoplasm, are regarded as female. Careful examination of the dimensions and figures given for the two forms of parasites shows that they merely form a continuous series. Where, then, in a series is the limit for, say, male forms to be applied? It is obviously impossible to fix any such limit, and size as a criterion of sex fails entirely.

The occasional occurrence of two *Leucocytozoa* within one leucocyte has been observed in the case of *L. funambuli* and also in certain *Leucocytozoa* of birds. This is probably merely a case of double infection of the leucocyte, and, though some would consider the two parasites to be sexual forms, neither conjugation nor fertilisation have ever been seen, and until these have been witnessed the question of sex cannot be decided.

VII. Mode of Transmission

The exact method of transmission of the mammalian *Leucocytozoa* to fresh hosts has been determined in a few cases only, and even in some of these it is not absolutely certain that the means proved experimentally is the one that occurs naturally or that it is the sole means. Invertebrates—ticks, mites, and lice—have been recorded as the intermediaries for the transference of *Leucocytozoa* from host to host.

Christophers (1907) described the sexual cycle of *Leucocytozoön canis* in the dog tick (*Rhipicephalus sanguineus*), and this arthropod appears to be the agent in spreading the disease from dog to dog. The exact method whereby the tick infects a new host is not known. Adult female ticks were the ones carrying the supposed vermicules of *L. canis*, and, as these ticks are supposed not to suck blood again, the possibility of inoculation of the dog by regurgitation of the small sporozoites seems out of the question, and hereditary transmission of the parasite has not been demonstrated.

Gerrard (1906) also reported that when clean Malay puppies and others infected with *L. canis* were placed together the clean animals contracted the disease, ticks on the infected dogs being the intermediaries.

Miller (1908) described the infection of white rats with *Leucocytozoön muris* (*Hepatozoön perniciosum*) as occurring in two ways. The rapid method is attained by the rats devouring the mites (*Lelaps echidninus*) containing oöcysts of the parasite. This was shown experimentally by feeding rats on crushed mites. The slower means of infection is by the bite of the mites, which wander from the rats into the straw, and thence, possibly, to different rats. Whether the rapid method of infection is a natural one is open to criticism. If it were so, the fæces of the rats should contain the chitinoid remains of the mites, but we are not told that such was the case. If this method of infection occurs naturally, then we have an entirely new method of infection, whereby the vertebrate host eats the ectoparasitic invertebrate carrier.

Miller's experiments were most ingenious. However, in such a complicated subject, mistakes cannot be entirely excluded, and it is just possible that Miller's sporogonic stages of *Hepatozoön perniciosum* in the mite were stages of natural parasites of the mite (*e.g.* a *Gregarina* or *Coccidium*, or both). This remark is not intended as in any sense derogatory to Miller's work, which was most clear and original. One cannot, however, overlook the fact that many most capable observers have for several years failed to find any evidence of a sexual cycle of a *Leucocytozoön* in an invertebrate (arthropod) carrier of infection.

Lice act as agents of transmission of disease in the case of *L. musculi*. Lice fed on infected white mice showed vermicules in their gut, and similar ones placed on clean mice produced a slight infection. No sexual cycle of the *Leucocytozoön* has been seen in the louse (*Hæmatopinus spinulosus*), and it is probable that the hemipteran acts merely as a mechanical agent in the transference of the disease.

The mode of transmission of the *Leucocytozoa* of birds and amphibia is unknown.

VIII. Distribution

The *Leucocytozoa* occur in all parts of the world, though they are more abundant in the leucocytes of animals living in tropical regions. Several *Leucocytozoa*—e.g. *L. canis*, *L. funambuli*, *L. felis domesticæ*, *L. leporis*, and *L. ratti* (a variety of *L. muris*)—occur in various parts of India. Also, in dogs from Malay and Tonkin, *L. canis* has been found. So far as mammalian *Leucocytozoa* are concerned, the majority are found in Asia.

Leucocytozoa also occur in Africa, for many of the avian *Leucocytozoa* are found there. The chief mammalian form from Africa is *L. muris*, described by Balfour from the leucocytes of *Mus decumanus* at Khartoum.

Up to the present the only *Leucocytozoön* described from European mammals is *L. musculi*, which occurred in white mice obtained in London. However, I know that *L. muris* has been found in England.

The fauna of the New World has been less examined for protozoal parasites than that of the Old World, but this deficiency in our knowledge is now being supplied. So far as mammalian *Leucocytozoa* are concerned, Miller (1908) recorded *Hepatozoön perniciosum* (probably a virulent form of *L. muris*) from white rats at Washington, and Theobald Smith (1895) found *Leucocytozoa* in turkeys.

Among the *Amphibia*, Carini (1907) recorded *L. ranarum* which occurs in a South American frog (*Leptodactylus ocellatus*).

From Australia, Cleland has recorded *Leucocytozoa* from the blood of rats in Perth, Western Australia.

From the above it will be seen that the *Leucocytozoa* are distributed over a very large area of the world, though there are many tracts from which none have yet been recorded. This would seem to indicate that these highly specialised parasites have been in existence for a long time, and the fact that in many cases they are not fatal to their hosts also points in the same direction.

We may now note the systematic zoological position of animals harbouring *Leucocytozoa*.

The chief hosts of *Leucocytozoa* among mammals are carnivores and rodents. The chief carnivores are various breeds of dogs and the Madras bazaar-cat. The rodents are more numerous. They include various rats, white mice, Indian hares, and the Indian palm squirrel.

Numerous *Leucocytozoa* have been found in the blood of birds. The chief hosts are the great tit, the raven, the magpie, various species of owls and crows, the Congo grey hawk, the capercailzie, the Scotch grouse, the pheasant, the turkey, the Abyssinian guinea-fowl, and the Francolin partridge. These avian hosts of *Leucocytozoa* have been found in many parts of the world.

In the case of mammalian *Leucocytozoa* the general effect upon the host seems to be that of producing leucocytosis. Patton worked this out carefully in the case of *L. funambuli*. Miller stated that there was marked anæmia in the case of his infected rats, and considered that the parasite was fatal to them. This is the only case in which much mortality among the infected animals has been reported. Some of the white mice harbouring *L. musculi* soon died, but I would not be certain that the *Leucocytozoön* was the sole cause of death.

IX. Nomenclature

Some difficulty has arisen latterly regarding the exact name of the genus to which the *Leucocytozoa*, especially those of mammals, are to be referred. Laveran and Mesnil place the mammalian forms in the genus *Hæmogregarina*. With all due respect to the wide knowledge and authority of the French protozoologists, I cannot agree with them; but follow James and Patton in referring the mammalian parasites to the genus *Leucocytozoön*. James and Patton have the advantage of first-hand knowledge of the group.

The genus *Leucocytozoön* was suggested by Danilewsky for the parasites of the leucocytes of owls; and Lühe has given the name *L. ziemanni* to the parasite first described by Danilewsky, which Ziemann had called *L. danilewskyi* in a different species of owl. The exact nature of the host-cell of Danilewsky's parasites of birds is still doubtful. Laveran believes that the host-cells are really young red blood corpuscles, and refers avian *Leucocytozoa* to the genus *Hæmamœba*, though it must be admitted that the avian *Leucocytozoa* are *not* markedly amœboid in shape, and melanin pigment is not present in them, as it is in most *Hæmamœbæ*, for Laveran includes the strict malarial parasites in the genus Hæmamœba.

Regarding the genus *Hæmogregarina*, Laveran uses it in a wide sense to include the genera *Hæmogregarina* (*sensu strictu*), *Karyolysus*, and *Lankesterella* of other authors. Laveran's genus *Hæmogregarina* is really more of the nature of a subsection of the *Hæmosporidia*, and is wider than a real genus. Further, the genus *Hæmogregarina* otherwise only includes parasites inhabiting the red corpuscles of the blood. The red corpuscles parasitised may be those of reptiles, amphibia, or fishes, as well as the remarkable parasites from the jerboa found by Balfour, from the gerbil or Indian field rat by Christophers, and from certain marsupials by Australian workers. Surely the genus *Hæmogregarina* is very large in whatever sense it is used. The Hæmogregarines of red corpuscles only occur in leucocytes rarely, when about to undergo the process of phagocytosis.

It seems to me that the difference in habitat of the *Leucocy-*

tozoa—namely, occurrence in the leucocytes—is not to be minimised or overlooked, and the genus *Leucocytozoön* should be retained for these parasites.

Again, the asexual multiplication or schizogony of the *Leucocytozoa* of mammals is somewhat specialised. It occurs in a tissue-cell of some internal organ, such as a cell of the liver (*L. muris*, fig. 4, A), a cell of the lung (*L. funambuli*, *L. leporis*), a cell of the bone marrow (*L. musculi*, fig. 4, C). Schizogony of the parasites of red blood corpuscles most often takes place within the red blood cells themselves, and only rarely in tissue-cells (spleen, liver, etc.).

However, as the structure and life-history of avian *Leucocytozoa* are still subjects of controversy, and as the name *Leucocytozoön* was first applied to parasites of birds, and Lühe seems to restrict the name thereto, the generic name *Leucocytogregarina* might be used for the highly specialised parasites of mammalian leucocytes, which have a different habitat from the strict Hæmogregarines of red corpuscles.

In connection with the vexed question of nomenclature, protests regarding the abuse of the rule of priority, etc., have been made by several of the most eminent zoologists of the day. Sir Ray Lankester in 1896, in a discussion at the Zoological Society of London, suggested that an international committee should be formed "to produce an authoritative list of names—once and for all—about which no lawyer-like haggling should hereafter be permitted." Nothing of this kind has yet been done. Boulanger (1908), in expressing disapproval of the strict application of the rule of priority, recalls this statement of Lankester, and also states that "names with which all general zoologists, anatomists, and physiologists are familiar should be respected, should be exempted from the rule in virtue of what may be termed the privilege of prescription."

I feel that the *Leucocytozoa* are well included in this category. The name is well established, it is descriptive and self-explanatory, and has been used continuously in literature by many able workers. For these reasons, together with those cited before, the name *Leucocytozoa* should be retained for *all* protozoal parasites of the leucocytes of vertebrates.

In conclusion, the *Leucocytozoa* are minute parasites, having a remarkable habitat, namely, within the leucocytes of the blood.

They pass a portion of their life as small, more or less vermiform or gregariniform bodies, enclosed within the host-cells, but later they may become free in the plasma of the blood. Multiplication of the mammalian *Leucocytozoa* occurs in the internal organs of the host (*e.g.* the liver, bone marrow, and lung). Avian *Leucocytozoa* are said to exhibit sexual characters, but this is somewhat doubtful. A sexual cycle of the mammalian forms has only been described for *Hepatozoön perniciosum* (*L. muris*) by Miller in a mite, and for *L. canis* by Christophers in the dog tick. These sexual cycles need confirmation. It is certain that lice spread *L. musculi* from mouse to mouse.

Such is a general survey of a very interesting group, the *Leucocytozoa*, which perhaps is not homogeneous, as the forms in birds differ in some respects from those occurring in mammals. This survey of the *Leucocytozoa* is the first that has been attempted, and it is hoped that it may draw attention to, and stimulate research upon, an interesting group of parasites which are probably more numerous than we know at present. They should be searched for among reptiles, amphibia, and fishes.

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PROFESSOR RIDGEWAY AND RACIAL ORIGINS

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As even the most jejune of amateur astronomers is aware, the light on the moon in its monthly phases does not advance with a measured and nice precision. At times, owing to the movements known as librations, the advancing line of light halts and actually recedes for a space. In like manner, although the public has with justice come to regard the Presidential Addresses at the British Association as so many milestones in the march of Science into the unknown, there are occasions when they deviate widely from the path established by known facts, and end finally in some erratic retrogression. Of such was the address read at the Dublin meeting by Prof. Ridgeway on the Application of Zoological Laws to Man. In this oration the Professor, so justly renowned for his researches in the archæology and ethnology of the Latins and Greeks, has attacked and to his own satisfaction vanquished problems of such wide and commanding interest as the origin of the Aryans, the application of European politics and institutions to coloured races, and the tendency of our present social laws to engender an (assumed) deterioration of the English race. The deep human interest, the grave importance, attaching to such topics will not be gainsaid. And these pronouncements of the Professor on them, delivered as they were under the ægis of a scientific association, announced, so to speak, *ex cathedra*, must sound in the ears of many with a tone of quite pontifical authority. *The Times* has quoted and endorsed his conclusions, utilising them, indeed, in the support of some characteristic reactionary views. An attempt therefore to show that the arguments used rest on foundations of quicksand, and that the inferences do not really arise from the facts adduced, will perhaps, even for the non-scientific, be neither altogether devoid of interest nor empty of instruction.

Lest any reader should shrink in alarm from a threatened inquiry into the origin of the Aryans, that Holy Grail of anthropological research, let me at once assure them that they will finish this paper with exactly that amount of nescience, knowledge, or prejudice on this point with which they began. So far as I am concerned, the original speakers of the Aryan language or languages may have been tall, blond, long-headed Teutons (Prof. Ridgeway's men of straw), tall, round-headed men of the Alpine or Celtic race, or they may have derived from the short, dark, and long-headed race that has its home on the Mediterranean. That is not the question which in the first of Prof. Ridgeway's theses lies open to the challenge of criticism. The fundamental error in his 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—what our German friends style the *Ursprache*. He predicates also a similar local permanence of idiosyncrasy, polity, and social and religious ideas, but of this more anon. 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. In this view the brunette dolichocephaly of the Mediterranean race resulted from the climatic conditions of that sea, whatever the original type of the races that now inhabit its vicinity. Similarly, the brachycephaly of the Alpine stock is attributed to their mountain origin, and the blondness of the Teutons to the bitter cold of the Baltic shores.

On what zoological facts is it proposed to base this surprising subversion of accepted views? Passing by the difference in fur of the Corean and the Indian tigers, obviously due to the colder habitat of the former, and the sandy hue of the animals in Cutch, a simple case of protective colouring, we find the case of *Equidæ* adduced as a proof of alteration of colouring due to climate. Stress is laid on the fact that the southern zebras and the quagga are of a somewhat dun colour, resembling that of the Central Asian Prjevalsky's horse and kiang, whilst the zebras of the intervening tropics affect, as is well known, a brightly striped livery. The Somali ass, it is also pointed out, resembles its Asiatic, but not its African, congeners in the absence of a shoulder-stripe.

Now, in the first place, it is quite incorrect to say, as Prof. Ridgeway does, that the horse family has not migrated. To even the non-scientific reader it will occur as a genus eminently capable of migration. But the zoologists have proof that the extinct South American horse wandered there from North America, whilst as regards the species in the Old World the only point at issue is whether they originated in Europe, in Asia, or in Africa; no one seriously asserts a common development in all three continents. In discussing, however, Prof. Ridgeway's view of the effect of climate on colour—there is a strong tang of Erasmus Darwin about this—one is met by the difficulty that he adduces no reason and formulates no hypothesis why climate should of itself possess such thaumaturgical virtues, why a tropical sun should favour black and white stripes, or a dun colour should evolve under more temperate skies. No tittle of reason is given to support this alleged nexus of climate and colour; it is, and remains, as much an assertion as, say, the supposed "sweet influence of the Pleiades" on mundane affairs. Protective colouring does not apparently find favour in the Professor's eyes, in fact he expressly denies it in the case of the zebra. To combat his views is, indeed, like combating the spectre of the Brocken; the lacuna in the argument, the absence of the major premiss, imparts to them the air of invincible intangibility affected by theological dogmas. But most inquirers nowadays demand, and demand imperatively, a reason why a cause should produce a specified effect. As a matter of fact, the main factors affecting colouration, as a myriad painstaking inquiries amply demonstrate, are explained by the necessity for concealment in the struggle for existence, combined to a lesser extent with the influence of heredity and the advantages of ornaments attractive in courtship. And by concealment is denoted concealment at the time of greatest danger, especially at that of breeding. Without attempting an exhaustive discussion of the colouration of the *Equidæ*, one may note that in this respect they differ no whit from the remainder of observed living creatures. As various experiments and other facts demonstrate, they spring from a striped ancestor, which explains the survival of stripes on the back and shoulders in otherwise uniformly coloured varieties. (Does Prof.

Ridgeway seriously suggest that the absence of shoulder-stripes in the Somali and Arabic asses is due to the effect of the scorching Red Sea climate?) Again, we have Prof. Galton's authority for the fact that zebras, however conspicuous in the open plains by day, appear hardly visible at a short distance at night—a fact that must seriously embarrass their enemy the lion in his final assaults. Such striping would, on the other hand, spell rapid extermination for the species in any place inhabited by wolves. These carnivora do not, of course, exist in Africa; but before discussing the reasons for other variations of colour such as in the quaggas there, one would have to consider closely the habitat of the species concerned, the methods of the carnivora that prey on it, and the times and periods of its greatest peril. Thus, for instance, in the case of asses their grey colour harmonises admirably with their surroundings under the intense white light of the desert country they inhabit. And in addition to the zebras already noted, the example of the Burmese barking deer furnishes a warning against hasty judgments by first appearances. Nothing can be brighter or more conspicuous than the orange coat of this animal when in a green field. Closer examination shows, however, that it habitually lives in dense jungle, that when this jungle sheds its leaves in the hot weather the deer's coat matches fairly well with the dead foliage, and that at night, when its enemies are on the prowl, it is indistinguishable at a short distance. We may safely postulate similar explanations in the case of the remaining *Equidæ*.

In addition to his remarks on colouring, Prof. Ridgeway lays some weight on the physical changes known to have occurred in horses in the Basuto Mountains, Java, and (he might have added) in the Falkland Isles. But the horses originally introduced into these localities must have been artificially bred animals, and we all know how rapidly such varieties tend to retrogress when exposed to conditions which do not favour the qualities artificially developed. In none of these countries would size or its concomitant speed confer any superiority; in fact, in mountains large horses are at a distinct disadvantage. In such conditions the artificial qualities so laboriously obtained quickly disappear, and the animal reverts to its ancestral type. Man, it is needless to say, has never yet attempted to apply to himself the methods by which he has differentiated and perfected the various breeds of his domestic animals. The argument from analogy does not therefore apply.

Turning now to mankind, Prof. Ridgeway endeavours to argue that the dark, long-headed Mediterranean race is not ethnically one species, but represents the result of an admittedly charming climate and environment on different stocks settled on its shores. But in the name of all that is logical, why should this environment favour dolichocephaly? Through what dim necromancy or subtle magic do men tend to become long-headed under the influence of blue skies and a very dry and delightful climate? On the shores of the Black Sea live races with a distinct trend to brachycephaly. Why should the Black Sea climate conduce to round-headedness? or, for that matter, why should brachycephaly dominate one side of the Bay of Bengal and dolichocephaly the other? A few questions such as these abundantly demonstrate the absurdity of Prof. Ridgeway's position and the gratuitous character of his hypotheses.

When the latter goes on to say that the skins of mankind tend to get lighter in gradations from the equator to the poles, 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. Thus we find in tropical or sub-tropical regions yellow, brown, reddish, or black men, but never blue or purple men; for these latter colours would clearly fail to protect. When a racial colour has once established itself, varying degrees of heat do not, however, seem to affect it in a marked degree. Witness the comparatively slight alteration in colouring between the Mongoloids in the Malay Peninsula and those in Northern Asia, or between that of American Indians in different latitudes. The Irulars in the Neilgherry Mountains attract attention by their excessive blackness, and the Brahuis in the mountains of Baluchistan are also excessively dark. Racial types once fixed alter only with secular slowness.

To attribute the production of the blond race of North Europe, as Prof. Ridgeway does, to the same causes that produced the white hares and white bears, implies a singular inability to grasp the relevant facts of the case or to frame inductions upon them. The whiteness of animals inhabiting the 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? If so, their capacity for endurance must have aroused the amazed envy of neighbouring Mongoloids, who (poor fellows!) probably then, as now, protected themselves from the wintry cold by robes of skins. But apart from the Laps and Esquimaux, the fact that other races living in similar climatic conditions in North Asia, North America, and Patagonia have never developed any tendency to blondness sufficiently disposes of this bizarre suggestion. Nor does the case of the fair-haired Berbers in the Atlas or of the Amorites in the Syrian Mountains in any way eke out the meagreness of the argument. Where we find islets of blond-haired people living in similar climatic conditions, but separated by broad channels of melanochroids from the great mass of people of the former type, the obvious cause which suggests itself is that they are the remnants of colonies which, with the cessation of the stream of immigration, have failed to maintain themselves in the plains against the surrounding melanochroic population. The facts of the Arctic-Alpine flora furnish an analogy—not, however, to be pushed too far. Two causes for this failure suggest themselves. If the Mendelian law applies to the human race—and far be it from me to express an opinion on this *causa teterrima belli*—a probable recessiveness of blond hair and dominance of black would account for the phenomenon. But it seems more likely that the fair races succumbed to zymotic diseases in the sub-tropical climate of the lowlands—diseases to which their autochthonous inhabitants would be largely immune. The examples of the Himalayas, of the Andes, and of a hundred other mountain ranges inhabited by races as dark-haired as those in the neighbouring plains sufficiently dispose of the supposed effect of altitude on blondness. In Scotland, as a matter of fact, the pigmentation survey shows blondness to be predominant in the valleys and dark hair in the mountains.

For a similar reason the Professor's theory regarding the brachycephaly of the Alpine or Celtic race falls to the ground. This theory finds its only support in the head-shape of the local ox. I do not for a moment desire to asperse the virtues of this estimable beast, which, like other four-footed animals, must perforce carry its head up and down hill stuck out in front, but I really cannot accept its existence as proof of an inherent power of mountains to cause brachycephaly in human beings. As well attribute the long-headedness of the Esquimaux to the aurora borealis. Indeed, on this view, how would one account for such dolichocephalic phenomena as, say, the Scottish Highlanders (index 76), the Corsicans (index 73), the mountaineers of the Pyrenees (index 77), or those of Kashmir (index 72–4)? Prof. Ridgeway is careful to add that he is far from suggesting that altitude is the only cause of brachycephaly. One is glad, sincerely glad, of this reservation. We can still, it seems, light fires without phlogiston.

In frequent jeopardy from its failure to harmonise with or account for known facts, the attempt to attribute the racial differences of man to physical surroundings finally suffers shipwreck upon another shore. As Sir Ray Lankester 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. The process was completed by the cultivation of grains and the fortification of villages. Henceforth mankind could spread, multiply, and migrate, subject only to checks due to inter-tribal collisions and (owing to his migrations) to an enormously increased mortality from the most terrible enemy of all, the pathogenic protozoa. By the inventions specified man had indeed grasped the overlordship of the world, only to find himself subject to new and calamitous agents of selection. But though the development and progress of the human race are still subject to selective checks, these differ radically and in essence from those under which the life of the forest, air, and stream have evolved. To argue from one set of facts to the other involves a logical blunder.

Having in fancy vanquished and utterly crushed the miserable anthropologists who in their base reliance on facts had attributed a certain permanence and continuity to head-shape and to other physical characteristics of race, Prof. Ridgeway next carries his victorious banners into the domains of language and of social customs. He first alleges on very modern and meagre data the fixity of language and the continuance locally of primitive tongues. In historic times certain dark-haired races—*e.g.* the Arcadians and other Greek tribes—always spoke an Aryan language, whilst other similar tribes—*e.g.* the Picts, the ancient Irish, and the Illyrians—founded their marriage systems on a polyandrous basis. Hence he concluded that the primordial Aryan-speaking people were neither blond in colour nor patriarchal in their social polity. A double fallacy lurks beneath the surface of this argument. In the first place, abundant historic evidence exists to show the ease with which savage or half-civilised peoples change their language. The rapid Latinisation of Spain and Gaul is a case in point. In less than a hundred years the victorious Arabs imposed their language on the north coast of Africa. "Even at the present day," says Risley in his Report on the Census of India, "we see absorption of the aboriginal tribes by the Aryans [*sic*] going on before our eyes, and the first thing to yield seems to be the language." As good illustrations may be mentioned Brahui, a Dravidian language now spoken by Turko-Iranians, the Manda languages spoken by Dravidians, and Assamese, an Aryan dialect spoken by Mongoloids. In a precisely similar manner Burmese in Burma continues to dominate and incorporate the languages of various tribes. Countless similar examples can be drawn from other parts of the world; but so well has the impermanence of language been established that it is quite unnecessary to labour the point. Nor do the few examples to the contrary quoted by Prof. Ridgeway carry any weight. Their mountainous environment—an environment notoriously preservative of dialects—accounts for the survival of the Gaelic language in Scotland and Wales, and for the continued diversity of tongues in Switzerland; that Irish survived in days prior to the Gaelic League was due solely to the unattractive and infertile country where it lingered on. It is ill, too, to postulate conditions springing from the considerate delicacy of modern civilisation, of those vast and barbaric times commencing with the dawn of the human race and ending with the period when the Ottoman conquerors thundered at the gates of Europe. No tolerance, but a callous brutality of suppression, too often awaited the mother-speech of the conquered. Of languages it may truly be said, "tout casse, tout passe." Just as Heligoland stands the sole remnant of a vanquished country, so do isolated tongues like Basque in the West Pyrenees, and Hunza, south of the Pamirs, remind us of realm upon realm of lost languages and forgotten dialects.

But apart from these considerations, the mere fact that at the dawn of history certain dark-haired tribes used Aryan dialects is by no means inconsistent with a previous conquest or subjugation by a blond-haired race. In this matter we deal with times of immemorial antiquity stretching far beyond the earliest twilight of the Latin and Greek nations. In the gloom and uncertainty which enshroud this remote period, an uncertainty lifted only fitfully by the discoveries at Halstatt and at Knossos, who can say what kingdoms rose and waned, what turbulent streams of tribal conquest, after sweeping with irresistible force over European countries, checked, halted, and finally ebbed away, leaving only the dimmest vestiges of their power and glory or perhaps no trace at all? But for the accident of a dry climate, who would have suspected the very existence of such cultured languages as Sumerian and Assyrian, or even that of Khotan? You might as well argue that the Pliocene horse was the original type of *Equidæ* as allege Aryan to be the *Ursprache* of the Arcadians or Picts.

Not content with this rapid career over fields anthropological and linguistic, Prof. Ridgeway next plunges hardily into the thickets of sociology and of comparative legislation, and from these brings back spoils even more surprising and unusual. He alleges that social institutions and even religion are derived from physical environment. "We might," he says, "as well ask the Ethiopian to change his skin as to change radically his social and religious ideas"; and he goes on gravely to attribute the success of Mahommedanism in tropical Africa to the fact that it was "evolved"—save the mark—from a Semitic stock living in a low latitude. Christianity is equally a Semitic religion, so possibly it was the ten degrees of latitude between Jerusalem and Mecca which in the tropics have trammelled and impaired its progress. But if so, how account for the Christians in tropical America, not to mention the flourishing churches in Uganda and South Madras, or contrariwise the Mahommedan races such as the Tartars or Kerghiz on the bleak mid-Asian plain? 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. And in respect to the old animistic beliefs, the Silurian strata of religion, if one fact more than another emerges from their study, it is the wide diffusion, the catholicity, of various concepts in countries diverse as the Poles in all conditions of physical environment. No serious student of the history of religions will discern any nexus between the latitude and race of their origin and the countries where they prevail.

In the face of the recent metamorphosis of Japan and the bright promise of a new order in China and in Turkey, it scarcely seems necessary to combat the alleged racial fixity of ideation or permanence in national politics. But history teems with similar examples: witness the sudden development from barbarism of Greece and Rome, their equally rapid decadence, the marvellous changes wrought in Arabia and North Africa by Mahommedanism, the swift uprise of such culture centres as Baghdad under the Caliphs or Spain under Moorish domination. In two hundred years the Seljuks changed their state from barbarism to Christianity and from Christianity to Mahommedan civilisation. To the Egyptian at the commencement of our era what institutions could seem more irrevocably fixed or more native to the soil than the worship of Isis and Osiris and all the peculiar civilisation of the Nile? Yet in two hundred years their religion had yielded to Christianity, and four hundred years later the Arab conquerors tore from the Egyptians every characteristic they possessed, save only their physical type.

Conceptions in social states such as those put forward in the address, though not uncommon, spring from an obvious confusion of ideas, and from short-sighted views of history and of existing politics. Religions, for example, or at least the great religions of culture, are not "evolved" any more than were gunpowder, the Ptolemaic system of astronomy, or Darwinism. By evolution we denote the adaptation of a race to its environment through the elimination of those members unfitted to that environment, not those moral syntheses or conceptions of cosmos which constitute the foundations of a religious system. 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 when human sacrifices lit up the banks of the Thames; subsequently free parliaments were held in London what time the Italians groaned under the most grievous of tyrannies. And so with every other institution that crystallises momentarily in the shifting magma of intellectual progress. The thoughts and ideas of mankind wheel and soar in a sphere wholly different from that where work the slow forces which evolved his somatic cells. Give a man or woman education in the modern sense of the term, freeing his mind from the gyves to freedom of thought imposed by dogma and tradition, and within certain broad limits he will think and reason much the same all the world over. On this point the case of Japan is decisive. True, modern ideas often shape ill and awry at first introduction to half-civilised peoples. But the causes of such misfits are usually not far to seek. One fundamental defect, one cause of the failures which lead so many ardent reformers to despair, lies, it would often seem, in the omission to educate and enlighten the feminine portion of a race. It does not suffice to open the doors of intellectual progress to the manhood of a nation; unless the women equally tread the path of progress and enlightenment, it follows of necessity that, owing to the latter's influence over childhood and the home, progress will be hindered, warped, and restricted.

By way of conclusion Prof. Ridgeway, after a playful reference to alcohol and flesh-food as necessities imposed by a cold or temperate climate, indulges in some diatribes on the folly of preserving the "lower" class, ending with the customary prophecy of race degeneration. Here is no place for a discussion either of temperance or of vegetarianism. But such temperate races as the American Indians and 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 Japanese peasantry, and the Highlanders and Irish (until recent years), from enjoying on a vegetarian diet remarkably robust health. This, however, by the way. The real question is, are the bulk of the lower classes truly inferior by heredity to those above them in the social scale? And with all deference to Mr. Galton and Prof. Karl Pearson, the existing evidence does not permit an answer in the affirmative, however flattering such might be to our vanity or soothing to our conscience. No doubt 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. But such evolution only reflects in a dim and distorted fashion natural selection as understood by modern biologists.

In a herd of deer or troop of monkeys 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 environments under which the progeny of different classes enter on the battle of life, between the surroundings of a 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 environment have received constant suggestions to lie, to steal, and to drink. The character of the youth of "gentle birth," on the other hand, comes to maturity in a sheltered atmosphere of culture and of intellectual refinement. Knowing as we do the enormous effect of "herd suggestion" on the human mind, who can ascribe with any certainty the failure of many of the lowly born to inherited defects? Who will assert that many a slum-born criminal and wastrel is by heredity inferior to numbers of the upper and middle classes who pass through life, if not with credit, at least without conspicuous blame? Only when there is approximate equality of opportunity, only when the present extravagant handicaps have passed away, and men—aye, and women too—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, their multiplication is injurious to our polity. 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.

THE CHEMISTRY OF THE MORPHINE GROUP OF ALKALOIDS

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Up to the beginning of the nineteenth century it was not known that basic substances could exist in plants, for it was not until the year 1803 that Derosne isolated from opium an impure crystalline substance (probably narcotine) which he named "*opium salt.*" He did not, however, carry his investigations very far, since he ascribed such basic properties as he had observed to an impurity arising from the alkali used in the purification of the "salt," and was therefore quite unaware of the important chemical properties which the new substance possessed. Three years later Sertürner[1] also obtained a crystalline product in a pure state from opium, and recognised that it exhibited undoubted basic properties.

The latter substance—*morphine*—was the first of that large and important class of compounds now known as alkaloids to be definitely recognised, and was assigned the formula $C_{17}H_{19}O_3N + H_2O$ by Laurent[2]; it is present in opium to the extent of 3 to 23 per cent. Owing to the labours of a large number of chemists, whose energies were directed to this special field of work, it soon became apparent that in opium morphine was associated with several other alkaloids, and of these, two bases, *codeine* and *thebaine*, now known to be closely related to morphine, were subsequently isolated. The former, discovered by Robiquet[3] in 1832, occurs in proportions varying from 0·3 to 2 per cent., and its formula was found by Gerhardt[4] to be $C_{18}H_{21}O_3N + H_2O$, whilst thebaine, of which opium contains 0·2 to 1 per cent., was isolated three years later by Thiboumery, and, being thought

[1] *Tromsdorff's Journ. d. Pharm.* **13**, i. 234.
[2] *Journ. de pharmacie* (3), **14**, 302.
[3] *Ann. chim. phys.* (2), **51**, 259.
[4] *Ibid.* (3), **7**, 253.

to be isomeric with morphine, was at first designated "*para-morphine*." Anderson,[1] however, and also Hesse,[2] by the analysis of a number of its salts, proved that its correct formula was $C_{19}H_{21}O_3N$.

These three alkaloids form what is called the morphine group. The first member of this group crystallises from alcohol in small colourless prisms and melts, with decomposition, at 230°; it is odourless, has a bitter taste, is slightly soluble in water, is lævo-rotatory, and is a powerful narcotic. The second, codeine, crystallises in colourless prisms or octahedra belonging to the rhombic system, and melts at 153°; it is only slightly soluble in water, is lævo-rotatory, possesses a bitter taste and also narcotic properties. Thebaine, the last member, crystallises in glistening tablets, and melts at 193°.

Morphine is so readily oxidised that it reduces gold and silver salts in the cold, whilst its solution in alkali is capable of absorbing oxygen from the air. It is also easily attacked by such oxidising agents as nitric acid, potassium permanganate, and potassium ferricyanide. In these reactions a non-poisonous substance soluble in alkali and variously designated *oxymorphine*, *oxydimorphine*, or *dehydromorphine* is formed. Hesse[3] has shown that this body is identical with the *pseudo-morphine* discovered by Pelletier and Thiboumery in opium. Some doubt appears to exist as to its correct formula, which, according to Polstorff[4] and Hesse,[5] may be either $C_{34}H_{36}O_6N_2$ or $(C_{17}H_{18}O_3N)_2$. Thus the action of mild oxidising agents upon morphine may be represented by the equation—

$$\underset{\text{Morphine}}{2C_{17}H_{19}O_3N} + O = \underset{\text{Pseudomorphine}}{(C_{17}H_{18}O_3N)_2} + H_2O$$

The stronger oxidation of morphine with nitric acid results in the formation, according to Chastaing,[6] of a tetrabasic *acid* of the formula $C_{20}H_9O_{18}N$, whereas when the action is allowed to become still more energetic, the molecule is broken down and *picric* acid obtained.

Dehydrating agents such as oxalic acid, sulphuric acid, hydrochloric acid, phosphoric acid, the alkalis, or a concentrated

[1] *Annalen*, **86**, 186.
[2] *Ibid.* **153**, 69.
[3] *Ibid.* **141**, 87.
[4] *Ber.* **19**, 1760.
[5] *Annalen*, **235**, 231.
[6] *Compt. rend.* **94**, 44.

solution of chloride of zinc act, according to Wright[1] and his co-workers, in a twofold manner upon morphine. Under certain conditions a heterogeneous mixture of condensation products is obtained—*trimorphine*, *tetramorphine*, etc.—whilst under other conditions a molecule of water is abstracted according to the equation:

$$\underset{\text{Morphine}}{C_{17}H_{19}O_3N} = \underset{\text{Apomorphine}}{C_{17}H_{17}O_2N} + H_2O$$

Apomorphine, the product of this reaction, is an amorphous base, slightly soluble in water, and very readily oxidisable. Its physiological properties are quite different from those of morphine; it is not a narcotic but a very powerful emetic.

The Constitution of Morphine

Turning now to the question of the structure of the morphine molecule, we find that the three oxygen atoms exercise different functions. One belongs to a phenolic hydroxyl group, confers upon morphine its acidic character, and has its hydrogen replaceable by a metal, an acid radicle, or an alkyl radicle. It is this hydrogen atom which has been substituted by a methyl group in codeine, which is therefore the methyl ether of morphine—a relationship discovered in 1869 by Matthieson and Wright,[2] and represented by the formulæ:

$$\underset{\text{Morphine}}{C_{17}H_{17}ON(OH)_2} \qquad \underset{\text{Codeine}}{C_{17}H_{17}ON(OH)(OCH_3)}$$

Morphine was converted into codeine in 1881 by Grimaux,[3] who treated it with methyl iodide in the presence of alkali.

$$\underset{\text{Morphine}}{C_{17}H_{17}ON(OH)_2} + CH_3I + KOH = \underset{\text{Codeine}}{C_{17}H_{17}ON(OH)(OCH_3)} + KI + H_2O$$

In view of this near relationship, it is obvious that the elucidation of the constitution of morphine establishes that of codeine also.

That both morphine and codeine contain also an alcoholic hydroxyl group is shown by the investigation of Matthieson and Wright,[4] who obtained, by the action of concentrated

[1] *Chemical News*, **19**, 289, 302; **27**, 317. *Ber.* **2**, 286, 336; **4**, 121; **5**, 336, 538, 1109; **6**, 828.

[2] *Proc. Roy. Soc., Ann. Spl.* **7**, 364.

[3] *Compt. rend.* **92**, 1140, 1228; **93**, 67, 217, 591.

[4] *Loc. cit.*

hydrochloric acid upon codeine, an amorphous chlorinated product which they named *chlorocodeide*.

$$C_{18}H_{21}O_3N + HCl = C_{18}H_{20}ClO_2N + H_2O$$

Codeine Chlorocodeide

They further state that if this compound is heated with water to 130°, codeine is regenerated; if, on the other hand, it is heated with hydrochloric acid to 150°, methyl chloride is split off and apomorphine remains.

$$C_{18}H_{20}ClO_2N = C_{17}H_{17}O_2N + CH_3Cl$$

Chlorocodeide Apomorphine

On examining these two equations, it is seen that when hydrochloric acid acts upon codeine at 150°, a CH_3 group and a molecule of water are split off, the final product being the same as that obtained by the action of dehydrating agents upon morphine.

The third oxygen atom in morphine and codeine is indifferent and, according to Vongerichten,[1] is doubly bound to carbon as in the ethers; it is sometimes referred to as the "bridge" oxygen atom and will be dealt with in detail later.

The nitrogen of morphine has been proved by Grimaux,[2] Hesse,[3] Vongerichten,[4] and Schrötter to be cyclic, and to be bound to three carbon atoms; it is therefore tertiary.

In order to make perfectly clear the *rationale* of the decomposition processes to which morphine has been subjected in the course of investigations made to determine its structure, it is necessary to say something about a reaction or series of reactions known as "exhaustive methylation." Shortly after the discovery of the quaternary ammonium bases, Hofmann[5] observed that when the hydroxides of these bodies are heated, water is split off with the simultaneous formation of a tertiary amine and a hydrocarbon—

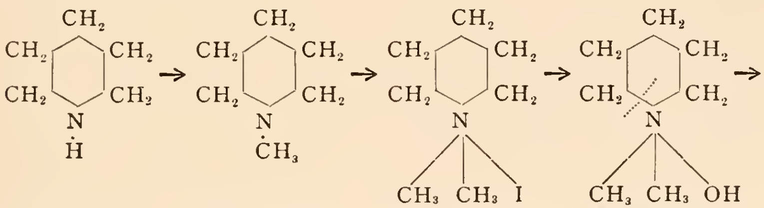
$$(C_2H_5)_3N{<}^{C_2H_5}_{OH} = (C_2H_5)_3N + C_2H_4 + H_2O$$

Tetraethylammonium hydroxide Triethylamine Ethylene

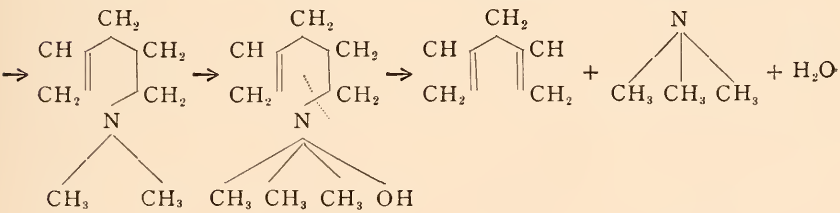
This reaction was applied by Hofmann[6] in 1881 to piperidine. This substance is a secondary base, and it is therefore

[1] *Annalen*, **210**, 105.
[2] *Compt. rend.* **93**, 591.
[3] *Annalen*, **222**, 223.
[4] *Ber.* **15**, 1480, 1279.
[5] *Annalen*, **78**, 263.
[6] *Ber.* **14**, 494, 659.

possible to replace the hydrogen atom united to the nitrogen, by a methyl group to form methylpiperidine. By treating the latter with methyl iodide, dimethylpiperidylammonium iodide is obtained, and this may be converted into the corresponding hydroxide by the action of moist silver oxide. When the dimethylpiperidylammonium hydroxide is subjected to dry distillation, the piperidine ring is opened and water splits off, leaving a substance named dimethylpiperidine. This is a tertiary base, and can be combined with methyl iodide to form dimethylpiperidylmethiodide, which again by treatment with moist silver oxide yields the corresponding hydroxide. If, now, the latter is strongly heated, it is decomposed into water, trimethylamine, and a hydrocarbon which Hofmann named *piperylene.* These reactions may be illustrated as follows:

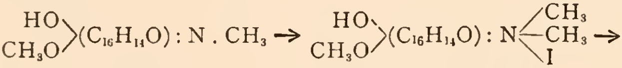


Piperidine — Methylpiperidine — Methylpiperidine methiodide — Methylpiperidine methohydroxide

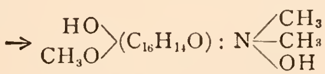


Dimethylpiperidine — Dimethylpiperidyl methohydroxide — Piperylene — Trimethylamine

By applying a perfectly analogous series of reactions to codeine, the ring which contains the nitrogen has been opened and the base thereby resolved into nitrogen-free products and amines, thus:



Codeine — Codeine methiodide



Codeine methohydroxide

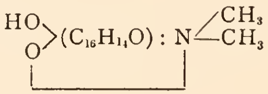
When either codeine methiodide or codeine methohydroxide is treated with hot caustic soda solution a tertiary base is obtained which Hesse has named *methylmorphimethine*:

$$\begin{matrix}HO\\CH_3O\end{matrix}\!\!>(C_{16}H_{13}O)\,.\,N\!<\!\begin{matrix}CH_3\\CH_3\end{matrix}$$

[Morphine is not quite analogous to codeine or piperidine in that the methohydroxide—

$$\begin{matrix}HO\\HO\end{matrix}\!\!>(C_{16}H_{14}O) : N\!\begin{matrix}\diagup CH_3\\ - CH_3\\ \diagdown OH\end{matrix}$$

is very stable towards alkalis, and does not yield the expected *morphimethine* when heated with strong soda. This is due, Vongerichten[1] suggests, to the probable formation of a comparatively stable *phenolbetaïne*.



.]

By the action of hydrochloric acid or acetic anhydride, methylmorphimethine can be decomposed, as Knorr[2] has shown, into hydroxyethyldimethylamine and a nitrogen-free body which is a derivative of phenanthrene and is identical with Vongerichten's *methylmorphol*.

$$\underset{\text{Methylmorphimethine}}{\begin{matrix}HO\\CH_3O\end{matrix}\!\!>(C_{16}H_{13}O)\,.\,N\!<\!\begin{matrix}CH_3\\CH_3\end{matrix}} = \underset{\text{Methylmorphol}}{\begin{matrix}HO\\CH_3O\end{matrix}\!\!>C_{14}H_8} + \underset{\text{Hydroxyethyldimethylamine}}{HO\,.\,CH_2\,.\,CH_2\,.\,N\!<\!\begin{matrix}CH_3\\CH_3\end{matrix}}$$

When acetic anhydride is used in the above reaction, there is formed a considerable proportion of a body isomeric with methylmorphimethine, *β-methylmorphimethine*, which by exhaustive methylation yields the methyl ether of *morphenol*, a non-nitrogenous substance differing from morphol by two atoms of hydrogen.

[1] *Ber.* **30**, 354.
[2] *Ibid.* **22**, 181, 1113 ; **27**, 1114.

The Nitrogen-free Products of Exhaustive Methylation

MORPHOL is obtained in the form of the acetyl derivative of its monomethyl ether by heating methylmorphimethine with acetic anhydride, as represented by the equation:

$$\left.\begin{matrix}CH_3O\\HO\end{matrix}\right\rangle(C_{14}H_9O)C_2H_4\,.\,N(CH_3)_2 + 2(CH_3CO)_2O =$$

Methylmorphimethine

$$\left.\begin{matrix}CH_3O\\CH_3\,.\,CO\,.\,O\end{matrix}\right\rangle C_{14}H_8 + CH_3\,.\,CO\,.\,O\,.\,C_2H_4\,.\,N(CH_3)_2 + 2CH_3COOH$$

Acetylmethylmorphol Acetoxyethyldimethylamine

When acetylmethylmorphol is oxidised by means of a solution of chromic acid in glacial acetic acid, it is converted into *acetylmethylmorpholquinone* which is readily shown to be a derivative of phenanthraquinone:

$$\left.\begin{matrix}CH_3O\\CH_3CO\,.\,O\end{matrix}\right\rangle C_{14}H_8 \rightarrow \left.\begin{matrix}CH_3O\\CH_3CO\,.\,O\end{matrix}\right\rangle C_{14}H_6O_2$$

Acetylmethylmorphol Acetylmethylmorpholquinone

In view of the fact that acetylmethylmorpholquinone is obtained by this reaction, *neither of the hydroxyl groups of morphol can be attached to the "bridge" carbon atoms of the phenanthrene complex*, for if such were the case they would disappear in the process of oxidation. Hence they must either be distributed between the two remaining rings or be *both attached to one of them.* The latter assumption has proved to be correct since acetylmethylmorpholquinone, as also morpholquinone, yields phthalic acid on further oxidation. This conclusion is supported by the fact that Barth and Weidel[1] obtained *protocatechuic* acid by fusing morphine with caustic alkali.

MORPHENOL may be obtained as a methyl ether by heating α-methylmorphimethine methohydroxide, but a better yield is secured if, as mentioned above, the corresponding β-derivative is employed. According to Vongerichten,[2] the decomposition takes place as follows:

$$\left.\begin{matrix}CH_3O\\HO\end{matrix}\right\rangle(C_{14}H_9O)C_2H_4\,.\,N(CH_3)_3OH = (CH_3O)C_{14}H_7O + 2H_2O + C_2H_4 + N(CH_3)_3$$

β-Methylmorphimethine methohydroxide — Morphenol methylether — Ethylene

[1] *Wien. Monatsh.* **4**, 700. [2] *Ber.* **29**, 67.

Morphenol methyl ether yields a monobromo derivative which, on treatment with hydriodic acid, is converted into morphenol:

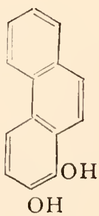
$$\underset{\text{Bromomorphenol}}{(CH_3O)C_{14}H_6BrO} \rightarrow \underset{\text{Morphenol}}{(HO)C_{14}H_7O}$$

Morphenol must also be a derivative of phenanthrene since it is converted into that hydrocarbon by heating with zinc dust; moreover, by the use of suitable reducing agents, two hydrogen atoms only can be added with the formation of morphol:

$$(HO)C_{14}H_7O + H_2 = \underset{\text{Morphol}}{\begin{matrix} HO \\ HO \end{matrix}\!\!>C_{14}H_8}$$

Morphenol is therefore closely related to morphol, and a determination of the position of the hydroxyl group in the latter must throw light on the constitution of the former.

Since protocatechuic acid can be obtained by fusing morphine with caustic alkali, it seems probable that the two hydroxyl groups in morphol are in the *ortho* position to one another, and that one of them is derived from the "ether-like" oxygen atom of morphine. A similar conclusion as to the relative position of the two groups results from a consideration of the behaviour of morphol as a dye-stuff. Vongerichten[1] showed that morphol possesses the property of dyeing mordanted fibres in contradistinction to its monomethyl ether; as Liebermann and Kostanecki[2] had observed that only those hydroxy derivatives of anthraquinone which have two hydroxyl groups in the *ortho* position possess this property, the constitution represented by the following formula is on these grounds assigned to morphol:

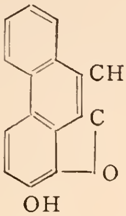


[1] *Ber.* **32**, 1522.

[2] *Annalen*, **240**, 245.

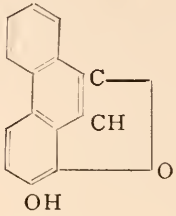
On this assumption, morphenol may be represented as follows:

(1)

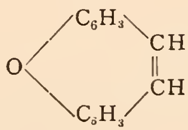


or

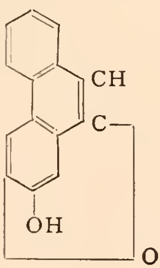
(2)



since an alternative diphenyleneoxide formula—



is not considered to be compatible with the ease with which morphol can be reduced to phenanthrene, nor yet with the difficulty experienced in oxidising it to a quinone. Furthermore, the formula in which the ring-oxygen of morphenol serves as a connecting link between one of the "bridge" carbon atoms and the carbon atom in the para position with respect to that attached to the "bridge" carbon atom

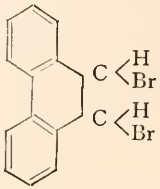


must be dismissed as impossible, for, with such a constitution, morphenol would stand in near relationship to the aromatic oxides described by Zincke and Auwers,[1] which differ from it in being highly reactive.

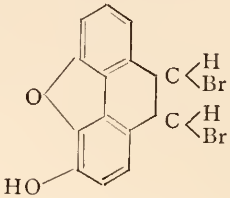
Vongerichten[2] has recently shown that the *ring-oxygen of morphenol cannot be attached to either of the "bridge" carbon atoms,*

[1] *Annalen*, **301**, 203; **303**, 76. [2] *Ber.* **33**, 1851.

by preparing the monobromo additive compounds of morphenol-methylether and acetylmorphenol which, by the further action of bromine, are converted into the dibromo derivatives corresponding to dibromophenanthrene—*i.e.* the bromine atoms must be attached to the "bridge" carbon atoms:

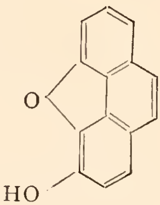


Dibromophenanthrene

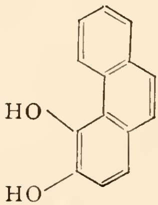


Dibromomorphenol

The validity of this conclusion has been confirmed in the following manner by the same investigator.[1] By first converting morphenol into an acetyl compound, and then oxidising by chromic acid in glacial acetic acid, he has succeeded in preparing a quinone which yields a *phenazine* on treatment with *orthodiamines*. The body so obtained has properties similar to the phenazine of morpholquinone, and is therefore to be regarded as a derivative of phenanthrazine. Hence it follows that both the "bridge" carbon atoms of morphenol have been oxidised to form an ortho-diketone, while the oxygen ring has remained unopened, and therefore the only possible formulæ for morphenol and morphol are:



Morphenol

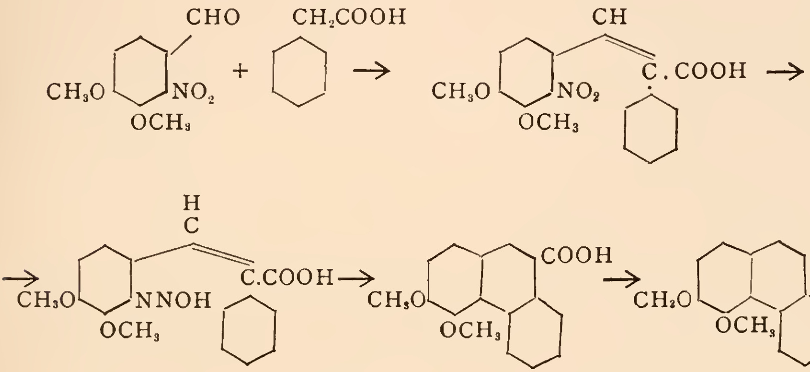


Morphol

This was confirmed by Pschorr and Sumuleanu,[2] who succeeded in synthesising dimethylmorphol. They obtained *a*-phenyl-2-nitro-3, 4-dimethoxycinnamic acid by the condensation of *vic.*o-nitrovanillin-methylether with sodium phenylacetate by means of Perkin's reaction. This was converted into the

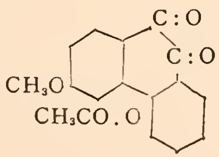
[1] *Ber.* **33**, 352. [2] *Ibid.* **33**, 1811.

corresponding amino-acid and diazotised. The diazo-body was then treated with dilute sulphuric acid, which caused nitrogen and water to split off, forming 3, 4-dimethoxy-phenanthrene-9-carboxylic acid, and by distillation the latter was resolved into carbon dioxide and dimethoxyphenanthrene:



Under the well-known conditions of this reaction, it is certain that the methoxyl groups must be in positions 3 and 4 respectively, and this synthetic body is identical with the dimethylmorphol prepared by Vongerichten[1] from methylmorphol.

Following on similar lines, Pschorr and Vogtherr[2] have prepared acetylmethylmorpholquinone by condensing *vic*.o-nitro*iso*vanillin with phenylacetic acid. The resulting product was then converted into the diazo-compound as before, and finally into 3-methoxy-4-hydroxyphenanthrene-9-carboxylic acid, which was acetylated and afterwards oxidised by means of chromic acid, whereby 3-methoxy-4-acetoxyphenanthraquinone was obtained:



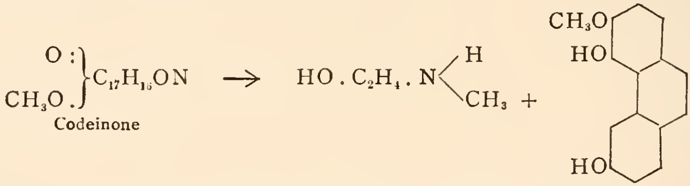
This proved to be identical with the acetylmethylmorpholquinone prepared by Vongerichten[3] from morphine.

[1] *Ber.* **33**, 1824. [2] *Ibid.* **35**, 4412. [3] *Ibid.* **31**, 52.

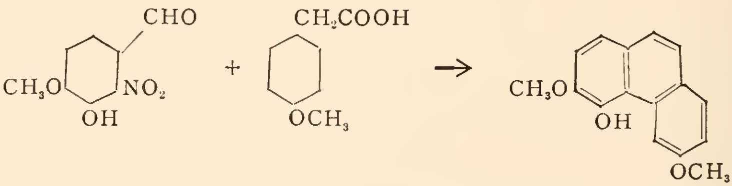
The Positions of the Oxygen Atoms of Morphine

To gain a deeper insight into the constitution of morphine, it was very important to determine *which of the two hydroxyl groups of morphol corresponds to the phenolic hydroxyl of morphine*, or, in other words, the methoxyl group of codeine. To this end, Pschorr and Sumuleanu prepared 3-acetoxy-4-methoxyphenanthrene by a reaction analogous to the above, and from this they obtained 3-hydroxy-4-methoxyphenanthrene, which they found to be quite different from Vongerichten's methylmorphol. Hence it follows that the non-nitrogeneous decomposition product obtained from *a*-methylmorphimethine can only be 3-methoxy-4-hydroxyphenanthrene, and that consequently *the phenolic hydroxyl of morphine occupies position* 3 *in the phenanthrene complex.*

That the *alcoholic hydroxyl of morphine occupies position* 6 in the phenanthrene complex is proved by the following considerations. A hydroxymethylmorphol was obtained by the action of acetic anhydride on codeinone :[1]



This body has been synthesised in a manner entirely analogous to that described for dimethylmorphol. Instead of *vic.*o-nitrovanillin-methylether and sodium phenylacetate, Pschorr started with *vic.*o-nitrovanillin and the sodium salt of p-methoxyphenylacetic acid—



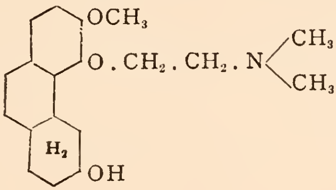
whereby he obtained 3, 6-dimethoxy-4-hydroxyphenanthrene, and ultimately arrived at 3-methoxy-4, 6-dihydroxyphenanthrene, the identity of which with the hydroxymethylmorphol obtained from codeinone is beyond question.

[1] *Ber.* **36**, 3074.

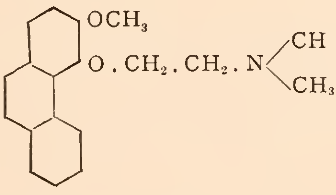
The hydroxyl group in position 6 undoubtedly corresponds to the alcoholic hydroxyl group of morphine, since the other is in the *ortho* position to the methoxyl group occupying position 3, and cannot therefore be alcoholic. Moreover, as will be seen later, the hydroxyl group in position 6 forms part of a ring which in morphine is in a reduced condition, and would therefore be expected to exhibit alcoholic properties.

By a process of elimination, the hydroxyl group in position 4 must correspond to the "indifferent" oxygen of morphine and morphenol.

The nature of the "indifferent" oxygen atom and the point of attachment of the nitrogen-containing side chain to the phenanthrene complex are closely bound up with one another. At one time Knorr thought that the oxygen formed a bridge between the two groups, especially as morphine in many of its properties closely resembled a base morpholine (*q.v.*) which contains the grouping $\overset{O}{\overbrace{H_2C \quad CH_2}}$. On this assumption methylmorphimethine would be represented by the formula



a structure of the same type as that of the phenanthrol ethers,[1] prepared by the interaction of the sodium salt of methyl morphol and chlorethyldimethylamine,



Knorr has compared the behaviour of these two bodies when treated with a solution of sodium ethoxide, and finds that whilst

[1] *Ber.* 38, 3172.

under these conditions methylmorphimethine is decomposed with the formation of dimethylaminoethylether, the phenanthrol ether proved itself completely stable even at a temperature of 150°. This characteristic difference in behaviour affords a proof that the binding of the complex $C_2H_4 . N(CH_3)_2$ in methylmorphimethine cannot be similar to that of the hydroxyethyldimethylamine in the synthetic body, and therefore *cannot be attached to the phenanthrene complex through the medium of the "indifferent" oxygen.*

This conclusion is in agreement with the joint observation of Knorr and Pschorr[1] that *thebainone*, when treated with acetic anhydride, decomposes with the formation of hydroxyethyldimethylamine in a manner similar to methylmorphimethine, notwithstanding the fact that this ketone contains no "indifferent" oxygen.

Thus, on the above grounds, the assumption of the presence of an oxazine ring in morphine is untenable, and there is no doubt that the "indifferent" oxygen atom must be regarded as a member of a furane ring, as in the case of methylmorphenol, the decomposition product of β-methylmorphimethine, and as *forming a bridge between positions* 4 *and* 5 *of the phenanthrene complex. The complex* $C_2H_4 . N . CH_3$ *must therefore be united by means of a carbon binding to the phenanthrene ring both in methylmorphimethine and morphine.*

The Nitrogen Complex

In order to obtain further information as to the manner in which the basic portion of the decomposition products of methylmorphimethine, codeinemethiodide, and thebainemethiodide (see p. 297) are attached to the phenanthrene complex, Knorr[2] has recently made a special study of these bodies. When α-methylmorphimethine is decomposed with hydrochloric acid gas, it yields methylmorphol and chloroethyldimethylamine (see p. 284). As a matter of fact, the latter base is not actually obtained, but, instead, a mixture of tetramethylethylenediamine, $(CH_3)_2N . CH_2 . CH_2 . N . (CH_3)_2$, and hydroxyethyldimethylamine, $HO . CH_2 . CH_2 . N(CH_3)_2$. Knorr[3] has, however, found that a mixture of the same bases is obtained when the salts of

[1] *Ber.* **38**, 3172. [2] *Ibid.* **37**, 3494. [3] *Ibid.* 3507.

chlorethyldimethylamine are heated with an excess of caustic soda.

The decomposition of *α*- or *β*-methylmorphimethine with sodium ethoxide gives, in addition to methylmorphol, the hitherto unknown dimethylamino ether, $(CH_3)_2N . CH_2 . CH_2 . O . CH_2 . CH_3$, which, according to Knorr, may be obtained synthetically by the interaction of dimethylamine and iodo-ether,

$$\begin{matrix} CH_3 \\ CH_3 \end{matrix}\Big\rangle NH + ICH_2 . CH_2 . O . C_2H_5 = \begin{matrix} CH_3 \\ CH_3 \end{matrix}\Big\rangle \underset{H \;\; I}{N} . CH_2 . CH_2 . O . C_2H_5$$

or by heating chlorethyldimethylamine with sodium ethoxide,

$$\begin{matrix} CH_3 \\ CH_3 \end{matrix}\Big\rangle N . CH_2 . CH_2Cl + NaOC_2H_5 = \begin{matrix} CH_3 \\ CH_3 \end{matrix}\Big\rangle N . CH_2 . CH_2 . O . C_2H_5 + NaCl$$

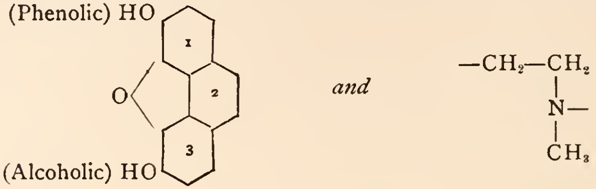
When codeinonemethiodide or thebainemethiodide is heated with alcohol at 160°—165°, they yield respectively a mixture of 3-methoxy-4, 6-dihydroxyphenanthrene and dimethylaminoethyl ether in the first case and thebaol and dimethylaminoethyl ether in the second. These reactions suggest that the methiodides of codeinone and thebaine possess a very labile linkage, from which the three-membered complex, C.C.N, may be expelled with great ease. It is of great interest to note that this three-membered complex is obtained in the form of dimethylaminoethyl ether in the two reactions described above, for it is clear that this substance cannot be the primary decomposition product. According to Knorr, the only reasonable assumption is that the three-membered complex is first liberated in the form of an unsaturated compound—probably a vinyldimethylamine, and that this, in the nascent state, combines with alcohol to form the ether base:

$$(CH_3)_2N . CH : CH_2 + C_2H_5OH = (CH_3)_2N . CH_2 . CH_2 . O . C_2H_5$$

In a similar manner, the formation of the acetic ester of a hydramine in the case of an acetic anhydride decomposition may be accounted for by the combination of acetic acid and the originally formed vinyl base:

$$(CH_3)_2N . CH : CH_2 + CH_3COOH = (CH_3)_2N . CH_2 . CH_2 . O . COCH_3$$

The foregoing considerations show that the morphine molecule may be regarded as built up from the complexes:



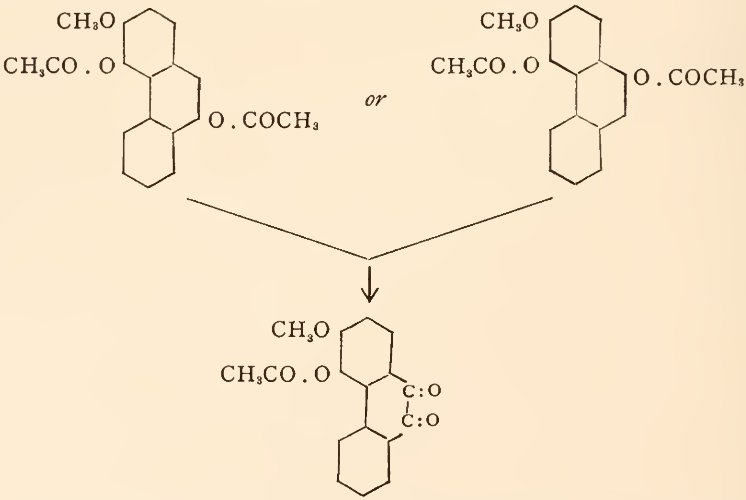
which together will be seen to contain four hydrogen atoms less than morphine.

It remains to be shown—

(*a*) How these additional hydrogen atoms are distributed;

(*b*) The manner in which the two complexes are united.

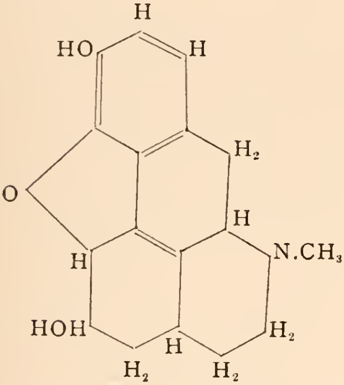
Inasmuch as nucleus 3 contains a hydroxyl group possessing alcoholic properties, it would appear to be of the nature of a reduced or partially reduced benzenoid ring; and the work of Knorr and Schneider,[1] and of Pschorr and Kuhtz,[2] shows that nucleus 2 must be in a similar condition. These investigators found that methoxy*di*acetoxyphenanthrene, which is obtained from hydroxycodeine in a manner similar to that whereby methoxyacetoxyphenanthrene is derived from codeine, loses its additional acetyl group on oxidation, and is converted into methoxyacetoxyphenanthraquinone (cf. p. 285).



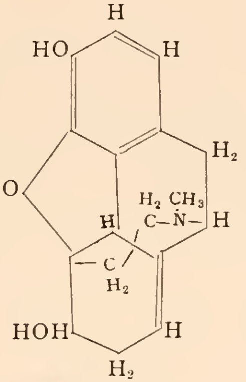
This acetyl group must, therefore, be attached to one of the

[1] *Ber.* 39, 1414. [2] *Ibid.* 39, 3137.

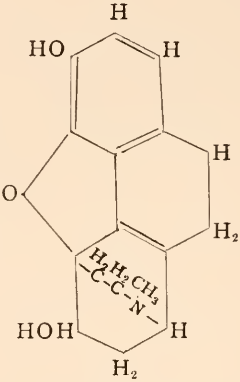
"bridge" carbon atoms of the phenanthrene complex, and corresponds to a hydroxyl group, which in both hydroxycodeine and hydroxymethylmorphimethine possesses alcoholic properties. Hence the "bridge" nucleus must be reduced—an inference which is supported by the fact that hydroxycodeine is formed from codeine by oxidation with chromic acid (see p. 300). The way in which the hydrogen atoms are distributed between the two reduced nuclei and the position of the double bond depend upon the points of attachment of the nitrogen complex, regarding which there is some difference of opinion. Thus Pschorr suggests that it is joined to positions 8 and 9; Knorr to positions 5 and 9; while Freund considers that it forms a bridge between positions 5 and 8. These views have found expression in the following formulæ:



Morphine (Pschorr)

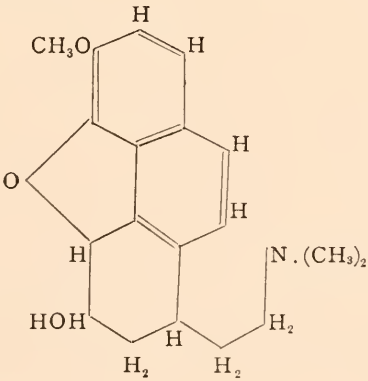


Morphine (Knorr)



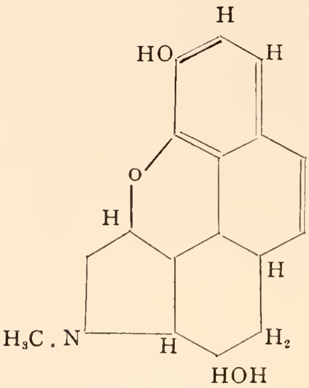
Morphine (Freund)

Pschorr's formula necessitates the following expression for methylmorphimethine:

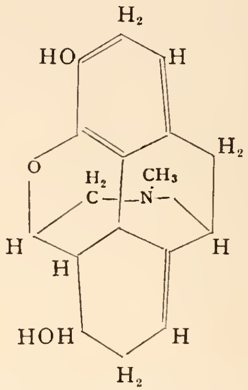


and is incompatible with the existence of a hydroxymethyl-morphimethine in which the hydroxyl, possessing alcoholic functions, is attached to either position 9 or 10.

Quite recently, Bucherer has proposed an entirely novel formula which has not yet been thoroughly discussed; Knorr thinks that, with certain modifications, this may be worth considering:



Morphine (Bucherer)



Morphine (Bucherer, modified by Knorr)

Thebaine

Attention must now be directed to thebaine, the third member of the morphine group. It has been shown that morphine and codeine stand in close relationship to one another, codeine being the monomethyl ether of morphine, and it will now be proved that there is a connection betwen thebaine and codeine, albeit not quite so simple as that between the latter and morphine.

An inspection of the empirical formulæ of the three members of the group shows that each contains three atoms of oxygen and one atom of nitrogen:

$C_{17}H_{19}O_3N$	$C_{18}H_{21}O_3N$	$C_{19}H_{21}O_3N$
Morphine	Codeine	Thebaine

By treatment with concentrated hydriodic acid according to Zeisel's method, Roser and Howard have proved that thebaine contains two methoxyl groups, and have also shown it to be a tertiary base since it combines with one molecule of methyl iodide only. The above formulæ may therefore be amplified as follows:

$$\left.\begin{matrix}HO\\HO\end{matrix}\right>(C_{16}H_{14}O):N.CH_3;\quad \left.\begin{matrix}CH_3O\\HO\end{matrix}\right>(C_{16}H_{14}O):N.CH_3;\quad \left.\begin{matrix}CH_3O\\CH_3O\end{matrix}\right>(C_{16}H_{12}O):N.CH_3$$

Morphine — Codeine — Thebaine

The existing knowledge of the thebaine molecule is largely due to the work of Freund,[1] who has subjected the base to a series of decompositions similar to those carried out with morphine and codeine. By heating with acetic anhydride, thebaine is resolved into methylhydroxyethylamine and the acetyl derivative of a nitrogen-free compound which, by analogy with morphol, has been named *thebaol*:

$$\underset{\text{Thebaine}}{\left.\begin{matrix}CH_3O\\CH_3O\end{matrix}\right>(C_{16}H_{12}O):N.CH_3}+2(CH_3CO)_2O=\underset{\text{Acetylthebaol}}{\left.\begin{matrix}CH_3O\\CH_3O\end{matrix}\right>(C_{14}H_7)O.COCH_3}+CH_3COOH+$$

$$+\underset{\text{Diacetylmethylhydroxyethylamine}}{\left.\begin{matrix}CH_3CO\\CH_3\end{matrix}\right>N.CH_2.CH_2.O.COCH_3}$$

Similarly, by treating thebaine methiodide with acetic anhydride in the presence of silver acetate, acetylthebaol and the acetyl derivative of dimethylhydroxyethylamine are obtained:

$$\underset{\text{Thebaine methiodide}}{\left.\begin{matrix}CH_3O\\CH_3O\end{matrix}\right>(C_{16}H_{12}O):N(CH_3)_2I}+(CH_3CO)_2O=\underset{\text{Acetylthebaol}}{\left.\begin{matrix}CH_3O\\CH_3O\end{matrix}\right>(C_{14}H_7)O.COCH_3}+HI+$$

$$+\underset{\text{Acetyldimethylhydroxyethylamine}}{\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right>N.CH_2.CH_2.O.COCH_3}$$

These reactions are entirely analogous to the decomposition of methylmorphimethine into acetylmethylmorphol and dimethylhydroxyethylamine.

Constitution of Thebaol

By distillation with zinc dust, thebaol is converted into phenanthrene, and, as it can readily be demonstrated that thebaol contains two methoxy groups and one phenolic

[1] *Ber.* **30**, 1364.

hydroxyl group, this body must be regarded as a tri-substituted phenanthrene of the formula:

$$(CH_3O)_2C_{14}H_7OH$$

When oxidised with a mixture of glacial acetic acid and chromic acid, acetylthebaol is converted into a compound which possesses the formula $C_{18}H_{14}O_6$ and exhibits all the properties of a quinone. This body has been designated *acetylthebaolquinone* and yields thebaolquinone on saponification (cf. p. 285):

$$\begin{matrix} CH_3O \\ CH_3O \end{matrix}\Big\rangle(C_{14}H_7)O\,.\,OC\,.\,CH_3 \rightarrow \begin{matrix} CH_3O \\ CH_3O \end{matrix}\Big\rangle(C_{14}H_5O_2)O\,.\,OC\,.\,CH_3 \rightarrow$$

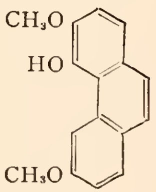
Acetylthebaol — Acetylthebaolquinone

$$\rightarrow \begin{matrix} CH_3O \\ CH_3O \end{matrix}\Big\rangle(C_{14}H_5O_2)\,.\,OH$$

Thebaolquinone

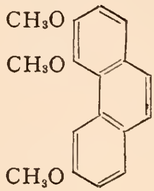
Thebaolquinone exhibits the characteristic reaction of an ortho-diketone in that it combines with ortho-diamines, in view of which fact, as also on account of its other properties, it must be a tri-substituted phenanthraquinone. *None of the substituting radicles in thebaol can therefore be attached to the "bridge" atoms* of the phenanthrene complex, for any groups so attached would necessarily have disappeared in the process of oxidation. An insight into the position of the methoxyl groups in thebaolquinone is given by the oxidation of that body with permanganate. Under this treatment an acid of the formula $C_9H_8O_5$ and identical with o-methoxyphthalic acid has been obtained, from which it follows that the *two methoxyl groups in thebaolquinone cannot both be bound to the same ring.*

Pschorr, Seydel, and Stöhrer[1] have recently synthesised thebaol and have proved that that body is a 3, 6-dimethoxy-4-hydroxyphenanthrene of the formula:



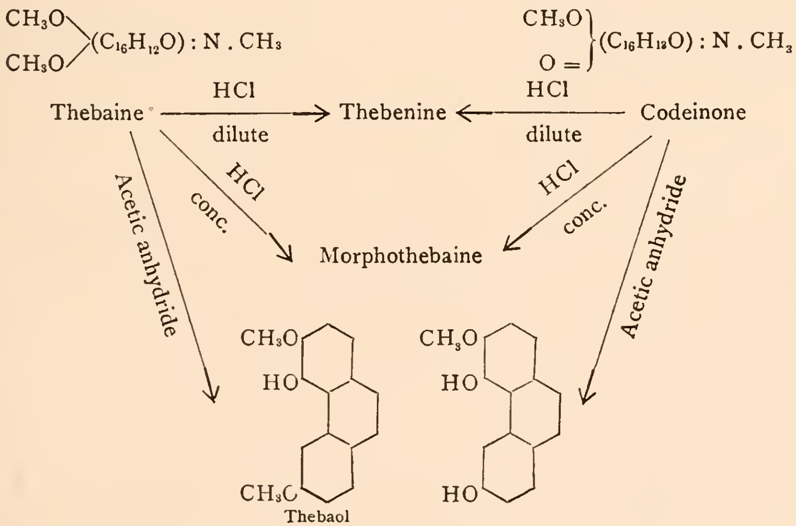
[1] *Ber.* **35**, 4400.

This synthesis was conducted on the same lines as that of morphol (cf. p. 289), the initial materials being *vic.*o-nitro-vanillin-methylether and p-methoxyphenylacetic acid, and the final product thebaol-methylether:

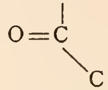


Thebaol itself was synthesised in a similar manner, *vic.*o-nitrovanillin being used instead of the corresponding methyl ether (see p. 290).

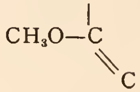
When thebaine hydrochloride is heated for some time with dilute hydrochloric acid, a secondary base *thebenine* is formed, whilst if concentrated acid be used a tertiary base *morphothebaine* results. Under precisely similar conditions, codeinone can be made to yield the *same products*, and there must therefore be a very close relationship between the two bodies — a conclusion which is further substantiated by the fact that on treatment with acetic anhydride, the nitrogen-free product from thebaine—thebaol—is the monomethyl ether of that resulting from the action of the same reagent on codeinone:



These facts show that the group



in codeinone corresponds to the group

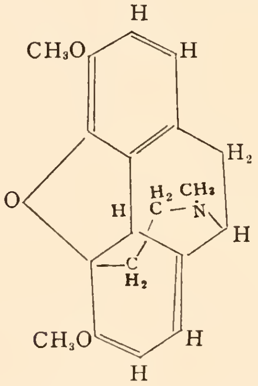


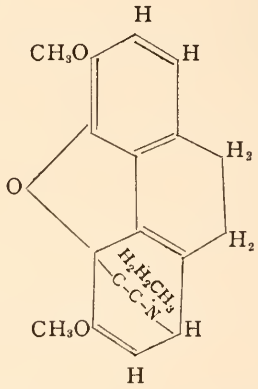
in thebaine. Codeinone is a ketone derived from codeine by the oxidation of its alcoholic hydroxyl group, thus:

$$\left.\begin{matrix} CH_3O \\ HO \end{matrix}\right\}(C_{16}H_{14}O):N.CH_3 + O = \left.\begin{matrix} CH_3O \\ O: \end{matrix}\right\}(C_{16}H_{13}O):N.CH_3 + H_2O$$

Codeine Codeinone

It follows that *thebaine is the methyl ether of the enolic form of codeinone* and, by analogy with morphine, probably possesses one of the two following formulæ:





Thebenine is a secondary base, and when subjected to exhaustive methylation yields *trimethylamine* and a nitrogen-free substance named *thebenol*; the latter is very stable towards alkali, for even on fusion only the methyl group is split off and another substance, *nor-thebenol*,[1] obtained:

$$\begin{matrix} CH_3O \\ CH_3O \end{matrix}\!\!>(C_{16}H_{12}O):N.CH_3 \rightarrow \begin{matrix} CH_3O \\ HO \end{matrix}\!\!>(C_{16}H_{11}O).NHCH_3 \rightarrow$$

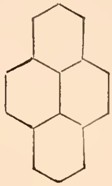
Thebaine Thebenine

$$\rightarrow \begin{matrix} CH_3O \\ HO \end{matrix}\!\!>(C_{16}H_{10}O) \rightarrow \begin{matrix} HO \\ HO \end{matrix}\!\!>(C_{16}H_{10}O)$$

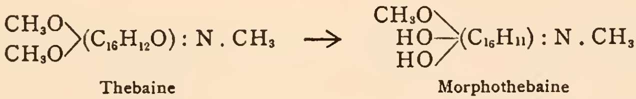
Thebenol Non-thebenol

[1] *Ber.* **30**, 1382.

When energetically reduced,[1] thebenol yields the hydrocarbon *pyrene* which, according to Bamberger and Philip,[2] possesses the configuration:



In the formation of morphothebaine, the oxygen ring of thebaine is opened, thus giving rise to a hydroxyl group:



The second hydroxyl is, of course, produced by the decomposition of a methoxyl group.

Apomorphine

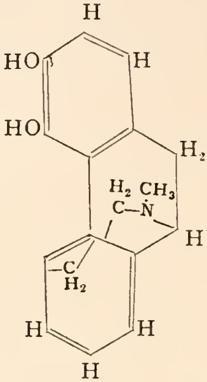
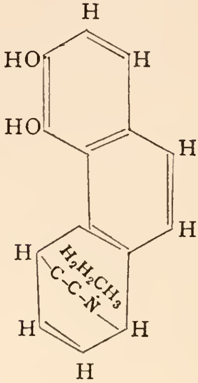
As previously mentioned (p. 281), apomorphine is produced by the action of dehydrating agents upon morphine:

$$C_{17}H_{19}O_3N - H_2O = C_{17}H_{17}O_2N$$

Dankwortt[3] succeeded in preparing a *mono*acetyl derivative of apomorphine, whence he concluded that only one of its two oxygen atoms was present as a hydroxyl group, while the other was combined in the same manner as the "ether-like" oxygen in morphine. Recently, however, Pschorr, Jaeckel, and Fecht[4] have proved that both of the oxygen atoms in apomorphine are present as hydroxyl groups. Dimethylapomorphine combines with only one molecule of methyl iodide, so that the nitrogen in apomorphine is tertiary. One of the

[1] *Ber.* **30**, 1383.
[2] *Annalen*, **240**, 147.
[3] *Annalen*, **228**, 572.
[4] *Ber.* **35**, 4377.

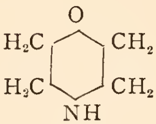
two following formulæ may therefore be regarded as representing apomorphine:

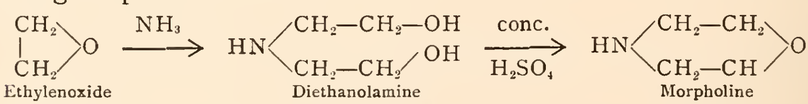
Knorr and Raabe[1] have recently shown that ψ-apocodeine (obtained by heating a mixture of dry codeine and anhydrous oxalic acid at 150°)[2] is identical with the 3-methylether of apomorphine prepared by Pschorr, Jaeckel, and Fecht; and thus apomorphine and ψ-apocodeine stand in the same relationship to one another as morphine and codeine.

Morpholine and its Derivatives

The base *morpholine*



is of historical interest in that it was at one time regarded by Knorr as the parent substance of morphine, and in order to test the accuracy of this view he synthetically prepared the base and several of its derivatives and elaborated a general method for their preparation. This consists in splitting off water from diethanolamines (prepared by the condensation of ethylene oxide with amines), by heating with strong sulphuric acid[3]:

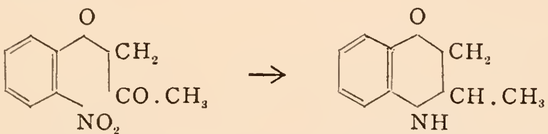


This method can be varied in two directions: ammonia may may be replaced by various mono-substituted ammonias of

[1] *Ber.* **41**, 3050. [2] *Ibid.* **40**, 3355. [3] *Ibid.* **30**, 909; **31**, 1070, 1969.

the type $R.NH_2$ whereby N-substituted morpholine bases are obtained, or compounds of the type $\begin{matrix}R.CH\\ |\\ R.CH\end{matrix}\!\!>O$, of an aliphatic or alicyclic nature, may be employed. By means of this latter modification, substances which closely resemble morphine in properties are obtained.

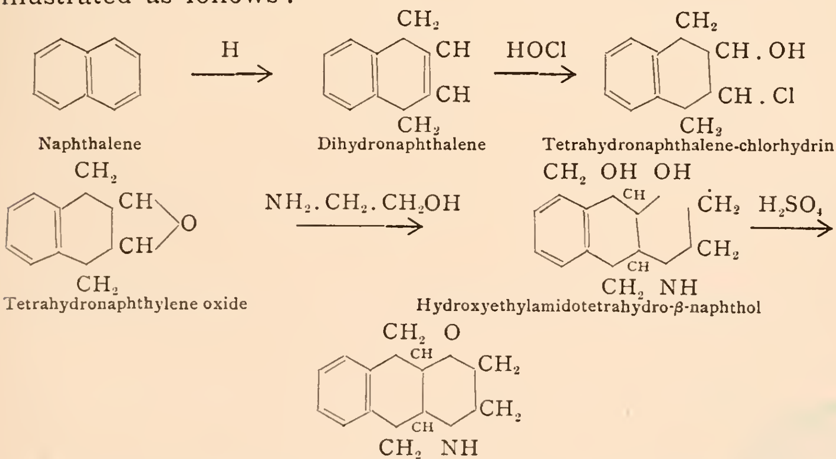
R. Störmer[2] has worked out a second method by which a morpholine derivative can be very quickly obtained—*i.e.* the reduction of o-nitrophenacetol in boiling alcoholic solution with tin and hydrochloric acid, whereby a ring is formed and a methyl derivative of phenmorpholine, together with other products, obtained:



o-Nitrophenacetol 2-Methylphenmorpholine

In a precisely similar manner, methylnaphthomorpholine may be obtained from *a*-nitro-*β*-naphthacetol.

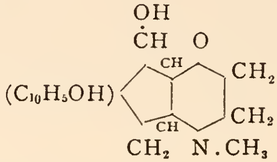
One of the most interesting morpholine derivatives prepared by Knorr[1] is *naphthalanemorpholine*, which was obtained by the combination of tetrahydronaphthylene oxide—an alicyclic analogue of ethylene oxide—with hydroxyethylamine.[2] Starting from naphthalene, the complete synthesis may be illustrated as follows:



Naphthalane-morpholine

[1] *Annalen*, **307**, 171; *Ber.* **32**, 743. [2] *Annalen*, **288**, 89.

Now naphthalanemorpholine, when subjected to exhaustive methylation, gives as ultimate products naphthalene and hydroxyethyldimethylamine—morphine, it will be remembered, gave under similar treatment a derivative of dihydroxyphenanthrene and also hydroxyethyldimethylamine—and it was on these grounds and also the close similarity in properties shown by naphthalemorpholine and morphine that Knorr originally suggested



as the constitutional formula for morphine—a formula which was disproved by his own subsequent work.

Isomerides of Morphine and Codeine

It has been shown by Vongerichten[1] that by the interaction of codeine and phosphorus pentachloride, the alcoholic hydroxyl group of the former is replaced by chlorine and a crystalline base, *chlorocodeide* $C_{18}H_{20}O_2NCl$, obtained. Recently, Schryver and Lees[2] have made an extended study of this and similar compounds and have prepared *chloromorphide* $C_{17}H_{18}O_2NCl$, *bromomorphide* $C_{17}H_{18}O_2NBr$, and *bromocodeide* $C_{18}H_{20}ONCl$ by the interaction of morphine and codeine with phosphorus halides; the same two investigators have also demonstrated that when these halogen derivatives are hydrolysed by water, morphine and codeine are not simply regenerated, but that in each case at least two bases isomeric with, but not identical with, morphine and codeine respectively are produced.

Vongerichten[3] (and independently Knorr and Horlein[4]) has proved that chlorocodeide on hydrolysis yields an isomeride of codeine which is identical with the so-called "*pseudo*codeine" first obtained by Merck[5] by the interaction of dilute sulphuric acid and codeine. Lees[6] has also shown that when chloromorphide is methylated, a chlorocodeide identical with the

[1] *Annalen*, **210**, 107.
[2] *Trans.* **77**, 1024; **79**, 563.
[3] *Ber.* **36**, 159.
[4] *Ber.* **39**, 4409.
[5] *Arch. Pharm.* **229**, 161.
[6] *Trans.* **92**, 1408.

product of the interaction of phosphorus pentachloride and codeine is obtained, and that when bromomorphide is methylated bromocodeide is produced; thus chlorocodeide and bromocodeide are simply the methyl ethers of the corresponding morphides. On hydrolysis (*loc. cit.*), chloromorphide yields *β-iso*morphine and a second isomeride of morphine which, when methylated, is converted into the above-mentioned "*pseudo*codeine." As pointed out by Knorr and Horlein (*loc. cit.*), the latter name was not happily chosen by Merck, since it is not the methyl ether of the base known as *pseudo*morphine, which is an oxidation product of morphine (see p. 280). The position becomes still more unfortunate now that the morphine analogue of "*pseudo*codeine" has been isolated, inasmuch as the new base cannot, of course, be termed "*pseudo*morphine." Lees has therefore proposed for it the name *neoiso*morphine, and similarly suggests the name *neoiso*codeine for the corresponding methyl ether.

When bromomorphide is hydrolysed, *β-iso*morphine and *iso*morphine are produced, and the corresponding methyl ethers are obtained by the hydrolysis of bromocodeide. Thus the genetic relationship between the halogen derivatives of morphine and codeine and the bases produced therefrom by hydrolysis, may be represented as follows:

*β-iso*morphine	←	Chloromorphide	→	Neo*iso*morphine
↓		↓		↓
*β-iso*codeine	←	Chlorocodeide	→	Neo*iso*codeine
*β-iso*morphine	←	Bromomorphide	→	*iso*morphine
↓		↓		↓
*β-iso*codeine	←	Bromocodeide	→	*iso*codeine

THE GENESIS OF IGNEOUS ROCKS

By JOHN W. EVANS, D.Sc., LL.B.
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In recent years considerable attention has been given to the study of the microscopical characters of igneous rocks as well as to the investigations of their mode of occurrence in the field. The work, however, has been in the main descriptive, and much remains to be done in interpreting the data that have been accumulated. It is impossible, in this instance, to follow the procedure usually adopted by geologists and decipher the memorials of the past by the experience of the present; for the processes involved are, as a rule, far removed from our observation, and even in the case of volcanic phenomena which are visible at the surface there are obvious difficulties in submitting them to close examination. The aid of other sciences has accordingly been invoked, and by the application of physical and chemical principles, many of them of comparatively recent development, results of considerable interest and importance have been arrived at. These have hitherto been so scattered among different publications as to be to a large extent inaccessible to those interested in the subject, and we owe a debt of gratitude to Mr. Harker, who, in his *Natural History of Igneous Rocks*, has presented them to us in a readable and attractive form.[2]

No one could have been more fitted for the task. In his classical researches on the igneous rocks of North Wales and the Lake District, and in later years on those of Tertiary age in the West of Scotland, he has demonstrated the relations that

[1] *The Natural History of Igneous Rocks.* By A. Harker. [Pp. xvi + 384.] (London: Methuen & Co., 1909. Price 12*s.* 6*d.* net).—*Igneous Rocks, Vol. I., Composition, Texture and Classification* [pp. xi + 464]. (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1909. Price 21*s.*)

[2] Two articles by Mr. Harker with the same title appeared in the earlier issue of *Science Progress*, vol. vi. 1896, pp. 12–33, and vol. vii. 1898, pp. 203–18. He had previously contributed other petrological articles to the same publication.

exist between igneous phenomena and earth movements, and has at the same time been able to trace to a considerable extent the progress of differentiation in igneous magmas.

The distinguishing character of an igneous rock is that it has consolidated from a state of fusion. If the solidification is rapid, it results in the formation of a glassy rock which does not differ in its essential nature from the fluid mass from which it has originated. There has been no break in continuity, but the viscosity has increased to such an extent that the magma may for practical purposes be regarded as a solid.

If, on the other hand, the process of solidification has been sufficiently prolonged, the molten magma crystallises out into silicates and other minerals. Simultaneously with the crystallisation, or on a decrease of pressure at a previous period, a number of volatile substances, which also formed part of the magma, separate from it and make their way through the surrounding rocks towards the surface. The most important of these "volatile fluxes" or "mineralisers," as they are termed by different writers, is water, which even at the high temperature of the magma retains, if the pressure be sufficiently great, many of the physical properties that characterise it at ordinary temperatures, but is endowed at the same time with far greater chemical activity. Volatile fluxes are usually present in greatest amount in acid magmas, especially those under great pressure at a considerable depth.

It was formerly assumed that the chemical constituents of the silicates were present in the magma in the form of simple oxides, and that these combined to form minerals when the rock consolidated. But we can only explain the manner in which the oxides are distributed in the minerals as they successively crystallise on the hypothesis that the former were already in a state of combination in the magma.

"Thus the bulk-analysis of an ordinary granite shows more alumina than is required to make felspars with the alkalies and lime present, and this excess is contained in micas or aluminous hornblende, minerals of variable composition. But the last-named minerals, with others of minor importance, have crystallised before the felspar. By their abstraction the composition of the remaining magma was accurately adjusted, so that the molecules of alumina equalled the sum of the molecules of potash, soda, and (remaining) lime. In other words, it was reduced accurately to the composition of a mixture of felspars and quartz; which is inexplicable, except on the supposition that it was actually a mixture of felspars and quartz" (pp. 166–7).

Mr. Harker's last assumption goes, however, somewhat farther than is warranted by the facts. Potash, soda and lime are able to form, without the assistance of silica, definite compounds with alumina in the same proportion as in felspars and other silicates of similar composition, such as nepheline, analcime, leucite and most of the zeolites, that is to say, with one molecule alkali or lime for each molecule of alumina. The potassium and sodium aluminates are freely soluble in water at ordinary temperature, and the former yields crystals with the composition $Al_2O_3 . K_2O . 3H_2O$.[1] The corresponding lime compound is insoluble. The possibility that it is these aluminates and not the felspars or other allied silicates that are present in the magma must not be overlooked. If this be the case the silica will no doubt occur in combination with water, for which it possesses a great affinity at high temperatures, and the excess of alumina will probably be present in a similar hydrated form. In exceptional cases, where there is an excess of alkalies over alumina, the trisodic aluminate $Al_2O_3 . 3Na_2O$, which has also been isolated, may be present and give rise to minerals of the sodalite group. It is, however, usually convenient in discussing the physical principles that govern the consolidation of an igneous rock to assume, for the purposes of exposition, that the minerals that crystallise out were present with the volatile fluxes in a state of mutual solution in the magma.

The crystallisation of a magma is, as a rule, partly the result of cooling and partly a consequence of the escape of the volatile fluxes. The latter is, however, in some cases the result and not the cause of the crystallisation. The laws that regulate the crystallisation of silicate magmas on cooling in the absence of the volatile fluxes have been worked out by Vogt, Doelter and others from the examination of artificial slags and a comparison of the results thus obtained with the microscopic characters of igneous rocks. It has thus been shown that the order in which the constituents of a slag or igneous magma crystallise out is largely dependent on the proportions in which they are actually or potentially present.

[1] These analytical formulæ are adopted for practical convenience without any assumption that they represent the structural constitution of the substance.

If, to select a simple example, we suppose that a magma which represents a combination of olivine and diopside in the proportion of 40 to 60 is slowly cooled from a state of complete fusion, olivine will at a certain temperature begin to crystallise out, and as it does so the percentage it forms of the magma will diminish until it is as low as 32 per cent. If now there is any further loss of heat, the two minerals will crystallise out together in the ratio of 32 to 68 until the whole rock is solid.

If there had been originally more than 68 per cent. of diopside, this would have crystallised out alone until the same ratio of 32 to 68 was reached.

A magma therefore containing olivine and diopside in this proportion remains liquid at a lower temperature than any other combination of those minerals and is said to be a eutectic mixture. The theory of eutectic mixtures is of considerable importance in the interpretation of the characters of igneous rocks, and as was pointed out by Mr. Teall as early as 1888, the association of quartz and felspar known as graphic granite may be regarded as an example of such a eutectic combination.

In the case of three or more substances there is in like manner a eutectic mixture which consolidates at a lower temperature than a magma containing them in other proportions. As the cooling of such a magma progresses first one and then two minerals will crystallise out, and so on till the eutectic mixture is reached, when all will crystallise out together. The order in which any two of these minerals crystallise is, however, not necessarily the same as if they alone were present, but is dependent on the nature and amount of the other minerals. If two minerals have an "ion" in common, such as magnesium in the case of spinel $MgO.Al_2O_3$ and olivine $(MgO)_2SiO_2$ (with a portion of the magnesium replaced by iron), their solubility will be markedly diminished, and each will be liable to crystallise out earlier than if the other were not present.

In an ordinary igneous magma containing the elements of a number of minerals most of the basic minerals have ions in common, which is not the case with the more acid minerals. The former are therefore more sparingly soluble in the magma. In this way the empyrical rule enunciated by Rosenbusch that the basic minerals crystallise out before those which are more acid in composition is in part at least explained.

These principles are, however, complicated by a number of other considerations which can only be briefly alluded to here, but which will be found clearly explained in detail in Mr. Harker's book.

Under certain circumstances, which have been investigated by Principal Miers and other workers, a mineral does not begin to separate out when the temperature of saturation is reached, and the magma may become supersaturated to a considerable extent before crystallisation begins. It can be shown that this may result in the crystallisation alone of first one and then the other mineral before the two crystallise out together. In some cases, too, the crystals which were first formed may be partly "resorbed" while the second mineral is in process of crystallisation.[1]

Another modification is introduced in cases where two substances are capable of forming mixed crystals or "solid solutions," for combinations of this character separate out in the place of the simple minerals. If the two substances are imperfect isomorphs, each can only hold in solid solution a certain percentage of the other; thus orthoclase can contain not more than 28 per cent. of albite, and albite not more than 12 per cent. of orthoclase. The succession of events in this case presents considerable analogies to that of two substances that are not isomorphs, but the composition of the crystals that separate out is dependent on, though not identical with, the composition of the magma at the moment they are formed. A eutectic magma is finally formed which solidifies with the simultaneous crystallisation of orthoclase containing a maximum of albite, and albite containing a maximum of orthoclase. It is believed that the well-known perthitic intergrowth of these minerals, in which orthoclase contains streaks and patches of albite, or in some cases *vice versa*, is produced in this manner.[2]

Where two substances are perfect isomorphs, as in the case of albite and anorthite, they can form mixed crystals in every proportion, and the final product is no longer a eutectic mixture of two distinct kinds of crystals.

[1] The symbolical representations of events given on p. 214 are not quite complete. They should read (A) ; (B) (– A) ; (B) ; (AB) and (B) ; (A) (– B) ; (A) ; (AB).

[2] In some instances, however, it is believed to be a secondary structure due to the decrease, with the temperature, of the capacity of one mineral to hold the other in solid solution.

Still another complication presents itself where the magma comprises two silicates which are capable of forming with each other a combination in the nature of a double salt. This acts to some extent as an independent substance, with the result that there will be a different eutectic mixture according as the original magma was intermediate in composition between the double salt and the one or the other of the simple constituents.

The influence of the volatile fluxes on crystallisation and other phenomena connected with the consolidation of igneous magmas has not yet been worked out in any detail, but there is little doubt that they must profoundly affect the characters of the rock ultimately formed. It is highly probable, for instance, that they play an important part in determining the late crystallisation of the more acid minerals, to which allusion has already been made. Silica, we know, combines freely with water under pressure at high temperatures, and this appears to be also the case with the constituents of the alkali felspars, for these with quartz make up almost entirely pegmatoid veins, which are undoubtedly formed from magmas containing a large proportion of water. The affinity of the alkali felspars for water is easily explained by the supposition already advanced that they are present in the form of silica and the soluble aluminates of the alkalies.

These felspars have, as a matter of fact, never been obtained by the crystallisation of an artificial magma without the elements of water or something capable of playing a similar part, and this is usually explained by the fact that a magma consisting largely of the elements of the alkali felspars is too viscous at the temperature of consolidation to allow the molecules the mobility necessary for crystallisation.[1]

Volatile fluxes are also needed for the formation of other minerals, such as quartz, amphibole and mica. These are termed "low temperature minerals" because they are transformed at high temperatures into other minerals, and it is possible that the volatile constituents may operate, in some cases at least, by

[1] Mr. Harker suggests that the abundance of orthoclase (sanidine) in many lavas shows that in some magmas volatile constituents are not necessary for the crystallisation of that mineral. It is, however, doubtful whether there are any liquid lavas which do not still contain an appreciable amount of water. Simultaneously with the loss of this, the whole rock crystallises out, so far as it had not already done so. See, however, note, on p. 312.

enabling the magma to remain liquid at a sufficiently low temperature to admit of these minerals crystallising out.[1]

Recent researches have thrown considerable light on the question of the temperature of the crystallisation of igneous magmas. It is obvious that no mineral can have crystallised out at a temperature and under conditions in which it is incapable of a stable existence. The "low temperature minerals," for instance, have been employed to fix a higher limit to the temperature of the consolidation of the rocks in which they are found. Quartz is stable only below 800° C., at which temperature it tends to pass into tridymite, and it is claimed that no rock containing the former mineral, at least as an original constituent, can have been formed above that temperature.[2] The temperatures at which the amphibole and mica become unstable are not exactly known, but they are probably at least as low.

In considering the value of these arguments it must, however, be remembered that our experimental data were obtained under atmospheric pressure. As the density of quartz (2·65) is considerably greater than that of tridymite (2·3), it is probable that under increased pressure quartz would remain stable at a higher temperature. Again, the instability of the amphiboles and micas at elevated temperatures may be attributed to the fact that they contain an appreciable amount of water. They have apparently crystallised under great pressure in a magma rich in volatile fluxes, and in many cases, it would seem, at a far higher temperature than that at which they become unstable at ordinary pressures for the dark borders seen in porphyritic crystals in volcanic rocks indicate, in all probability, decomposition due to the loss of volatile constituents on release of pressure when the lava was erupted. It would seem that some, at least, of these "low temperature minerals" might with at least equal propriety be termed "high pressure minerals."

An upper limit for the consolidation of a rock is also fixed by the melting point of its most fusible constituent mineral. The

[1] Morozewicz has shown that the presence of tungstic acid operates in the same manner as the volatile fluxes, the addition of one per cent. to a simple fused magma enabling orthoclase (sanidine), quartz and biotite to crystallise out. This amount is, however, rarely present in igneous magmas.

[2] Wright and Larsen have employed the change of the crystallographic class of quartz at 575°C. to show that granites consolidate above that temperature. *Am. Journ. Sci.* 1909, **27**, 421–47.

temperature of the crystallisation of a dolerite must therefore be below 1170° C., the melting point of augite. As a consequence, however, of the eutectic principle the magma must have remained liquid to a still lower temperature, and the determinations of the temperature of the molten lavas of Vesuvius, which place it in the neighbourhood of 1000° C., agree very well with theoretical considerations.

The presence of a large amount of volatile fluxes will also diminish considerably the temperature of solidification, and we may accordingly expect it to be lower in deep-seated than in volcanic rocks, which have lost the greater part of these constituents. Some pitchstone dykes yield water to the extent of over 20 per cent. by volume, and it is probable that a granite magma contains even more. Sorby as long ago as 1858 showed how the temperature of the crystallisation of a rock may be calculated from the proportion which the volume of the liquid inclusions in quartz and other minerals bear to the cavities in which they occur. In this way it has been found that the temperature of crystallisation of the Cornish granites lay between 200° C. and 350° C. (But see note 2 on p. 312.) The data obtained for the associated quartz-porphyry dykes indicates, Mr. Harker believes, a temperature some 50° higher, the result no doubt of a smaller amount of volatile fluxes.

The pegmatoid veins, which are also connected with granite, are believed, on the other hand, to have crystallised from a magma still richer in the elements of water and other volatile constituents than the granite itself, and they no doubt solidified at a much lower temperature. They may sometimes be followed until they pass into quartz veins which must have been deposited from a magma consisting mainly of silica and water, without alumina and the alkalies, at very moderate depths, and at a temperature little above the boiling point of water under atmospheric pressure. The extreme limit is represented by the waters of geysers that deposit siliceous sinter at the surface as they cool.[1]

Mr. Harker also discusses the question of the temperature of magmas at the time of intrusion underground, or extrusion at the earth's surface. Where there is evidence that crystallisa-

[1] The zeolites are in many cases among the latest deposits from magmatic water. The presence of fluorine in some form is indicated in those cases in which apophyllite occurs.

tion had already commenced, as is often the case with lavas, this furnishes us with a maximum limit, but the porphyritic crystals of intrusive rocks have often been formed *in situ*, and accordingly give no evidence of the temperature of intrusion. He considers the rare existence of evidence of the melting of the rocks that once enclosed a molten igneous magma to be an indication that the temperature was little in excess of that of consolidation. It must be remembered, however, that the adjoining rocks contain a comparatively small amount of volatile fluxes, so that their fusion point may be much higher than that of a perfectly fluid magma which is rich in these constituents. There is little to warrant any definite conclusion on the subject; yet it is difficult to traverse Brögger's contention that the temperature of intrusion of a deep-seated magma must be greater than that of its apophyses or any volcanic rocks with which they are connected, but it is probable that the difference is comparatively small.

In estimating the temperature of the crystallisation of rock-forming minerals, the influence of the high pressure that may have prevailed must not be forgotten. If crystallisation be accompanied by contraction, as is the case with all rock-forming minerals, increase of pressure results in an increase of the temperature at which crystallisation takes place. But although every magma or glass having the composition of a simple rock-forming crystal has a larger volume and lower density than its crystalline equivalent, this is not always the case with one representing a mixture of different minerals, for there are some rock glasses which are found to possess a greater density than that of the combination of minerals which would have been formed if the circumstances had been favourable for crystallisation. The temperature of crystallisation of such a magma will be lowered instead of raised by increase of pressure. There is reason, moreover, to believe that as the pressure is increased, the decrease in volume of individual minerals on crystallisation gradually becomes less and ultimately passes into an increase. We may therefore expect that under very high pressures the temperature of crystallisation of all magmas will be indefinitely lowered so that at a sufficient depth no crystallised minerals will exist, even though the temperature of the earth's interior is not so high as has been supposed. It does not follow from this that such magmas are in a liquid state in the ordinary sense of the word. They are, it is true, non-crystalline, but at the same time they must, as recent researches have shown, be as rigid as a solid so far as regards rapidly changing forces like those of earthquake tremors, or even the varying stresses due to the attraction of the moon and sun, though there is reason to believe that they are capable of yielding slowly to continuously operating forces. Under the enormous pressures of the earth's interior the distinctions between the states of solid, liquid and gas would seem to cease to exist, and all discussions as to whether one condition or the other prevails are probably beside the point. To say that the magmas of the earth's interior are in a gaseous state if the temperature is above the critical point is as misleading as to assert that they are in a liquid or solid state, for we have no reason to suppose that at the pressures that must prevail the critical temperature corresponds to any discontinuous change of physical condition.

We cannot here follow the author in his interesting descriptions of the varying microstructure or texture of igneous rocks and its relation to the phenomena and principles which we have briefly considered. But besides the differences which rocks present in this respect there are important variations in chemical composition which we have every reason to suppose have been developed by a process of segregation, and a number of suggestions have been made as to the means by which they may have been brought about. All are agreed that in many cases at least these variations arise in the course of the crystallisation of the magma in intercrustal reservoirs. The first crystals that form appear on the margin where the magma is cooled by contact with the neighbouring rocks. Here it is mainly the more basic minerals that separate out, leaving the magma poorer in their constituents, which are replaced to some extent by diffusion from other parts of the reservoir. In process of time crystallisation extends towards the interior, the minerals formed becoming gradually less basic, until in the centre of the mass the most acid variety of the rock occurs. The gabbro of Carrock Fell in Cumberland, described by Mr. Harker, is an interesting example. At the actual border it is "a very dense ultrabasic rock, with as much as 27 per cent. of titaniferous iron-ore (silica-percentage 32½)," whereas "in the centre the rock is a quartz-gabbro of relatively acid nature (silica-percentage $59\frac{1}{2}$)." "There is throughout a gradual transition from one variety to another, and, for the most part, the constituent minerals are the same, but associated in different proportions." He has shown that the chemical relations of the different types thus linked together may be illustrated by diagrams, in which the silica percentages are represented by the abscissæ and the percentages of the other oxides by the ordinates. It is found that with rocks differentiated from the same magma the points corresponding to each of these oxides in the different types form a continuous curve; which is not the case when unconnected igneous rocks are dealt with in the same way.

In some igneous masses the rock shows similar local variations, but these are not arranged in the same regular concentric fashion. In other areas a number of separate intrusive or volcanic rocks show a similar relation in chemical composition. It is supposed that in these cases the magma was differentiated by crystallisation in an intercrustal reservoir, and that subsequently while still in a plastic condition the different types were forced into new positions by pressure due to earth movements.

In some instances under the stress of such movements the liquid portion of a partially consolidated magma has been strained off from the more basic material that had already crystallised, and the magma separated in this way has crystallised out as an acid igneous rock. In other cases the minerals which have first crystallised are believed to have sunk, under the influence of gravity, to the lower portion of the reservoir.

But though no one can doubt the importance of crystallisation as a factor in rock differentiation, it appears to be insufficient to account for all the cases in which rocks of varying composition appear to have been derived from the same magma. In some areas we have evidence that both acid and basic products of segregation have crystallised out from the fluid state after separation has been effected, and in others the complexity of the relations between the different types is so great that it cannot be explained as the result of a single crystallisation.

Mr. Harker and others have suggested that igneous masses may be remelted and again crystallised, and that this process may be repeated indefinitely as in the laboratory operation of fractional crystallisation. The temperature of the magma and rock is supposed to remain throughout in close proximity to the melting point, so that a slight variation in the conditions may bring about crystallisation or fusion. A serious difficulty, however, presents itself, especially in the case of the more acid rocks. Crystallisation is accompanied by the elimination of the greater part of the volatile fluxes, and without these a much higher temperature would be required to bring the rock again into the liquid state. Many rocks, too, would in the absence of these fluxes, unless the temperature were very elevated, give rise to magmas so viscid that it would be impossible for differentiation to take place.

The possibility of segregation in the liquid magma must therefore be considered. This has been chiefly discussed in connection with Soret's principle, which declares that a substance with which a solution is nearly saturated tends to accumulate in any portion of the solution which is colder than the rest, or, more exactly, that "equilibrium will be attained only when the concentration at every point is inversely proportional to the absolute temperature." The differences of temperature likely to occur in an igneous magma bear, however, so small a ratio to the absolute temperature that the results of the application of this principle may almost be disregarded.

It is probable that segregation under the action of gravity is of more importance. An ordinary aqueous solution would, we are told, have to be at least 100 metres high before there would be a sensible difference in the degree of concentration between the top and bottom. Many intercrustal reservoirs must be far deeper than this; there is evidence, too, that the separation occurs more readily in the case of silicate magmas.

So far it has been assumed that the contents of each subterranean reservoir consist of a single magma, whose constituents are miscible in all proportions. Some liquid substances, however, such as aniline and water, are only completely miscible above a certain temperature, below which one cannot absorb the other except to a limited extent. The result is that as the mixture cools it separates into two liquids which arrange themselves in layers according to their density. Each of these liquids consists mainly of one of the constituents with a certain proportion of the other.

Vogt, however, concluded from his experiments that the constituents of igneous rock magmas are as a rule miscible in all proportions at all temperatures at which they remain liquid. The only exceptions he admitted were certain sulphides, of which the magnetic sulphide of iron, pyrrhotite, often found associated with norite, is the most important for the present purpose. Mr. Harker, however, is disposed to explain the segregation of corundum and the spinels from peridotites in the same way. Moreover, Vogt's experiments were made on anhydrous mixtures, and it does not follow that his results apply to magmas rich in water and other volatile fluxes. It is true that the elements of the alkali felspars appear to be miscible, even at moderate temperatures, in all proportions with water, and the same seems to be true of other magmas rich in alkalies, such as pitchstone; there is nothing, however, to show that water is at similar temperatures completely miscible with the majority of silicates in the absence of a considerable amount of the alkalies.

It is therefore possible that a magma rich in water and not containing an excess of alkalies over the alumina might separate on cooling into two layers, the first containing the greater part of the water, the elements of quartz and the alkali felspars with a small amount of the other constituents, the second consisting mainly of the silicates of calcium, magnesium and iron.[1]

The separation of norite and granite in the saucer-shaped igneous mass in the neighbourhood of Sudbury, Lake Superior, is probably a case of gravitational differentiation anterior to crystallisation. Here the total thickness is as much as a mile and a quarter. Similar relations appear to exist between the norite and red granite of the Bushveldt Plutonic Series of the Transvaal, which is also enormously thick. In these cases the great thickness would probably enable an effective separation to take place even if the magma were continuous. If, however, there were two discontinuous magmas, each would also suffer a certain amount of continuous gravitational separation, so that it might be difficult to say from an examination of the rocks whether there had been a separation into two magmas or not.

On the question of the classification of rocks Mr. Harker agrees with Becker that it should be based on the eutectic ratios. He would also pay special regard to rock structures which, like "the pegmatoid, the graphic, and (in part) the porphyritic, have

[1] Arrhenius takes up a somewhat similar position, but Mr. Harker is inclined to think that the experiments of Barus, who showed that glass was miscible with water in all proportions at as low a temperature as 200° C., are inconsistent with his views. Barus does not mention the composition of the glass he employed, but it was doubtless ordinary glass containing a large percentage of alkali.

a real significance from the petrogenetic standpoint." "Structures which depend on the special conditions under which a magma consolidates, and are not directly related to its composition and origin, are clearly of a lower order of importance."

There is no space to do justice to the earlier chapters dealing with the geological relations of igneous rocks, a subject which the author has made peculiarly his own. He lays great stress here, as in some of his previous writings, on the importance and widespread occurrence of the succession: volcanic rocks, plutonic rocks and minor intrusions; though he would not deny that some intrusive rocks must be connected with contemporaneous volcanic phenomena. We are indebted to him for the introduction of the term "minor intrusions" (also employed in his memoir on the igneous rocks of Skye) for the so-called "hypabyssal" rocks, which are often no less deep-seated than the plutonic or "abyssal" rocks. These latter might, by an extension of the same nomenclature, be more happily referred to as "major intrusions."

The book can be confidently recommended to all advanced students of petrology. It furnishes abundant material for thought, and indicates the directions that research must take in the immediate future. Its appearance should be followed by a striking development in this country of original work on the genesis and relations of igneous rocks.

Almost simultaneously another treatise dealing with the general principles of petrology has been published in New York by one who has for many years been identified with petrological research in the United States. While Mr. Harker set himself to discuss the more important recent advances and the problems awaiting solution, Professor Iddings has in this, the first volume of his *Igneous Rocks*, given us a comprehensive text-book on the principles of the subject. It has a wider scope, and subjects dealt with in both treatises are necessarily treated more briefly.

Considerable attention is given to chemical data and principles connected with magmas and igneous rocks. A number of typical analyses are furnished, and the different forms of diagrams employed to express the chemical composition of rocks are explained and illustrated in some detail. There is a useful discussion on the part played by the different elements in igneous magmas and in the minerals that crystallise out from them. An interesting suggestion is made to explain why leucite is associated with the intermediate plagioclase felspars but not with albite. If there is not enough silica present to form both orthoclase and albite with the alumina, potash and soda, the potash as the stronger base will take the larger amount of silica, so that orthoclase ($K_2O . Al_2O_3 . 6SiO_2$) and nepheline ($Na_2O . Al_2O_3 . 2SiO_2$) are formed. If, however, the elements of anorthite are present its crystallisation induces the simultaneous formation of its isomorph, albite ($Na_2O . Al_2O_3 . 6SiO_2$), and the two crystallise out in solid solution of one another, forming a member of the plagioclase series. As there is now insufficient silica to form orthoclase, leucite ($K_2O . Al_2O_3 . 4SiO$) appears in its place.[1] A concise but adequate account is given of the chemico-physical principles applicable to silicate magmas and their consolidation to form rock masses.

The chapter dealing with the crystallisation and texture of igneous rocks, illustrated by excellent photographs, is one of the most valuable in the book. It includes a description of the different "habits" of the principal rock-forming minerals, according to the conditions under which they were crystallised. A number of novel terms are introduced, and some at least of these might be adopted with advantage.

After dealing with the mode of occurrence and larger structures of igneous rocks, special attention being given to the mechanics of columnar jointing, the author devotes a large section of the work to the subject of the nomenclature and classification of the igneous rocks. We are given in the first place a "qualitative mineralogical" classification based on the works of Rosenbusch and Zirkel, which may be found useful for reference, though the essential constituents are not always sufficiently distinguished. This is followed by a detailed description of the newly elaborated "quantitative classification" of igneous rocks, already reviewed in SCIENCE PROGRESS (vol. i. 1906, p. 259). It is unnecessary to add anything here to the severe judgment which Mr. Harker has passed upon this scheme: "The rounded completeness, the measured precision, the finality, which would be admirable in a mathematical treatise, serve only to condemn a classification of igneous rocks, since they make more evident its aloofness from the scheme of nature, based not on arithmetical but on physical and chemical principles."

[1] This agrees very well with the supposition (*ante*, p. 308) that the alkalies are usually present in magmas in simple combinations with alumina and not as felspars.

THE EVOLUTION OF ANIMAL FUNCTION

PART II

By KEITH LUCAS

In an article published in this Journal at the beginning of the present year,[1] I discussed in a preliminary way some aspects of the relation between animal physiology and the hypothesis of evolution. I pointed out how, in my belief, the study of animal function has been prevented from developing along comparative lines by a somewhat loose application of the morphological doctrine of homologous parts; and I suggested that there was no valid reason known why, provided only that certain specified precautions were properly observed, likeness of function should not serve as a legitimate basis for tracing the course of phylogenetic development. The precautions to be observed consisted first in the reference of function to the individual cells in which it might be manifested; and second, in a rigid distinction between the functional behaviour of a cell, as gauged by its actual performance under the normal conditions of life within the organism, and its functional capability as discovered from its reaction to certain standard conditions and stimuli.

The preliminary discussion of these necessary distinctions was cast in the form of an inquiry into the use of functional characters as a basis of phylogenetic research, for the reason, indicated at the time, that the historical development of the present position seemed to demand such treatment. But it need not be inferred that the exclusion of functional considerations from the problems of phylogeny is to be regretted only, or even in the main, in the interests of phylogeny. I suggested briefly at the time that the neglect of a comparative study of function was to be deplored, rather because it has left us in the dark about the evolution of function itself, than because phylogenetic history might have been more completely and accurately traced. The statement was, however, made without more discussion. I wish now to attempt to justifiy what was said

[1] Science Progress, 1909, 3. 472-83.

and to formulate somewhat more clearly the problems with which comparative physiology has to deal.

How far, then, may we expect the comparative study of function to yield results of real importance to phylogeny? I venture to think that we cannot hope by this method to achieve anything of the first magnitude. It is hard to believe that peculiarities of functional capability will have outlasted profound morphological change, so as to bridge over those great gaps by which our present knowledge of phylogeny is interrupted. True, we have such facts as the preponderance of uric acid excretion in the reptiles and birds and its relative diminution in mammals, or the presence of creatin in the vertebrates and its absence from the invertebrates, pointing to the belief that certain tricks of metabolism may remain true over long stretches of evolution. But our knowledge of phylogenetic history is already such that facts of this order may be expected rather to confirm old knowledge than to make new. Comparative physiology, at the best a difficult and tedious method, comes too late into the field, and can hope for no large share in the enterprise.

Even if the course of phylogenetic development were wholly known and catalogued, so that we could assign all known living forms to their places in an historic scheme, still it would be but a slight beginning made in our knowledge of the past history of evolution. Though we should know the order of succession of evolutionary changes, we might remain almost wholly ignorant as to what the nature of those changes had been; and for the removal of that ignorance morphological observation would prove, as it seems to me, of very subsidiary importance, while the comparative study of function would be absolutely imperative.

If we try to discover broadly what is the nature of the differences which the course of evolution has established between one organism and another, we may for the purpose of analysis recognise two distinct modes of differentiation. On the one hand, evolution has produced changes which have affected only the functional opportunities of the cells constituting an organism, by changing their relation to other cells within the organism. Such a change is made by the morphological formation of a digestive cavity, which limits the opportunities of the lining cells to the function of secretion or absorption. On the other hand there have been direct changes made in the actual physiological properties of the cells within the organism, as when a nerve-cell has acquired the capability of more rapid conduction, or a gland-cell has become peculiarly sensitive to the stimulus afforded by the products of activity of certain other cells within the organism. The latter changes are to be recognised as distinct from the former because they constitute change of the actual inherent properties of the cells. This distinction is not invalidated by the obvious fact that, in the complex of organic reactions, a change of functional capability will seldom occur, without in some way involving also a change of opportunity for cells other than those directly concerned.

If we accept provisionally this grouping of the evolutionary changes which organisms have undergone, we see at once that, for the investigation of the first category of changes, those of functional opportunity, there are two methods of observation available, the morphological and the physiological. It needs no showing that the functional opportunities of cells may be altered either by morphological change, as in the example given above of the formation of a digestive cavity, or by the physiological change of other cells, as for example if one group of cells should secrete a chemical substance which should act as a stimulus to others contained in the organism. For the study of changes in functional capability the direct observation of function is the only possible method, and inference from morphological observation can have no voice whatever in the matter. The knowledge that certain cells forming part of an organism have been set apart for digestive purposes by the morphological formation of an alimentary canal, may tell us what functions those cells have to perform, but can tell us nothing whatever about their properties. The cells may have retained unchanged all the properties possessed by their predecessors in phylogeny at the time when no special cavity was set apart for digestion. They may, on the other hand, have been profoundly modified for the special duty to which the morphological change has limited them.

Here then we find two categories of evolutionary change open to inquiry—the one presenting problems both to morphology and to comparative physiology, the other capable of investigation by the methods of physiology only. The question I wish to ask is whether the contribution which the comparative study of function might make towards the solution of these problems would be of real assistance towards the understanding of the history of animal evolution.

Let us consider first the tracing out of those evolutionary changes which have been characterised as changes of functional capability. Is the knowledge of such changes really essential to the understanding of the history of evolution? In other words, has the evolution of functional capability been a factor of the first importance in the differentiation of organisms? We may suppose for a moment that it has not, in order to discover where such a supposition will lead. If the supposition be true, then the only physiological difference between the various parts of an organism lies in the functional opportunity which the various cells encounter. To take a single example: the heart muscle and the limb muscle of the frog behave differently only because they are subjected to different conditions and stimuli in the organism. The absurdity of this inference is at once manifest. Every student of elementary physiology has made the experiment of observing the behaviour of a limb muscle and of heart muscle when placed under like conditions, namely, when excised and immersed in Ringer's fluid. This simple experiment in functional capability shows the heart muscle beating rhythmically, the limb muscle quiescent. There can be no doubt that the two sorts of muscle are endowed with properties inherently different. In fact it is almost absurd to raise the question. The whole selective action of drugs is dependent on the differentiation of functional capability. The assumption that such differences are of fundamental importance lies at the very root of modern physiological research.

Though we may recognise clearly enough that the cells of an organism do differ in functional capability, and that such differences have been potent factors in the evolution of the organism, yet our knowledge of those differences is wanting in precision. Only in the case of very few types of cells—practically only of those types which we group together as excitable cells—are we beginning to get any clear definition of the existing differences of functional capability; while as to the evolutionary history of the differentiation we know scarcely more than nothing. There is no need to look for such remote problems as the past functional history of those cells of the suprarenal body whose secretion is so essential to the working of the mammalian organism. Even in such a simple and well-defined case as the cardiac muscle-cell of the amphibia, we can make but a poor guess at the successive steps of past functional evolution.

Our present knowledge of the history of animal evolution is, in fact, a singularly lifeless affair. It is as lifeless as was our knowledge of embryological development before the method of experimental morphology came into being. Our only acquaintance with the effect of evolution upon the actual working of the organisms concerned is that limited inference, which morphological observation will support, as to the functional opportunities enjoyed by various groups of cells. We have hitherto taken little account of the qualification of each cell to use its opportunities.

Here, then, as it seems to me, is the primary problem of comparative physiology—a problem whose investigation is wholly necessary for the understanding of the evolutionary process. It may be stated in general terms as the question to what extent and along what lines the functional capabilities of animal cells have been changed in the course of evolution. It would not be profitable at this early stage to enter upon any schematic and formal discussion of the many subsidiary questions which are included in this general statement. The better way, perhaps, to define the problem, and to make it more real, will be to attempt some preliminary consideration of the methods of investigation.

After such constant insistence upon the difference between functional behaviour and functional capability, it is scarcely necessary to repeat the warning that, if the investigation is to keep clear of confusion between questions of function and questions of structure or of the arrangement of parts, functional capability must be tested by experimental methods under chosen standard conditions and stimuli, and must never be inferred from normal behaviour under the special conditions presented by each organism. This inquiry means, in fact, the investigation of the reactions of cells under conditions which will necessarily be in a certain sense artificial, because if they are to be as nearly as possible similar for all cells investigated, they cannot be for all cells the normal conditions of life in the organism.

A simple example may make this point clear. Suppose that we have observed the excitatory process to proceed with greater rapidity in the motor nerves of the frog than in those of the snail. The observation is of no value as a comparison of functional capability in the two types of cell until we are assured that in the two cases the conditions of experiment were alike. But the normal tissue fluids of the two animals contain different percentages of inorganic salts, which may well have been the factors essential to the differences observed. The comparison cannot therefore be made strictly until both cells have been brought into a condition of equilibrium in similar fluids. If this is to be done the conditions must be artificial for one cell or for both.

This use of artificial conditions is at once the essential method of investigation, and the chief source of difficulty which the inquiry presents. In the first place we cannot deny that the need for testing different cells under uniform conditions will often present grave obstacles from the experimental side. As long as comparison is being made between the different cells of a single species there will not be much to fear; but when the comparison is between the cells of animals widely remote in phylogenetic relationship, the normal conditions may be so far different that it will be scarcely possible to find a single set of conditions under which both types of cell will survive. As a particular example of the sort of difficulty which may be encountered from this quarter, one thinks of the wide difference of composition which the tissue fluids of different animals present. The concentration of the blood of marine invertebrates and of the lower fishes is about equal to that of sea-water; that of animals living in fresh water is often no more than one-fifth of this. Recent physiological research shows, indeed, that it is possible to bring cells accustomed to one concentration of tissue fluid into steady equilibrium with a widely different concentration in a time not greater than a few hours. But the necessity of such a process obviously adds much to the difficulty of comparing cells taken from different animals.

Quite apart from such experimental difficulties, the use of artificial conditions will undoubtedly offend that prejudice which the human mind so often shows against any investigation of the phenomena of life under conditions other than those normal to the organism. The basis of this objection seems to be the idea that, so long as we investigate and describe the normal sequence of biological phenomena, we are doing all that can possibly be done towards the scientific explanation of life, whereas to in vestigate the behaviour of organisms or of their parts, under conditions to which they are not normally subjected, is to burden biology with so much useless fact. The question was fought out recently over Roux's programme for the study of experimental embryology. The clearest possible answer has been given to all who sought to oppose the artificial method, by those fundamental facts which the school of experimental embryology has already contributed to the understanding of embryological development. Indeed, as far as I am aware, no sound argument has ever been adduced why biology should be condemned to struggle along without making every possible use of that abstraction from special conditions which is the very essence of the experimental method. Biology surely, beyond all other sciences, finds the phenomena which she has to investigate entangled in special conditions. The organism presents to every cell a most limited routine, and only by breaking through that routine shall we begin to disentangle the essential properties of the cells from their chance associations.

If, then, the general problem is stated, and the essential method of observation is determined, we have yet to decide at which end of evolutionary history investigation should begin. Are we to learn first to know the properties of the unicellular animal, and then to trace these as they diverge and become isolated in the specialised cells of the higher organisms? Or is the method to be the reverse of this? This question will certainly be answered in practice by one consideration only—at which end it is easier to begin. On this matter it has been urged by Verworn that cell-physiology should begin with the unicellular organism. The reasons given for this decision are, in the main, the statement that every cell, even the most physiologically specialised, has to perform all the elementary functions of life, and the fact that physiological specialisation brings with it morphological complexity. As to the first of these reasons, one need only recall that the nerve fibre is freed from the duty of ingesting solid food particles, and from the performance of contractile changes, to realise that physiological specialisation does actually carry with it the suppression of some elementary vital phenomena. The question of morphological complexity is beside the point when the object of study is not form but function. I venture to think that when the investigation of any function is in hand the first object of study will be the type of cell in which that function is most obvious and most highly elaborated. Never shall we penetrate *de novo* into the complex of functional capabilities which a unicellular organism presents, select one capability and trace it upward until it emerges as the specialised property of some group of highly evolved cells. We should fail simply for want of experience and technique gained on the more specialised case. The physiology of the cell must begin, not with that maid-of-all-work the unicellular organism, but with such cells as are as nearly as possible unifunctional. This decision is as much a matter of history as of prophecy. When we speak of the conduction of the excitatory state in unicellular organisms we are speaking of phenomena which were first seen and traced in specialised nerve-cells, were afterwards recognised by similar methods in muscle-cells, and were only later detected in unicellular organisms by investigators who had the previous knowledge to guide them. The only practicable method for the study of any function will be to investigate that function first in some cell in which it appears in a highly elaborated state, and with the help of experience and technique so gained to trace it back through succeeding degrees of less specialisation and greater obscurity.

Here we encounter a grave difficulty. If we were acquainted with every modification which any one functional capability exhibited throughout the animal kingdom, we should still be unable to arrange the various modifications found in the order of their succession in evolution. We have no sure ground for asserting that it would be just, for example, to say that every function had developed along the line of increasing complexity. The idea presents itself, of course, that we might make use of the phylogenetic succession of species and arrange the functions observed in the order of the species from which they were taken. This might be a practicable method, if we were so fortunate as to have at our command a number of animals which formed a continuous series of successive steps in a process of evolution. As a fact, it is not to be expected that the animals living at any one time will ever form such a series. The animal kingdom is made up of a number of species, each of which represents the end of one line of a branching system. The condition of things which we are seeking would demand that a species, whenever it had by variation produced a new species fitted to survive, should itself continue to exist without further change, and mark time on the main line of evolution. Such an expectation is clearly absurd. The most that we can hope is that some species now existing have not diverged very far from the main line along which more recent species passed on the way to their present condition, and so may in a rough way be regarded as earlier stages of a continuous process. This is a serious difficulty in the way of tracing out the course of evolution of any functional capability. For even if we were able to estimate with some approach to accuracy the extent to which any given species had diverged morphologically from the main line of evolution, we should still be as far as ever from knowing whether the peculiarities of functional capability which that species might present had or had not been the subject of much recent modification. We have no means of knowing *à priori* whether the course of functional evolution is likely to follow in any way that of the morphological.

May we not look for some help towards the solution of the problem from the study of ontogenetic development? May we not make some use of the Biogenetic Law, that ontogenetic development is a recapitulation of phylogenetic? The method is a tempting one, for it removes at a stroke that fundamental difficulty which we have already mentioned—the difficulty of finding a single set of conditions under which cells taken from diverse animals could be compared. In comparing cells from different stages of any one embryonic development we should not meet those differences of concentration of tissue fluids which we have to face in the comparison of cells taken from different animals. Unfortunately it has become amply clear in recent years that the Biogenetic Law is not a generalisation which can be applied in any such crude and direct manner. The study of ontogeny from the morphological side has shown often enough that the law breaks down when brought into touch with the details of development, seems in fact to reduce itself to the simple observation that the early stages of development in different animals are more closely similar than the later stages, and refuses any responsibility beyond this fact.

This failure of the Biogenetic Law need not mean that ontogenetic study is to render no help to the understanding of functional evolution. The very fact that the Biogenetic Law was ever stated in the form to which we are accustomed seems to indicate the strange, one-sided view of the process of evolution which has passed into common thought. We have come almost to think of phylogeny as a thing to be contrasted with ontogeny. We are in danger of forgetting that the process of phylogenetic development, as considered apart from the ontogenetic, is a pure abstraction, existing nowhere except in our own minds. What we have to deal with in evolution is not two separate processes—an ontogenetic and a phylogenetic—but simply a succession of ontogenetic processes usually each exactly like its immediate predecessor, but sometimes undergoing a permanent modification. From each of these successive processes we have mentally selected the final stage; we have arranged these stages in the order of the ontogenetic processes from which they were taken, and have raised them to the dignity of a separate biological process—the process of phylogenetic development. Let us suppose for a moment this abstraction never made, and let us regard the course of evolution in the simple way as a succession of ontogenetic developments into which variations of procedure have been introduced from time to time. From this point we gain at once a clearer view of the facts which really underlie the Biogenetic Law. If that law were rigidly true in the sense that ontogenetic developments were always an exact reproduction of phylogenetic history, then the meaning of the law, in terms of our present method of regarding evolution as a succession of modifications of ontogenetic development, would be simply this: that no modification had ever been introduced into ontogenetic development except the addition of a further development subsequent to the completion of the process hitherto normal. We might, in fact, state the Biogenetic Law more simply in these terms: In the course of evolution ontogeny is modified only by the addition of new processes at the end of development. It needs no showing that such a method of procedure would inevitably result in a condition of things rigidly consonant with the strict interpretation of the law stated in the usual manner. Every stage of phylogenetic development, or, in other words, the final stage of every successive ontogenetic development, would be naturally embalmed in subsequent ontogenetic procedure. We recognise at once that the law, as we have stated it above, is not true. Ontogenetic development is sometimes modified by a change which appears at a comparatively early stage. Yet we cannot fail to see that there is some truth in the Biogenetic Law. On the whole the successive stages of ontogeny do in a rough way follow the lines of phylogenetic development, though there are many short cuts and modifications. It would seem then that the underlying fact, on which the Biogenetic Law rests, is simply that modifications introduced into the course of ontogeny do not always affect all stages of the process, but tend to appear rather in the later stages than in the earlier. This is probably the form in which we must accept the law for our present purpose.

If we now return to the difficulty under consideration we shall find that this statement may, with reasonable probability, be of real assistance in the inquiry. If every modification introduced into ontogenetic development necessarily affected all stages of the process, we should be in no better position, after examining all stages of ontogeny, than we are after examining only its final result. We should only find in different species a number of wholly different ontogenies between which the connection of succession would not be traceable, just as we have a number of different results of ontogeny. If, however, some modifications appear at a comparatively late stage of ontogeny, we have groups of ontogenies whose earlier stages are alike though their later stages are different. As soon as we begin to recognise such groups we are getting free from the bewildering state of things in which the functional capabilities differ with every species. We may even, to put the matter somewhat hopefully, find such groups co-extensive with different phyla of the animal kingdom, though the adult functional capabilities found within any one phylum are differentiated one from another to such an extent that their inclusion in one group on their own merits would not suggest itself. If we can only recognise such groups as these, we shall be on the road towards such a classification of functional ontogenies as will bring us into touch with those broad facts of evolutionary succession which are tolerably ascertained on morphological grounds. In this way and for these reasons, I believe that we are likely to learn far more of the past evolution of any functional capability by examining its ontogenetic development in comparatively few species, than we could hope to know if we were acquainted in a vastly greater number of species with the final condition which functional capability reaches in the adult organism.

THE TRANSMISSION OF PHOTOGRAPHS BY TELEGRAPHY

By T. THORNE BAKER, F.C.S., F.R.P.S.

Many attempts were made to telegraph pictures some twenty years ago, but the methods then conceived were not developed, in all probability on account of the lack of possible application. Modern journalism is such, however, that a continual and liberal supply of what are called "news photographs" is demanded, and the time has thus arrived when photo-telegraphy can hold a useful position and perform a commercial function.

There are two ways in which a photograph can be made to control or actuate the electrical currents necessary to transmit it over a distance to a suitable receiver; one is optical, the other mechanical. We shall consider each of these in turn.

Optical Method of Transmission

In the optical method we must depend on the density of the photographic image to control the amount of light falling upon some body whose electrical properties vary with intensity of illumination. Selenium, in the crystalline form, is extremely sensitive to light, its resistance being reduced by approximately fifty per cent. when illuminated by the maximum amount of light suitable for influencing it. Its sensitiveness is greatest towards the orange region of the spectrum, though Ruhmer claims to have sensitised it for special regions by the aid of optical sensitisers such as are used in orthochromatic photography. The sensitiveness to different regions of the spectrum, according to measurements made by me in 1907, are as follows:

	Region of Spectrum.	Ratio of Resistance of Selenium Cell to its Resistance when unilluminated.
Blue-violet	3800–4700 A.U.	11 : 21
Green	4700–5800	11 : 22
Yellow	4500–end of red	11 : 26
Orange	5700– „	11 : 26
Red	6100– „	11 : 25

Attempts to sensitise the selenium "cells" were not sufficiently successful to lead me to continue the experiments, especially as the inertia appeared to remain practically unaltered with light of different wave-lengths. The "cells" are made by distributing selenium over two coils of platinum wire wound round a plate of steatite, not touching each other, and the cell is then cooked at a constant temperature for several hours until the selenium assumes the crystalline form, in which its conductivity is enormously increased.

Professor Korn, of Munich, was the first to make successful use of selenium, by compensating for the high inertia by the combination of two cells of opposite characteristics, both illuminated simultaneously. His method of procedure is as follows. A photographic transparency, on a celluloid base, is

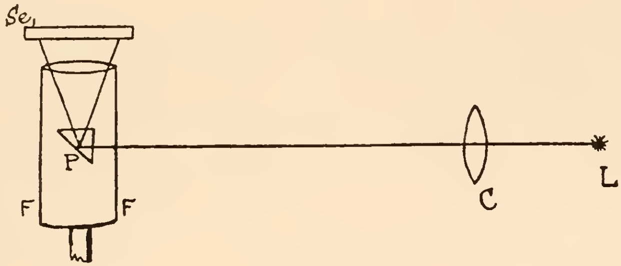


FIG. I.

attached to a glass cylinder, which is revolved spirally in front of a Nernst lamp. In fig. I, L is the lamp, C a lens, F F the glass cylinder with film attached. The rays from L cross at the front face of the cylinder, and as the latter revolves, and different successive parts of the picture intercept the light, the intensity is varied, and the light which traverses the film is reflected from the prism P on to a selenium cell Se, of low inertia and high resistance. The resistance of course varies each instant according to the density of the film at the point where the beam of light traverses it.

To understand the system of compensation, we must now turn for a moment to the receiver, which in transmission is taken into use as the compensator. The light from another lamp L (fig. 2), passes through a hole bored in the poles of the electromagnet M M, and is then concentrated upon a point of a sensitive film attached to the drum D, which revolves synchronously with the cylinder FF in fig. 1. But in transmission, a prism is placed at Q, which turns the light downwards upon the compensating cell Se_2. Between the poles of the magnet MM is fixed a galvanometer unit consisting of two very fine silver wires, to which are attached, where they cross the

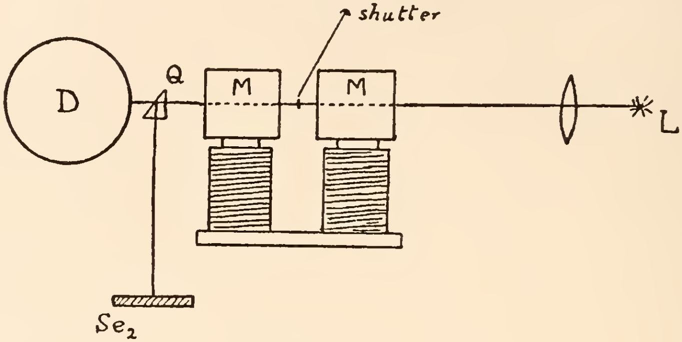


FIG. 2.

optic axis, a tiny magnesium foil shutter, about $\frac{1}{60}$th inch square. The shadow of this is made by optical arrangement just to cover the cell Se_2 in transmission, or, in receiving, to cover a triangular aperture in a diaphragm fixed in front of the dark

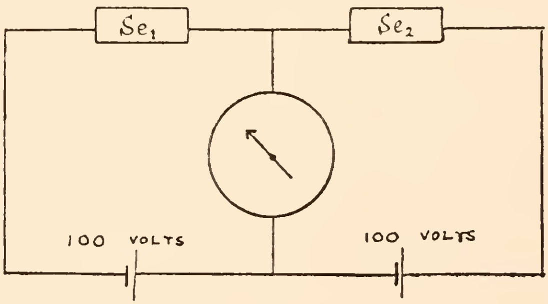


FIG. 3.

box in which the receiving drum rotates. When current passes through the silver wires, a lateral displacement is produced, which enables the light to reach the cell Se_2. With maximum current, the whole cell is illuminated, with 1/xth of the maximum current, 1/xth of the cell is illuminated, and so on.

The two cells are connected on the two sides of a bridge, as will be seen from fig. 3. Here, if w_1 and w_2 are the resistances of the two cells, the displacement in the galvanometer is approximately proportional to D in the equation $D=Ck\left(\frac{1}{w_1}-\frac{1}{w_2}\right)$, where C is the current and k a constant depending on the galvanometer. The resistance of Se_1 is usually very high, about 150,000 ohms, while that of Se_2 is much less, about 30,000 ohms, etc. The great inertia is largely overcome by the use of the two cells, especially as the light only reaches the compensating cell a fraction of a second after it has affected the first.

The function of the Korn transmitter is thus to send into

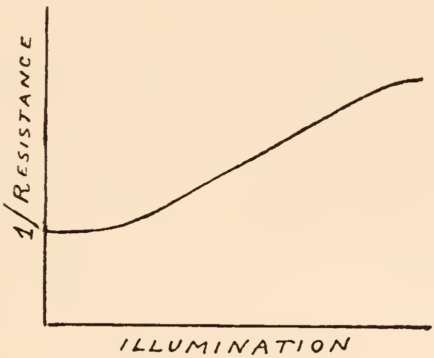


FIG. 4.

the line an ever-varying current which depends always on the density of the film being telegraphed. In the receiver, this current is made to displace the magnesium shutter, so that the Nernst lamp can illuminate the revolving sensitive film more or less. The aperture in front of the film is, as already stated, triangular. This is because the stronger the light acting on the selenium cells, the less comparatively is their resistance altered; hence as the shutter uncovers the triangular hole, moving always towards the base, more and more light (comparatively) is admitted to the lens behind it which concentrates it upon the film. The curve representing the effect of the illumination on the resistance is usually somewhat as shown in fig. 4. The form of the curve varies with each individual cell, and the shape of the aperture, *i.e.* the obtuseness of the angle, must be altered to suit the cell.

The time of the transmission is long by this method, and for commercial value it can hardly be compared with the two methods now to be described.

Mechanical Methods of Transmission.

The idea of a telegraphic transmitter for photographs consisting of line drawings or "half-tone" prints on metal attached to a cylinder revolving beneath a metal tracer or style is a very old one. Yet until Prof. Korn adapted to such a transmitter the "string" galvanometer nothing of a really practical nature was ever accomplished. The arrangement of his telautograph can be seen at a glance from fig. 5. Here C is the metal cylinder of the transmitter, which revolves spirally

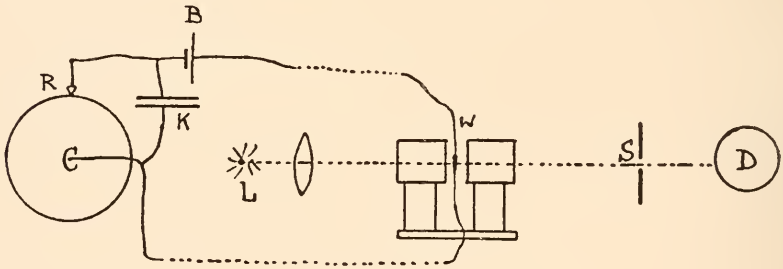


Fig. 5.

in the manner of a phonograph cylinder under the steel tracer R. On the cylinder is put a piece of copper foil on which a sketch has been drawn with some insulating ink, such as shellac solution tinted with an aniline dye. B is a battery of from thirty to sixty volts; one element of this is connected to the style, the other to the line, the cylinder (or base of the instrument) being earthed, or attached to the second unit of a telephone line. The received current, which is of course interrupted every time a shellac line comes between tracer and cylinder, passes through a flat silver wire W stretched across the field of an electromagnet with bored poles, as in the case of the selenium machines. The wire crosses the optic axis, light from the Nernst lamp L being intercepted by the wire, so that the shadow of the latter covers the slit S. When current flows, the shadow shifts, and light passes through the slit and is concentrated on a point of the revolving sensitive film attached to the receiving drum D, which is revolving synchronously with that of the transmitter.

Prof. Korn also utilises a weak current flowing through the galvanometer in the opposite direction to that of the received current, so that an actual break of current never takes place. A condenser K is placed across the transmitting units to prevent sparking at the style.

Here it is interesting to note that, working between Berlin and Paris, when 60 volts is used over telephone lines, about 15 milliampères of current usually enters the receiving apparatus. Using an earth "return," instead of having a metallic circuit throughout, as much as 5 milliampères has been received. The rate of transmission is ten minutes for pictures 12 × 12 centimetres in size. M. Chatenet, who operates the apparatus in Paris for the *Daily Mirror*, has done much experimental work with me in preparing half-tone photographs on metal-foil for transmission, but despite the very low moment of inertia of the moving part of Korn's galvanometer, an oscillation is set up owing to the regular period of the lines in the half-tone image, and the results so far have not been altogether satisfactory. M. Chatenet and myself have both come to the conclusion that single-line half-tone photographs are the best for transmission, and such results are solely used for transmission in my own telectograph.

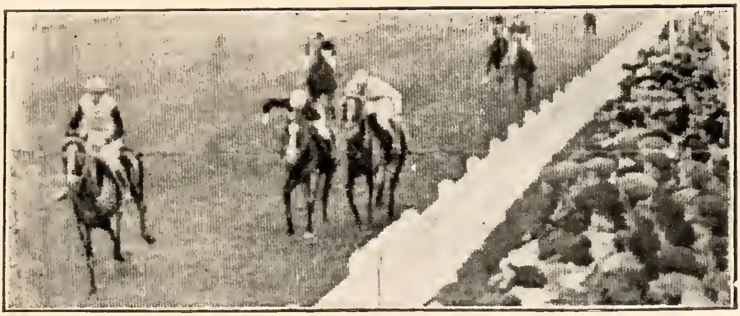
Half-tone photographs may be prepared in a variety of ways. The most suitable for electrical transmission are those made with a single-line screen, by copying the original in a camera fitted with a glass screen ruled with thirty-five or forty parallel lines to the inch. This screen is placed a short distance in front of the plate, and the latter on development renders the subject as a series of lines varying everywhere in width according to the densities in the photograph. A print is made from the half-tone negative so obtained upon a sheet of metal-foil coated with fish-glue, sensitised to light by means of potassium-dichromate. The print after exposure is washed in water, when the unexposed parts (the "whites") dissolve away, leaving the insoluble gum lines of varying width constituting the image.

The gum lines, fine though they may be, are sufficient to make the style vibrate as it passes over them, when the print is attached to the rotating drum of the transmitter. This vibration is very detrimental to the telegraphing of the picture, as instead of the impulses telegraphed depending for their duration solely on the width of the lines, they tend to endure for a time t/n, where t is the time taken for one revolution and n the number of lines traversed by the style in one revolution. t/n is of course constant, while the widths of the insulating lines vary, and on their variation depends the formation of the details in the picture.

I overcame this as follows. First, the prints were made on fairly thick lead-foil instead of copper, and afterwards they were pressed so as to sink the lines completely into the metal, and thus give a smooth surface. The style passes over the photograph thus without any vibration.

It will be readily seen that by passing the electric current through the cylinder and style these lines are made to act as interrupters, so that at the receiving station we get a series of short-period currents, each of whose duration depends on the thickness of the line corresponding. Instead of utilising the received currents to cause a shutter to rise and so allow the light from a lamp to act on a photographic film (as in the case of the Korn telautograph), I pass them through a cylinder and style similar to that used in the transmitter, but the cylinder has attached to it a piece of electrically sensitised paper, which discolours by electrolytic action every time the current flows through it, thus leaving a black dot on the paper. These dots form into lines as the transmission proceeds, and finally a replica of the original half-tone photograph is obtained, the thickness of the lines depending on the length of time of flow of current, and thus corresponding exactly with those of the picture transmitted.

But here several line phenomena have to be dealt with. The method adopted for strengthening the attenuated currents I do not propose to discuss now, but the effects of the cable capacity are worthy of notice. Oscillatory currents are apparently formed in the line, and these flow into the receiver and effectually blur and spoil the results; indeed, they prevent the



Finish of the St. Leger, Doncaster Races, 1909. Telegraphed in $5\frac{1}{2}$ minutes by the Thorne-Baker telectrograph.

p. 338]

received image from being recognised. They were eventually overcome by introducing counterbalancing oscillations, which completely damped them. An adjustable back, E.M.F., with variable capacity, enables the operator to counteract the line-constants, with the result that crisply defined pictures are being regularly received from Manchester at the London office of the *Daily Mirror* at the present time.

This method of photo-telegraphy is particularly adapted to wireless transmissions, and I have arranged a satisfactory installation for experimental purposes in the following way. In the diagram (fig. 6), D is the drum of the transmitter, and the

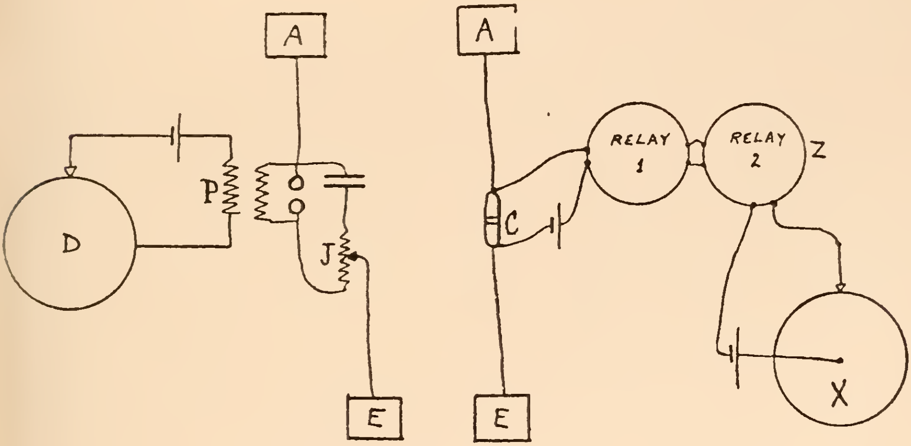


FIG. 6

intermittent currents from it are made to actuate the primary P of the induction coil. A is the antenna and E the earth-plate of the oscillating circuit, K the condenser battery, J the inductance. In the receiver, the coherer C actuates the relay through a small battery, and shunted to the decoherer is a second relay Z, through the local battery side of which the current is passed into the receiver-drum X, to which is attached the sensitive paper. Synchronisation is effected by a simple arrangement, which I am not at liberty to enlarge on at the moment, but the chief feature of the apparatus is the decohering instrument, which after considerable experimental work I have made to work exceedingly crisply, so that the effect is very "dead beat," the hammer only striking once to each signal sent from the transmitting station, *i.e.*, to each traversal of a given line by the style. The definition of the telegraphed pictures is not interfered with seriously, and the results have been most promising.

It should be observed that the wireless apparatus is in a purely experimental stage at present, and I do not anticipate any application of it to commercial uses until wireless work generally becomes more efficient.

REVIEWS

The Cambridge Natural History. Edited by S. F. Harmer and A. E. Shipley. **Vol. IV. Crustacea and Arachnids.** By the late Prof. Weldon, Geoffrey Smith, Henry Woods, A. E. Shipley, Cecil Warburton, and Prof. D'Arcy W. Thompson. [Pp. xviii + 566.] (London: Macmillan & Co., 1909. Price 17*s.* net).

The *Cambridge Natural History* is now completed by the issue of this, the fourth in order of the ten volumes, and the editors are to be congratulated on having brought to a successful conclusion the enterprise begun some sixteen years ago. In several respects the work as a whole occupies an important place in scientific literature; not least in that it is the only "Natural History" in the English language which, while written for the non-specialist reader, devotes a due proportion of space to the invertebrate groups of the Animal Kingdom.

As in the case of several of its predecessors, the various sections composing the latest volume differ considerably in method of treatment, and do not all reach the same standard of excellence. The longest section is that dealing with the Crustacea, and it has suffered somewhat from the melancholy circumstance which, as explained in the preface, has delayed the appearance of the volume. The Crustacea were to have been dealt with by the late Prof. Weldon, of Oxford; but at his untimely death only the chapter on the Branchiopoda was left ready for publication, and the remainder of the section has been written, at very short notice, by his pupil, Mr. Geoffrey Smith. Prof. Weldon's chapter provides a very full and lucid account of the group with which it deals. It is illustrated by a large number of exceptionally fine original figures, and the few inaccuracies which may be detected would doubtless have been corrected had the author lived to revise it. As it stands, however, this chapter, extending to thirty-seven pages, is out of proportion to the rest of the section, and this has made it necessary to deal more briefly than was desirable with the other orders of Crustacea. Mr. Smith's contribution bears evidence of having been somewhat hastily compiled, and its various parts are of very unequal value. On some subjects, of which the author has made a special study, he writes in a very interesting fashion; and the accounts which he gives of the wonderful life-history of the Rhizocephala (p. 95), of the phenomena connected with growth and sex (p. 100), and of the structure and modifications of the eyes (p. 146) are especially well worth reading. Unfortunately, when on less familiar ground, he is here and there betrayed into statements which seem to admit of no other explanation than that said to have been offered, on a certain celebrated occasion, by Dr. Samuel Johnson. In stating that the Stomatopoda have "ten hepatic diverticula given off segmentally from the alimentary canal" (p. 142), Mr. Smith is only repeating a venerable error of the text-books; and in attributing to *Nebalia* a brood-pouch like that of the Mysidæ (p. 112) he has been misled by the statement of a recent writer; but he might have hesitated before describing *Caligus lacustris* as "common in fresh-water lakes and streams" (p. 74), or *Carcinus mænas* as absent from the Mediterranean (p. 194). *Stenochotheres*, three times repeated on p. 76 for *Stenothocheres*, and "retinaculæ" on p. 142, may perhaps be attributed to the printer. The tangle of nomenclature may entrap the most careful, and it is merely an unlucky slip that the only example given of the genus *Hippolyte* (p. 164) is the type-species of the previously mentioned *Virbius*; but it is less excusable to find that Sir Ray Lankester's figure of *Cymonomus normani*, copied on p. 185, is labelled *C. granulatus*, although the distinguishing characters of the two forms are given on the next page, with the added error that *C. normani* is said to come from the "East African Coast" instead of the North Atlantic. Still more astonishing and altogether inexplicable are the statements that in the crab *Ocypoda* "the gills have entirely disappeared" (p. 194), and that "*Crangon antarcticus* occurs at the two poles and apparently not in the intermediate regions" (p. 200). Some of the figures illustrating this section are excellent, but a few, notably the diagram of a Cirripede on p. 96, are very poor.

The chapters dealing with the Trilobites and Eurypterida are by Mr. Henry Woods, and give very full and well-illustrated accounts of these important fossil groups. Mr. Woods dissents from the view, adopted by Sir Ray Lankester among others, that the Trilobites are specially related to the Arachnida, and he regards them as approximating to the primitive stock of the Crustacea.

A brief "Introduction to Arachnida" and sections dealing with the King-Crabs (Xiphosura) and with those curious and degenerate groups the Tardigrada ("Water Bears") and Pentastomida, are written by Mr. A. E. Shipley, who has brought together a great many interesting details which the student will look for in vain in the ordinary text-books. In discussing the King-Crabs the author refers to, but does not adopt, Pocock's classification, objecting to the disappearance of the name *Limulus* and also to the subdivision into genera and sub-families of a group containing so few species "differing *inter se* comparatively slightly." It may be suggested, however, that questions of classification are not necessarily connected with those of mere nomenclature. Many zoologists will applaud Mr. Shipley's defiance of the "priority purists" in retaining the familiar name *Limulus*; but it is important to have a classification which draws attention to the significant fact that the Oriental King-Crabs are more nearly related to one another than they are to the American species.

The air-breathing Arachnida (Scorpions, Spiders, Mites, and the like) are dealt with by Mr. Cecil Warburton. Three of the seven chapters in this section are devoted to the spiders, and provide, what has long been wanted, a tolerably full summary of what is known of the habits and classification of these attractive animals. A long account is given of spiders' webs and nests, and the varied methods of their construction—a subject to which the author has previously made noteworthy contributions. The chapter on the classification of spiders, largely based on the work of Simon, will not escape criticism, especially since the author adopts no divisions of higher rank than families, regarding those wider groups which have been proposed as "not of great importance."

The other Orders of Arachnida are dealt with more briefly, and in some respects the accounts given of them are hardly up to date. Thus, no mention is made of the stridulating organs possessed by many Scorpions, and the section dealing with the Pedipalpi (Whip-scorpions) seems very insufficient. The author does not appear to have consulted the important memoir on the Tartarides, published in 1905 by Hansen and Sörensen, otherwise he would hardly have stated that the carapace in this group is "two-jointed" (p. 310) or that the species are found only in Burma and Ceylon (p. 312). No authority is given for the name *Graephonus* mentioned on p. 309 as that of a fossil Tarantulid, and we are therefore left to conjecture what may be its relation to the better-known *Geraphrynus*.

Prof. D'Arcy W. Thompson contributes a scholarly and interesting chapter on the Pycnogonida, of which the affinities were discussed recently in SCIENCE PROGRESS (Vol. iii., 1909; pp. 687-93). He adopts the view, which was contested in the article referred to, that the recently discovered ten-legged forms, *Decolopoda* and *Pentanymphon*, are the most primitive members of the group. A pleasing feature of this chapter is the prominence given to the observations of the older naturalists. A distinguished zoologist recently told the reviewer that "one never needs to refer to the older writers now except for purposes of nomenclature." This opinion is evidently not shared by Prof. Thompson, who finds the accurate descriptions and acute comments of Linnæus, Brünnich, and O. Fabricius still worth quotation from the century before last. Not much of importance relating to the Pycnogonida has escaped notice in this chapter, but there seems to be no mention of the fact that some deep-sea species are known to be phosphorescent.

W. T. CALMAN.

Microscopy: The Construction, Use, and Theory of the Microscope. By E. J. SPITTA. [Pp. xxii + 502, 16 plates.] (London: John Murray; 2nd edition, 1909. Price 12*s*. 6*d*. net.)

THE first edition of this book appeared in August 1907, and the early issue of a new edition indicates a well-deserved popularity. The aim of the author has been to give a clear and useful account of the whole subject, and though this is a somewhat difficult task in a book of this size, Mr. Spitta has succeeded very well. But it is the practical rather than the theoretical side that is prominently brought out; this book will enable the student to obtain an intelligent grasp and will lead him to a skilful manipulation of that beautiful instrument—the microscope. Mr. Spitta is an authority on his subject, and his wide practical experience is obvious throughout the book, which abounds in useful hints. To any one desiring a reliable and serviceable guide to the microscope, whether he be a microscopist or merely using the instrument as an aid to work in some other branch of science, we can recommend the book.

A few words may be said on one or two important features. The value of Mr. Spitta's work is increased by numerous excellent illustrations and plates; any small difficulty that may be experienced in the text is easily overcome with the assistance of these figures. For permission to use many of the diagrams, the author is indebted to several firms of optical instrument makers, and perhaps, under such circumstances, a suspicion of advertisement is unavoidable.

The author has wisely refrained from using a large amount of mathematical work in the body of the book, though, where necessary, it has been introduced. In every case, moreover, the physical interpretation is clearly brought before the reader. Mr. Conrady has contributed two chapters, one on the undulatory theory of light, the other on the various theories of microscopic vision. The theories of Airy, Abbe, Altmann, and Stoney are discussed; and emphasis is laid on the differences between the case of the telescope and that of the microscope, arising from the fact that in the former the objects examined are usually self-luminous, while in the latter they have to be in some way illuminated. The insufficiency of Airy's "spurious disc method" is insisted upon. In a simple, yet fairly comprehensive manner, Abbe's experiments, which at one time were so vigorously attacked—largely, it may be said, on account of the critics' ignorance of the subject—are described, and the work of that physicist vindicated.

Two subjects, somewhat difficult and often neglected in books of this kind, are well treated in the chapters on the numerical aperture, and the testing of objectives. In the latter chapter, among other methods, Abbe's test plate is very clearly described.

One of the most useful chapters, especially for the private student, is that devoted to microscopes and objectives for different purposes. The distinctive features of the instrument as modified for special subjects, such as botany, physiology, etc., are well described and illustrated.

It may be useful to indicate briefly the ground covered by the book. After a short introductory chapter on prisms and lenses, there follow discussions on the simple microscope, the compound microscope with its various fine adjustments, objectives—semi-apochromatic and apochromatic, "dry" and "homogeneous" systems—and eyepieces. These give very detailed accounts, and contain illustrations of the work of the more important makers. Then follow chapters on magnification—the difference between what Abbe termed "empty magnification" and useful magnification is emphasised—substage condensers and diaphragms, methods of illumination (including Siedentopf's method of oblique and dark-ground illumination for ultramicroscopic particles, but, unfortunately, not Cotton and Mouton's simple and convenient modification), the use of the microscope, the binocular microscope, and stereoscopic vision. The rather long chapter on the instrument as modified for use in the particular subjects follows, succeeded by that on the testing of objectives.

The two chapters by Mr. Conrady precede one on accessories, and the last chapter contains a number (thirty-eight) of "hints upon correcting several common faults" met with in using the microscope and its accessories, evidently the work of a skilled and experienced microscopist.

H. THIRKILL.

An Elementary Treatment of the Theory of Spinning Tops and Gyroscopic Motion. By HAROLD CRABTREE, M.A. [Pp. xii + 140, with 3 plates.] (London: Longmans, Green & Co. Price 5*s*. 6*d*. net.)

THIS book is primarily designed "to bring within the range of the abler mathematicians at our Public Schools and of first-year undergraduates at the Universities, a subject which has hitherto been considered too difficult for any but the more advanced students in mathematics." Appearing at a time when investigations of the conditions determining dynamical stability are very much "in the air," this work can be recommended strongly to those concerned with the applications of the gyroscope to practical engineering problems.

The book can be divided roughly into two parts. In the first part (chapters i. to v.) the necessary mathematical equipment of the student has been reduced to a minimum. Most of the phenomena with which we have been familiar since our "spinning-top days" are here shown to be direct consequences of the existence of a gyroscopic torque when the top or gyroscope is free to precess. The author emphasises the necessity for freedom of precession before any gyroscopic resistance is called into play—a fact too often lost sight of by practical engineers. A short treatment of moments of inertia is given. This part might have been extended very profitably. Considering that a large number of the moments of inertia used in subsequent parts of the book are about axes not through the centre of gravity, the inclusion of the theorem relating to change of axes is very desirable. Although a minor point, the distinction drawn in chapter i. between the angular velocity of a point and that of a line is misleading, considering that the velocity is really about an axis in each case. Too great praise cannot be given for the excellent manner in which the author illustrates the importance of gyroscopic forces in various cases. In the fifth chapter a brief presentation of practical applications of the gyroscope is given. Obry's method of directing a torpedo, Schlick's method of steadying a ship, and Brennan's mono-rail are here lucidly described. The plate illustrating Schlick's apparatus gives a good idea of the dimensions of the gyroscope employed.

The remaining portion of the book is concerned with a more complete mathematical treatment of the subject, and maintains the same high level of clearness. Chapters are devoted to the steady motion, general motion, and stability of motion of a top. Chapter viii. is devoted to the deduction of the equations of motion referred to moving axes. The occasional use of the symbol M for the resultant angular momentum—usually designated by h—is to be regretted. The deduction of the component velocities when referred to moving axes is not convincing. We should much have preferred the method in which a vector is resolved along any arbitrary direction, this direction being then made to coincide with each of the axes in turn.

Throughout the work the author insists upon a careful study being made of the physical dimensions of all quantities involved in any equation. An excellent feature of the book is the manner in which the quantities involved in angular motion are compared with the more familiar but similar quantities in linear motion. The book contains an excellent collection of examples, drawn largely from practical applications. Answers are given to all the unworked examples. The diagrams are particularly clear, and the text is practically free from errors. The book is one that will repay careful study, and forms a welcome addition to the literature of this subject.

J. S. G. THOMAS.

An Introduction to the Science of Radioactivity. By CHARLES W. RAFFETY. [Pp. xii+208.] (London: Longmans, Green & Co., 1909. 4*s*. 6*d*. net.)

MR. RAFFETY'S book is, as he modestly describes it, a popular introduction to the subject for the general reader. The student will do better with more authoritative works.

To come to detail; the author would, on p. 7, leave one with the impression that Cornish pitchblende has an abnormally low activity. It has recently been shown that the value for the ratio of radium to uranium content is the same for all pitchblendes. The determination of what has been called the "Thomson Constant"—*e*—the charge on the ion—is dismissed with a few lines (p. 26): the account is so brief as to be nearly useless and unintelligible Page 28 might profitably be rewritten.

Definitions of anode and cathode should have preceded the short account of electrical discharges through gases. Indeed, Faraday's original "sun-rising and sun-setting" definitions would have been interesting and appropriate in a book of this nature. One finds (p. 144) the old and wrong explanation of the rotation of a paddle-wheel subjected to bombardment by cathode rays. The effect is a radiometer one, and is not a measure of the momentum of the cathode ray stream.

Mr. Raffety's views on insulators are not practical: paraffin wax is *not* very good for electroscopes—its natural hygroscopicity, and the almost invariable presence of a residual charge, render it taboo for quantitative work. We read further and find ebonite condemned: we disagree entirely—it is excellent, and has the advantage of being easily workable. Quartz should find a place with sulphur among the best insulators, and reference might well have been made to the ageing and accompanying deterioration of most insulators.

In his treatment of electroscopes the author mentions that weight for weight aluminium leaves are more durable than gold. He does not tell us that when sensitiveness is aimed at there is little to choose between the two so far as weight is concerned, for although gold leaf can be obtained thinner than aluminium leaf, its lesser thickness is counterbalanced by greater density. With the ordinary methods of mounting, the thinner gold leaf holds a considerable advantage by reason of its greater flexibility near the hinge.

All these are minor blemishes, and the volume, for its size, takes a remarkably comprehensive survey of the subject in its most recent phases. Mr. Raffety writes easily and pleasantly, there is a good deal that is meritorious, and this book will, no doubt, find readers who will be encouraged to ask for something more.

G. W. C. Kaye.

The Photography of Coloured Objects. By C. E. Kenneth Mees, D.Sc. [Pp. vi + 69.] (Croydon: Wratten & Wainwright, Ltd., 1909. Price 1*s.* net.)

As is generally known, there is a great difference between the sensitiveness of the eye and of the ordinary photographic plate to light of different colours. By treating a plate with certain dyes its lack of sensitiveness can be partially corrected; but for it to attain the same relative sensitiveness a colour screen must be used in addition to diminish the excessive action of the light of the blue end of the spectrum.

In the first three chapters of this little book, Dr. Mees gives clearly and concisely a general introduction to the principles underlying the photography of coloured objects, and in the subsequent chapters discusses the application of these principles to the more important branches of work. The chapters on "photography of coloured objects for reproduction," "landscape photography," and "portraiture" are contributed by authorities in these particular branches of photography.

The author also explains how, by the use of appropriate colour-screens and panchromatic plates, it is possible to overcome difficulties such as the rendering in monotone of colour contrasts in the copying of pictures and in microphotography, also the photography of subjects having a uniform colour, such as articles of furniture. A method of suppressing certain colours is described, the example being the entire elimination of red ink corrections from a typewritten sheet. The great advantage that can be effected by the special treatment of these particular cases is strikingly shown by the illustrations. Finally, a chapter is devoted to the theory underlying the processes of three-colour photography.

This work will no doubt prove both interesting and instructive to all concerned in the subject of photography.

A. E. Andrews.

The Theory of Valency. By J. Newton Friend. [Pp. ix + 180.] (London: Longmans, Green & Co., 1909. Price 5*s.* net.)

This treatise, which is one of the series of chemical text-books edited by Sir William Ramsay, contains a concise and very readable summary of the views which have been held at various times concerning valency; it is moreover the only

English work devoted entirely to this subject. In the opening chapters the early theories of chemical combination are passed in review with special reference to the parts played by the doctrines of constant and variable valency.

The importance of the periodic classification as a guide to valency may be gauged by the fact that the author devotes twelve chapters to a consideration of the habits of combination of the elements arranged in their natural families. The evidence collected in support of the views expressed is generally accompanied by references to the original papers. This bibliography will greatly enhance the value of the book to the serious student, who will also read with appreciation the author's exposition of Werner's theory of valency.

Under the heading of electro-chemical theories the author deals with the ideas put forward from time to time regarding the nature of valency. The modern electronic theories of valency are discussed, and the author elaborates his own views, which afford an alternative explanation of the constitution of the metallic ammines studied by Werner and others.

The concluding chapter gives a brief outline of one or two of the most important theories other than those based on electrical hypotheses. It is to be regretted that Barlow and Pope's theory is not discussed in greater detail, inasmuch as this conception, unlike all the others, is based on crystallographic considerations.

The work may be recommended as a useful introduction to the present knowledge of the phenomenon of chemical valency.

G. T. MORGAN.

Rational Immunisation in the Treatment of Pulmonary Tuberculosis and other Diseases. BY E. C. HORT, B.A., B.Sc., M.R.C.P. [Pp. 75.] (London: John Bale, Sons, & Danielsson, Ltd., 1909. Price 3*s*. 6*d*. net.)

UNDER this title the author gives a sketch of natural recovery from bacterial infections, and treatment by means of artificial inoculation, together with an account of autolysis, enzymes and antienzymes, and cognate matters. He holds that auto-inoculation involves inoculation against the products of tissue-change present in bacterial lesions, and that this is neglected in vaccine therapy.

He contends that the presence or absence of auto-inoculation and the presence or absence of response are both indicated in temperature charts in febrile cases, which are thus a reliable register of the progress of immunisation. Charts in support of this view are reproduced, chiefly from cases of phthisis.

Advantages are claimed for auto-inoculation over vaccine therapy, the chief being that the former is based on the natural model, that it deals with the products of tissue-change, that the right organisms are employed, and that it is easier to regulate dosage.

The valuable suggestion contained in this book is that in auto-inoculation action is taken against the products of tissue-destruction which result from bacterial infections. The details of the accounts of immunisation and auto-inoculation are open to criticism, and the language throughout is somewhat obscure.

The interpretation of temperature charts is a highly controversial subject, and even if the author's view be accepted a chart appears to be more interesting as a record than useful as a guide.

With the views expressed on the advantages of auto-inoculation we are only in partial agreement. Because auto-inoculation occurs in Nature and frequently leads to recovery it does not follow that it is the best artificial method. The advantage of using the products of tissue-change and of the certain use of the correct organisms is clear, but we respectfully demur to the suggestion that dosage is more easily regulated. Auto-inoculation calls out a quite unknown dose, and that too of living organisms.

We gather that in the author's opinion rational immunisation consists in leaving patients alone who are doing well, and employing auto-inoculation with the others, especial care being required with patients who are going down hill.

D. W. CARMALT JONES.

On the Poison of Venomous Snakes and the Methods of Preventing Death from their Bite. Reprinted papers by the late SIR JOSEPH FAYRER, Bt., K.C.S.I., M.D., F.R.C.P., F.R.S.; SIR LAUDER BRUNTON, Bt., LL.D., M.D., F.R.C.P., F.R.S.; and MAJOR LEONARD ROGERS, I.M.S., M.S., F.R.C.P., F.R.C.S. [Pp. 174.] (London: Macmillan & Co., Ltd., 1909. 2*s*. 6*d*.)

IT is to Sir Lauder Brunton that the reissue, in book form, of this important series of six pioneer articles, originally published between 1873 and 1904 in the *Proceedings of the Royal Society*, is immediately due. Five of the papers are the joint production of Fayrer and Brunton, and the remaining one that of these writers and Rogers, by whom the later experimental work was carried out.

The first two deal particularly with the effects of cobra- and krait-poison, and with that of the Indian viper, *Daboia Russellii*. In the third the venom of the American rattlesnake is compared with the above-mentioned Indian snake-poisons, both on animals and on various forms of animal and vegetal protoplasm. Pulsation of the *Venæ cavæ* and pulmonary veins, independently of the heart, is the subject of the fourth article. This had been noted, in the second paper, in animals killed by cobra-poison, and is here shown to arise under other circumstances. The fifth article deals with some antidotes to cobra bite, viz. platinic chloride, chloride of gold, and permanganate of potassium, all of which are shown to be chemically, rather than physiologically, antagonistic to the poison.

Nearly thirty years elapsed between the appearance of the fifth and sixth papers. During this interval the value of permanganate of potash had been redescribed, and re-emphasised by Couty and Lacerda, and by Vincent Richards.

The sixth paper describes work by Leonard Rogers, with an ingenious instrument due to Lauder Brunton, on the antidotal value of the permanganate which had been described by Brunton and Fayrer in 1878, and still earlier by Fayrer in his work on the Thanatophidia of India. These experiments, carried out in the Physiological Laboratory of the University of London, confirmed and extended the original finding, showing that, *in vitro*, the drug destroys most (if not all) snake venoms. *In vivo*, circumstances restricted the work to the venoms of two snakes—the cobra and the krait. The venom was injected into the part most frequently bitten—a limb—the limb bandaged above the injection, the site of the injection freely incised, and crystals of the permanganate rubbed well into the wound. The results are set forth in tabular form, and show *inter alia* the dose of poison per kilo body-weight, and the influence of delay between inoculation and treatment. They are, as claimed, most encouraging and, if less striking than those yielded under favourable circumstances by antivenomous sera, point to an exceedingly valuable "first aid," owing to the stability and antiseptic action of the drug and the extreme simplicity of the apparatus and procedure.

The work is of great interest and value both to physiological and clinical students of snake poisons, and is rendered doubly accessible and helpful by the very complete index which the issue of this volume has made possible.

W. L. SYMES.

Studies in Fossil Botany: Vol. II. Spermophyta. By D. H. SCOTT, M.A. Ph.D., F.R.S. [Pp. xiv + 355-676, 85 illustrations.] (London: Adam & Charles Black. 2nd Edition, 1909. Price 5*s.*)

THIS volume completes the second edition of the author's well-known *Studies*, the first half of which appeared last year and was reviewed in SCIENCE PROGRESS of July 1908. Our knowledge of the plants with which it is concerned, more especially the Pteridosperms and Bennettiteæ, has been so greatly extended during the last decade, that this section, as the author points out, has "required an even more drastic revision than Volume I., and is, to a great extent, a new book."

The first two chapters are devoted to the Pteridosperms. They contain a full and thoroughly up-to-date account of the anatomy and fructifications of this interesting group, from the pen of one who, perhaps more than any other, is especially well qualified to deal with these matters. It would indeed be difficult to suggest any improvement in the manner of presentation of the facts and inferences discussed in this section.

Passing next to another Palæozoic group, the Cordaitales, the author presents a greatly amplified account of these plants as compared with that found in the first edition. We notice a brief mention and figure of the first British *Poroxylon* as yet undescribed, in which connection between the stem and leaf-bases can for the first time be demonstrated.

The author has also placed side by side with the drawings of the ovule and pollen-chamber of *Cordaianthus*, Griffiths' figures, first published in 1852, of the same organs of *Cycas*, which afford a striking resemblance to the fossil seed.

The last group discussed is the Mesozoic Gymnosperms, especially the Bennettiteæ, an excellent summary of Wieland's recent work on American specimens being included. This section is however open to serious criticism as regards nomenclature. As is well known to palæobotanists, a difference of opinion at present exists between European workers and their American brethren, as to whether certain fossils should be called *Bennettites* or *Cycadeoidea*. It is not necessary to enter here into the arguments, and it is obvious that Dr. Scott has wished to avoid any expression of opinion as to which name is to be preferred. With this intention, apparently, he uses both names for fossils which admittedly belong to the same genus, his choice being dependent on the particular locality from which a certain specimen was derived. We fancy that this compromise will puzzle the reader and form a serious difficulty to the student, despite the author's footnote on p. 578. Undoubtedly it detracts from the clearness of the account given of this group. Whichever name may be adopted, even provisionally, it should be used uniformly throughout.

The concluding chapter is devoted to a masterly survey of the whole field covered by the two volumes, especially in relation to phylogenetic problems. The author adopts a threefold primary classification—the Sphenopsida, Lycopsida, and Pteropsida—and the review of these groups will prove of the greatest value to botanists, whether their researches lie towards the "living" or the "fossil." As regards this section, it would hardly appear that the author, in discussing the bearing of our present knowledge of the Bennettiteæ on the problem of the origin of Angiosperms, has sufficiently emphasised the fact that the whole question really turns on what our conception of the primitive type of Angiospermous flower may be. It is obvious that if the flowers of *Casuarina* and the Piperaceæ are really primitive, the discovery of the amphisporangiate strobilus of *Bennettites* has little or no bearing on this problem. It is only if we can show that such flowers as those of *Liriodendron* and other members of the Ranales have retained the larger number of archaic features, that the Bennettitean strobilus has any special significance in this direction.

The completed *Studies*, as we now have them, form undoubtedly the best introductory text-book to Fossil Botany in existence from the botanical standpoint, although it makes no pretence of covering the whole ground. It has been rewritten with all the author's well-known charm, clearness and accuracy. The printing is careful, the type excellent, and the wealth of illustrations generous, no less than thirty-six new figures appearing in this volume alone.

E. A. Newell Arber.

Œcology of Plants: an Introduction to the Study of Plant-communities. By Eug. Warming, assisted by Martin Vahl. Prepared for publication in English by Percy Groom and Isaac Bayley Balfour. [Pp. xi + 422.] (Oxford: Clarendon Press, 1909. Price 8*s*. 6*d*. net.)

About ten or twelve years ago, the Delegates of the Oxford Press announced their intention of publishing a translation of Warming's *Plantesamfund*. The promise has not been fulfilled: and English students have become familiar with Warming's work in the German translation, under the title *Œcologische Pflanzengeographie*, by Knoblauch. In default of the promised translation, there now appears the present volume, which, states Warming in the author's preface, is practically a new work. It has evidently been written in English by Warming; and to the English manuscript Dr. Groom has, we are informed by Prof. Balfour in a note, "applied with untiring patience his skill in interpretation and in apt expression."

Although British ecology is, at the present time, in a flourishing condition, its early inspirations come from continental botanists. The influence of Flahault, through his pupil Robert Smith, is well known; and, whilst no British ecologist appears to have ever studied under Warming, his influence on the growth of ecology in these islands has been no less marked.

At the time of its publication, Warming's earlier work was the only text-book on the subject. "When I wrote it," says Warming, "I had no models to study." It must be stated, however, that every subsequent author has been able to find in Warming's *Plantesamfund* an excellent model of what a text-book should be. Simple, clear, and logical, it has had enormous influence. It set out four great types of vegetation; and based these on the water-content and to some extent on the mineral content of the soil. The four types were Hydrophytes, Xerophytes, Halophytes, and Mesophytes. Later writers have criticised, and some have severely criticised this classification; and the open-mindedness of Warming is seen in various references to these criticisms. For example, Warming now emphasises the view that "a halophyte is in fact a special form of xerophyte, as Clements repeatedly urges, and Wiesner and Schimper recognised" (p. 134). Whilst Warming, however, has evacuated certain of his earlier positions, he has not dismantled them; for we read (p. 228) that "there is always a question" with regard to the vegetation on sandy sea-shores "as to whether this formation must be regarded as halophilous or psammophilous." The importance of the recognition of physiological dryness co-existing with physical wetness is insisted on throughout the book, as it was by Schimper. Hence peat-bog plants are no longer treated under hydrophytes; though, even in his earlier work, Warming stated quite fairly the relations of the structure of what have been termed "bog xerophytes" to their habitat. Warming, however, still retains his term "mesophytes," and refuses to admit what many ecologists regard as indisputable, that Schimper's term "tropophytes" (*i.e.* plants which are hygrophilous in summer and xerophilous in winter) represents an advance on previous conceptions. Warming also reiterates his contention that the physical characters of the soil are more important than the chemical.

In Warming's present work, thirteen ecological classes are given. The classification of the major units of vegetation is undoubtedly less simple and clear than the earlier one, and it is more than doubtful whether the loss in simplicity and clearness is counterbalanced by any gain in accuracy. The attempt to place every plant in a verbal class with the termination "-phyte" is probably foredoomed to failure; for, as Schimper has shown, many plants come in one such class during one period of the year and in another such class during another period; and, as Warming himself has shown, certain plants possess some structures characteristic of one such class and other structures characteristic of another. Even in the present scheme, certain plants come in two of the classes. The class Chersophytes is hopelessly unnatural, including such diverse plant communities as Alpine "meadows," "Montenegrin expanses of poor grassland occurring on stony ground," "alvar-vegetation" of Sweden, a peculiar type of "waste herbage" in Madeira, and "bushland on dry soil," such as "Hippophaëta" and "similar thorn bushland," Chodat's "garide," "palm-bushland," and "fern-heath," which is "likewise a kind of bushland and is produced by the widespread bracken-fern"! "Coniferous formations," as an ecological class, cannot possibly be admitted; for it represents a confusion of the floristic and ecological points of view, and a wholly unnecessary confession of the failure of the application of certain definite ecological principles. The last class, "mesophytes," would appear to be indefinable except in terms of itself. To the reviewer, it is clear that a formal classification of the major units of vegetation on the lines attempted by Warming and Vahl is either impossible or premature. In spite of this defective classification, there is under each class a very valuable discussion on the nature and requirements of various sub-classes, in addition to the more general treatment of "growth-forms" (the "plant-forms" of some authors) and of ecological factors in the first three sections of the book. The book, like its predecessor, concludes with a section on the struggle between plant-communities, in which the author briefly states his opinions concerning the origin of species.

The terminology used in this book with regard to the various plant-communities is most unfortunate. For this terminology Warming probably cannot be held wholly in fault. How many British botanists or even British ecologists could state the significance of the following terms: "Elfin-scrub," "fell-field," "fell-heath," "fern-heath," "grove-dell-formation," "high-moor," "low-moor," "orchard-scrub," "sand-field," and "succulent steppe" (*sic*)? Most of these terms can only be regarded as *quasi* equivalents obtained by the simple process of literal translation. "Skill in interpretation and in apt expression" does not here force itself obtrusively on the reader's mind. It may be fully acknowledged that this matter of ecological nomenclature is a difficult one, particularly with regard to the finding of good equivalents in different languages. It may, however, be suggested that the formulation of a terminology suited to the vegetation of this country is not likely to be successfully attempted except by those who are actually working at the problems presented by the vegetation in question. Several terms already in general use in this country have been ignored. It is highly probable that the great majority of the peculiar terms of this book have been still-born ; but even so, the attempted introduction into the language of a large number of clumsy technical terms is to be regretted.

An attempt is made in the introductory note to justify the very frequent omission of references to recent British work ; but the reasons stated do not serve to explain some rather serious mis-references. The citation of Yapp on page 217 is ridiculous : the context refers to "Hochmoor," whilst Yapp's paper only has reference to "Niedermoor." Similarly, the citation of R. Smith, on p. 326, as an authority on pasture on cultivated soil, is plainly in error ; for R. Smith was describing mainly land not under cultivation. Woodhead has not described "an association of Pteris with *Holcus lanatus*" (see p. 146). Many of the references to British papers are to points of trivial importance, and seem to indicate that these papers have not been considered as a whole. Marcel Hardy's interesting and useful papers are completely ignored, doubtless by an oversight. Darbishire's paper on Mamillaria should have been mentioned on p. 118 ; and there are many other omissions of similar papers.

Many statements in the general body of the work require qualification. The following are a few such statements : pasture in the tropics "is always artificial" (p. 327) ; hochmoor "is mainly formed by bog-moss (Sphagnum)" (p. 200) ; "moor-soil is probably always acid" (p. 196) ; Hochmoor peat "is rich in air" (p. 204) ; "the beech is incapable of natural regeneration" (p. 363) ; the Juncoid leaf, seen in species of Juncus, "is devoid of furrows" (p. 111) ; "wet soil is cold, and therefore physiologically dry" (p. 195).

In spite, however, of many defects, the book must always form a portion of the equipment of British ecologists, who are extremely fortunate in being placed in possession of a book written in their own language by such a master as Prof. Warming. The book contains a very lengthy index and an almost complete bibliography. There are several more or less serious misprints, especially with regard to the citation of British authors and papers ; and the very frequent use both of italics and of the interpolation of synonyms in brackets are out of place in a book of this nature. Apart from these points, the book is well got up ; and, there being no illustrations, the price has been kept commendably low.

C. E. Moss.

FACTORS WHICH DETERMINE FERTILITY IN SOILS

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The profound effect on the fertility of soils which is exercised by their organic contents is not yet sufficiently appreciated. When plants die, they fall back on the soil as decaying masses of substances of varying stability, and give rise to products many of which are highly complex in character: some of the products, the simpler ones in particular, are of great value as plant food whilst others may be deleterious.

Undecomposed plant residues, leaves, stems, etc., "open up" the soil if present in any quantity, destroying that continuity of the soil masses on which a satisfactory supply of water and a proper root range for the plant depend. If the soil mass be continuous, water tends to distribute itself uniformly; if it be discontinuous, one part may be too wet while another is too dry. As the organic matter is decomposed, not only does this detrimental mechanical effect cease but a distinctly beneficial effect is exercised. One of the products—the black substance or mixture of substances known as humus, to which the colour of a rich garden soil is due—has the remarkable property of increasing the water-holding capacity of the soil, thus reducing the loss by drainage, which is one of the most serious of soil losses. Humus also facilitates the production of the fine crumbly condition or "tilth" essential for successful cultivation.

The effect of the decomposition of organic matter is therefore that the productiveness of the soil is increased by the conversion of useless or even harmful substances into others which are valuable either as plant food or as agencies for improving the texture and water-holding capacity of the soil. The process of change has always interested agricultural chemists. In early years it was thought to be purely chemical in character—a slow combustion or "eremacausis." Even after Pasteur had demonstrated the presence of micro-organisms in the soil, and

Boussingault, in 1859, had recognised the existence of a "mycodermic vegetation . . . not always visible to the naked eye, the progress of which must be followed by aid of a microscope," there was no suspicion that this "vegetation" was in any way connected with soil fertility. Not till the nature of putrefaction was elucidated and the old doctrine of spontaneous generation finally disproved, was it possible to make much advance. Soil bacteriology may be said to date from 1878, when Schloesing and Müntz observed a delay of twenty days in the commencement of nitrification in an artificial soil in contact with sewage, and argued that nitrification must therefore be a biological, not a chemical, process. The new hypothesis was put beyond question by Warington and in course of time the suspected organisms were actually isolated. A vast amount of subsequent work has shown that the decomposition of plant remains in the soil is mainly the work of micro-organisms and has also revealed to some extent the broad outlines of the change.

It is necessary at the outset to appreciate the conditions under which decomposition goes on in the soil. The greater part of the soil is simply inert mineral matter: the decomposable part is only small; normally there is not more water, frequently there is less, than can be held by surface forces on the particles. In a not unusual case the soil consists of about 80 per cent. of inert mineral matter, 15 per cent. of water, and 5 per cent. of organic matter, only part of which, however, can be readily decomposed. The top six inches of soil thus constituted is inhabited by a teeming population of the most varied kind, from large earthworms down to organisms only 0·5 μ in length. Bacterial counts show that some millions are present in each gram of soil. The micro-organisms show great diversity in their food requirements, their mode of life and in the way they are influenced by external conditions. All are competing in the struggle for existence; multiplying with enormous rapidity whenever the conditions are favourable, disappearing equally quickly when they are unfavourable or changing into spores which may lie dormant during long periods and yet revive as soon as the conditions are again suitable. In the struggle, no one species exterminates the rest; instead the different forms seem to settle down to a rough kind of equilibrium, each being hampered by others yet each surviving in some degree; apparently the equilibrium does not vary much so long as the soil conditions remain fairly constant.

The general outline of the changes, so far as they have been traced out, is somewhat as follows. The organic matter is finally resolved into carbon dioxide, water, nitrogen, ammonia, calcium carbonate and other mineral matter, as well as into more or less stable organic substances which tend to accumulate in the soil. Decomposition takes place slowly, however, a number of intermediate products of the humus type, of varying degrees of stability, being found; the low temperature and rather special conditions apparently favour unstable molecular groupings. The most remarkable feature of the decomposition and the one most in need of further elucidation is the evolution of considerable quantities of gaseous nitrogen.

Normally ammonia does not remain as such in the soil but is either absorbed by some of the clay constituents to form a curious compound not yet investigated, or it is oxidised by bacteria to nitrite, and finally to nitrate. The nitrifying organisms derive their carbon not from organic matter, which indeed is rather injurious to them, but from carbonic acid, which they assimilate and convert into complex cell substances without the aid of sunlight or the intervention of chlorophyll; apparently they utilise the energy set free by the oxidation of ammonia.

Another set of organisms possesses the remarkable property of absorbing gaseous nitrogen from the air and converting it into protein. A considerable amount of energy is of course necessary and is derived from the oxidation of organic matter. Between this fixation of nitrogen and the liberation of nitrogen already mentioned there is generally an equilibrium. The steps in these changes are entirely unknown.

There are three general methods of studying the effects of these changes on soil fertility. The separate changes may be traced out; the effect of the whole series of changes may be estimated; the micro-organic flora may be altered and the results observed. All are indirect; unfortunately no direct means of studying the separate bacterial processes at work in the soil is yet available. It is impossible at present either to make soil artificially or to reconstruct the bacterial flora in a sterilised soil; it is even impossible to sterilise soil without profoundly altering its character.

The first method has been developed along the lines of selective cultures adopted by the earlier investigators in isolating soil organisms. A solution, made to favour one group of organisms, is inoculated with a sample of soil; the favoured group develops while the rest die or form spores; finally a fairly pure culture may be obtained. Thus in working with the nitrogen-fixing organisms the culture solution used is one containing sugar, potassium phosphate and calcium carbonate but no nitrogen compound; organisms incapable of utilising gaseous nitrogen are therefore unable to develop. The solution used in studying nitrifying organisms contains an ammonium salt, phosphates, etc., but no organic matter; whilst in investigating the decomposition process it is customary to use a solution of a highly nitrogenous organic compound such as peptone. The results obtained are very valuable but they throw more light on the morphological and physiological characteristics of the soil organisms than on the changes actually going on in the soil.

A study of the whole resultant effect gives the kind of information wanted for some fertility problems without, however, the detail necessary for further developments. The total chemical change may be ascertained by determining ammonia and nitrates in the soil. The total bacterial activity has been estimated in two ways. Hiltner and Störmer count the number of colonies developing on gelatine or agar plates inoculated with known weights of soil and reduce the results to numbers per gram of soil. The method fails to discriminate between spores and active organisms in the soil and also takes no account of organisms active in the soil which do not develop on gelatine plates; nevertheless it gives very useful results. The second method, devised by the writer, consists in measuring the rate at which oxygen is absorbed by the soil. The desirable changes are oxidations or are brought about by aerobic organisms, and the rate at which they take place is therefore some function of the rate at which oxygen is absorbed. These rates for a series of comparable soils are found to run in the same order as the relative productiveness.

The third method, which has been used in the Rothamsted laboratories by Dr. Hutchinson and the writer, is really a combination of these two. A change is induced in the micro-organic flora of the soil, usually by partial sterilisation with toluene or by heat. The alteration in the flora and the change in the course of the soil decomposition are observed and, as far as may be, correlated.

The Effect of Partial Sterilisation

The earliest observations that soil is altered by an apparently inert antiseptic arose out of attempts to kill insect pests in the soil by means of carbon disulphide. This substance, which for fifty years has been known as an insecticide, was used in 1877 by Oberlin, an Alsatian vine-grower, to kill phylloxera; Girard used it in 1887 to clear a piece of sugar-beet ground badly infested with nematodes. In both cases the subsequent crops were such as to show that the productiveness of the soil had been increased by the treatment. The experiments appear to have been made quite independently and the results were published in 1894; they naturally attracted a good deal of attention, and similar trials were made by a number of investigators.

The first piece of scientific work came from A. Koch in 1899. He worked with varying quantities of carbon disulphide, and concluded that it stimulates the plant root to increased growth. Four years later Hiltner and Störmer showed that the bacterial flora of the soil undergoes a change. The immediate effect of the antiseptic was to decrease by about 75 per cent. the number of organisms capable of developing on gelatine plates; then, as soon as the antiseptic had evaporated, the numbers rose far higher than before, and there was also some change in the type of flora. They argued that the increased numbers of bacteria must in any case result in an increased food supply for the plant, and they further claimed that the new type of flora was actually better than the old in that denitrifying organisms were killed, nitrogen-fixing organisms increased, and nitrification only suspended during a period when nitrates were not wanted and might undergo loss by drainage. In a later publication Hiltner shows that other poisonous substances which can be removed from the soil behave like carbon disulphide. The important part of this work is unquestionably the discovery that the organisms in the treated soils ultimately outnumber those in the original soil. The hypothesis that the new type of flora is actually more efficient than the old rests on less trustworthy evidence, and has indeed been modified in some of its details by Hiltner himself.

The effect of heat on the productiveness of the soil was first noticed by the early bacteriologists. It had at first been assumed that heat simply sterilised the soil and produced no other change, until Frank in 1888 showed that it increased the soluble mineral and organic matter and also the productiveness. Later work by Pfeffer and Franke and by Krüger and Schneidewind showed that plants actually take more food from a heated than from an unheated soil. Heat undoubtedly causes decomposition of some of the soil constituents quite apart from its effect on the soil flora.

Experiments by Dr. F. V. Darbishire and the writer at Wye showed that the rate of oxidation was considerably reduced after the soil had been heated to 130° C., but was more than doubled after it had been heated to 100° C.; it was also increased by treatment with small quantities of volatile antiseptics. It follows from what has already been stated that the bacterial activity is increased and consequently the amount of decomposition. The increased quantity of plant food thus formed is shown by the amounts taken up by the plant: the following is a typical series of results:

	Dry weight of crop, grams.	Percentage composition of dry matter.			Weight of food taken by the plant from soil, grams.		
		N.	P_2O_5.	K_2O.	N.	P_2O_5.	K_2O.
Buckwheat.							
Untreated soil	18·14	2·75	1·87	5·62	·499	·339	1·019
Soil treated with carbon-disulphide	23·27	3·15	2·34	5·97	·733	·544	1·389
Mustard.							
Untreated soil	15·88	2·30	1·00	4·20	·367	·159	·668
Heated soil	24·33	4·43	2·08	5·02	1·077	·506	1·221

Fig. 1 shows photographs of wheat plants grown in heated soil and in soil treated with toluene.

The process has been more fully traced out in experiments made at Rothamsted in conjunction with Dr. H. B. Hutchinson. The most striking change that sets in after partial sterilisation is an accumulation of ammonia as shown in fig. 2. Nitrification ceases, but the ammonia is much in excess of the sum of the

a *b* *c*

Fig. 1.—Wheat grown in (*a*) heated soil, (*b*) untreated soil, (*c*) soil treated with 0·25 per cent. of toluene, which was subsequently allowed to evaporate.

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ammonia and nitrate in the untreated soil. The total nitrogen as ammonia and nitrate found at the beginning of an experiment,

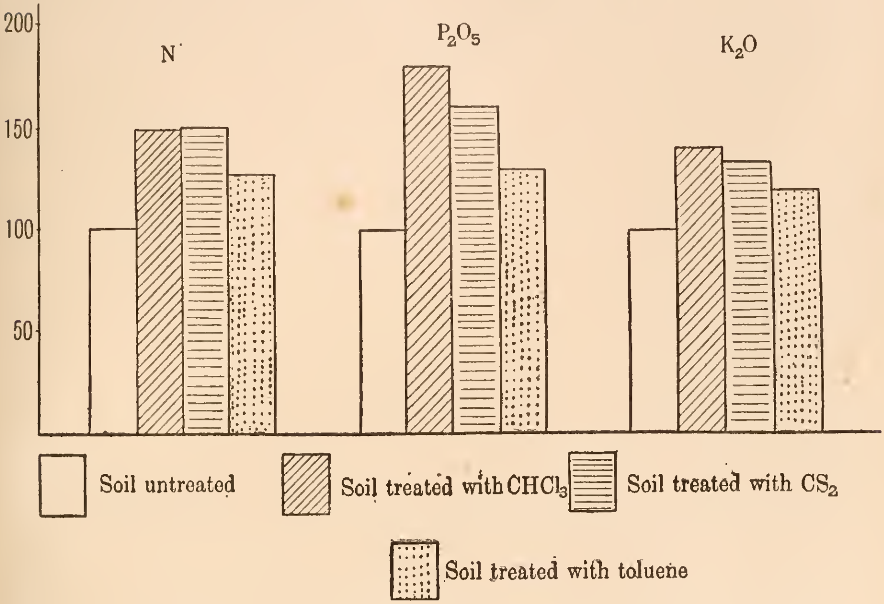

FIG. 2.—Relative amounts of plant food taken by buckwheat from variously treated soils.

and after the soil had been kept twenty-three days at about 15° C. in a moist condition was:

	Nitrogen present as ammonia.		Nitrogen present as nitrate.		Total nitrogen present as ammonia and nitrate.		
	At beginning.	After 23 days.	At beginning.	After 23 days.	At beginning.	After 23 days.	Gain in 23 days.
Untreated soil	1·8	1·7	12	16	13·8	17·7	3·9
Soil heated 2 hours to 98° C.	6·5	43·8	13	12	19·5	55·8	36·3
Soil treated with toluene, which was then evaporated	5·0	27·8	12	12	17·0	39·8	22·8
Soil treated with toluene, which was left in	7·2	14·5	11	10	18·2	24·5	6·3

The results are expressed as parts per million of dry soil.

The accumulation of ammonia might be due either to an increased production in the treated soils or to the removal by the treatment of some agent, other than the nitrifying organisms, which is always consuming ammonia. The second supposition falls to the ground, because when small quantities of ammonium salts are added to untreated soils the whole of the added nitrogen is recovered as ammonia and nitrate. Hence we must conclude that the treatment has induced an increased *production* of ammonia.

Several considerations show that the production of ammonia subsequent to the small initial gain on heating or treating with toluene is mainly the work of micro-organisms. The curves belong to the type associated with bacterial, rather than purely chemical, change. Soil which has been heated to 125° C. (at which temperature all organisms are killed) shows no increase in ammonia-content after the first small gain. There is no rapid period of gain if enough toluene is left in to inhibit bacterial action nor if the water supply is insufficient. There may be some enzyme action—indeed the result of leaving toluene in suggests that there is—but the chief factor is clearly bacterial activity.

Further evidence was obtained by counting the number of bacteria in the various soils that develop on Koch's gelatine plates. The numbers given below show that the bacteria increase *pari passu* with the amount of ammonia; we may therefore associate the increased production of ammonia with the increased numbers of bacteria. Direct experiments showed that addition of ammonium salts to unheated soil led to no such increase as is observed here.

	Number of organisms per gram of dry soil, in millions.			Ammonia produced in 9 days, in parts per million of dry soil.
	At beginning.	After 9 days.	Increase during 9 days.	
Untreated soil	6·7	9·8	3·1	0·7
Soil heated to 98°	·0003	6·3[1]	6·3[1]	3·2[1]
Soil treated with toluene which was subsequently evaporated	2·6	40·6	38·0	17·1
Soil treated with toluene which was left in	2·3	2·6	0·3	5·5

The new flora arising after partial sterilisation was found to be more active than the original flora in effecting the decomposition of nitrogenous organic matter such as peptone and in hydrolysing urea. But there is no evidence that the species

[1] After 4 days; 9 days count lost by plates liquefying.

surviving the treatment have become more active—indeed the contrary is rather the case, since organisms isolated from the partially sterilised soils proved less active than others of the same kind from the untreated soil. Nor can the difference in the rate with which ammonia is produced be attributed to a change in the type of bacterial flora. Examination of the gelatine plates showed that the flora which becomes established in the soil after heating is altogether different from that originally present; on the other hand, the flora of the soil treated with toluene did not appear to have altered very much. The curves

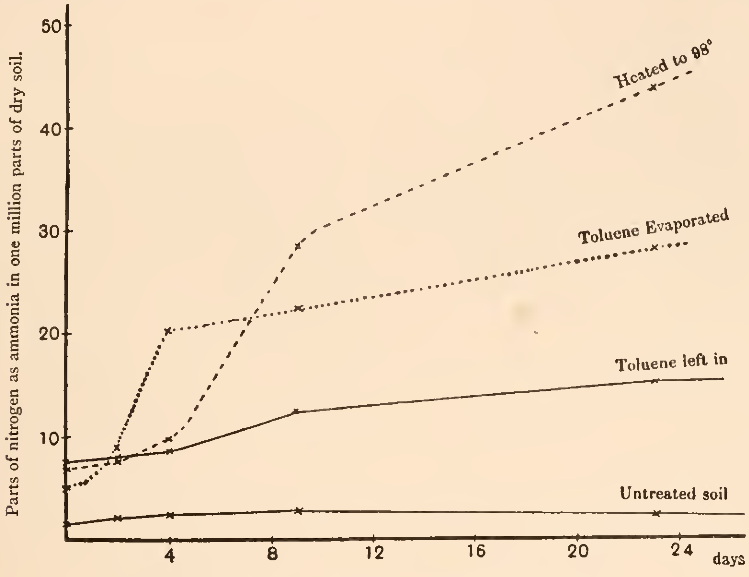

FIG. 3.—Amount of ammonia in variously treated soils.

showing the amount of ammonia produced in the soil treated with toluene and in the heated soil (fig. 3) are very much alike but the bacterial flora of the soils is very different; the curves for the untreated soil and the soil treated with toluene are fundamentally different, whilst the bacterial flora is not. Our experiments indicate that the increased production of ammonia in the partially sterilised soil is due to the increased numbers of the bacteria rather than to any other cause; the problem reduces itself to finding out why the bacteria can increase so much more rapidly in the partially sterilised than in the untreated soils.

Attempts to bring the partially sterilised soil back to its original condition by inoculation with organisms from the untreated soil led to some remarkable results. Addition of *small* quantities (0·5 per cent.) of untreated soil or of a filtered aqueous extract of the soil containing bacteria, considerably *increased* the amount of decomposition and the total number of organisms. But addition of *large* quantities (5 per cent.) of untreated soil gave no such increase; instead the numbers were reduced much more than if the added soil were a mere inert diluting mass; they were actually *halved.* The depressing effect was not shown at once but only after the lapse of some time.

	Gain in ammonia and nitrate in 57 days.	Numbers of bacteria, in millions per gram of dry soil.		
		After 20 days.	After 38 days.	After 61 days.
Toluened soil alone	24·3	28·0	31·8	60·1
,, ,, + extract from untreated soil . . .	43·7	61·3	45·2	166·6
,, ,, + 5 per cent. untreated soil . . .	20·3	32·0	46·9	48·0

The result of the small inoculation is what might be expected from the enfeeblement of the organisms brought about by partial sterilisation. But the effect of the addition of 5 per cent. of untreated soil (fig. 4) can only be interpreted as showing that something actively detrimental to bacteria has been introduced. The conclusion may be drawn that the untreated soil contains a factor limiting the development of bacteria, this factor being put out of action by toluene or by heat.

Since the reduction in the rate at which ammonia is produced is not at once operative when untreated soil is added to soil treated with toluene, we may infer that the limiting factor is probably biological; this conclusion was strengthened by other experiments. The factor does not appear to be bacterial, since its effects do not show in the aqueous extract of the soil; and it does not come into evidence in proportion as the bacteria develop. Search was therefore made for larger organisms such as infusoria, amœbæ and other protozoa. None were found in the heated soil and only small ciliate infusoria in the soil treated with toluene. But the untreated soil contained a variety of them; and some half-dozen have already been picked out by Dr. Hutchinson. Ordinary soil, indeed, seems to have quite a large population of protozoa which have been hitherto overlooked. All such organisms existing in active forms must be severe competitors with the soil bacteria by reason of their large size. Some, *e.g.* *Colpoda cucullus* and *Amœba nitrophila*, are known to devour bacteria; Colpoda seems particularly active. Dr. Hutchinson has found

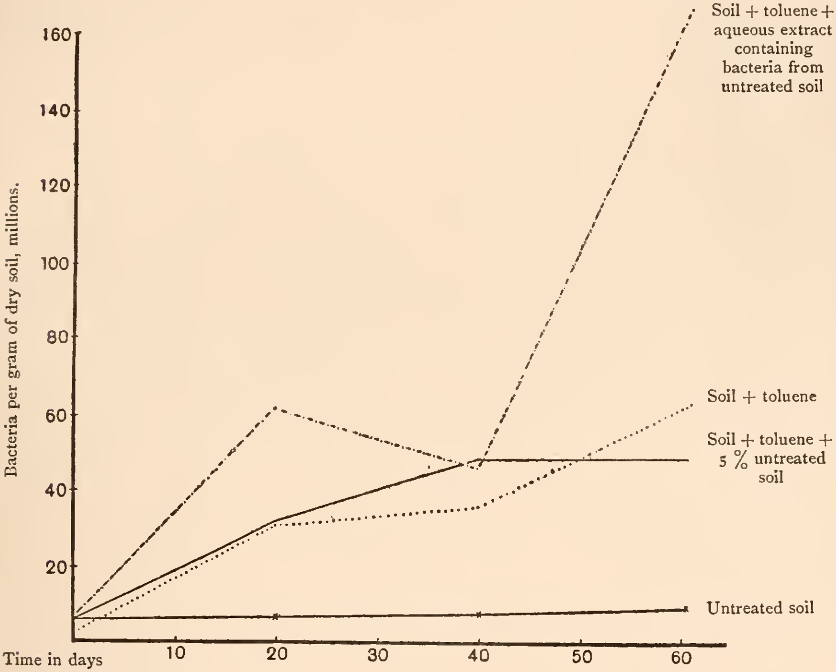

FIG. 4. Effect of untreated soil, and of aqueous extract containing bacteria from untreated soil, on the bacterial activity in soil treated with toluene.

these organisms in every soil so far examined: their wide distribution may also be inferred from their common occurrence in hay infusions. The evidence at present available goes to show that these large organisms constitute the factor or one of the factors—for there may be others—limiting bacterial activity and therefore fertility in ordinary untreated soils.

Further studies of these organisms in their relation to the changes in soils are now in progress. Experiments are also in hand to discover whether these organisms or the inhibitory factor can be suppressed in ordinary field soils by any economical and practicable method.

It is possible in the light of these results to form a clearer picture of the microscopic life of the soil. The micro-organic flora of an ordinary soil is very mixed, including as it does a wide variety of organisms performing very different functions. They may be divided roughly into two classes: saprophytes which live on and effect the decomposition of organic matter, and a class comprising (*a*) phagocytes which consume living bacteria, (*b*) large organisms inimical in other ways to bacteria. The action of the saprophytes tends to increase the fertility of the soil, *e.g.* they produce ammonia, fix nitrogen and so on. It is true that some of them cause a liberation of nitrogen during the decomposition of organic matter and are to this extent injurious. Such action, however, is either much restricted or is counterbalanced by fixation processes. On the other hand the phagocytes and similar organisms are detrimental to fertility, because they limit the number of bacteria and therefore the rate at which ammonia is produced.

Between these two classes of organisms there is an equilibrium under natural conditions; the bacteria cannot multiply indefinitely but are kept in check by the phagocytes, which, in turn, are kept down by the limited amount of food, water, etc. In these circumstances bacteria effect only a limited amount of decomposition—much less, in fact, than might be expected from the quantity of organic matter present.

When toluene is added or when the soil is heated to 98° C. the phagocytes are killed but not the bacterial spores. On removing the toluene and moistening the soil, the spores germinate and the resulting organisms multiply with great rapidity, since they are now freed from the attacks of their enemies and the competition of other larger organisms; they even appear to decompose the dead organisms. There is evidence that the individual species may be less virulent than the old races, but they more than make up for any deficiency in this direction by their enormously increased numbers. The rate of decomposition is considerably hastened and a larger amount of ammonia is produced. Some of the groups of organisms suffer, such as the nitrogen-fixers, whilst the nitrifying organisms are exterminated.

It might be thought that the removal of nitrifying organisms would seriously interfere with the growth of plants; as a matter of fact it seems to have little effect. Experiments showed that plants readily took up the decomposition products—ammonia, etc.—and were not dependent on nitrification, although they made rather better growth per unit of nitrogen absorbed if supplied with nitrate than if supplied with ammonia. Considerable care is necessary to prevent reinfection; the nitrifying organisms are so small and so widely disseminated that, unless special precautions are taken, they speedily gain an entry into the treated soils and nitrify the ammonia as it is formed.

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"THAT BAD BEVERAGE, BEER"

By ADRIAN J. BROWN

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Although the use of beer in one form or another has been common among us for many generations, there is a persistent recurrence of interest in its manufacture and ingredients, due on the one hand to the efforts of those who regard it as an entirely pernicious drink, capable of inflicting untold harm on the community, on the other hand of those who take a less rigorous attitude but are still eager to profess their anxiety lest a drink so popular should fall below a standard of purity which they conceive to be attainable only by the use of malt and hops. Between the two parties, who may be described respectively as "No Beerites" and "Pure Beerites," there is a wide gulf fixed; but their united voices are sufficient to keep the public mind on the alert and to make it worth while to consider the matter of beer-making from the standpoint of science.

Although the word "beer" occurs in various forms in several languages, such as *bière* in French, *bier* in German, and *beor* in Anglo-Saxon, its etymology is obscure. It probably shares the origin of the word "brew" and is always used in the sense of implying a beverage fermented by artificial means. This mode of preparation serves to distinguish it from any variety of wine which is produced by the natural or self-fermentation of the juice of the grape or vine fruit. The use of the word "beer," as implying an artificially-fermented beverage, is seen in names such as "spruce beer," "ginger beer," etc., whilst the term "beer," used alone, is customarily employed to indicate a fermented beverage obtained by the use of some form of malted cereal or other saccharine substance in conjunction with hops. As will presently be seen, the process of artificial fermentation is of great antiquity; but it is certain that the preparation of wine is a process of yet earlier origin. When primitive man was engaged in harnessing Nature to his needs, he would find the making of some sort of wine a simple task. It may well be supposed that he stumbled upon the process by accident, since it is easy to imagine that a vessel containing crushed grapes might be set aside for future use, and discovered later to contain a new drink, far more exhilarating than the ordinary fresh juice of the grape. Thus may have arisen the first connoisseur in vintages, tasting with delight the results of Nature's handiwork, and realising, however dimly, that alcohol could be produced by the mere crushing and leaving together of the juice and skin of certain fruits, such as the grape, the juice of which contains fermentable sugar, the skin carrying a yeast capable of turning the sugar into alcohol and of converting the sober juice into an intoxicating wine.

In its widest sense, however, the term "wine" must be held to include all alcoholic beverages which are derived from sugar-yielding vegetable juices. Thus the coco-nut palm yields from the cut stalks of its young inflorescences a juice called toddy, which may be boiled down to sugar or allowed to ferment, forming, when distilled, the drink known as "arrack." Other palms yield similar beverages. Again, from the agave, or American aloe, which is known to us as the "century plant" and belongs to the Amaryllideæ, the Mexicans obtain their national drink "pulque" by extracting the sap of the flower bud and allowing it to ferment. By these methods the juices of various plants have been made palatable and their original use as beverages belongs to a period of history so remote as to be past reckoning. The common feature of the methods used in the preparation of wine in its earliest form will be seen to lie in the simple character of the processes employed, Nature being allowed to operate with comparative freedom, undisturbed and uncontrolled by any great exercise of man's intelligence.

Such processes were easy enough in lands where fruit or plants of a suitable character happened to be plentiful or easily grown. In other regions one may suppose that the discovery of a fermented drink had to await the development of human intelligence up to the point of discovering how a fermentable sugar could be obtained by the treatment of grain. That this point was reached in very early times is shown by references in Egyptian papyri dating back to three thousand years before the Christian era; while Diodorus Siculus, in his history of the world, written about 44 B.C., affirms that "wherever the vine was not found in Egypt, Osiris taught the method of preparing a corn wine from grain." Albeit Diodorus is held to be an historian of easy veracity, his reference to Osiris is enough to show that the origin of beer had already been lost in the mist of ages; for Osiris was a legendary king of Egypt, whose beneficent deeds had passed into mythology, and had led to his deification at a period already remote beyond human reckoning.

The Question of "Pure Beer"

Beyond the vague account of Diodorus little is known of the more ancient modes of preparing beer, but the frequent references which occur in Pliny and other Latin authors show that the drink was extensively used in Western Europe. It is well known that the Saxons had it in common use long before they settled in Britain and founded the English nation, of which beer was for centuries almost the sole, and certainly the staple, form of liquid food. Used at every meal, partaken of as a matter of course by men, women, and children, beer was the sole drink of all save the very wealthy. It was prepared as a thing of common domestic use, like bread; and the housewife was expected to be skilled in brewing no less than in baking. The process was largely traditional, recipes being handed down in verbal form, and doubtless with some pretensions to secrecy wherever a successful innovation had been discovered and a variation of the common method had been found to have a good result in winning approval from the family and neighbours. Built up thus during many years of empirical practice, the art of brewing appears to have called forth no systematic treatise until the year 1585, when a certain Thaddeus Hagecius, a native of Bohemia, wrote in Latin a booklet, *De Cerevisiâ, ejusque conficiendi, ratione, natura, viribus et facultatibus, opusculum.* This little work is full of interest, since it contains a detailed account of the practice of brewing as followed at that time, with a description of the preparation of beer from wheat and barley in the first place and also some noteworthy information as to the practice of using such substitutes as oats, millet, anise, fennel, and currants. It will be observed that in this little treatise there is a direct contradiction of the opinion, so commonly held and expressed nowadays, that "pure beer" is a product of hops and malted barley, and of these only. It is evident that in the past the term "beer" was applied more widely, and included any manufactured beverage prepared from sugar-yielding materials. These latter were, for the most part, but by no means solely, derived from malted cereals of various kinds, the flavour being given by hops or by such ingredients as the anise or fennel mentioned by Hagecius, and doubtless added in conformity with the demands of custom or fashion. It is evident that the glib modern use of the term "pure beer," with its narrow application to a product of fancied merit, to be composed solely of hops and malted barley, is without historical sanction. Nor is it unreasonable to inquire on what grounds the modern brewer is to be forbidden to utilise 10 or 15 per cent. of the starchy endosperm of maize or of rice in the place of a similar quantity of malted barley. Even if he should elect to use cane-sugar or dextrose as a substitute, it is difficult to see that he thereby violates the purity of the resulting product; unless, indeed, we are to interpret the term "pure beer" in the pedantic and misleading fashion of certain extremists, whose steadfast abhorrence of alcohol apparently fails to yield either a wholesome sobriety of judgment or a temperate expression of opinion. The limitation of the description "pure" to the preparation derived from hops and barley is comparatively recent and was due in the first instance to the interested efforts of those who were concerned with maintaining the selling price of barley at a competitive figure.

With regard to the vaunted excellence and purity of the ale of our grandfathers, so often quoted by the "pure beer" party, interesting information may be gathered from the instructions given by one Alexander Marrice in his *Art of Brewing*, published in 1837. For "Reading Beer" the following substances are recommended: Malt, hops, grains of Paradise, coriander seeds, sugar, Indian bark. For "London Ale" the addition of orange powder, salt of tartar, and bean flower is suggested; while for "Windsor Ale," orange pea, honey, and licorice were added. To a generation anxious for purity, the recipe for "Scurvy-grass Ale" should prove interesting. It runs thus: "Malt (5 qr.), garden scurvy grass (5 bushels), hops (25 lb.), Alexandrium senna (2 lb.), molasses (10 lb.). During the fermentation one pound of sliced horseradish to be placed in a net and thrown into the tun." These efforts are wholly eclipsed, however, by the *olla podrida* suggested by a Mr. Mackenzie, and designated by him "London Porter." This concoction was to contain malt (1 qr.), hops (8 lb.), treacle (6 lb.), licorice-root (8 lb.), essentia bina (8 lb.), colouring (8 lb.), Spanish licorice (2 oz.), capsicum ($\frac{1}{2}$ oz.), cocculus indicus (2 oz.), ginger (3 oz.), heading—a mixture of alum and copperas ($\frac{1}{4}$ oz.), lime (4 oz.), linseed (1 oz.), salts of tartar (2 dr.), cinnamon bark (2 dr.). Truly it may be said that the early Victorians were a hardy race if they could quaff with steady lip and undaunted heart a beverage such as this. By comparison, any product of a modern brewery is harmless indeed, and ignorance alone serves to excuse those who deplore the present use of harmless malt substitutes and profess to desire a return to the brewing methods of our grandfathers.

Such an attitude ignores two facts, viz. that current taste demands a beer of lighter gravity, and also that modern scientific research has placed at the disposal of the practical brewer a wealth of knowledge which his predecessors lacked. Whereas formerly beers were brewed at a very high original gravity, and contained a large proportion of alcohol, the modern product is required to be about thirty per cent. lighter in original gravity and much less alcoholic than that of the past. Public taste has made an advance in the right direction, and has presented to the brewer a series of problems at once commercial and scientific. The success of his business demands that he shall gauge and satisfy public requirements, and in his efforts to do this it becomes necessary for him to enlist the aid of the laboratory expert. This latter necessity has given rise to the suspicion that brewing has become a kind of black magic, carried on with subtle and unholy craft, to the end that the producer of beer may gain vast wealth at the cost of the well-being of innocent consumers. Such a view of the matter is nothing more than the inevitable result of the deplorable ignorance of modern science which pervades every class of society, and leads the average man to suspect any process which he cannot at once understand. Hence it is that the "Pure Beer" question has made an extensive appeal, and has even been the subject of a parliamentary inquiry, designed to enquire into the alleged adulteration of beer. It is generally forgotten that the government control of brewing processes is rigid and minute, the excise officer being constantly at the brewer's elbow, and ever on the alert to check any transgression of the law. It may be counted for righteousness to the brewer that, although the presence of a chemist on his staff has often resulted in public odium, he has nevertheless persisted in his determination to use the resources of modern science, and to keep abreast of public needs. It is doubtless true that his enterprise has been rewarded, although less abundantly than that of a successful chocolate-maker. The latter has the additional satisfaction of winning praise for his shrewdness in employing chemists where the brewer incurs only suspicion, and of gaining approval for a product containing much sugar and starch, while these ingredients are held suspect in beer. Such diverse verdicts are to be expected so long as scientific processes are so dimly understood, and while men continue to live chiefly on catchwords.

The man who desires to comprehend something of the operations of modern brewing will do well to study the pages of some up-to-date text-book on the art, following up this study by a visit to any large brewery, where he may see the various processes carried out. He will of course observe the presence of an Inland Revenue official, and may be told that at every stage in the production of beer there is the exercise of public control acting through a trained officer, so that the quality and quantity of every material are supervised, and anything in the nature of illegality prevented. He will probably be led to contrast this state of things with that which attends the production of countless other food-products, such as jam, potted meats, bread, or temperance drinks. There, instead of the constant presence of a government official, we have no supervision beyond the occasional visit of a local inspector or the intermittent attentions of a medical officer of health. Under such conditions adulteration is possible and indeed not infrequent; but in the case of beer the addition of any harmful or unauthorised ingredient is practically impossible. When one considers further that beer is of all beverages the one most free from disease-carrying germs, such as are often found in water and aerated drinks, it is not difficult to agree with Prof. H. E. Armstrong's opinion that "Beer is the only safe drink at the disposal of the public all over the world. It is the only sterilised drink available for public consumption."[1]

Research in connection with Brewing

The foregoing apology for beer has furnished testimony as to the dependence of modern brewing processes upon scientific

[1] *Journal of the Institute of Brewing*, 1908, vol. xiv. p. 145.

investigation, but it is not out of place to record the fact that brewing in its turn has contributed not a little to the progress of science. As a practical art, it is concerned with the physiological processes of germination in seeds, the complexities of the chemistry of carbohydrates and of protein compounds, the action of micro-organisms and enzymes, to mention only a few of the fields of investigation which the process of brewing opens out to the scientific worker. Should we seek an example of the successful utilisation of these opportunities, the most conspicuous will be found in the work of Louis Pasteur, whose researches into the history of micro-organisms have given him a standing no less eminent than that won by Darwin in the field of evolution. Whereas Darwin's work altered completely the orthodox attitude of mind throughout the domain of science, that of Pasteur created a new world and gave to mankind direct benefits in the shape of exact knowledge concerning the cause and prevention of infectious diseases and the value of aseptic surgery. It should not be forgotten, moreover, that Pasteur's investigations had their origin in a study of the phenomena of brewing. At the end of the war of 1870 he began to study the science of brewing, with the object of assisting the development of a national industry, and thus furnishing to his native land a means of recovery from the effects of a national disaster. The outcome of his studies was his well-known work, *Etudes sur la Bière*, in which he set forth his great theories of fermentation, afterwards elaborated until they formed the magnificent structure which constitutes his title to fame. Too often this origin of Pasteur's work is forgotten, being obscured by his later work in the region of pathology. A regrettable consequence has been that the true science of bacteriology is now to some extent displaced in favour of a special branch, and the department of pathological bacteriology usurps the attention of many workers, who fail to recognise the importance of a continued and deeper study of micro-organisms in general, whether pathogenetic or beneficial. While it may be conceded that the immediate and visible results of the specialised study which now prevails are far-reaching and valuable in their scope, yet it must be urged that these results are gained at the cost of progress in other branches, no less important, although less striking to the public mind. A premature concentration upon one aspect of a subject not only hinders the progress of the subject as a whole, but inevitably hampers the worker after a time even in the selected branch.

In addition to the epoch-making work of Pasteur, one may mention that of Peter Griess, whose notable researches were carried out in the laboratory of Messrs. Allsopp at Burton-on-Trent, and that of Cornelius O'Sullivan, the re-discoverer of maltose, whose labours indicated the line of investigation which is still being followed by students of the composition and transformations of starch. His efforts have been extended and supplemented by the independent investigations of Horace Brown, who has also gained universal regard among scientific workers by reason of his brilliant researches into the chemical and physical aspects of vegetable physiology and the ingenuity he has shown in the selection of fresh paths of inquiry. The name of the late Dr. Hansen is but one of the many that might be mentioned in proof that the brewing industry has supplied a long and honourable list of workers in the field of science. The opportunity may be taken to give a brief account of a piece of work done recently by the writer, which had its origin in the study of a technical problem connected with brewing; the results, however, have proved to be of much wider interest.[1]

The Seed Coats of Barley

During an investigation of the water-absorbing properties of the dry seed of barley, it was observed that the seed possesses semi-permeable properties of a remarkable kind. It is, of course, well known that when the dry seeds are immersed in water they absorb the liquid to an extent of about 80 per cent. of their original weight. But when the seeds were immersed in a 5-per-cent. solution of sulphuric acid, it was found that the water of the solution was alone absorbed, leaving the acid outside, a result which seemed to indicate that the seed-coverings possessed the property of semi-permeability. This was worthy of attention, since hitherto it had been supposed that among naturally-formed membranes the property of semi-permeability was confined to those formed of living protoplasm,

[1] *Annals of Botany*, 1907, vol. xxi. p. 79; *Proceedings of the Royal Society*, 1909, B. vol. lxxxi. p. 82.

and was absent from such non-living structures as the coverings of the seed of barley. It now appeared that these coverings were able to effect a complete separation of the water from the acid, and to produce a proportional concentration of the excluded acid solution. Nor is this so in the case of a 5-per-cent. solution only. With solutions of varying strengths, up to 36 per cent., a result accrues which is similar in kind, but different in degree, for as the acid solution becomes more concentrated, less water enters the seed. This difference is to be ascribed to the higher osmotic pressure of the stronger solutions. In short, a seed of barley behaves as a cell completely enclosed in a semi-permeable membrane, this membrane being, as investigation has shown, the inner skin or testa of the seed. The same behaviour of the seed is to be observed when it is immersed in solutions of chlorhydric or phosphoric acid, in dilute alkalies, in solutions of many salts—such as sodium chloride, calcium chloride, cupric sulphate or ferrous sulphate, or in solutions of sucrose, dextrose or glycerine.

This property of semi-permeability is found to extend to the seed-coverings of all the other species of Gramineæ yet examined, and its probable value to the seed is a matter of botanical interest, since a property of this unique character is not unlikely to have some important use. This may consist partly in the benefit which the seed derives from being able to absorb water alone when placed in a moist situation where the soil contains soluble matter likely to injure the seed in the early stages of germination. A more important use of the property will probably be that it serves to retain soluble matter within the grain during fermentation. The starchy endosperm which constitutes the food-reserve of the embryo of seeds of the Gramineæ is rendered soluble by enzymic changes in the course of germination; and it is obvious that if the seed-covering were permeable to such substances as sugar, into which the endosperm is largely converted, much of the food-reserve would be lost by diffusion before it could be absorbed by the young plant. The presence of a semi-permeable covering, on the other hand, ensures the preservation of the whole of the food supply to the end designed by Nature. This view of the matter is further strengthened by a consideration of the highly-specialised shape of the seed, with its re-entering ventral furrow, suggesting a necessary provision for the expansion of the contents of the seed, such as takes place when it is moistened. This expansion would burst the seed-coverings; and the fact that the shape of the seed prevents any rupture suggests that the coverings are to be maintained entire for some such purpose as can be carried out by means of their semi-permeable property.

But even this unique and hitherto unsuspected feature of the seed-coverings is less remarkable than the one which came to light in the course of further investigations, and which revealed the power of selective permeability in the presence of certain solutes. Thus, if the seed of barley is immersed in a mixture of solutions of mercuric chloride and sulphuric acid, the former salt passes through the seed-coverings with apparent ease and itself diffuses into the seed, while the sulphuric acid is kept outside. In a similar fashion acetic acid and the fatty acids generally are able to pass through the seed-coverings; but the salts of these acids, such as sodium acetate, are excluded. More remarkable still is the fact that, whereas acetic acid enters and diffuses through the seed very readily, the corresponding amino-acid, glycine, will not enter at all. Aqueous solutions of ethyl alcohol, aldehyde, acetone, and ethylic acetate diffuse into the seeds very readily; but in the anhydrous state these liquids are excluded. The only explanation of this property of selective-permeability which can be suggested at present is that it is related in some way to the mode in which the molecules of the various solutes are united with the molecules of solvent water. Such an explanation appears to support the views on solution which have been advanced by Prof. H. E. Armstrong.

The investigations just described have not yet been brought to the final stage, but enough has been done to furnish yet another illustration of the value of the brewing industry as an incentive to scientific research. Of its importance in the world of commerce and its antiquity as a craft, business men and historians can supply evidence. It has remained to certain classes of politicians and ill-balanced enthusiasts to hamper the provision of a pure and wholesome drink and to surround "that Bad Beverage, Beer" with an odium which is as harmful as it is undeserved.

PALÆOLITHIC RACES AND THEIR MODERN REPRESENTATIVES[1]

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PART VI. (*Conclusion*)

Chronology: Recent Discoveries

We now approach the last and most difficult portion of our task, and must endeavour to assign the stages of human industry we have already discussed to their appropriate places in some scale of time, such as that to which we were led by our studies of the glacial epoch.

But, as if this problem were not complicated enough in itself, it involves another not less so, for the scale we have adopted is by no means universally accepted by those who have given most attention to the subject. Some there are, like Mr. Lamplugh among English geologists, and Prof. Geinitz in Germany, who, adopting the views of the late Prof. Carvell Lewis, recognise only a single undivided glacial epoch, and reject in the most uncompromising fashion all notion of intervening genial episodes. It would be impossible to enter here into the elaborate arguments which have been adduced on both sides in this thorny controversy, but on a review of the facts it does not appear that any evidence has yet been adduced sufficiently cogent to invalidate the conclusions set forth in our first article. In the camp of the "Interglacialists" themselves, however, differences of opinion are not wanting; thus Prof. James Geikie, who is not content with less than six glacial ages, has a numerous following, especially in America, where it is even said that as many as seven of these ages can be recognised; on the other hand, Prof. Marcellin Boule and the

[1] The preceding articles of this series, now concluded, have appeared in Science Progress: Parts I. and II., 1908, **3**, 326-53; Part III., "Early Pleistocene Man and the Tasmanians," 1909, **3**, 500-33; Part IV., "Solutrean Man and the Bushmen," 1909, **3**, 667-86; Part V., "Magdalenian Man and the Eskimo," 1909, **4**, 16-45.

majority of French and German geologists cannot find evidence of more than three. Pending further investigation we may adhere provisionally to the classification of Prof. Penck (*anteà*, vol. iii. p. 326).

The first attempt of recent years to bring the subdivisions of the glacial epoch into relationship with the several stages of Palæolithic industry was made by Prof. Maurice Hoernes, who found a comfortable pigeon-hole for each of the industrial stages recognised by him in each of the three genial episodes of Penck. The Magdalenian, as the latest, naturally found its place in the third of these episodes, and the Solutrean in the second; there remain then the Chellean, Acheulean, and Mousterian, three industries and only one pigeon-hole; it was contended, however, that these were not entitled to separate recognition, and in opposition to the views of Mortillet they were lumped together into a single industry, docketed with the name of Chelleo-Mousterian, and inserted in the first episode.[1] Thus we are presented with the following scheme:—

IV Glacial	episode	
3 Genial	„	Magdalenian or Reindeer period.
III Glacial	„	
2 Genial	„	Solutrean or Mammoth period.
II Glacial	„	
1 Genial	„	Chelleo-Mousterian period.
I Glacial	„	

This correlation is so obviously opposed to the best established facts in archaeology that it now possesses a merely historical interest; nevertheless the work in which it is set forth is still of great value as a store of information admirably arranged.[2]

The next essay we owe to Prof. Penck; it is generalised in the following table:—

IV Glacial	episode	Magdalenian	industry	Tundra	phase
3 Genial	„	Solutrean	„	Steppe	„
				Forest	„
III Glacial	„	Mousterian	„	Tundra	„
2 Genial	„	Chellean	„		

[1] As we have already pointed out, M. Comment's investigations have completely confirmed Mortillet's views.

[2] M. Hoernes, *Die Diluvial Mensch in Europa*, Brunswick, 1903, p. 227.

It will be seen that the industries have all moved one step upwards, and the Palæolithic epoch as a whole is thus brought nearer to our own times. The correlation is based on a variety of evidence which we can only briefly pass in review.

Great weight is attached by Penck to the geographical distribution of the implements in France. The Magdalenian, Solutrean, and Mousterian are all abundantly represented outside the region which was covered by the ice of the ancient glaciers; the Magdalenian also extends into this region, crossing the moraine of the third (III) glacial episode, and even in some cases of the fourth (IV), which lies within the third moraine. Hence the Magdalenian is certainly more recent than the third, and than the fourth also, except in so far as it may be contemporaneous with the fourth. The occurrence of an arcto-alpine fauna (reindeer, musk-ox, etc.) in association with the Magdalenian industry seems to point to contemporaneity, but that the Magdalenian is on the whole post-glacial is one of the very few facts upon which all archaeologists seem now to be agreed. As regards the Mousterian the case is said to be very different; according to Penck, not a single Mousterian station is known in the neighbourhood of the moraines of either the third or fourth glacial age. The Mousterian avoids the glaciated region altogether. Hence this industry is certainly older than the fourth (IV) glacial episode, and either older than the third or contemporaneous with it; in this case also contemporaneity is inferred from the nature of the fauna. As we shall see later, this conclusion is not final.

The Löss.—Under this term more than one deposit is included, and they are not all of the same age. Penck recognises a distinction into a younger and an older löss; neither is ever found resting upon the moraines of the last glacial episode; indeed, when the löss comes in contact with these moraines it sinks beneath them. Thus the löss as a whole is certainly older than the last glaciation, and the younger löss probably belongs to the last interglacial episode. But it is within the younger löss that numerous stations, which have been assigned on good grounds to the Solutrean, have been found. In a terrace of löss on the Danube is the famous station of Krems, which has afforded more than 25,000 implements of jasper, chalcedony, and chert. They lie in layers within the löss, associated with the charcoal left by numerous fires, and with a rich herbivorous fauna. No laurel-leaf points (Solutrean) have been found here, but they occur in similar deposits at Předmost in Moravia; in the löss of Brünn ivory statuettes have also been met with. If the age of this löss has been rightly determined, then the Solutrean must evidently be assigned to the last or third genial episode.

River Terraces.—The gravels of the river terraces were deposited, as we have seen, during the successive genial episodes, and we might naturally look to them for the most certain evidence on the question before us. Unfortunately the terraces which have afforded human implements occur in regions so remote from the Eastern Alps and from the ancient glaciated mountain regions generally that they cannot be brought into comparison. An exception seems to be provided by the district watered by the Garonne north of the Pyrenees. Four terraces can be traced along the course of this river, and they have been correlated by Obermaier [1] with the four terraces of the Eastern Alps. The lowest is about 15 metres above the alluvial plain, and ends towards the mountains in a moraine; the next is 55 metres above the plain and contains quartzite implements of Acheulean type; the remaining terraces, which stand still higher, have not afforded any traces of human industry. It would thus appear that Acheulean man inhabited this part of Southern France during the third glacial age. The contemporary fauna as shown by fossils was arcto-alpine (mammoth, *Rhinoceros tichorhinus*, and reindeer). This correlation is at variance with the views of Penck, who has consequently discussed in detail the relationship of the terraces and is led to conclude that the third terrace is really on the horizon of the second of the East Alpine terraces.

On the completion of his great work on the Alps in the Ice-age Penck modified his earlier views to some extent,[2] with the effect of rendering the several industrial stages slightly less discontinuous and bringing them nearer our own times. The following diagram (fig. 1) represents Penck's latest attempt at correlation.

[1] Hugo Obermaier, "Beiträge zur Kenntniss des Quartäre in den Pyrenaen,' *Arch. f. Anthrop. N.F.* 1906, 4, p. 299.

[2] *Die Alpen in Eiszeitalter*, by Albrecht Penck and Eduard Bruckner, Bd. III. p. 1172; and Penck, "Das Alter des Menschengeschlechtes," *Zeits. f. Ethn.* 1908, pp. 390–497.

The views of the leading French geologists, and particularly of Prof. Marcellin Boule,[1] differ from those of Prof. Penck in much the same manner as those of Prof. Penck differ from those of Prof. Hoernes; as will be seen from the following

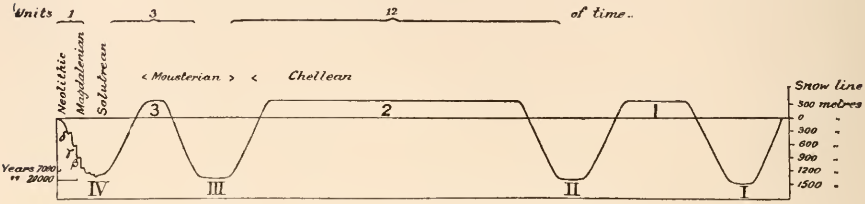


FIG. 1.—Curve of climate for the Glacial Epoch.

table the series of human industries is advanced by Prof. Boule one stage nearer the present:—

Post-glacial	{ Magdalenian Solutrean
Glacial IV.	Mousterian
Genial III.	Chellean

The Chellean with its accompanying warm fauna is placed in the last interglacial episode, and by this proceeding the facts are brought into greater harmony with those which have been discovered in this country by British observers.

In support of his correlation Prof. Boule insists in the first place that implements of the lower Palæolithic period are not, as is asserted, rigidly excluded from the region once occupied by the ice of the third glacial episode. In 1887 M. Tardy found an Acheulean boucher on the right bank of the Ain, above alluvium which overlay undisturbed moraines of the great Rhône glacier; and in 1908 M. Lebrun found a little boucher (3 inches long), also of Acheulean type, near Conliege, 5 kilometres south-east of Lons-le-Saunier, *i.e.* in a region occupied by the ice during its greatest extension (glacial episode III). Thus it follows that the Acheulean industry is more recent than the third glacial episode. Prof. Penck objects to this argument that the occurrence of isolated implements within the glaciated area is of less importance than the com-

[3] M. Boule, "Paleontologie stratigraphique de l'Homme," *Rev. d'Anthrop.*, 1888, 1889 ser. 3 to 4, and "Observations sur un silex taillé du Jura et sur la Chronologie de M. Penck," *L'Anthropologie*, 1908, t. 19, pp. 1–13 (separate copy).

plete exclusion of stations where implements are at all common, and further that bouchers of Acheulean form might have been used by the later and even Neolithic races.

The puzzles presented by the Mousterian industry are very embarrassing. It is said to be associated generally with the arcto-alpine fauna, and according to M. Boule it lies within the moraines of the third (III) glacial episode and outside those of the fourth (IV). Hence the position assigned to it in his scheme. Unfortunately, however, the rule as to the fauna is so often broken by exceptions that its value as evidence is greatly impaired; in the caves of Grimaldi, for instance, a Mousterian industry is found in company with a Chellean or warm fauna. Penck welcomes such facts as supporting his views; according to him the warm fauna of the Chellean stage marks not so much a particular epoch in time as a particular set of conditions—those, namely, of a genial or interglacial episode; they might therefore be expected to recur, provided the intervals between the episodes are not of excessive duration. The same of course is true, *mutatis mutandis*, of the arcto-alpine fauna.

This theory of recurrence is strongly opposed by Prof. Boule, who asserts that in France, where the Pleistocene mammals are richly represented by fossils in thousands of localities, the succession is always, and without exception, the same—first Pliocene, then the warm, and afterwards the cold Pleistocene fauna, and finally the fauna characterised by the reindeer.

The caverns of Wildkirchli, situated on the Säntis at a height of 1599 metres above the sea, and in the midst of the glaciated region, have lately been investigated by Herr Bächler[1]: they contain Mousterian implements, and according to Prof. Boule the customary cold fauna; this statement is contested by Prof. Penck, however, who asserts that the cold fauna is absent; at such a station the mammoth and woolly rhinoceros are scarcely to be expected, but the reindeer also is unrepresented, and of the twelve species which have been recognised at Wildkirchli, seven are known from the lower interglacial horizon of the Prince's caves at Mentone, where they occur in company with *Elephas antiquus* and *Rhinoceros*

[1] L. Bächler, "Die prähistorische Kulturstätte in der Wildkirchli-Ebenalphöle": *Verh. d. Schw. Naturforsch. Gesells. in St. Gallen*, 1906.

Mercki and along with Mousterian implements. These species are: *Ursus spelcus*, *Felis leo* var. *spelea*, *Felis pardus* var. *spelea*, *Canis lupus*, *Capra ibex*, *Capella rupicapra*, *Cervus elaphus*. It is pointed out that Wildkirchli would not have been inhabited during a glacial episode.

Evidently the last word has not yet been said on this question, and we must leave it to future investigation to decide. Meanwhile we may turn to our own islands, which are not altogether destitute of facts bearing on this inquiry.

It has long been known that Chellean and Acheulean bouchers, often spoken of as the river type of implement, occur in fluviatile gravels, which are younger than the great chalky boulder clay, a widespread glacial deposit formed probably during the third glacial episode of Penck. In spite of the most persevering search bouchers have never yet been found in deposits which could be shown to be older than the chalky boulder clay. Whatever doubts might have been felt as to the relation of the older palaeolithic industries to this deposit must, I imagine, have been dispelled by the investigations made by Mr. Clement Reid, on behalf of a Committee appointed by the British Association, at the classic locality of Hoxne, where John Frere discovered abundant palæolithic implements in 1797. The implements are typical Acheulean bouchers, and were originally very numerous—as many as five or six to the square yard; at present the locality only yields about one to about ten square yards.[1] By means of trial pits and borings the following succession was made out: The lowest deposit is the chalky boulder clay; a hollow in this is filled first with lacustrine beds 20 ft. in thickness, these are succeeded by lignite with a temperate flora; then come lacustrine beds 20 ft. in thickness, but containing an *arctic* or *subarctic* flora (arctic willow and arctic birch), and finally the implement-bearing brick earth and gravel. Representing this in a vertical column we have—

Acheulean industry.
Cold climate (*Betula nana*, *Salix polaris*).
Temperate climate (*Alnus glutinosa*, *Rosa canina*).
Glacial climate (chalky boulder clay).

[1] "The Relation of Palæolithic Man to the Glacial Epoch," Report of the Committee, etc., drawn up by Clement Reid, *Rep. Brit. Assoc.* 1896, Liverpool, pp. 400–15.

The chalky boulder clay is said to be the most recent glacial deposit in Norfolk. If, as we have suggested, it may be referred to the third (III.) glacial episode of the Continent, then the Acheulean in England may be placed in the third (3) genial episode, and the Chellean may belong to the same episode, but to an earlier portion of it, perhaps represented by the temperate Alder bed of lignite. If this correlation is correct then we ought to find evidence somewhere of the fourth (IV) glacial episode.

Of late years observations have been made in the neighbourhood of Oxford which have an important bearing on this question. At Wolvercote, a little north of the city, a river terrace, lying about 40 ft. above the Thames, is admirably exposed in an extensive brick pit. The section [1] shows that a river channel excavated in the Oxford clay, which here forms the country rock, was subsequently converted by some unknown means into a lake. Lying on the bottom of this, *i.e.* on the Oxford clay, Mr. Bell found a number of very beautiful lower palæolithic implements. They are in a perfect state of preservation, with sharp edges, and unworn by erosion. Overlying them is a bed of coarse gravel, followed by a series of lacustrine deposits, clay, laminated sandy silt, and occasional layers of fine gravel, altogether about 15 ft. in thickness. A lenticle of peat is intercalated near the base, above the coarse gravel; it has afforded numerous remains of plants belonging to species which indicate a temperate climate. Over the whole sweeps a bed of coarse gravel which here forms the surface of the river terrace, 40 ft. above the present level of the Thames. This gravel presents very peculiar characters, recalling the contorted drift of glaciated regions. It is, as it were, folded in with the immediately underlying deposit, whether this be the clay of the lacustrine series or the Oxford clay. The tongues of clay which are intruded into the gravel show signs of violent movement; they are overthrust, slickensided, and rudely foliated. These phenomena have been referred by Mr. Bell to some form of ice-action. Similar characters are to be seen in several other gravel pits of the district, notably at Coombe, about seven miles N.N.E. of Oxford (137 ft. above the Evenlode), and Picket's Heath Farm, on

[1] A. M. Bell, "Implementiferous Sections at Wolvercote," *Quart. Journ. Geol. Soc.* 1904, **60**, pp. 120–30.

Cumnor hill, two or three miles south and west of Oxford (356 ft. above the Thames). In the gravels of all these localities, including Wolvercote, I have found glaciated boulders. At Picket's Heath Farm the gravels acquire the characters of a boulder clay, and afford numerous erratics; many of these consist of igneous rocks, such as spherulitic rhyolite and rhyolitic breccia, which must have come from a considerable distance, and yet retain a subangular character and present flattened faces scored with glacial striae.

The contorted gravel at Wolvercote has afforded striated pebbles of quartzite, and from the gravel "tip" thrown out by the workmen a large subangular striated boulder of igneous rock was obtained, the glacial markings of which were sufficiently well marked to carry conviction to so critical an observer as the late Sir John Evans.

Whether the deposits on Cumnor hill are contemporaneous with those of Wolvercote may be regarded as an open question, but in any case evidence of glacial action exists at both of these localities.

The implements discovered by Mr. Bell are bouchers of large size possessing special characters of their own; they are certainly not Chellean; in the excellence of their workmanship they are allied to the Acheulean, from which, however, they differ in form, since most of them present a flat face on one side, and are thus, as Sir John Evans expresses it, "shoe-shaped" (fig. 2). Some rare ovate forms occur with them, which may be paralleled with similar forms of Acheulean age, and to this age the whole assemblage may with great probability be assigned.

We have seen already that the Acheulean is younger than the great chalky boulder clay (glacial stage III.?), and this holds good for the Wolvercote implements, which may indeed be even slightly younger than the Acheulean. But the Wolvercote implements are older than the contorted gravels (glacial stage IV?), and may therefore be provisionally assigned to the third (3) interglacial episode. The associated fauna includes *Elephas primigenius*, *Equus caballus*, *Bos priscus*, *Cervus elaphus*, and *Rangifer tarandus*.

Alternations of warm and cold climates seem to have been the rule during the Pleistocene epoch. These were sometimes more contrasted (glacial and genial), sometimes less (beds with

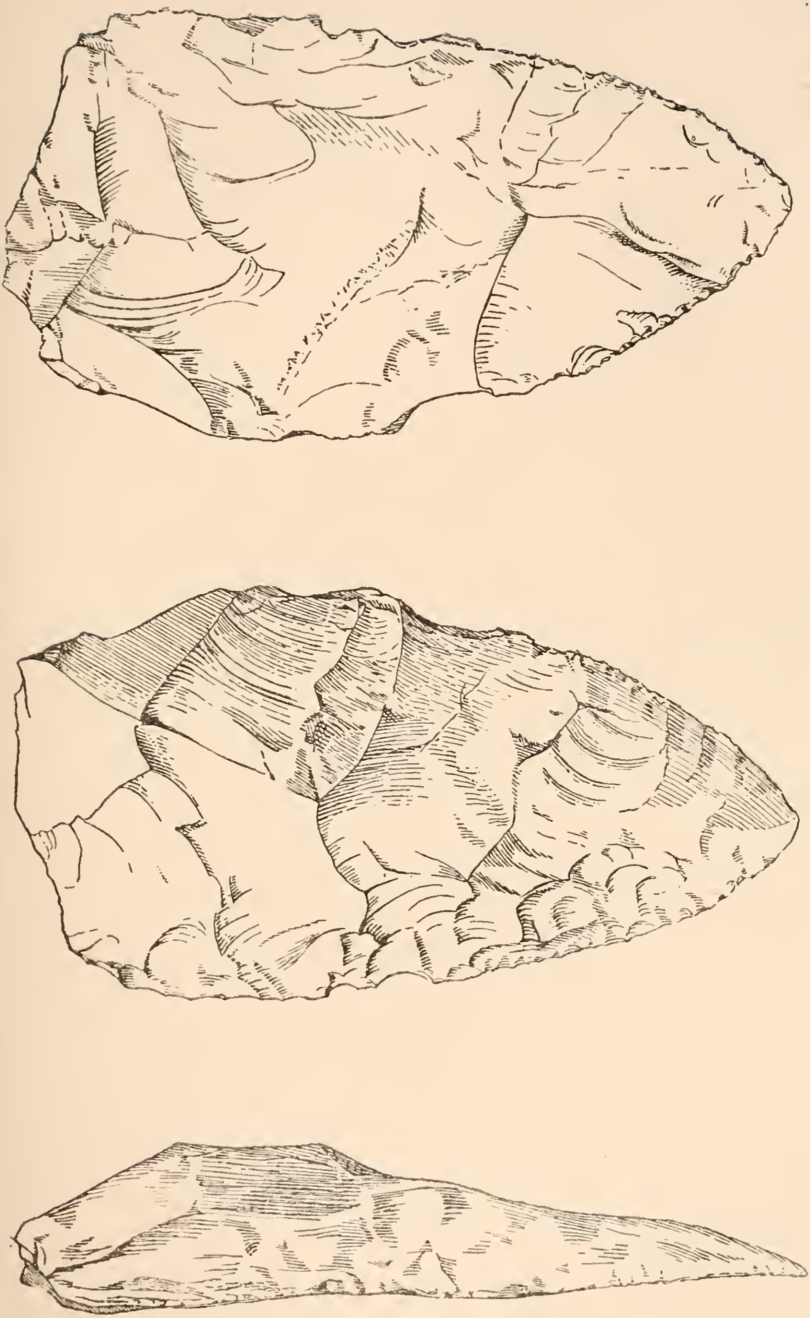


FIG. 2.—A Boucher from Wolvercote.

The figure at the top shows the flat face, that in middle the convex face, that at the bottom is a profile view (× ½). The original specimen is in the collection of Mr. A. Montgomery Bell, M.A., F.G.S temperate and sub-arctic plants), and it seems possible that the Wolvercote horizon may correspond to one of the warmer minor undulations which filled the interval between the great cold waves of the third and fourth glacial epochs; we might then represent the succession by the following scheme:

Contorted gravel	glacial episode IV.
Wolvercote bouchers (Oxfordian)	temperate climate.
" " "	cold climate?
Acheulean of Hoxne . . .	temperate climate.
Betula nana beds of Hoxne . .	cold climate.
Alder beds of Hoxne (Chellean?)	temperate climate.
Chalky boulder clay . . .	glacial episode III.

The stratigraphical relations of the older industries have been investigated by Prof. Rutot.

One of the motives for writing the preceding articles was the belief that the chronology of the palæolithic industries had at length been placed on a sound basis through the investigations of Prof. Penck; now, with the progress of discovery, we are again beset with doubts. In spite of the persistent efforts of gifted observers, extending over many years, the history of the Pleistocene epoch, especially those parts of it concerned with the evolution of the human race, still remains an open question.

The progress of artificial excavations is, however, constantly affording fresh material for study, and with the increasing accumulation of evidence the day cannot be far distant when the later stages of human evolution will be provided with such dates as the stratified sequence can supply; and it is even possible that in the remote future these may be brought into some connection with the recognised standard of time. This hope is stimulated by the fact that our knowledge has been increased even so recently as within the last twelve months by three discoveries of great importance.

We may refer first to the beautifully preserved lower jaw of a palæolithic man (*Homo Heidelbergensis*), found by Dr. Schoetensack at Mauer, 10 kilometres south-east of Heidelberg. This is evidently of great antiquity. It was extracted from a bed of fluviatile sand exposed in a sand-pit at a depth of 24 metres (say 80 feet) from the surface. The section exposed in the pit is as follows:

Younger löss . .	5·74 metres	(over 18 feet).
Older ,, . .	5·18 ,,	(about 17 ,,).
Mauer sands . .	15·62 ,,	(,, 50 ,,).

The fauna associated with it includes the characteristic Chellean species *Elephas antiquus*; the accompanying rhinoceros however is not, as we might have expected, *R. Mercki*, but *R. etruscus*, a species found elsewhere in the Upper Pliocene, as in the Val d'Arno, Italy, the Forest bed at Cromer, and the Siwalik hills of India. Two species of bears are represented (*Ursus arvernensis*, Croizet, and *U. Deningeri*, Reichenau), the lion (*Felis leo* var. *spelea*), a species not distinct from the existing African lion, which survived up to historic times in Southern Europe, a dog (*Canis Neschersensis*, Croizet), which is almost identical with the existing wolf of the Pyrenees, a boar (*Sus scrofa*, cf. *prisca*), several deer (*Cervus latifrons*, *C. elaphus*, var. *C. capreolus*), a bison, the beaver (*Castor fiber*), and the horse. The horse is represented by a number of teeth, which are said not to be identical with those of the existing species (*Equus caballus*), but intermediate between it and the Pliocene *Equus Stenonis*.

Some of the species of this fauna suggest an Upper Pleistocene horizon, but *Elephas antiquus* would seem to take us back at least to the Chellean, and *Ursus arvernensis* and *Rhinoceros etruscus* to a still earlier date. A general discussion of the question leads M. Rutot to conclude that the jaw may be assigned to his Mafflian stage or the Lower Chellean of Dr. Obermaier.[1]

Let us now turn to the jaw itself. It presents a combination of characters which are truly remarkable. The dentition is completely human, the teeth forming a close, regular series uninterrupted by a diastema, with the crowns, so far as we can judge from their worn condition, all rising to a common level; the canines are no more projecting than the other teeth; and we may add, as an equally important fact, that the incisors are of a comparatively small size, no larger than the average of existing men. In the Anthropoid apes these teeth are distinguished by their relatively large dimensions. *The dentition is in some respects less simian than that which may be sometimes*

[1] A. Rutot, "Note sur la Machoire humaine de Mauer," *Bull. Soc. de Géologie Belge*, 1909, t. 22, pp. 117–69, in particular p. 129.

observed in existing primitive races, such, for instance, as the Australians.

The front teeth are not "projecting," but set squarely in the jaw; they are curved, however, especially the roots, in accordance with the generally rounded contour of the front end of the jaw—precisely recalling, in this respect, the curvature of the teeth in the upper jaw of the Neandertal race as represented by the Gibraltar skull. They show considerable signs of wear, much more so than the back teeth (molars and premolars); and since the dentition is complete, the wisdom teeth having been "cut," this shows that the front teeth probably played a more important part than in existing and even primitive races.

The jaw which bears this thoroughly human dentition is itself surprisingly simian; it stands, indeed, almost midway between that of *Homo sapiens* and that of an anthropoid ape such as the chimpanzee. The differences between a human and a simian jaw are most salient at the anterior extremity. In existing men the profile of this part of the lower jaw is usually, though not always, a more or less sigmoidal curve, concave above, just below the teeth, and convex below where it follows the chin. The chin is a characteristic human feature. A line drawn from the upper to the lower extremity of the curve is more or less vertical, varying a few degrees on one side or other of a perpendicular let fall from the upper extremity when the general alveolar surface of the jaw is placed horizontally.

In the case of the apes there is no inflexion below the incisors and there is no chin; the profile is a simple rapidly retreating curve.

It has long been known, from observations on the jaws of Spy and Krapina, that the chin was very much reduced or even altogether absent in the Neandertal race; in the Heidelberg jaw, however, not only is this the case, but the profile has returned to the simple rounded outline which is met with in the apes, differing chiefly by its more gradually retreating slope.

The inner face of the anterior extremity of the jaw also presents several interesting peculiarities. In modern races this surface slopes steeply downwards from the back of the incisors and exhibits no marked subdivision into different regions. In the anthropoids its slope is far less steep, and the upper portion corresponding to the lingual basin can generally be distinguished from the remainder, either by its gentler inclination or by presenting a concave instead of a convex outline in profile. In regard to this character also the Heidelberg jaw occupies an intermediate position, a somewhat sudden increase in inclination marking the termination of the lingual region. The interval between the higher races and the Heidelberg jaw in respect to this character is filled, however, by an almost infinite series of gradations.

A second important peculiarity is presented by the lower part of the inner surface, about two-thirds of the way down. In this region two pairs of muscles are attached, the genio-glossal above and the genio-hyoid below; each muscle of the pair is symmetrically placed on each side of the middle line and close to it; in some cases the place of attachment is marked by a roughened oval area, but usually, in modern races, a spine (*spina mentalis interna*), or spines, is developed as the most conspicuous part of the connection. Great importance was given to this spine by de Mortillet, who regarded it as essential to speech, a view which, though it has been refuted by Topinard, frequently recurs in the works of later writers.

In the apes this spine is absent, and in its place we find a depression or pit. This simian character is now admitted, after much controversy, to occur in several primitive lower jaws of ancient date, but in none of them is it so conspicuous as in the Heidelberg example.

Thus we perceive that in all the characters which distinguish the anterior extremity of the lower jaw, *Homo Heidelbergensis* stands midway between man and the anthropoid apes.

In its robustness and general characters this jaw is equally primitive. The extraordinary breadth of the ascending ramus is a remarkable feature implying great muscular development and a large zygomatic arch.

We now pass to the next important discovery, that of a well-preserved example of the Neandertal type of skull. This was found on August 3, 1907, at La Chapelle aux Saints (Corrèze) by Messrs. Boyssonie and Bardon; a preliminary description has been published by Prof. M. Boule.[1] The remains are those of an individual who had actually been

[1] M. Boule, "L'Homme fossile de la Chapelle aux Saints, Corrèze," *L'Anthropologie*, 1908, tom. 19, pp. 519-25.

interred in a primitive tomb made of rough slabs of stone; that some ideas of a future life were already in existence at this early date, is shown by the fact that the bones of the leg of a bison were found immediately overlying the skull, and so related in position as to show that the leg when interred must have been entire and probably still clothed with flesh. No doubt it was intended to provide food for the departed in his journey to the next world.

The skull was found broken into several fragments; these have been very skilfully and successfully pieced together, so that the original form is very accurately reproduced. That part of it which corresponds with the original Neandertal calotte is of almost precisely the same form, so that we can have no doubt as to its racial identity. In our previous account of the face of Neandertal man our data were derived from the Gibraltar skull,[1] and it is extremely gratifying to find that the new skull from Corrèze repeats the features of the Gibraltar skull in almost every particular; the only exception of any importance being the presence of marked facial prognathism, which, as we have seen, does not exist in the Gibraltar skull. There is nothing very remarkable in this—it is indeed just what might have been expected; and it may be added that the range of variation with respect to this character within the Neandertal race is fully equalled among some existing primitive races such as the Australians.

Passing over a multitude of details of great interest and importance, we will only refer now to the cranial capacity, which is unexpectedly great and indeed astonishing. Prof. Boule has ascertained by measurement that it amounts to no less than 1600 cubic centimetres. In the Gibraltar skull the capacity is 1250 c.c. or thereabouts. Thus the range so far as it is known extends from 1250 to 1600 c.c. The average capacity greatly exceeds that of the Australians, which is 1230 c.c. In this, as in some other respects, the Australians appear to be the more primitive.

It is fortunate that the new skull is accurately "dated"; the implements which occur with it are typical Mousterian forms, and it is to the Mousterian horizon that we can now definitely assign the Neandertal race.

It is a long way from the fragmentary Neandertal calotte

[1] *Science Progress*, 1909, **4**, pp. 525–6.

of uncertain age which Fulhrott discovered in 1857 to the beautiful skull from La Chapelle aux Saints which M. Boule has made known to us; the doubts and difficulties of the past half-century have now completely disappeared, and the accumulation of fragmentary evidence becomes synthetised by this fortunate discovery into an organic whole.

The latest of these recent discoveries was made on March 7, 1909, by Herr O. Hauser, who found a Neandertal skeleton in the lower cave of Le Moustier, under circumstances which pointed clearly to an interment. The body had been laid on a carefully arranged pavement of flint implements; it rested on its right side, with the right arm bent under the head and the left arm extended; burnt bones and flint implements were found lying about the skull; and an Acheulean boucher, the most beautifully worked of all that were found, lay within reach of the left arm. Prof. Klaatsch, who has described these remains,[1] states that they represent a young man of about sixteen years of age. The skull had unfortunately suffered considerable distortion owing to the pressure of the overlying deposits, so that for this or some other reason it was found impossible to piece together on removal the fragments of which it consisted with sufficient accuracy to restore completely the original form. This at least is the conclusion to which I am led by an examination of a cast of the skull and lower jaw supplied me by Dr. Krantz of Bonn; when the lower jaw is fitted to the skull by placing the condyles in their sockets its incisors lie about 10 mm. behind those of the upper jaw; and *vice versa*, when the teeth of the two jaws are made to bite together in their natural position the condyles are about 10 mm. in front of the glenoid cavities.[2] The error seems to be connected with the position of the upper jaw, which is made to advance too far, presenting in consequence a prognathism that is truly extraordinary. In other respects the skull affords a welcome confirmation of the results obtained from other material; it is evidently of great capacity, thus agreeing with the skulls from Spy and La Chapelle aux Saints. The frontal torus presents the usual

[1] H. Klaatsch, "Homo Mousteriensis, Hauseri," *Arch. f. Anthrop. N.F.* Bd, 7. pp. 287–97; and "Die primitive Mensch der Vergangenheit u. d. Gegenwart," *Verh. Ges. Deutsch. nat.-forsch.* Coln, 1908, p. 94.

[2] Prof. Ruggeri has also called attention to this fact. V. Giaffrida-Ruggeri, "Fossili umani Scimmieschi," *Monitore Zool. Italiano*, Anno XX. N. 7, pp. 214–25; in particular note 5, p. 221.

Neandertal character, though it is less marked in relief—a fact which may be connected with the comparative youth of the subject. The face, so far as can be judged from the restoration, resembles that of the Gibraltar skull, except as regards its prognathism.

The bones of the extremities resemble in fundamental characters those of other Neandertal skeletons, and indicate a stature of from 1450 to 1500 mm. The adult, probably fifty years of age (Boule), from La Chapelle aux Saints, was probably about 1600 mm. in height. All the evidence goes to show that the Neandertal men were of short stature with disproportionately large heads.

The connection between cranial capacity and intellectual power is a very vexed question; *ceteris paribus*, we should expect to find a direct relation between the two; but other things are not equal, the quality of the cerebral substance and the degree to which the cortex is folded must be taken into account, and of these factors the skull can tell us nothing, whether we examine it from the outside or the inside. The interior should throw some light on the relative volume of cerebrum and cerebellum, and we therefore look forward with great interest to the full description of the skull from La Chapelle aux Saints which we may expect to receive shortly from the pen of M. Boule, for this will include an account of the interior of the skull, and as much as we can learn of the regions of the brain from a cast.

In the great drama of existence our interest is chiefly centred in the self-manifestation of intelligence. Its history as recorded by the development of its companion the brain is the chief theme of the Tertiary era. The growth of the brain in size and complexity which accompanies the flow of time is a general phenomenon, not confined to any one branch of the mammalian stem, though it culminates in man. It does not produce itself: it is produced. Its cause is hidden from us, but its best results can be conserved, and it is to be hoped that in the course of evolution the practice of eugenics may come to be recognised as an essential part of religious discipline.

THE SCIENCE AND PRACTICE OF PARA-RUBBER CULTIVATION

THE NEW TROPICAL INDUSTRY OF THE EAST

By JOHN PARKIN, M.A., F.L.S.

Part I

Caoutchouc,[1] the elastic gum prepared by the Indians of tropical America from the milky juice (latex) of certain trees, became known in Europe during the seventeenth century as a curiosity. It remained so in England till the year 1770, when Priestley recommended its fitness for erasing lead-pencil marks, hence the origin of the name "india-rubber," now often shortened to "rubber."

This substance first assumed commercial importance about the year 1823 through the method of waterproofing patented by Macintosh. Its uses became greatly extended later by the discovery of vulcanisation, a process invented by Goodyear in America in 1839, and independently by Hancock in England about the same period. These pioneers showed that when caoutchouc is intimately mixed with sulphur and subjected to a temperature of about 150° C., its elasticity is not only increased but remains practically uniform through a wide range of temperature; further, its durability is greatly prolonged. Raw caoutchouc, on the other hand, softens with an increase and hardens with a decrease of temperature. Without the discovery of vulcanisation india-rubber would have remained of quite minor importance; very little unvulcanised rubber is now used in manufacture. The demand for raw rubber from this time onwards steadily rose. New uses for it were continually being found, and finally the incoming of the rubber tyre, especially the pneumatic variety, has caused an ever growing demand.

The great basin of the Amazon has always supplied the largest quantity, as well as the finest quality, of this now in-

[1] The word "caoutchouc" is a corruption of the native name for this substance.

dispensable raw material. This caoutchouc, known in commerce as "Para," after the Amazon port of that name, has been the recognised standard for fully half a century. It is obtained from the euphorbiaceous tree, *Hevea brasiliensis*, otherwise known as the Para-rubber tree. Though this is the most important rubber tree, yet several other laticiferous plants furnish commercial caoutchouc—notably among these may be mentioned: *Manihot Glaziovii* (Ceãra rubber) of Eastern Brazil; *Castilloa elastica*, of Mexico and Central America; *Funtumia elastica* (Lagos rubber), and species of *Landolphia* of tropical Africa; *Ficus elastica* (Rambong) of Assam and Malaya, the familiar "rubber plant" of our greenhouses. At the present time Brazil furnishes about 60 per cent. and Africa 30 per cent. of the world's supply.

The consumption of india-rubber has augmented so rapidly within the last few years, owing largely to the great increase in rubber-tyred vehicles of all kinds, that the supplies are becoming quite unequal to the demand. Hitherto the world's crop of rubber has come solely from wild sources. In all probability if the cultivation of caoutchouc-yielding trees had been delayed much longer, a rubber famine would be imminent. As it is, there will be a shortage in supplies for a few years to come. Prices for the raw material will rule high, and rubber goods will tend to become dearer or of lower quality through admixtures. The extended use of caoutchouc will be prevented, and its much-needed employment for flooring and pavement, where wear and tear is great or silence desired, will have to be postponed. The manufacturers are now paying an unprecedented and quite unforeseen price for this raw material. Fine hard Para is at present quoted in the London market at about 7*s*. 6*d*. per lb.[1] A year ago it was only 5*s*.; in fact, it is now nearly double the average figures for the twenty-five years preceding 1909. In July 1909 the price rose sensationally from 6*s*. to 8*s*. per lb. It was generally expected that it would drop considerably in the autumn with the incoming of the Amazon supplies. These, however, turned out to be smaller than anticipated, and instead of a fall, a rise of another shilling per lb. took place.[2] At present there seems to be a probability of a 7*s*. basis instead of a 5*s*. one

[1] London price, 7*s*. 6½*d*., December 24, 1909.

[2] The record figure, 9*s*. 2½*d*., was reached in early November, 1909.

being maintained for some time. The sooner the price can be brought down to 4*s.* or 5*s.* per lb. the better for the progress of the world generally.

Let us glance at the possible future supplies of wild rubber. Tropical Africa in recent years has supplied a considerable quantity of low-grade rubber, largely owing to the drastic measures ruling in the Congo State. The supplies from this source appear to be on the decline. The ruthless destruction of the Landolphia vines will prevent these plants from furnishing much rubber for some years to come. "Red" rubber ere long will be a horror of the past, and the incoming of the plantation variety will hasten its extinction. The exploiting of fresh areas of Funtumia, and possibly of Landolphia, may make up temporarily to some extent, but the amount of African wild rubber is not likely to increase but rather to diminish.

The only source of real importance is the Amazon region. Statements have been made to the effect that the supply there is wellnigh inexhaustible. Doubtless untapped areas exist, and, further, the Hevea tree quickly regenerates itself naturally from seed. Yet under existing circumstances the output from the Amazons does not seem capable of much expansion. The Brazilian Government, however, appears at last to be arousing itself, now that it sees a formidable rival in the plantation rubber of the East. A Congress is shortly to be held at Manaos, the great rubber port of the Upper Amazon, to debate such questions as the extent of rubber lands in unexploited Brazilian territory and whether cultivation offers the best means of maintaining the preponderance of Amazon rubber in the markets. But Ceylon and Malaya have several years' start in the way of cultivation. Caoutchouc can there be turned out at the cost only of 1*s.* to 1*s.* 6*d.* per lb., whereas it is calculated that a pound of wild Para rubber costs 2*s.* 6*d.* to collect, a sum more likely to increase than decrease when less accessible regions are approached. The lot of the native rubber-collector is not an envious one. Unless he can exchange his hard-gotten commodity to some considerable advantage to himself, he is not likely to be induced to exploit less inviting districts. Further, the supply of labour itself for the industry is by no means large. Still there is little doubt that Brazil could considerably increase its production by opening up its remoter rubber lands, by granting increased facilities to the collectors, and by reducing the tax on this export, which is one of the main productions of the country at the present time. This policy would undoubtedly pay with the price of rubber at anything like the present figure. But as far as one can foresee, the more distant future of Brazil as a great rubber-producer must lie largely in its adoption of cultivation.

At present plantation rubber forms a mere fraction of the world's supply—perhaps 5 per cent. Since 1905, when about 200 tons of it were exported from the East, the output has doubled year by year. If this continues the yield in four years' time will be equal to the total annual output of wild rubber at the present time—viz. 70,000 tons. Such a quick rate of increase may not be maintained. Half a million acres, however, will be in bearing in 1914, and, even allowing only a crop of 100 lb. per acre, a low estimate, this would mean an output of 22,000 tons—a considerable part of the world's present supply. In ten years' time the amount of plantation rubber, on a conservative basis, can hardly be less than 100,000 tons per annum. At this period probably supply may begin to overtake demand, with an inevitable drop in prices. Rubber might then descend to 3*s.* per lb., a price, judging from present conditions, quite remunerative to the planters, but not to the collectors of the wild product. Thus it seems that the world's supply sooner or later will be derived from plantation sources. The possibility of a synthetic commercial caoutchouc appears as far off as ever, and no adequate substitute seems forthcoming. Rubber, then, like the majority of economic plant-products, will in all probability in the near future be obtained largely, if not solely, from cultivated sources. The study, therefore, of the cultivation of rubber trees is of great importance. The methods in use at present for extracting the latex and preparing the rubber therefrom, though fairly satisfactory, cannot be regarded as final. Everything connected with this novel form of cultivation is still in the experimental stage, requiring not only the close attention of the practical agriculturist, but also the services of the botanist, chemist, and physicist, and especially of that much-needed but rarer expert, the biochemist.

It is the purpose of this article to describe briefly the methods employed on the estates, and to dwell somewhat on the problems connected with them, hoping thereby to arouse a general scientific interest in the subject. Before doing so, a short account of the history of rubber cultivation may not be out of place.

Historical

Although the modern industry of rubber-planting may be considered to date back only some ten or eleven years, yet to trace this new cultivation from its inception we must revert to the year 1876. The seeds destined to become the source of most of the Para-rubber trees now growing in the East were in that year collected in Brazil, brought to England, and sown at Kew. The young plants raised from these seeds were transhipped to Ceylon. This introduction of *Hevea brasiliensis* to the eastern tropics was due chiefly to the energy and foresight of two men, both happily with us at the present day, Sir Joseph Hooker, then Director of Kew, and Mr. Wickham, at that time engaged in planting pursuits in tropical South America. Drawings of the foliage and fruit of the tree made by Wickham were seen by Hooker, and the latter did not rest until he had persuaded the India Office to grant Wickham a commission for the collection and conveyance of the seed to England. How this was successfully accomplished has recently been retold by Mr. Wickham himself.[1] The story forms the romance of tropical agriculture. Owing to the short vitality possessed by this oily seed,[2] no time had to be lost in conveying the quantity collected across the ocean. Some seventy thousand seeds reached Kew Gardens, and from them quickly sprang a goodly array of seedlings. Ceylon was chosen for their reception, and two thousand young plants reached this favoured isle in 1876. They were mainly planted in a special plot of ground at Henaratgoda in the low country. Soon a small forest of young Heveas grew up. This grove is now historic, for from it the first planters to take up rubber cultivation obtained their seed; in addition these trees afforded the means for carrying out the early work in tapping and the preparation of rubber, upon the results of which the estates have largely based the methods now in use. The total cost of the introduction of the Para-rubber tree to the East amounted

[1] H. A. Wickham, *Para Indian Rubber* (1908). London, 1908, pp. 45–59. (See review in *Science Progress*, 1909, **3**, 705–6.)

[2] By careful packing in powdered charcoal the vitality of the seed can be somewhat prolonged.

to £1500, a trifling sum considering the wealth it is now producing and is likely to produce in the near future.

About the same period Cross was instrumental in bringing to the East two other important caoutchouc-yielding trees of the New World—viz. *Manihot Glaziovii* (Ceãra rubber) and *Castilloa elastica* (Central American rubber).

After the collapse of the coffee industry in Ceylon, the planters for a short time in the early eighties turned their attention to the Ceãra-rubber tree. However, its cultivation never attained great dimensions, and was soon extinguished by the rush into tea planting. The general consensus of opinion was that Ceãra rubber paid to collect, but not to grow.

Castilloa has never been largely planted in Ceylon or elsewhere in the East. It does not grow with the same vigour as Hevea, nor has it taken so kindly to its new home. It is fortunate that Ceylon planters adopted Hevea rather than Castilloa, as all recent returns have shown the former to be far and away the better yielding tree, even though at one period results seemed in favour of the latter. Castilloa, however, has been largely planted in Mexico—in fact, its cultivation commenced there a year or two in advance of that of Hevea in the East. Little, however, is heard at present of plantation Castilloa rubber on the London market. This is partly due to the fact that the trees do not come into bearing as soon as those of Hevea and yield less when they do; and partly because the United States mainly receives what rubber is produced by the Mexican plantations.

In 1888 the late Dr. Trimen, then Director of the Ceylon Botanic Gardens, commenced tapping experiments on the Hevea trees at Henaratgoda, grown from the Kew seedlings. He strongly advocated rubber planting, and was supported by Mr. John Ferguson, editor of the *Ceylon Observer*, who influenced planters, especially in the Malay States, by the publication of a manual on the subject. Dr. Willis, who succeeded Trimen as Director in 1896, took up the subject of india-rubber energetically and enthusiastically. In a circular[1] published in January 1898 he advocated the cultivation of the Para-rubber tree as deserving the attention of the Ceylon planters. He induced the Government to sanction the appointment of a scientific assistant. With this help a year's work

[1] J. C. Willis, *Circular*, *Roy. Bot. Gardens*, *Ceylon*, No. 4, Series I. (1898).

devoted to rubber tapping and preparation placed Hevea in a still more favourable position as a yielder of caoutchouc. The discovery of the so-called "wound-response" and the elaboration of a ready means of preparing clean rubber from the latex afforded the planters a basis for future procedure. The rush into rubber began then, and has continued ever since with increasing force. The tree has exceeded the most sanguine expectations regarding its producing capacity and vigour of growth. A few fortunate planters who laid down areas with this tree before this period have been the first to reap their reward.

About 750 acres had been planted with Hevea in Ceylon previous to the year 1899. Now at least 180,000 acres are under rubber in the island, either pure or planted amongst tea and cacao. Ceylon, though the pioneer, has had to give place to the Malay States in respect to area and yield. Planters soon saw an excellent opening for Para-rubber cultivation there. The soil and climate have been found especially suitable for the rapid growth of this tree, and the success already achieved is phenomenal. The premier company, Selangor, formed in 1899, paid a dividend of 75 per cent. for 1908, and is calculated to have earned one of 250 per cent. for the year just closed. Such extraordinary profits are of course partly due to the high market price of raw rubber at the present time, but partly also to increased output. No estate, as yet, has reached its full producing capacity.

Over 300,000 acres are now under Para rubber in the Malay States. Its cultivation has also extended to Java, Sumatra, Borneo, etc. At least 600,000 acres must now be planted with Hevea in the Middle East.

Attention in certain quarters has been redirected to Ceãra rubber (*Manihot Glaziovii*).[1] As this small tree will grow on dry ground where Hevea would not flourish, and as it produces good caoutchouc at an early age, it may perhaps become of some importance as a source of cultivated rubber. It is being planted largely in German East Africa, Nyasaland, and the Zanzibar Protectorate.

Rambong rubber (*Ficus elastica*) has received from the

[1] Three other species of Manihot (*M. dichotoma*, *M. heptaphylla* and *M. piauhyensis*) are now attracting attention as rubber plants. It is too early yet to decide as to whether any of them may be superior to *M. Glaziovii* for cultivation.

Dutch in Java considerable cultural attention in the past, and where growing now on any estate the trees are a valuable asset, as the rubber when well prepared commands a price only a little lower than fine Para. It is, however, likely to be replaced gradually by Hevea. Its banyan nature is a drawback to easy tapping, and in other respects it is inferior to Hevea.

The only other arborescent form of possible value for cultivation seems to be *Funtumia elastica* of tropical Africa. Attempts to grow it, however, have not met with much success, and Hevea is being introduced into West Africa in its stead.

The Landolphias are unlikely to become a cultivated source of india-rubber, as they are climbers (lianes) of slow growth.

Experience all points to *Hevea brasiliensis* as the best tree for cultivation. Manihot may take its place in drier tropical regions. Castilloa, Ficus, and possibly Funtumia might be useful as subsidiary sources, and be grown as trees in protective belts on estates.

General Cultivation

It is not within the scope of this article, nor is it within the writer's province, to deal in a complete manner with the general cultivation of the Para-rubber tree. However, as this is a crop of a special and novel kind, a few remarks respecting its peculiarities and the problems it offers for solution may not be without interest.

Close v. *Wide Planting.*—Hevea is a forest tree, and its cultivation might therefore be deemed a branch of forestry and so conform to the rules of sylviculture; but timber production is not the aim, hence close planting with the object of producing long straight poles is not necessarily the best means of growing this tree.

As the latex (rubber milk) is obtained from the bark of the trunk, the main purpose to be striven after is the production of as large an area of bark as possible in a given time. Further, since the greatest yield of latex is from the basal part of the trunk, thus making tapping above six feet, as a rule, inadvisable, it would seem expedient to grow the tree so as to throw the main increase of girth into the basal six to ten feet of bole. In sylviculture the great length of unbranched stem is secured at the expense of its thickness. As soon as the maximum height has been reached, thinning is commenced, in order to give room for a greater development of leaf-canopy to hasten the increase in girth. In rubber cultivation, however, thickness rather than height of trunk is desired from the beginning, so the trees must be planted much wider apart than appertains in ordinary forestry, in order to afford room for an early and ample production of foliage.

The first areas laid down with Para rubber were planted with trees ten feet apart, roughly 400 to the acre. It became evident in a few years that the growth in thickness would be greatly retarded if the trees were allowed to continue so crowded, consequently thinning has had to be practised. Wider planting is now more generally undertaken, and a distance apart of twenty feet (100 trees per acre) is commonly followed. Even at this interval the shoots of adjoining trees will often begin to interlace in five or six years' time, about the period when tapping can be commenced. Judicious thinning might be started now by removing the least desirable trees after thoroughly tapping them.

The correct planting distance to be pursued is still an open question. Time will doubtless show whether room for the ultimate possible extension should be provided at the outset, or whether a closer distance should be adopted while the trees are young, to be followed by thinning later. In the latter case probably more rubber may be obtained per acre during the first couple of years of tapping, but this may be at the expense of future yields. The present view appears to be rather that within reasonable limits a closely planted acre of rubber is worth no more than, if as much as, a widely planted one.

Straight v. *Forked Trunks.*—Since tapping is usually confined to the basal six feet of trunk, and since trees which fork early have generally a greater basal girth than those of the same age which remain straight, attention has been turned to the desirability of artificially inducing trees to fork early, in order to hasten their increase in girth. This can be accomplished with the least injury and trouble by what is known as "thumb-nail" pruning. The terminal bud is pinched out when the young tree has reached a height of about ten feet; forking then takes place, and further pruning can be practised if necessary to reduce the number of main branches. Wickham[1] strongly recommends this practice, and considers the ideal tree-form for Hevea to be

[1] Wickham, *loc. cit.* p. 22.

three main primary limbs, and to each of these three secondary branches—nine in all. Wright[1] also favours it, though pointing out that it is unwise to practise it on trees growing in a light soil and exposed situation, for they would in time be liable to be blown over by strong winds, owing to the weight of leaf canopy produced; they would, in fact, be more top-heavy than unforked trees.

Petch,[2] mycologist to the Ceylon Government, fears that such treatment will favour the entrance of the fungus *Corticium javanicum*, a somewhat dreaded bark disease. Ryckmann states that in Java and Sumatra this fungus attacks chiefly the forked trees, either natural or induced. The cleft affords a lodging-place for spores. A split in the wood is liable to occur here through wind or other causes, thus allowing the entrance of the fungus-hypha, and so the commencement of disease.

Catch Crops.—As a newly planted area of Para rubber will give no return for at least five or six years, and as between the widely planted trees there is much unoccupied ground, some profit may be immediately secured by growing what are called catch crops. Though their cultivation may lead to the payment of small dividends before the rubber comes into bearing, their value is to some extent doubtful, as these inter-planted crops frequently retard the growth of the rubber trees. Cassava (*Manihot utilissima*) has been largely used, but it is hardly a desirable plant to employ, as botanically it is too nearly related to Hevea. A disease or insect enemy which attacks the one will most likely spread to the other. The subject of catch crops and their desirability is dealt with fully in Wright's book.[3] There is much to be said for and against their cultivation.

Protective Belts.—A large uninterrupted area occupied by a single species of plant offers a most suitable field for the spread of a fungus or an insect foe. There is nothing to check a disease commencing at one point from spreading rapidly over the whole plantation. Consequently a system of blocks, separated from one another by screens composed of other

[1] H. Wright, *Hevea brasiliensis or Para Rubber*, 3rd edit. 1908, 48-51.

[2] Petch, *Circular and Agric. Journ., Roy. Bot. Gardens, Ceylon*, 1909, No. 21, vol. iv. p. 193.

[3] H. Wright, *Hevea brasiliensis or Para Rubber*, 3rd edit. 1908, pp. 51-6 (the standard work on rubber cultivation).

trees, is recommended for rubber estates. A disease or pest observed in one block might then be overcome before it had time to penetrate to a neighbouring area.

These protective belts may be formed by the retention of strips of the virgin forest, or they may be planted specially. If the latter course be adopted, trees of economic importance should, if possible, be chosen, care being taken not to select any nearly related to that composing the main cultivation. For example, Castilloa or Ficus might be used in connection with Hevea, thus affording extra quantities of caoutchouc; or trees useful for supplying timber for the estate might be planted. Manihot would not be advisable, as it not only belongs to the same family, the Euphorbiaceæ, but also to the same tribe. Fungi and insects often confine themselves to nearly related groups of species.

Seed-Selection.—Another matter worthy of brief mention is seed-selection. Too little attention has hitherto been paid to this. Estates, as a rule, have been planted with seeds from Hevea trees irrespective of their rubber-producing qualities. Selection might have been commenced ten years ago to the great advantage of those now about to engage in Para-rubber planting. It is not too late to begin, since even if the laying down of new areas in rubber should soon cease, old estates will doubtless require some renewing in course of time.

Suggestions[1] thrown out about the advisability of selection nine years ago were not heeded; in the interval nothing systematic in this direction has apparently been done. Now several are beginning to see the importance of turning attention to seed-selection. Sandemann[2] has recently advocated the practice strongly, and writes: "The matter was not perhaps so very pressing at the present moment, but would prove to be of very great importance if the price of rubber fell considerably, and especially so if that of labour rose coincidently."

Apparently there is a strain of Hevea now growing in the East which is a poor latex yielder but a great seed bearer. If care be not taken, estates may be planted with this variety, only to cause grave disappointment in a few years' time, when the trees reach the bearing age.

[1] J. H. Hart, *India-Rubber World*, October 1900, p. 6; J. Parkin, *idem*, January 1901, p. 105.

[2] Sandemann, *India-rubber Journal*, 1909, vol. xxxviii. p. 345.

Now that plant-breeding has almost become an exact science, largely through the application of the principles of Mendelism, comparatively quick results might be obtained even with a tropical tree. In ten to fifteen years' time seeds of a valuable strain might be forthcoming with which estates could replace worn-out trees or plant additional ground. The writer has referred in greater detail to this matter in a recent article.[1]

The Extraction of the Latex

The procedure employed in the East for the extraction of the latex from the stems of cultivated Hevea trees was elaborated independently, and not influenced by the native method still used in the forests of the Amazon.

The late Dr. Trimen in 1888 commenced tapping experiments at Henaratgoda in Ceylon on the rubber trees which had grown from the seedlings received from Kew in 1876. Vertical rows of V-shaped incisions were made in the bark of the trunk, from a height of six feet downwards, with a mallet and carpenter's chisel. The incisions were placed about a foot apart vertically, and the rows at a like distance horizontally. The latex oozing out of these cuts was made to trickle down the surface of the bark in a series of streams corresponding to the number of vertical rows of incisions. The whole of the milk was caught at the base by a clay gutter moulded round the trunk, and directed into one or more coco-nut shells placed around the foot of the tree. A second tapping was performed in a similar manner, the new incisions being inserted between the old ones, and so for subsequent bleedings. For details of this somewhat crude method, now almost obsolete, the reader is referred to one of the circulars published by the Ceylon Botanic Gardens Department.[2]

Dr. Willis continued the tapping experiments initiated by his predecessor, employing the same method. His results with respect to yield of rubber per single tapping brought out the remarkable fact that the second tapping gives a considerably larger quantity of caoutchouc than the first. Since his figures gave the first indication of the now well-known

[1] J. Parkin, *India-Rubber Journal* (Quarter-century No.), 1909, p. 63.

[2] Willis, *Circular*, *Roy. Bot. Gardens*, *Ceylon*, 1898, No. 4, Series I. 30–31.

"wound-response," it may be of interest to quote his remarks and figures from the circular [1] already referred to:

"The tappings may follow one another at intervals of a week for about four to eight weeks. The second tapping gives a much larger yield than the first, and the third and fourth tappings are usually very productive. In a series of experiments made during 1897 on trees of about two feet mean girth, the average yield per tree of the successive weekly tappings was as follows:

	oz		oz.
First week	·73	Fourth week	·80
Second week	1·48	Fifth week	·67
Third week	·97	Sixth week	·52
Total			5·17 oz."

Willis, realising that the methods he was then using for the extraction and preparation of the rubber were probably capable of much improvement, set the writer (who had just been appointed his scientific assistant) to work on these matters. The main outcome of this investigation,[2] carried out in Ceylon at both Peradeniya and Henaratgoda in 1898–99, consisted in the demonstration of "wound-response," and the introduction of an easy means of preparing rubber of high quality and purity from the latex. The subject of preparation is treated of in the next section of this paper. Our attention must now be turned to the phenomenon of wound-response.

Wound-response.—On general grounds it might be assumed that the trunk of a rubber tree would have yielded most of its store of latex after a single extensive tapping, so that none, or very little, would be forthcoming from a second tapping within a few days. This is practically what happens in the case of *Castilloa elastica.*

On the other hand, taking into account Willis's results, which show about double the weight of rubber from the second tapping, it might be conjectured that the injuries (incisions) stimulated in some way the accumulation of latex, so that a greater flow would issue from a similar number of incisions made a few days later. This, in fact, is how Hevea behaves.

Several simple experiments soon proved this. One was conducted in this wise. A piece of bark about an inch square was

[1] Willis, *Circular*, *Roy. Bot. Gardens*, *Ceylon*, 1898, No. 4, Series I. p. 32.

[2] J. Parkin, *Circulars*, *Roy. Bot. Gardens*, *Ceylon*, 1899, Nos. 12, 13, 14, Series I.

removed from certain Hevea trunks. After the lapse of two days, incisions were made near the wound and also on parts of the trunk at the same level farthest away from, *i.e.* opposite, the injury. Fully double the quantity of latex was obtained from the cuts near the wound, as compared with that yielded by those incisions made opposite.

Attention was then directed to the time-interval necessary in order to render this response recognisable. After twelve hours no difference was observed between the volume of latex yielded by the two classes of incisions. Sometimes after twenty-four hours and generally after two days, the effect of wounding on the yield, however, was marked. Apparently the drier the soil the longer the time required for the response to appear. The experience of planters and others since has shown that two days is, as a rule, the best interval between successive tappings, but some estates tap every day with good returns.

The following table gives in concise form the results of a somewhat elaborate experiment conducted at Peradeniya from March to June 1899.[1] Four trees were employed, and a horizontal row of ten similar incisions made per tree per tapping. Each fresh row of incisions was made near those of the preceding tapping, in order that the wound-response might take effect. The interval between tappings was usually five days. A less allowance would most likely have made the experiment still more striking.

	Volume of Latex in cubic centimetres.		Volume of Latex in cubic centimetres.
1st tapping	61·0	8th tapping	253·0
2nd ,,	105·5	9th ,,	264·5
3rd ,,	220·0	10th ,,	275·0
4th ,,	208·5	11th ,,	255·0
5th ,,	255·5	12th ,,	262·0
6th ,,	290·0	13th ,,	328·0
7th ,,	276·0	14th ,,	449·0

This experiment brought out the effect of wounding on the flow of latex in a still more favourable light; and considering that at the fourteenth tapping, when the experiment had to be brought to a conclusion, the volume was the largest collected, it would appear that the limit to the full advantage to be gained from wound-response had not yet been reached.

From a practical point of view this experiment is defective in

[1] Parkin, *loc. cit.* 135–40.

that no estimations were made as to the percentage of caoutchouc in the latex from the successive tappings. A large flow might mean a thin milk, yielding little solid rubber. Stanley Arden's work in the Malay States has supplied in some measure the deficiency. His results, published in 1902, have proved that the weight of caoutchouc itself is increased by the wound-response, and this increment is maintained or further augmented for a number of tappings. The following figures refer to one of his experiments,[1] in which ten trees were tapped every day for a fortnight:

	Wet Rubber in oz.		Wet Rubber in oz.
1st tapping	6¼	8th tapping	31⅝
2nd "	11⅜	9th "	29
3rd "	17½	10th "	30⅜
4th "	23⅜	11th "	31¼
5th "	26⅜	12th "	29⅜
6th "	26¾	13th "	30⅝
7th "	23⅞	14th "	33¾

Here, as in the Ceylon experiment, the yield per tapping has been well maintained throughout; and at the fourteenth and last tapping, instead of any diminution in yield, there is a slight increase, showing that the experiment could have been continued longer with profitable results.

The demonstration of wound-response, therefore, placed Hevea in a much more favourable position as a rubber producer, stimulating its cultivation. Before the discovery of this peculiarity, Hevea as a latex yielder in Ceylon did not look at all exceptional—in fact, it appeared less promising than Castilloa. From similar incisions made in untapped trunks of these two trees much more latex flows from Castilloa than from Hevea—roughly five to six times as much. But if after the lapse of one or two days fresh incisions are made in the trees quite near the old ones, it will be found that from the Castilloa no latex, or very little, oozes out, while from the Hevea about double the volume given by the first wounds can be collected, and further, this tree will continue giving this and even larger quantities for some time to come. Consequently a very much greater weight of rubber will be obtained in a year from a tree of Hevea than from one of Castilloa of

[1] S. Arden, *Report on Hevea brasiliensis in the Malay Peninsula*, 1902, p. 15.

a similar size. The yields from estates planted with these two trees bear this out in a striking manner.

The planting of Castilloa in Mexico, as already mentioned, commenced a year or two in advance of that of Hevea in the East. Wound-response not being then known, the former seemed the more promising tree, as it yielded its latex with greater ease. At the present time, however, little is heard of Castilloa plantation rubber, while that of Hevea is making a sensation unparalleled in the history of tropical agriculture. This is wholly due to the advantage taken of the wound-response, which appears totally absent in Castilloa. The one gives pounds of rubber per annum, whilst the other gives ounces.

The latest accounts[1] of Castilloa in Mexico are not over-encouraging, but with rubber at anything like its present price, the estates now coming into bearing will doubtless prove remunerative. A six-year-old Castilloa apparently gives only 2 to 3 oz. of rubber in the year without seriously injuring the tree, whereas a Hevea of similar age will yield about a pound. At ten years old the proportion appears to be about 4 or 5 oz. for Castilloa and 3 to 4 lb. for Hevea. It seems that the more Castilloa has been studied from the economic standpoint, the less satisfactory it becomes, while, on the contrary, Hevea has ever continued to grow in favour.

This phenomenon of wound-response in Hevea is not only of great practical importance in rubber cultivation, but is also of considerable botanical interest, and requires more extended investigation. To what circumstances is the increased flow of latex arising from injury due? In Hevea the milk (laticiferous) tubes reside chiefly in the innermost third of the bark, *i.e.* in the youngest and most functional part of the bast (phloem). New tubes are continually being formed in the fresh phloem, produced by the actively dividing layer of cells, the cambium; these take the place of the older exterior tubes, which become compressed and eventually obliterated by the tree's expansion. If the laticiferous tubes in a definite area of bark were completely drained of their contents, two possibilities might happen. On the one hand, this region might yield little or no latex, until the cambium formed new tubes—a process occupying some time; or, on the

[1] *India-Rubber Journal*, 1909, vol. xxxvii, p. 701. An article on Castilloa cultivation in the Quarter-century No. (p. 85) of this Journal gives higher yields—viz. about half those of Hevea.

other hand, latex from the adjoining areas might flow in and refill the drained tubes, so that on retapping in a day or two an abundance of latex would exude. The first possibility may represent the behaviour of Castilloa, the second that of Hevea. In the latter tree the time would appear to be too short for any of the increased flow to be accounted for by the formation of new laticiferous elements. Probably in this case an injury causes an inrush of water into the surrounding intact tubes and perhaps also into the severed ones, which will be now plugged by hardened latex. This flow of liquid towards the injured spot may be required for the reparation of the wound.

The latex which oozes out from a primary tapping of a Hevea tree is thicker, containing less water and more caoutchouc, than that which flows from subsequent tappings; and further, it appears to give a poorer quality of rubber. In practice it is a disadvantage to have latex of a treacly consistency exuding, as much of it is apt to harden on the tree, before it reaches the receptacle, producing inferior scrap rubber. In fact, the initial tapping is of little value from a rubber-yielding point of view; it only serves as the guide to future work. Sometimes drip-tins are fixed just above the tapping area. These allow water to drop slowly upon the incisions and so prevent the latex in its course down the trunk from drying on the tree. A little ammonia or formalin added to the water makes this device more effectual, as the coagulation of the latex is prevented by these reagents.

The latex from the second tapping is thus thinner and more copious, and it continues so for many subsequent tappings. The percentage of caoutchouc in the latex resulting from this multiple tapping has, however, never been adequately worked out. Presumably the percentage (roughly 30 to 40) is fairly uniformly maintained for quite a long period (three to four months), but eventually falls, and a thin watery latex results, which does not pay to collect. This indicates that the tree requires a rest.

Wound-response appears to be a peculiarity of Hevea alone, or to speak more guardedly, it has not been shown to occur, as yet, in any other rubber tree to the extent that advantage may be taken of it in practice. It may exist in a much less marked degree, but this remains to be demonstrated.

Considering that the laticiferous system of Castilloa is of an essentially different construction from that of Hevea, it is perhaps not altogether remarkable that the two trees behave differently when tapped. In the former the laticiferous tubes are in mutual connection from the beginning. Special cells are differentiated in the embryo, and these produce by growth in length and ramification the whole laticiferous system of the plant. This is known as the non-articulate system.

In Hevea, on the other hand, the tubes arise from rows of cells through the breaking down of the intervening walls. The perforations are not always completely formed, so this, the articulate system, is relatively disconnected compared with the other.

A wound in a tree containing the first arrangement will therefore most likely drain a larger area of laticiferous tissue than one in a tree of the second type. This doubtless accounts for the greater flow of latex from an initial incision in the trunk of Castilloa compared with that from one in Hevea; but it is difficult to explain the wound-response in the one and not in the other. Perhaps Hevea has a much more extensive, though less communicative, system than Castilloa; or in other words, a trunk of Hevea has a much larger number of tubes, and so holds a greater quantity of latex, than a corresponding one of Castilloa. At the first tapping the latter gives up practically all its latex, on account of the tubes freely communicating with one another; whilst the former only yields up a very small portion of its total quantity of latex, through the comparatively disconnected nature of its system. Thus from a single trial Castilloa appears the better yielder. On re-tapping in a few days' time no more latex exudes. The tubes apparently do not refill with liquid, and so probably collapse. In Hevea, however, a fresh set of tubes will be severed at the second tapping, and if the new incision be made near the old one, the ducts here will probably be surcharged with latex owing to a great infiltration of fluid caused by the previous wounding; thus from such an incision an increased quantity of latex will flow.

A detailed microscopic study of the laticiferous systems of these two trees might shed some light on the above suppositions. Manihot, however, has a system similar to that of Hevea, and yet, as far as it has been investigated, it shows no wound-response. Johnson,[1] experimenting with this tree in Portuguese

[1] W. H. Johnson, *India-Rubber Journal*, 1908, vol. xxxv. p. 209.

East Africa, has failed to get it to respond to multiple tapping.

The Function of Latex.—A few words on the question of the function of latex are called for here. It is still largely a problem awaiting solution.

A nutritive function for the laticiferous tubes was at one time upheld. They were supposed to act as conductors of plastic material, especially of proteins, and were considered in some cases partly to replace the sieve tubes. Adherence to such a view has lost ground in recent years.

Spence,[1] however, has recently revived the nutritive view on somewhat startling lines. His studies on the oxidising enzymes of latex has led him to regard the caoutchouc as a food reserve, which by means of these ferments may be oxidised and broken down into simple carbohydrates for the plant's use. Physiologists will require much evidence before accepting such a novel theory.

That the tubes conduct or store food materials for the plant seems doubtful. Primarily the latex may be regarded rather as a waste product, and the tubes containing it as genetically related to, and a further development of, secretory sacs. But the substitution of an extensive system of communicating tubes in place of isolated sacs apparently implies the adoption of some new function, in addition to that of removing the waste products of metabolism. A conducting function is the one which suggests itself. The tubes may form channels for the conveyance and storage of water. Laticiferous plants, at any rate the arborescent ones, are distinctly numerous in the tropics, where transpiration at times is excessive, especially during the dry season.

Again, the theory has been advanced that the latex serves as a protection against insects and fungi. In respect to an insect, a puncture or bite will result in an outflow of latex, which may interfere with its further operations or prove distasteful to it. The penetration of a fungus through a wound may be prevented by the latex which oozes out, forming an impenetrable layer. This supposed protective function for latex must be investigated separately for each species in its original surroundings. The laticiferous system may have been evolved to repel certain foes occurring in the natural

[1] D. Spence, *Bio-Chemical Journal*, 1908, iii. 179-81.

habitat of the plant, and yet be ineffectual against other enemies which the species may meet in a new environment.

The theory of water-storage and conduction is perhaps the most plausible. The watery nature of the latex in the tree trunk of Hevea has been noticed to be affected by the state of the soil. When dry, the latex is thicker and flows out less readily, suggesting that the tree is drawing upon the reserve of water accumulated in the laticiferous tubes. In the alluvial regions of the Malay States the tree yields latex very abundantly. Here there is a surplus of moisture in the soil, and so the tubes are always well distended with latex. There is, in fact, no need to draw upon this reserve.

The removal of latex from the Para-rubber tree appears to have little or no detrimental effect. A young tree judiciously tapped continues to grow almost as well as one which has not been touched. Some observations made by Macmillan and Petch[1] have shown, however, that the seeds from tapped trees are, on the whole, lighter in weight than those from untapped ones. Any prejudicial effect of the tapping is probably due rather to the injury to and removal of the surrounding tissues than to the extraction of the latex itself.

Tapping Systems.—The demonstration of wound-response quickly influenced the method of tapping Hevea trees followed in the East. To gain the full benefit of this response, the new incision must be made quite near the previous one. Thus was suggested the feasibility of reopening the old wound, rather than of making a fresh incision. Experience has shown that very satisfactory results can be so obtained. A thin paring of bark is removed from the lower edge of the initial groove at each subsequent tapping. By this means the bark down to the cambium is gradually shaved away. Thus excision instead of incision has come to control the tapping systems now in vogue.

The original small V-shaped cut has been completely abandoned. Attention to wound-response showed that a single slanting cut served just as well. Oblique incisions form the basis of the methods of tapping in use.

At first the latex from each incision was collected separately.

[1] Macmillan and Petch, *Circular, Roy. Bot. Gardens, Ceylon*, 1908, vol. iv. No. 11.

This involves more labour, and has been generally discarded for basal collection.

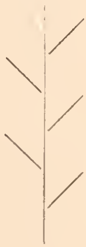
The two chief systems of tapping now practised on the estates are those known as the Spiral and the Herring-bone. The principle is the same in both. The initial grooving (tapping) forms a guide for all subsequent tappings performed during the year.

Spiral System.—A spiral groove is made in the bark of the trunk from a height of six feet to the base. If the incision is carried completely round the stem, then the system is called the Full Spiral; if only part way round, the Half Spiral. A bole of small girth will only require one spiral. If of larger circumference, then an extra spiral can be added for each additional foot of girth; thus a tree three feet round would need three. At each tapping a thin shaving of bark is removed from the lower edge of the spiral cut. The pressure of latex is thereby relieved, and a stream flows down the spiral into the receptacle placed at the base.

The full spiral, of all methods of tapping, yields the largest quantity of rubber in a given time, but is very drastic, as the whole of the cortical tissues from the height of six feet downwards is removed, most likely too quickly for the maintenance of the tree's general health. It is now considered the best system to adopt for trees which are subsequently to be removed as thinnings or for other reasons. Rubber in quantity is thus obtained with the minimum amount of labour.

Herring-bone Systems.—A vertical groove is made in the bark of the trunk extending from the base to a height of five or six feet. Then long oblique incisions about a foot apart are cut from it in an upward slanting direction. If the inclined cuts are made alternately on each side of the vertical groove, the method is known as the Full Herring-bone; if only on one side, the Half Herring-bone, thus:

Full Herring-bone



Half Herring-bone

The oblique incisions yield the latex, and the vertical groove serves as a channel of conduction to the basal receptacle. At each subsequent tapping a thin paring is taken off the lower edge of each oblique cut; the vertical groove is left untouched. Thus the extraction of latex from the area of trunk covered by the herring-bone can be continued, till the whole of the bark intervening between the original slanting incisions has been excised.

The half herring-bone is now generally preferred, as being less severe than the full herring-bone. A quarter of the girth of the trunk can be tapped on the former system each year. By the time (four years) the whole has been so treated, the renewed bark on the first area will be sufficiently mature to allow the multiple tapping to be recommenced.

Two important points should be observed in modern tapping. The wound should be reopened by as thin a paring as possible; and every care should be taken not to injure the cambium. The longer the bark can be made to last, the greater, as a rule, will be the yield of rubber. Shavings of bark one-twentieth of an inch or even less in thickness are now managed in practice. A foot of bark can, therefore, be made to last for about 250 successive tappings.

A wound which passes through the cambium into the wood heals badly. In the excision method of tapping careless manipulation results in an uneven bark renewal, producing a surface difficult to tap in a systematic manner. Consequently to guard against cambial injury and to ensure thin parings much ingenuity has been exercised in devising suitable tapping instruments. At least two dozen different knives have already been invented for the purpose.[1] Some of these have met with favour and are commonly used.

Pricking.—Since economy in bark excision is so important, the idea that puncturing might be substituted to some extent for paring was early mooted. A tool, termed the pricker, was brought out for the purpose. Good yields were obtained by the combined use of the parer and pricker. The bark was thus made to last longer.

[1] For details the reader is referred to Wright's textbook, pp. 79-88 (*Hevea brasiliensis or Para Rubber*, 3rd edit. 1908) and to the pages of the *India-Rubber Journal* for the latest knives.

Recently a Ceylon planter[1] has introduced a pricking system as a complete substitute for the paring method. It has not as yet met with much favour.

Pricking has been blamed for the production of burrs and nodules in the renewed bark. Petch[2] has stated reasons for this view. If correct, it is a serious drawback to the use of the pricker. Though opinion generally seems rather opposed to than in favour of pricking, either in conjunction with paring or alone, yet it is still a debatable point. The paring method on the half herring-bone system is giving excellent results on estates and is very systematic.

The question may be asked: Will the tree stand this somewhat severe treatment of removing gradually its bark up to a height of six feet? So far, no pronounced ill-effects have shown themselves. The bark on the excised area is renewed satisfactorily and this secondary covering is as rich or even richer in latex than the primary bark. This is on a par with the cinchona tree, which gives a greater yield of quinine from its renewed bark.

It has not yet been settled as to the time which should elapse before the reformed bark should be tapped. Four years has been considered a suitable period, but this may be possibly longer than is really required. Some results seem to show that if the new bark is tapped early, the rubber is of an inferior quality, even though the latex may be abundant.

Wickham,[3] in his criticism of plantation methods, views with disfavour this system of removing the bark, and thinks that, in the long run, the incision mode of tapping, as employed on the wild trees in the Amazon, will be found to be preferable. His views seem generally to run counter to the practices in the East. At the same time, the opinions of one who is so well acquainted with the Brazilian rubber industry are not to be lightly laid aside.

High Tapping.—It has already been mentioned that, as a rule, it is not advisable to continue the tapping of a Hevea trunk above six feet. In the first place, the yield of latex is much less from the upper parts of the stem, and secondly, high tapping

[1] Northway's Tapping System—see article in *India-Rubber Journal*, 1909, vol. xxxviii. p. 225.

[2] Petch, *Circular*, *Roy. Bot. Gardens*, *Ceylon*, 1909, vol. iv. No. 18.

[3] Wickham, *loc. cit.* p. 38.

requires the erection of scaffolding, which adds greatly to the expense.

Interesting experiments as to yield have been carried out by the Ceylon Botanic Gardens Department[1] on the original Henaratgoda trees. These bring out clearly the great rubber-producing capacity of the basal six feet of trunk and the small yield afforded by the higher parts.

The improbability of obtaining rubber from the young stems, leaves, and unripe fruits will be referred to in the concluding portion of this paper, which will deal chiefly with the preparation of the rubber from the latex.

[1] *Roy. Bot. Gardens, Ceylon, Administration Report*, 1906, p. 32.

THE FUTURE OF SCIENCE IN OUR SCHOOLS—THEIR COMPLETE RE-ORGANISATION A NECESSITY[1]

"The main thing which we ought to teach our youth is to *see* something—all that the eyes which God has given them are capable of seeing. The sum of what we *do* teach them is to *say* something. As far as I have experience of instruction, no man ever dreams of teaching a boy to get to the root of a matter; to think it out; to get quit of passion and desire in the process of thinking; or to fear no face of man in plainly asserting the ascertained result."—RUSKIN in *Modern Painters*.

I ASSUME that it is my privilege and office to-day to represent you; therefore I propose, for once, to break through that modesty which became you well in the past but which you cannot, in fairness to the public, allow to stand always in the way of your preferment.

Am I not voicing your conviction when I say that the future of our schools must depend on the position accorded in them to science—the acknowledged office of science being the elucidation of truth; that the men most competent to take charge of the schools will be the science masters—it being their business to study method and to be practical, therefore to solve problems and lead?

If shyness forbid you to recognise the picture, at least I should like to forewarn you of the greatness of your future responsibilities, so that you may prepare yourselves to bear the heavy burden which will be cast upon you.

Believe me, I am serious. I would ask you what the literary man is and does that he now should be held competent to undertake work which is essentially experimental and practical in its character, for which he has had no fit preparation, if indeed he be not unsuited to it by nature? It is clear that he owes his position not to special qualifications but to inheritance; he is as much out of place as is the stage-coach driver in these days of railways, electric cars, automobiles and aeroplanes. I venture to assert that ere long the literary man in charge of a school will be an anachronism;

[1] Presidential Address to the Association of Public School Science Masters, delivered at Westminster School, January 13, 1910.

the public will insist that headmasters shall have been trained in a wider school than that of letters alone.

But before science comes by its own in the schools, many bones must be broken; let us, if possible, fracture a few by way of a beginning.

In August last, at Winnipeg, which is on the confines of British-American civilisation, the absolute antipodes of this Westminster of ours, as I sat at the feet of a learned Headmaster and heard him discourse in sonorous periods of the imperfection and inadequacy of the classical system in vogue in our chief English schools, I marvelled at his courage, his energy, his breadth of view and the logical consistency of his arguments—all qualities rarely associated with the pronouncements of Headmasters. In moving a vote of thanks to him for his inspiring and invaluable address, I could not help calling attention to the fact—it was wicked of me, no doubt—that the one man who had proved to the public that he was able effectively to teach English boys Greek was now telling us that Greek was only for the very few—for those few who could appreciate it and who would find a use for it in later life. Properly speaking, we should there and then have sent an advisory telegram to Lord Curzon, Sir Wm. Anson & Co. at Oxford; unfortunately, we missed our opportunity and Oxford remains unrepentant and unreformed: it still insists upon lowering the moral tone of all entrants into University life by enforcing a test which is known to be farcical and futile, known to be one which spoils the young lives of pupils in preparatory schools. And doctors of divinity in charge of our schools smile blandly at such proceedings—so great is the influence of tradition, so great the deadening power of classical training in obscuring mental perspicacity, whatever its "cultural" effect may be.

I fear the name of my enthusiastic friend the Warden of Bradfield is anathema among his colleagues; yet after studying the conditions, both in our own country and in various parts of the great American continent, very carefully of late years, I cannot help feeling that men such as he, who go out into the world and consider its requirements, are the men we need to put moral purpose into our school system—that moral purpose which, more than anything else, is wanted in the schools of the world to-day.

On my return from America, on reaching Queenstown, I happened to see the *Daily Mail* of November 5, in which I found a reference in one of the editorials to opinions expressed by the new Headmaster of Rugby and by the Headmaster of the City of London School as to the value of classical education. Mr. David, I read, "had learnt that the classical system was quite the best but he had learnt more and more to realise that the modern was a very good second best."

We must all feel that it was very gracious and good of Mr. David to express such an opinion at the outset of his career as a Headmaster. If the modern be a very good second best already, as we are aware that the modern system—most of us scarcely dare call it a system yet—is at best but incompletely developed, we may well rest satisfied with the admission and await the time when we have a modern system. Mr. David will then perhaps be prepared to admit that it is quite the best; let us hope that he will co-operate with us in giving it this position.

Dr. Chilton is reported to have stated that Mr. David was right in assigning the higher value to a classical education.

"He recognised to the full the value of the modern side; but it is difficult—he said—to make a modern education a liberal education. He did not think that it is possible to exert by means of a purely modern system of education that liberal, broadening influence upon the mind which is the chief merit of a classical education. He hesitated to dogmatise but it was his own experience that a classical education makes the master, while a modern education makes the man."

Now much of this is mere verbiage. What is a modern education? What in particular is a *purely* modern system of education? What is a liberal education? Liberal is one of those shibboleths applied to education behind which the literary person always takes convenient shelter. What is the nature of the liberal, broadening influence upon the mind which the much-vaunted classical education gives? Judged by results, where and in what way is it manifest, at Oxford, for example? Its apparent effect is to make its votaries neglect, if not despise, other branches of learning. "The classical education makes the master, the modern the man." Good heavens! This while our masters are failing us in every direction and allowing the men to be masters, for the masters so-called show no alertness, no organising capacity; they lack ideas and ideals; the lessons of history—of classical history especially—are lost upon them. Classical training is supposed to be of value as leading to an appreciation of the meaning of words. I would challenge Dr. Chilton to ask himself if his words have any real meaning. I would challenge him to prove that he has real knowledge of, nay, even the will and power to appreciate, the possibilities before the modern teacher whose vision is not entirely obscured by classical spectacles.

On reading what Dr. Chilton had said, I could not help feeling that his attitude was far less satisfying than Mr. David's, but I admit I was under Dr. Gray's hypnotic influence. And I had once read Hazlitt's essay "on the Ignorance of the Learned."

I had in mind, too, a Friday-evening discourse delivered at the Royal Institution in 1868 by the late Dean Farrar when a classical master—therefore one of the elect—at Harrow School. Farrar did not hesitate to damn the literary system out and out.

"So far from being half finished, the real battle for educational reform has hardly begun. Latin and Greek still continue to be the all but exclusive staple of our education, and though a classical training conducted on wise principles and with reasonable methods is of the highest value, yet the many and serious evils which our present system of it involves have been resolutely ignored. The yoke of the Greek and Latin languages has been made needlessly humiliating and needlessly heavy; taken alone, it is doubtful whether they furnish the best mental discipline for any but certain that they do not furnish even a good discipline for all, and they remain to this day entrenched behind a mountain-heap of fallacies, of which no small number ought to have been banished ignominiously to the region of the most exploded errors. . . .

"The question then is, not whether the education is to be literary or scientific but whether it is to be scientific or nil; the struggle is not between science and literature but between something and nothing, between science and no science, between intellectual culture and its almost total absence."

This opinion, be it noted, was given more than forty years ago!

There came also into my memory the confessions made in the Upton Letters, issued from a not unknown school on the banks of the Thames, it will be remembered; I since find in the letter of July 16, 1904, in which T. B. writes to his confidant:

"I grow every day more despondent about the education we give at our so-called classical schools. . . . One sees arrive here every year a lot of brisk, healthy boys, with fair intelligence and quite disposed to work, and at the other end one sees depart a corresponding set of young gentlemen who know nothing and can do nothing and are profoundly cynical about all intellectual things. And this is the result of the meal of chaff we serve out to them week after week: we collect it, we chop it up, we tie it up in packets, we spend hours administering it in teaspoons, and this is the end. . . .

"What produces the cynicism about work so common in classical schools is that the work is of a kind which does not seem to lead anywhere and classics are a painful necessity which the boys intend to banish from their minds as soon as they possibly can.

"This is a melancholy jeremiad, I am well aware; but it is also a frame of mind which grows upon me; and to come back to my original proposition, it is the stupidity of virtuous men which is responsible for the continuance of this arid, out-of-joint system."

The inimitable description of "A Speech Day" given in Mr. A. C. Benson's *At Large* (p. 219) also occurred to me:

"Then the Bishop went on to talk about educational things and he said with much emphasis that, in spite of all that was said about modern education, we most of us realised as we grew older that all culture was really based upon the Greek and Latin classics. We all stamped on the ground and cheered at that, I as lustily as the rest, though I am quite sure that it is not true. All that the Bishop really meant was that such culture as he himself possessed had been based on the classics. Now the Bishop is a robust, genial and sensible man, but he is not a strictly cultured man. He is only sketchily varnished with culture. He thinks that German literature is nebulous and French literature immoral. I don't suppose he ever reads an English book, except perhaps an ecclesiastical biography; he would say that he had no time to read a novel; probably he glances at *The Christian Year* on Sundays and peruses a Waverley novel if he is kept in bed by a cold. Yet he considers himself and would be generally considered a well-educated man. I believe myself that the reason why we, as a nation, love good literature so little is because we are starved at an impressionable age on a diet of classics; and to persist in regarding the classics as a high-water mark of the human intellect seems to me to argue a melancholy want of faith in the progress of the race. However, for the moment we all believed ourselves to be men of high culture, soundly based on the corner-stone of Latin and Greek. . . .

"And thus, the tide being high, the Bishop went into harbour at the top of the flood. I don't even complain of the nature of the address; it was, frankly, such as might have been given by a Sadducee in the time of Christ. But the interesting thing about it was that most of the people present believed it to be an ethical and even a religious address. It was the ethic of a professional bowler and the religion of a banker."

Finally, I bethought me of the practices followed by those strange people whose doings are so well described in Samuel Butler's *Erewhon*, as related in the chapters on their Colleges of Unreason. You may be comforted, as I have been, by the comments of the visitor to these Colleges:

"Perhaps, after all, it is better for a country that its seats of learning shall do more to suppress mental growth than to encourage it. Were it not for a certain priggishness which these places infuse into so great a number of their *alumni*, genuine work would become dangerously common. It is essential that by far the greater part of what is said or done in the world should be so ephemeral as to take itself away quickly; it should keep good for twenty-four hours or even twice as long; but it should not be good enough a week hence to prevent people from going on to something else. No doubt the marvellous development of journalism in England, as also the fact that our seats of learning aim rather at fostering mediocrity than anything higher, is due to our unconscious recognition of the fact that it is even more necessary to check exuberance of mental development than to encourage it. There can be no doubt that this is what our academic bodies do, and they do it the more effectually because they do it only subconsciously. They think they are advancing healthy mental assimilation and digestion, whereas in reality they are little better than cancer in the stomach."

Not long after I reached home I became aware of signs of revolt in an unexpected quarter—in Germany, that land of perfection in all matters educational, according to Mr. Haldane, Prof. Sadler and others, where classics have long ranked with mother's milk. I found that the great Prof. Ostwald, the noted expositor of *Ionomania*, had written a book on Great Men—only departed ones, at present—in which he contends that classical studies are generally worthless and positively detrimental to the development of originality. The following passages display his attitude:

"As at present organised, the school is a machine for the destruction rather than the development of originality. Together with the good it does, it provides so much that may be dispensed with, if it be not directly harmful, *that it is to be shunned in all cases in which the development of special ability is in question.*

"Latin has long ceased to be the gate of culture; indeed it is become the greatest obstacle in the way of culture.

"Those who have plunged the intermediate school into its present deplorable condition are certainly not those who can rescue it therefrom.

"How is help to be obtained? Only by every one who is aware (of the condition of affairs) raising his voice without regard to the odium he may incur. *For the future of our people is at stake.*"

This has been the cry of a small party of realists among us for years past. Year after year, however, our Headmasters demonstrate their impotence in public—but no one heeds the writing on the wall. When the Royal Society formally addresses the Universities, nothing is done.

Even in Russia a protest has been raised against the classical system. Thus Prof. Tilden, in the recent lecture he delivered to the Chemical Society on the world-renowed chemist and physicist Mendeléeff, calls attention to his irreconcilable enmity to the system and quotes the following passage among others from his *Remarks on Public Instruction in Russia* (1901): "The fundamental direction of Russian education should be living and real, not based on dead languages, grammatical rules and dialectical discussions, which, without experimental control, bring self-deceit, illusion, presumption and selfishness."

Lastly, let me point out that the book, published recently, entitled *Schools and Schoolboys*, written by Dr. Burge of Winchester and the Rev. Mr. Lyttelton of Eton, is not without value to my argument. It is remarkable, in the first place, as an illustration of the cogency of Dr. Warre's reasoning that "Latin and Greek are the great instructors in English." If handed in as a sixth-form essay, I can imagine it returned with the remark, "Good in intent but full of illogical statements, lacking point and needing rearrangement to make it a logical treatise. You should spend more time over your work and give more thought to it, taking care that your arguments are less often mutually destructive. Leave out the twaddle about 'blooming earls' setting an example by indulging in hand work[1]; in future the working-man will expect the aristocrat to prove that he has learnt to use *his head* on all possible occasions." Intentionally or unintentionally, from beginning to end, the essay is an open confession of the failure of the classical system that Mr. David describes as the very best. Which, then, are we to believe—Mr. David and Dr. Chilton or Mr. Farrar, Mr. Benson, Prof. Ostwald, Mendeléeff, Dr. Burge and Mr. Lyttelton? The doctors evidently disagree—I propose to honour the opinion of those who are broadminded enough openly to admit failure. The position is well summed up by Mr. Lyttelton in the statement:

"We are still under the spell of a powerful and ancient tradition which dictates to us this and that way of dealing with boys' minds but has absolutely nothing to say as to what these minds require. That is why at the present day we are dominated by every sort of consideration in settling a curriculum except the right one."

Confession is said to be good for the soul! It is also said that charity begins at home!

A few days ago the Headmasters, in solemn conference assembled, adopted the somewhat futile resolution: "That a boy should not begin to learn Greek until the foundations of Latin and French have been securely laid and until he has received systematic training in English." These terms are almost tantamount to a general confession of failure. Why should Headmasters combine in agreeing to do what is obviously their duty? Who shall say when foundations are securely laid? What is "systematic" training in English? Why could they not agree definitely and honestly to abolish Greek from the preparatory schools—which is what is desirable, if not intended? Dr. Burge and Canon Lyttelton think that no improvement can be made in the teaching unless there be agreement among many public schools; that if school A were to admit boys without Greek, the schools preparatory to A's school would be relieved of nothing. Perhaps not imme-

[1] Mr. Lyttelton at times rises to sublime heights of eloquence in recommending hand work, *e.g.* "Badly taught, any subject may 'be soft'; but there is no lesson when a boy can possibly give more of his attention to the work in hand or can be so effectively braced for perseverance, as when he is shaping a freely curved surface of a cup for use. If his attention wanders he spoils his piece of wood and his hope of service or cuts his finger. Where is there any such stimulus in Latin prose?" One is forced by such classical bathos to confess that Latin cannot possibly compete with cut fingers.

diately. But "all we like sheep have gone astray." If A were Winchester and B, Eton, acted with him, the problem would be solved, the fashion being set. Schoolmasters are mainly affected by the vice of indetermination if not by cowardice; somebody must jump in to show them how deep the water is before they will attempt to swim.

State control of schools must soon be demanded by the public if the changes which have so long been asked for—and admitted by Headmasters to be necessary and possible—are not made without much further delay. I am one of those who believe that no effort should be spared to avert this fate from them—that a State-directed system of education would infallibly land us in mediocrity by repressing initiative and reducing our schools to one dead level of uniformity.

Dr. Burge and Mr. Lyttelton imply that the shrinkage of classics in our schools is in deference "to an uninstructed spirit of commercialism." The testimonials I have quoted are in no way tainted with commercialism; those who wrote them—the late Dean Farrar, Samuel Butler, Mr. A. C. Benson and Prof. Ostwald—scarcely come under such a criticism. *Pace* Dr. Burge and the entire host of the Headmasters' Conference, the spirit at work is the spirit of common sense. Those who advocate the reform, root and branch, of our school practices—I count myself among them—have had no thought of commercialism in their minds. The commercialism has been on the side of the Headmasters, the Schools and the Universities. The schools have allowed the Examinations and the Scholarship system to dominate them for purely commercial reasons; the Examinations have been developed by the Universities into eminently commercial enterprises in which there is no longer a vestige of altruism to be found. Dr. Burge jibes at the prospect of control by the Board of Education; he does not see that control by the Universities has "crept upon the schools by stealth"—to use his own expression—in a most insidious way.

In face, however, of the outspoken confession from Eton and Winchester with which we are favoured, it is time that we put an end to the farce of proclaiming the supreme value of a classical education; it is time that we recognised that a trade-union attitude is not one which the scholastic profession can worthily support much longer. We are all agreed that we wish to develop character in boys and girls, but parents now ask that we also seek to fit them, as far as possible, for life in the world and that we will not attempt to force all into an antiquated unsuitable uniform.

Here is the chance for the science masters—they are more concerned than any other branch of the scholastic profession in effecting the necessary changes; indeed, I am satisfied that effective changes will not be made unless they intervene and insist on complete reconstruction of the curriculum. What is your Association for but to form and formulate opinion? You cannot act individually; if you act collectively you must command attention. I counsel nothing less than that you act as revolutionaries.

To quote from an important book by a recent American writer[1]:

"What we need in many hide-bound institutions is revolution, panic and thorough reorganisation along common-sense lines, after a comprehensive study of the everyday problems of the day, from the students' standpoint and not from those of a hundred years ago."

What is to be the keynote of this revolution? Let me answer the question by reference to the same writer.

"The present condition of business, professional and other activities may be summed up in one word—'problems.' Life in itself has always presented these, but to-day . . . it seems to be nothing else. Our problems are immense, intense and inherent in the very texture of our modern civilisation. . . .

"Who then is the greatest among us? The problem solver. How then shall we give our college students the highest training? By training them to solve life's problems. We are not to turn out a well-stuffed graduate but one cultured, forceful, upright, with every good quality developed in him most successfully to solve his life's problems. This is the straight edge against which we may measure a man's whole college career—How far has it developed to the utmost his latent abilities and made him a cultured gentleman and worker, fitted to solve the problems that will arise in his life?"

I recommend the whole of the chapter (XXIX.) from which these passages are taken to your earnest attention. The argu-

[1] Clarence F. Birdseye, *Individual Training in our Colleges* (The Macmillan Co., 1907).

ments used apply as much to schools as to colleges—to the whole period of scholastic training, in fact.

To quote a few more of Mr. Birdseye's aphoristic passages :

"We realise that this is the age of new and gigantic problems, coming daily, thick and fast, any one of which would have staggered our forefathers. It is no longer the improbable but that which was (yesterday) the impossible which happens. . . . We should not attempt to make mental storehouses of men but mental factories, men so trained that they are not daunted by any difficulty, able to concentrate their best powers, at a moment's notice, upon any question that may arise. . . .

"As we go through cities we see crowds of corner loafers, young men probably of bad habits and with no special training in any one special line. . . . We are attempting to stuff our students with knowledge instead of teaching them to think. We are overlooking the fact that we can best train them when they see how they are to be directly benefited. There is the greatest difference between our trying to stuff all the learning possible into students and their willing and eager effort to absorb all the learning possible. The college should bring to bear every influence to make students eager to learn ; the rest of the training will be easy and delightful. . . .

"Before they can be successful in the world, students as individuals must learn to do things right and not to be satisfied until they have done them right. They must be made to appreciate the value of small things and of accuracy. . . . So our students should be content to be nothing less than the best possible in their lessons and in anything else they undertake. There should be no let up until they understand that 'moderately accurate knowledge is like a moderately fresh egg.' From this standpoint and with varied illustrations, they should be made to appreciate the vital necessity to them in the future of learning to go on only so fast as they are right up to that point. This one trait, thoroughly ingrained in them, will almost assure success in life. Not only should they be taught to do a thing over and over again until it is done right but also to verify things fully as they go along."

These things will be done, more or less effectually, when the teaching in our schools is conducted on scientific lines—neither according to mere tradition nor to modern syllabus but with forethought and good intent.

What can be done by way of revolution is to be seen at the Osborne and Dartmouth Colleges—although perhaps some things which should not be done may also still be seen there. These colleges are not in the hands of literary men. Frankly recognising that there is not time for everything in schoolboy life, the authorities have omitted classics from the curriculum. Another and perhaps the greatest step forward taken by these schools is the introduction of manual exercises as an essential part of the curriculum and the devotion of a very large part of the time to such work. The results obtained under this system are altogether remarkable: the qualities of self-helpfulness, alertness and exactness are encouraged and developed in a most successful manner; although the boys vary in aptitude, very few show themselves to be incapable of profiting from the workshop instruction and the whole outlook on life is altered by such training.

I believe we shall be forced, in the course of time, to admit that the plan adopted at the two Naval Colleges is one which must be followed generally and that every possible form of manual work will eventually find its place in our schools.

When I refer to these schools, I am told that their aims are entirely different from those of other schools. Of course they are; *their aim is definite*: it is to turn out boys who have learnt to help themselves and to think for themselves and to use their hands to some purpose—boys who have all their faculties developed. It may well be that the course has direct reference to the future of the boys—but so it should in every school; at present it has no reference to the future of any but a half-dozen or so who are being crammed like cattle to compete for some special pecuniary prize, some university scholarship. The curse of commercialism meets us at every step: instead of giving a liberal education to their best boys, our schools insist on turning them into the rankest specialists—classical or mathematical. What we need, in fact, more than anything else is to despecialise the teaching and make it general and liberal. Even our Universities are sinners in this respect, as almost every subject is taught from a purely professional point of view, although the majority of their students have no intention of taking up professional work. Attention is centred on the prize animals; the beasts of burden, who are the effective workers in the world, are in no way tended as they should be—much to their present advantage perhaps, pending the discovery of suitable, liberal methods of meeting their requirements. But the fact that we do neglect the majority should not be overlooked, even if it be not allowed to weigh in that obscure region of sentience, the conscience.

I have referred to the two naval schools as revolutionary enterprises which may well be taken as patterns.

As to the place of science, there is no example I can hold up before you. Some one is required who, in masterful manner, will follow the Admiralty authorities' example and take the bull by the horns—I should say, the Headmasters by the shoulders—and insist on proper treatment of the subject. It is conceivable but scarcely probable that some day we may have a Minister of Education who will rise to his opportunities. You all know perfectly well that you cannot do justice to the subject under present conditions in the time at your disposal.

And may I say—we are most of us not yet prepared to take up the right attitude and do the work properly? This is not our fault so much as it is our misfortune—the fault of Oxford and Cambridge, in most cases. How many have learnt to be problem solvers—to think even that there are such things as problems, except in the form of Shavian plays, perhaps? But "you never can tell": surely there is much talent latent among teachers which only needs developing?

The teaching of science in schools should consist primarily of instruction in the art of inquiry; in other words, the training should be given in the proper use of the experimental method. If this be not taught, the instruction has in no way served its purpose and is of little value. But how many will interpret this statement in the sense in which I make it? *Scarcely any, I know.* In fact, nothing more clearly demonstrates the complete failure of our present educational system than the existence of almost entire misunderstanding of the true meaning of the term "experiment." Teachers of science draw no distinction, as a rule, between the mere demonstration, verification, or practical exercise on the one hand and what is properly described as an experiment on the other. To speak of *showing* an experiment is a negation of terms—actually, a demonstration is given; students, as a rule, carry out practical exercises merely in order to verify statements made to them. If I tell my students that oxygen is a supporter of combustion and proceed to burn things in oxygen before their eyes, in order to persuade them of the truth of my statement and to impress the fact upon their slack memories, I give demonstrations: I am not experimenting.

We fail in making science teaching effective largely because it is not experimental and argumentative: certain facts are more or less impressed upon the memories of our pupils but nothing more.

The extraordinary value of experimental work is almost wholly lost under our present system, whether it be in school or college or university, owing to the general misunderstanding I have referred to. Our colleges need reform in this respect more than our schools—because the teachers are trained in them.

The system we adopt of lecturing to our students and then directing them to work from printed instructions in the laboratory is as vicious as it is possible to make it. We know it to be an absolute failure in practice, every examination proving this to be the case; yet we go on our way rejoicing. We science teachers, in fact, are often as hopelessly narrow in our outlook as are literary teachers—probably because we have allowed the literary example to influence us: we start under literary guidance and but few of us shake off the allegiance.

One great reason is that we begin too late; the child's natural desire to observe and experiment, to reason on the basis of observations made and from the results of experimental inquiry, must be developed, encouraged and fostered in every possible way. The habit is soon lost if in the least neglected—dogmatism has killed it throughout the ages; perhaps it kills it more effectively now than has been the case at any previous time, because it is aided by books and especially by the monstrously pernicious agency man has invented to oppress his intellect—our accursed examination system.

If proper habits are to be acquired, the foundation must be laid soundly in earliest youth and the mortar of practice never allowed to dry until the edifice be complete. An uncompromising resistance must be offered to all who deny the truth of this assertion; it raises the most important of all the issues to be considered by teachers. We were beginning to make the doctrine felt in schools when enthusiastic but uninstructed faddists came along with that rankest and most pretentious of hybrids, *Nature Study*—a subject which, as commonly taught, involves no study and has little, if any, connection with Nature; which is confessedly "unscientific" in its aims and methods.

Mr. Lyttelton's pronouncement on Science is as follows:

"Science, in the sense of chemistry, physics, biology, etc., is omitted from the preparatory school altogether. If mathematical measurements and handicraft combined with drawing are part of the curriculum and if the scientific language-teaching, grammar and syntax never outstrips the pupil's intelligence, the foundation for science specialisation is being securely laid. The question which requires more discussion than it has yet received is: How far can we safely postpone the beginning of science proper, laboratory work, etc.? Probably the answer would now be to this effect: That from fourteen to sixteen there should be an increased amount of rudimentary science work such as weighing and measuring and of the all-important work of accurate description."

I should like to analyse this passage line by line, almost word by word—to interrogate Mr. Lyttelton, so as to ascertain what meaning he wishes to be attached to his statements, to find out whether they have any clear meaning in his mind. Particularly, I should like to ask him why he heads the page which is about half covered by the passage I have quoted "A Bugbear"? What are we to suppose has caused him needless terror? Surely we have the right to ask literary magnates to be careful in their choice of words, to be clear and logical in their statements. If I am to extract any meaning from the passage, it is that there should be no "science proper" in the earlier years of school life. My contention is that there should be little else than *proper science*; but then my definition of science is "the business of knowing," in its best sense, without any equivocal meaning whatever. What Mr. Lyttelton calls science proper I should probably term science improper. The word science is generally misused: the connotation attached to it by schoolmasters (literary schoolmasters) is always too narrow. It is impossible, however, to be too emphatic that whether it be science proper or improper, systematic experimental science must be begun in the kindergarten and carried through every stage of the student's career. No schoolmaster will admit that literary work can be laid aside at any stage; we, on our side, cannot admit that experimental work can ever be omitted from the course.

Mr. Lyttelton, I fear, is not sound on the subject of science. I trust I do not do him an injustice if I assume that probably he has never made an experiment knowingly and does not know how to set about making one—therefore he cannot appreciate the influence of experimental work and its nature as a means of developing character; that although a literary man, he has not fathomed the full meaning of the statement made by the grave-digger, when discussing Ophelia's death, that "an act hath three branches: it is to act, to do and to perform"; that in defining an act, the grave-digger was defining an experiment, which is an act based upon definite argument, performed with a conscious purpose (a motive), carried out strictly with reference to that purpose, the results of which are finally discussed with direct reference to the purpose in mind. The ordinary laboratory "experiment" is usually something done in almost blind obedience to a more or less indefinite instruction given without any very clear motive, design or purpose, little if any attempt being made to apply the results in gaining an answer to the question proposed—the fact that a question has been (that is to say, should have been) asked being, as a rule, left out of account. The entry in the student's note-book is almost invariably restricted to a bare statement of what was done and the result.

The pernicious system of allowing rough notes to be taken, which are written up afterwards, is the cause of much of the difficulty—notes must be written out fully at the time, as the work proceeds; in no other way can lying—conscious and unconscious—be prevented; in no other way can it be made clear what was actually the motive in mind at the time when the work was begun.

Our object should be to develop the right attitude of mind in our pupils—if we take care of the pence, the pounds will take care of themselves, in the sense that, if we teach method, facts will necessarily be pressed into the service and graven upon the memory in a really effective manner—to a far greater extent than under the pressure of the examination system.

If the habit be acquired in school of solving problems, later on in life the facility gained in asking questions and seeking answers—the critical habit of mind—must inevitably prove to be applicable to the ordinary affairs of the world.

To deal now with the question of subject-matter. I have said that we need to despecialise: I would apply this to science teaching as well as to Latin, mathematics, geography and any one branch of science. We ought not to speak of teaching geology or physics or chemistry or biology in schools any more than we speak of teaching addition, subtraction, multiplication and division; just as we include all these latter under arithmetic, so ought we to include all that is necessary under the one term "experimental science." The point is one of great importance, as it will affect the training of teachers as well as school practice. The object in view must be that our pupils gain understanding of the world in which they live and work. The twaddle that is generally taught as science to-day is worthless for all practical purposes, both as discipline and as knowledge: it is far too didactic and lacks both point and argument; its educational value is therefore almost nil; literary critics are right in rating it low.

Physics and chemistry are as much inseparable subjects as are addition and multiplication; moreover, they are fundamental to all other branches of natural science—all other experimental and observational sciences are little else than applied physics and chemistry, differing mainly in the subject-matter treated. Physics, properly speaking, is that science which has to do with the intrinsic properties of materials; so soon as any change in molecular composition takes place, whether in individual substances or as a consequence of the interaction of molecules, the phenomena come within the province of chemistry. There is much misunderstanding on this subject. Much that is taught as physics belongs to chemistry—the changes which attend the passage of water into the solid or gaseous condition, for example, are essentially chemical changes, changes in molecular composition; unfortunately, as a rule, the physical phenomena are alone considered, the chemical are left out of account. This is largely because of the tendency to develop physics on the mathematical rather than on the rational side—of the tendency to be satisfied when formal expression has been given to outward and visible signs and to overlook inward and spiritual grace. True chemistry is the science of inward and spiritual grace.

Or to take another illustration, it is not improbable that the phenomena of magnetism are largely chemical—that is to say, that they include changes in composition, alterations in molecular structure or configuration. Oscillatory structural changes are probably at the root of the production of electric currents by the motion of a conductor in a magnetic field. I always teach that chemistry is synonymous with change—that chemistry is the science ot change, of change in molecular configuration or composition.

Physics deals with the pieces of which the world is made and considers their characters as separate individuals; chemistry deals with the games that can be played with their aid—the chief office of the chemist is to elucidate character by the study of the behaviour of the pieces when brought into interaction. Physics is therefore a far more limited science than chemistry.

Physics, in not a few respects, is the simpler science: everything in physics can be quantified sooner or later; there is much in chemistry that cannot, which requires for its appreciation that subtle sense commonly spoken of as artistic feeling. I am satisfied that chemistry is by far the most valuable of the sciences as an educational instrument, because comprehensive problems may be set and solved experimentally in a consistent, logical and thorough manner; instruction may be given in all that pertains to the art of inquiry in a way and to an extent which is not possible in the case of any other branch of knowledge. Whatever we teach in our schools, chemistry must not be neglected; it is the science of life, life being but a succession of chemical changes: it is therefore the basis of physiology.

I hope that I shall not be subject to the criticism that my opinion is given from the "nothing like leather" point of view. I have some working acquaintance with the other branches of science which come under consideration in schools, sufficient, I trust, to do justice to their relative values in the curriculum. My selection of chemistry as the subject of prime importance is made on broad educational grounds, both on account of its superior disciplinary value and from the utilitarian point of view: the knowledge it gives is more necessary to us than that derived from physics, if we are to understand ourselves.

It is impossible, on such an occasion as this, to formulate a school programme of studies in Natural Knowledge—the term definitive of the province of the Royal Society of London—but I may be allowed to give certain broad indications.

Work must begin in connection with the arithmetic class—the teaching of arithmetic must be made practical. Here comes the fundamental difficulty—the teachers are not practical.

Granted: but such idiots must no longer be allowed to attempt to teach in our schools'; the would-be teachers of arithmetic must be sent to school and learn to use centimetre and other scales and to weigh and measure, to cut out things from cardboard and sheet metal, to note that there are such objects as stones and other materials from which sermons can be drawn. They will be fingerless and fumble at first but if their positions be made dependent upon their efficiency, they will soon cease to be conservative; then they will become interested in the work on finding how much easier, how really delightful it is, when teaching is made practical—how, instead of being regarded as the natural enemy of the boy, they can gain his sympathy and come to be believed in by him as of some use on this earth.

At the earliest possible moment—namely, at the beginning—materials will be chosen for examination which are of natural origin and ready to hand. Nothing is better than the common, rolled garden pebble; the foundations of geology are laid when this is taken in hand and studied, sooner or later, from every possible point of view. Its appearance may be recorded, its texture or tact determined, its approximate size ascertained, its weight and volume found as accurately as possible with the measuring apparatus at disposal.

If the quest be continued, whenever possible, in field, by brook-side and at the sea, the natural history of the pebble may be worked out thoroughly and profitably; the first steps will then have been taken not only in geology but also in physiography and physics. Materials in common use may afterwards be worked with in all sorts of ways—in workshop and classroom. By recording every act as it is done (but never later)—why it is done, how it is done, with what result it is done, the lesson learnt by doing it—not only will training be given in English but also in writing; also in drawing if sketches be made in illustration of the written accounts. The systematic training in English, of which Headmasters are dreaming, will never be given effectually until given in some such way as that I picture—until boys and girls have something to write about which is actual fact within their own knowledge.

Nothing impresses me more than the blank ignorance of common materials displayed by the hundreds of nominally well-educated young fellows I meet with. The earth on which they have trodden all their lives is usually an unknown quantity to them—they can scarcely distinguish between sand and clay; the word "limestone" has no particular significance in their ears; the term "sedimentary deposit" conveys no meaning to them. Every intelligent person should have at least an elementary acquaintance with the fundamental ideas of geology, in order that he may put some meaning into natural scenery. That we neglect to give such training is truly surprising; the value of most of the geography we teach is *nil* in comparison with the value such knowledge should have. When travelling recently in the Yosemite Valley, a region of surpassing interest and loveliness, I could not help being impressed by the way in which my companions spent much of their time in tracing resemblances in the rocks to pigs, elephants and such like. The beauty of the scenery made no proper impression on them and they could see no meaning in the rocks or in the vegetation; neither did the stones preach sermons to them nor had the trees tongues. The significance of Wordsworth's lines came home to me:

> I heard a thousand blended notes,
> While in a grove I sate reclined,
> In that sweet mood when pleasant thoughts
> Bring sad thoughts to the mind.
>
> To her fair works did Nature link
> The human soul that through me ran,
> And much it grieved my heart to think
> What man has made of man.

The passage prefixed by Ruskin to his *Modern Painters* is even more expressive of the same feeling.

At as early a stage as possible, water should be taken as a common material worthy of study—in the first instance mainly with reference to its so-called physical properties. I have laid down a very complete scheme of work with water in my collected essays on the *Teaching of Scientific Method*, Chap. XXIII., to which I will venture to refer you.

When common materials have been studied very fully, mainly from the physical point of view, it will be time to lay the foundations of chemical belief. If metals have been examined, it will scarcely be necessary to seek an opportunity of drawing attention to the changes which some metals undergo when exposed. I am satisfied, after many years' experience, that no better way of beginning can be made than by inquiring into the rusting of iron, passing from this to the study of combustion in so far as is necessary to establish the existence in air of a constituent which is participant in such changes, which combines with metals to form earthy substances. I would advise that limestone be taken as the next subject of study and after this common salt, beginning with sea-water. Again I may refer you to suggestions I have made elsewhere as to the manner in which the problems these substances present may be treated.

I would lay particular stress on the importance of the thorough mastery of a single substance such as limestone, as its properties are typical of those of salts generally, including the rock-forming silicates; moreover, the properties of its two constituent oxides are equally typical of the two classes of oxides—the acidic and the alkylic. If once the method of arriving at the composite nature of such a substance be grasped—if the learner be duly seized with understanding of its properties in relation to those of its constituent oxides and elements—lessons of abiding value will have been learnt and a very fair appreciation of the problems which confront the chemist will have been gained. But to secure this end the work must be done, not only with utmost deliberation and understanding but exactly and with complete honesty of purpose—*by the pupil.*

The connecting link between chemistry and physics will be established when the interaction of a metal such as zinc and an acid such as chlorhydric or sulphuric is studied so as to make clear the conditions which determine the occurrence of chemical change and the essentially electrolytic character of the process. The simpler phenomena of the electric current may then be made the subject of inquiry. Energy and efficiency may afterwards come under consideration, in order that a sound knowledge may be acquired of the fundamental principles underlying the use of the steam engine, the internal combustion engine and the dynamo.

When, in examining salt, the discovery is made that spirit of salt is a composite gas and its composition is determined, opportunity is given to discuss the kinetic theory of gases, the existence of molecules and atoms, the determination of molecular weights and subsequently of atomic weights as distinct from equivalents. The meaning of formulæ may then be made clear and their use allowed. The fact that most of the formulæ in use are empirical and not molecular should be emphasised—that common salt, for example, is represented only empirically by the formula NaCl. The hideous habit of speaking of substances not by name but by naming the letters in their formulæ should never be allowed to grow up.

Now will come the stage at which the conception of structure should be developed by the thorough study of alcohol. The foundation will thus be laid for the profitable study of physiological problems, in so far as is necessary to obtain an insight into the all-important subject of food and its functions.

At an early period of the course a most careful study will have been made of plant growth from start to finish, in the sense that development will have been followed, watched and recorded through all its stages—but not elucidated. Now comes the time to elucidate the processes of growth. Flour is easily separated into starch and gluten, milk into fat and curd, meat into flesh and fat; the existence of two main classes of food materials being thereby established—the non-nitrogenous and the nitrogenous—the changes which attend germination may be followed with advantage. Commencing with the barley grain, its behaviour towards water alone and towards water containing acids, salts, etc., in solution may be studied from the point of view of Prof. Adrian Brown's recent striking observations, which demonstrate the existence, as an outer covering of the seed, of a membrane which, in a most marvellous way, prevents the entry of nearly all substances other than water into the grain and in like manner hinders the exit from the grain of soluble matters as these are formed during germination, thus preventing the loss of the food materials available for the development of the young plantlet. The disappearance of the starch—the loss of dry matter—is easily followed experimentally and the nature of the process made clear by studying the action of unboiled and boiled malt-extract on starch paste. The existence and mode of action of enzymes having been thus demonstrated, the oxidation process may be followed, qualitatively and quantitatively; it is desirable to use seeds in which fatty matters are the reserve material as well as starchy seeds for such experiments, in order to make clear the function of both starch and fat as foods.

The liberation of oxygen and the formation of starch under the influence of light are easily followed; the value of salts may be learnt by water-culture experiments. Yeast having been examined microscopically, alcohol may be prepared with its aid and proof obtained of the existence in yeast of enzymes capable of hydrolysing cane and starch sugar. In order that there may be clear understanding of the function of the albuminoids, their conversion under the influence of pancreatic juice into soluble diffusible materials should be studied; amino-acids such as glutaminic acid and glycine may be prepared, the one from gluten the other from gelatin, by hydrolysing these materials with chlorhydric acid. Finally, urea should be isolated from urine; it is even desirable to institute systematic observations on the amount of urine voided daily over a given period and to have determinations made of the amounts of urea and salt excreted in it.

The last subject to receive attention would be anatomy. The main disposition and functional importance of the various organs of the body should be understood by all. If time permit, the various chief types of the animal kingdom should be brought under review. And last but not least Darwin's work should not be left out of account.

The work must be done throughout by the boys, not by the teacher. To say that this is impossible is to confess that the task laid upon the boy is beyond his powers—tasks must be chosen which are not: in fact, all we need to do is to copy the example set by those who train boys to be skilful in games. No team of boys could ever succeed in cricket who merely watched masters play and only now and then hit balls this way and that according to an instruction given with every ball—they would never acquire any sense of independence; there would certainly be no outcry against over-indulgence in games under such a system—they would be as unpopular and ineffective as most school lessons are.

If we teach science at all in schools, we must teach it practically; our main care must be to develop the right spirit—the right attitude of mind in our pupils—a proper appreciation of scientific method and some power of applying it. The training must be thorough and exact—comparable in thoroughness and depth with that given in teaching the classical languages or geometry and algebra properly.

Teachers more than any one else will be benefited if the course of instruction be broadened and made thoroughly practical.

To some my programme may appear to be too ambitious; others will think one or other of its sections narrow. It is put forward only as a mere skeleton, to give emphasis to my contention that we must lay a broad foundation in school. Many probably will object to teaching the rudiments of so many branches of science and will contend that one treated fully is of more disciplinary value—they will not be prepared to undertake the task, being specialists. To these I would say, that every person of intelligence, in these days, must be something of a geologist, something of a physicist, something of a chemist, something of a biologist—to the extent of being able to appreciate common natural objects and common natural phenomena: nothing less can count as culture, nothing less will serve the ordinary requirements of life.

I am not placing experimental science in advance of other subjects but merely asking that it receive proper consideration and that its paramount importance as a means of forming character be recognised. My desire is to see proportionate attention paid to all necessary subjects of instruction: at present our schools and our universities are the seats of rankest specialisation, altogether illiberal in their tendencies, in no sense schools for masters but the homes of slaves.

I do not suppose for one moment that even when the best of systems are in operation in our schools and the best of teachers are engaged in them, that we shall succeed in carrying very many beyond a moderate degree of efficiency—the material will not allow of more. Intelligence—real intelligence — is born not made: at no time more than at the present has it been necessary to appreciate the truth of this axiom, for it is beyond question demonstrated scientific truth. I know, however, from experience, that results can be obtained far in advance of those to which we have long been accustomed. Far fewer will be sickened of learning, far fewer will be deprived of their mental independence by dogmatic teaching and by over-teaching — the two vices which are the most potent cause of breakdown in the schools of to-day and the reason why so little real progress is made.

Necessarily I shall be told that there is not time for a course such as I foreshadow—nor is there under present conditions. But then I contemplate a revolution in school procedure and that you will be revolutionaries. De-specialisation of subjects must be insisted upon—classical languages especially must be postponed to a late stage in the pupil's career, in order that a proper foundation may be laid—that method may be taught during the earlier years. Boys properly prepared in the way I contemplate will learn far more Latin and also far more Greek if desirable, than under the present system, even if the teaching be postponed until the last two years of school life—which, let me insist parenthetically and emphatically, should in no case whatever extend beyond seventeen years of age. Mathematics also must be de-specialised—it must be taught by practical men, with reference to its applications; moreover, we must recognise that the vast majority can learn but little and need but little and that only those who show some ability should be given special opportunity of mastering the subject.

In the case of classics and mathematics, as indeed of all subjects, specialised teaching must be reserved for the University or Technical School. Consequently, the whole scholarship system must be revised. We have it on no less authority than that of the distinguished President of the British Association for the Advancement of Science, Sir Joseph Thomson, that the present system satisfies no one and is admitted to be harmful. But it is of no use for a man in his position to give utterance to pious opinions on such a matter—Headmasters' Conferences and outsiders like myself may be left to do that. If Sir Joseph and those who think with him form a party and act within their University, the necessary changes will be made without difficulty; it is because there is no party organisation, no political system, that things drift from year to year, from bad to worse. What opinions must the masses have of the masters—of Dr. Chilton's hypothetical leaders in training at the Universities? The day cannot be far distant when they will force the Universities to meet the schools, so that the education of real masters may be possible.

But I am drifting from my point. In order that it may be possible to give proper instruction in scientific method in schools, not only must de-specialisation of all subjects be insisted upon—there must also be co-ordination of subjects to prevent the great overlapping by which, at present, so much time is lost.

So far as I can discover, there is no organisation of the teaching in our schools—scarcely any of our Headmasters are, in any sense of the term, masters of method. A new race of Headmasters is required, who will make the study of educational method their proper occupation—a race of broad-minded, practical men, who will be in touch with the world and prepared to put every form of commercialism aside. Such men will need the whole-hearted support not only of their staff but also of the public—as the public, at present, are as much in need of conversion to right methods and right objects as are the Headmasters.

But in order that justice may be done to all subjects we must not only de-specialise the teaching and co-ordinate and co-relate subjects—much other de-ing has to be done; the absurd notion that each particular subject must be taught week by week, throughout the entire period of attendance at school, should be abandoned in favour of the "block system" which prevails in places of higher instruction. Experimental studies cannot be carried on in any proper way, honestly and exactly, in the odd hour or two per week now grudgingly devoted to such work—time is the essence of success. It will therefore be necessary to bring all the teachers in a school into close co-operation in order that they may make arrangements to share the time which is available in different ways, week by week, so that at one time this, at another time some other subject, may receive the necessary attention.

Such proposed dislocation of the time table will, I know, meet with the most active opposition, as Headmasters and staff will no longer be able to enjoy that comfortable condition of repose so dearly loved by the profession when the change I insist on as necessary is made; but when at last they see that it is inevitable, they will actually be imitating the ways of the world and doing themselves no end of good by becoming live beings.

I make earnest appeal to you to disturb the existing condition of affairs, because you represent the cream of the profession—because you, of all teachers, are likely to be the most active, the most alert, the most ambitious and more willing and more competent to institute experiments than your literary or mathematical brethren.

Nothing is more certain than that we must make experiments, in order that we may find out ways of rendering our schools efficient. Speaking generally, the present race of Headmasters and teachers is characterised by inability to make experiments —inability to see even that experiments must be made; you, therefore, who have had some experience of experimental work are the elect upon whom is cast the serious task of saving the situation—of saving the country, as we are obviously at a period of national peril.

Meetings such as this should be devoted to the discussion of questions of prime importance in school organisation, many of which scream for consideration at the present time. I would suggest that papers on such subjects should be presented by the Association without the names of individual authors being attached to them: if you will allow me to say so, the bill of fare you are in the habit of providing is scarcely worthy of you.

In this world, people are usually taken fairly at their own valuation—let me urge that it is for you to place a higher value upon yourselves: the time for modesty is gone by; your mission is clear. I can only trust that I have voiced to you the call of a somewhat inarticulate country and that you will forthwith take up arms and be aggressive in her defence.

There is danger that as time goes on the evils of unemployment will increase rather than diminish; the value of ideas, the value of leadership, must in consequence increase more and more as the problems of life grow in complexity. The education of fit men—of men of free and generous mind fit to lead—rests mainly in your hands; it is for you to rise to the sense of your opportunities and responsibilities: may I add, to set an example as gentlemen in the highest and truest sense of the term. Whatever fault we may find with our system of higher education, it has had one great advantage—our boys have been in the hands of well-bred, well-behaved, clean-minded gentlemen; the example these have set has been of inestimable value. How great the value is perhaps obvious only to those who know something of modern American schools, which are almost entirely in the hands of women teachers—so that the example of gentlemen is lacking and little chance given of stemming the tide of commercialism.

H. E. A.

PROGRESS OF METEORIC ASTRONOMY

By W. F. DENNING, F.R.A.S.

Few departments of astronomy were neglected so long as that dealing with meteors and shooting stars, and no branch has attained so much importance in a comparatively short period. The study of shooting stars in their fugitive flights has opened out a new world in celestial physics and enriched our knowledge to a degree certainly never contemplated by the pioneers in this field of research. The little falling star which glides silently along the azure and forms but a momentary spark, can tell a wonderful story. Its career is ended, it is true, with its fall, and it will never more grace the brow of night with its glories; but its track amongst the stars has been recorded, and so we can glean much concerning it. Two observations supply the data for the determination of its real path in the air: the length of its luminous course and its velocity.

The shooting star is found to have taken fire within our atmosphere, and to have entered it from outside. In fact, the event signifies a collision between a small stony mass and the earth's atmosphere. The former is not formed of a highly combustible material; but it flies into the air with tremendous speed (10 to 44 miles per second) and the concussion generates sufficient heat to render the object incandescent and to destroy it. Every night, every hour, these meteors may be observed to fall. No part of the firmament is free from their incursions, no season of the year is without them; they are ceaseless, though variable in their times and numbers. In ancient days shooting stars were regarded as atmospheric in their derivations, and thought to indicate electrical phenomena exhaled from the earth and descending again. No one viewed them as being planetary atoms, cosmical in their origin, positions, behaviour, and speed. Our forefathers never dreamed that they formed the dust of the universe drifted, as it were, into dense clouds revolving in mighty orbits around the sun. They never contemplated that the nocturnal sparks which enliven the stillness of our autumn nights have such grand and far-reaching influences, racing in almost interminable paths from star to star, and disporting themselves amongst the planets, to rush into their envelopes and cause a momentary blaze like that so frequently pictured in terrestrial skies!

Old ideas regarding them were crude and altogether mistaken. Exact observations were lacking; there were really no reliable data from which a trustworthy theory of meteors could be constructed. Shrocter, the German astronomer of more than a century ago, often saw meteors during his vigils and calculated the height of one of them as more than one million miles!

A good many curious theories were current about meteors in former times. Halley thought them strata of inflammable vapour raised from the earth, and then (taking fire at one end) the running flame produced the apparent motion of the object. Olbers concluded that they formed fragments of an exploded planet. Hutton and Laplace believed they were thrown from volcanoes in the moon. Clap, Day, and Carvalla considered them to be terrestrial comets. Brewster and La Grange regarded meteors as bodies thrown off from the earth by volcanoes. Dr. Blagden said they were electrical phenomena, while Sir Isaac Newton supposed they proceeded from comets' tails. Herrick correctly wrote, "Millions of small planetary bodies of various magnitudes are revolving round the sun, and when any of these dart into our atmosphere they become ignited, and are seen in the form of shooting stars."

It is not the intention to give a sketch of the history of meteoric astronomy. The main facts are included in some of our text-books and mere repetition would serve no useful end. We must, however, mention certain features which appear to have influenced the advance of our knowledge in a material degree. In this connection the Leonid meteors of November deserve prominent recognition. There is no doubt that this recurring shower, more than any other, called the attention of scientific men to a careful consideration not only of the phenomenon itself, but also of the whole subject of meteors.

The display of 1799 was a fine one, but that of 1833 formed a marvellous meteoric storm which excited world-wide discussion. Olmsted thought (and correctly so) that the meteors formed a body similar to that of a comet. In later years

Herrick, Quetelet, Newton and others applied themselves to the investigation, and inferred periodic returns in about thirty-three years from a comparison of old records. The year 1866 was therefore awaited with great interest to see whether the shower returned in all its former brilliancy. It did not disappoint expectation.

Heis was the leader of the more accurate school of observers. He saw the necessity of recording meteors according to their definite paths amongst the stars. He accumulated thousands of such observations. Schmidt at Bonn and Athens imitated his example. Prof. Baden Powell in England began systematically to collect descriptions of such fireballs and meteors as were seen, and presented annual reports to the British Association. Neumayer at Melbourne (1858–63) also obtained observations, and many others at about this period applied themselves to the same end. Amongst them we may mention Prof. A. S. Herschel, Mr. R. P. Greg, Mr. J. Glaisher, Lieut.-Col. Tupman, G. Zezioli, G. V. Schiaparelli and F. Dinza in Italy, and A. C. Twining, Quetelet, and others in America. It will be appropriate to give a summary of the leading events in this field since the year of the rich shower of Andromedes witnessed by Brandes and others in 1798. Such a table may be of utility for reference, and interesting as indicating the periods rendered particularly memorable by the appearance of star showers or their associated comets, or by the work of some distinguished labourer in this branch. The list does not aim at completeness; in fact, to have given anything like an exhaustive *précis* of events would have required considerable space.

Only one really abundant swarm of Lyrids appears to have been seen in modern times. That the Leonids did not return in their old-time splendour in 1899 and 1900 was due to the stream having been diverted from the earth by planetary influence.

1798. Dec. 6. Abundant display of Andromedes witnessed by Brandes and others.

1799. Nov. 11. Brilliant shower of Leonids seen by Humboldt and Bonpland.

1803. April 19. Abundant shower of Lyrids seen in America.

1826. March 26. Biela discovers a comet with an orbit similar to the Andromedes of November.

1833. Nov. 12. Magnificent display of Leonids observed in America.
1838. Dec. 6. Plentiful shower of Andromedes.
1840. E. C. Herrick. Chronological list of star showers.
1845. Coulvier-Gravier publishes *Recherches sur les Étoiles Filantes.*
1845. British Association forms a Committee on Luminous Meteors, which collected observations and published annual reports to 1880. Secretaries, Profs. Baden Powell and A. S. Herschel.
1852. Schmidt. Results of observations of shooting stars.
1858–63. Neumayer observes 2,000 meteors in Melbourne; their radiant points are determined by Heis.
1860–67. H. A. Newton publishes various important papers on meteors.
1861. Quetelet. Catalogue of star showers.
1861. April 4. Thatcher discovers a comet which is afterwards found to be associated with the April Lyrids.
1862. July. Tuttle and Swift discover a comet which presents an orbital resemblance to the August Perseids.
1865. Dec. 19. Tempel discovers a comet which moves in the same orbit as the Leonids of November.
1866. Schiaparelli studies the August meteors, and is led to discover the identity of orbits of certain comets and meteoric systems.
1866. Nov. 13. Great display of Leonids seen in England.
1867. Nov. 13. Great display of Leonids.
1867–9. G. Zezioli at Berjams observes many thousands of meteors, and the radiants of the majority of them are derived by Schiaparelli.
1868. British Association Report contains full discussion and particulars of Schiaparelli's researches on shooting stars and their association with comets.
1868. Nov. 13. Great display of Leonids.
1868. Prof. A. S. Herschel and R. P. Greg publish a table of 88 radiant points of shooting stars (B. A. Report, 1868).
1871. Schiaparelli publishes a work entitled *Entwurf einer Theor Sternschnuppen.*
1871. Schmidt. Table of 152 radiants he had observed (*Ast. Nach.* 1756).
1872. Nov. 27. Very abundant shower of Andromedes.

1874. Tupman. Catalogue of 102 radiants of shooting stars observed in the Mediterranean.
1877. Heis publishes his results of forty-three years' (1833–75) observations of meteors.
1877. The motion of the Perseid radiant and its extent and direction detected at Bristol.
1884. Details of stationary radiation published by R. A. S. (*Monthly Notices*, Dec. 1884).
1885. Nov. 27. Very abundant shower of Andromedes.
1891. Kleiber publishes work on the orbits of meteor streams.
1891. Denning. Catalogue of 918 radiants.
1892. Nov. 23. Fine shower of Andromedes observed in America.
1898. Denning. General catalogue of radiants (4,367), of fire-balls, and shooting stars (*Memoirs of R.A.S.*, vol. liii).
1899. Nov. 23–4. Fine shower of Andromedes.
1901. Nov. 14. Rich display of Leonids.
1903. Nov. 14. Rich display of Leonids.
1904. Nov. 21. Fine display of Andromedes.

The year 1866 may well be considered as a brilliant epoch in the history of meteoric astronomy. And if we include a few other years, say from 1861 to 1872, there were many important developments in our knowledge of this branch, because a number of associated comets and meteoric showers were presented in the heavens, and the men were not wanting to observe, discuss, and interpret the facts which led up to very significant and far-reaching discoveries. The comets of the Lyrids, Perseids, and Leonids appeared, active displays of Perseids occurred, swarms of Leonids came in 1866, 1867, and 1868, and a great storm of Andromedes invaded our skies in 1872. These were epoch-making phenomena, and their significance was not lost upon those who were applying themselves to researches in this field. Many honoured names might be mentioned of those who contributed to the successful issue of the investigations, chief among them being G. V. Schiaparelli, H. A. Newton, J. C. Adams, E. Weiss, J. G. Galle, C. A. F. Peters, H. d'Arrest, Prof. A. S. Herschel, and others.

Shooting stars came to be regarded as well-ordered parts of the cosmos, and as equally entitled to regard with the planets and comets. They formed, in fact, the material of which comets were made, and the host of attenuated meteoric showers, of which our nocturnal skies gave clear evidence, were but the débris of ancient disintegrated comets distributed into streams of particles still owning allegiance to the sun. As the earth in her annual round became immersed in these streams, numbers of them encountered our atmosphere and were consumed. Their average heights while undergoing combustion were about 70 to 50 miles, and their velocities 34 miles per second.

There were four apparently very certain agreements between comets and periodical meteor showers found by Schiaparelli and his contemporaries, and further coincidences of a similar kind were expected to present themselves. The radiant points of comets and dates of rencontre or nearest approach of the cometary and terrestrial orbits were computed by Weiss and A. S. Herschel, but no further instances of a definite nature were observed. The January, October, and December meteors apparently had no parent comet which could be discovered. And though there were some fairly good cases of near resemblance between the orbits of certain comets and minor meteoric displays, yet there was no absolute proof of connection. Even to this day the list of such associated meteor showers and comets has not been extended if we except the doubtful case of the May Aquarids and Halley's comet. This year ought to provide us with new evidence on the latter, for there should occur a brilliant shower in May if the meteors are dependent on Halley's comet for their supplies. The shower was discovered by Tupman in 1869–70, and it certainly forms a complete ellipse, some of the meteors being visible every year. At the period Tupman recognised it as a specially brilliant shower of meteors the comet was near aphelion.

It is often the experience that after a branch of science has received a great impetus from several noteworthy discoveries, there results a period of quiescence—for some years, perhaps for a generation, no further striking advances are effected. This has been exemplified in meteoric astronomy. During the last forty years we have no brilliant successes to chronicle. Much has been done, it is true; many thousands of observations —more accurate and reliable than those of the older school— have been accumulated. Our knowledge of the radiant points both of the major and minor systems is now more complete and exact than before, and many hundreds of the real paths of meteors have been calculated from multiple observations of the some objects.

The data which have been gleaned will be of inestimable value to future inquirers. The materials we have been storing up will form the basis for the building of important theories when the time is ripe for the consummation. For it must be admitted that our knowledge is still far from perfect, and that certain observed features of meteoric showers have never received adequate explanation.

Though no very brilliant achievement can be said to have marked our progress in this field since the identity of cometary and meteoric orbits was fully demonstrated, yet many important items can be referred to as forming enduring links in the chain of our advance.

A generation and more ago the radiant point of the great display of August was supposed to be of normal character. Its position near η Persei had been pretty accurately fixed by Greg and Herschel, Schmidt, Tupman and others. The shower was supposed to last some days, possibly weeks; and it was uncertain whether the radiant was elongated, diffuse, mobile, or fixed. Twining in America had thought it moved to east from some observations he secured; but his materials were insufficient to prove anything. Greg considered the radiant an elliptical area, with major axis from north to south. Others regarded the centre as constant, but the area of radiation an extensive one.

The matter was set at rest in 1877 and 1878 by observations at Bristol. It was found that the radiant moved to the E.N.E. at the diurnal rate of 1°, and that the display certainly lasted a month if not more. These observations were shown by elaborate mathematical investigations by Kleiber (*Monthly Notices R. A. S.* B. 1. 341) to be remarkably in accord with theoretical deductions. The observed radiants and those computed agreed within 2°; and this must be held to be very satisfactory, when we remember that records of meteor flights necessarily depend upon hurried eye estimates of position. Moreover, the radiant often appears diffuse or scattered owing to deflected or perturbed meteors, and is not a point upon which the tracks sharply converge; so that its precise location is by no means easily fixed.

The radiant of the April meteors has shown similar indications of motion to that of the Perseids, but more evidence is required. The shower is brief and its meteors often few; and the difficulty in deriving the exact position of the radiant point from night to night is considerable.

Another feature of meteor streams which has received ample recognition in recent years is the apparently very long duration of many showers from a fixed or stationary radiant. Theoretically the radiant should show a displacement from night to night similarly to the Perseids and Lyrids. But Greg found many showers which seemingly continued a long period. Some of them were suspected to change their places somewhat, and a proportion of them apparently advanced in R.A. with the time.

The writer made many observations at Bristol in 1876 and following years; one notable outcome of his watching was that a great majority of the secondary meteor streams are visible during an abnormal length of time, and that no variation whatever takes place in the position of the radiant amongst the stars. Certain showers appear to be maintained for as long a period as six months, and instances were not wanting in which radiants continued in active play all the year round! It is true that meteors were not remarked emanating from these points on every night of the year; but they would be noticed at intervals—proving that there was at least an intermittent discharge, if not an absolutely continuous one. And the curious point was that the radiant remained precisely in the same position, allowing for slight and unavoidable errors attending observation. Thus there are displays of meteors from about $161° + 59°$, $262° + 62°$, and $310° + 79°$ in the Spring, Summer, Autumn and Winter; and I believe that with many additional observations the three showers named and many others would be found continuous or nearly so. The ordinary meteoric displays are however so exceedingly feeble that it is hard to define either their beginning or ending; and an observer—after perhaps 30 or 40 hours of careful watching within the same week—will find he has not recorded a sufficient number of tracks to indicate some of the radiants with precision. Yet as a rule five tracks are enough for the purpose. Such is the attenuated state of certain systems, however, that they do not provide one meteor in ten hours to the region of sky commanded by one observer.

The facts proving stationary radiation were embodied in a paper published by the Royal Astronomical Society (*Monthly Notices*, vol. x), and though the observations have not been refuted, no satisfactory explanation of the curious fact has ever been advanced. Many ingenious attempts have been made, either partly or wholly to accommodate the long-enduring showers, but without success.

When the first endeavours were made to match Comet 1861 I with the Lyrid meteor system, the resemblance of orbit was not very marked. This was, however, owing to a faulty position given for the radiant. From later observations, the radiant was proved to be precisely in the point which had been calculated for meteors directed from the comet.

The heights of the swift-moving meteors such as the Leonids and Perseids is proved to be decidedly greater than that of the slow-moving meteors. Perseids and Leonids commence their luminous careers at about 82 miles and end them at 54 miles. The Leonids move with a theoretical velocity of 44 miles per second, whilst that of the Perseids is 38 miles per second. The latter are usually about 2 miles lower in the air than the former. Slow-moving fireballs often penetrate to within 25 or 30 miles of the earth's surface; but I have never found a well-observed meteor of the Leonid type fall to within an altitude of 40 miles. The Orionids of the October display are of smaller type than the Perseids; but they have a radiant essentially different—for it appears to retain an immovable place between October 9 and 29.

It is a well-ascertained fact of recent years that nearly all the large fireballs have slow motions, and come from western radiants; thus they are overtaking the earth in her orbit, and revolving in direct orbits like the periodical comets of the Jovian family. There are many fireballs from radiants near the Zodiac; but the rule is not absolute, these bodies being sometimes directed from regions widely removed—proving that the inclinations of their orbits are occasionally considerable.

Photography has played a great part in assisting astronomical discoveries, and in allowing us to attain a higher degree of accuracy in the representation and measurement of celestial objects; but it has practically failed in the meteoric field. The fugitive durations prevent their being impressed on the plates, except in the case of the more brilliant specimens. It was hoped at one time that the camera would entirely supersede the rough estimates of the flight which only momentary glimpses afford us; but this has been far from realised. The ordinary shooting star falls, and the exposed plate shows no more sign of it than the sky after the last vestige has disappeared. But some excellent individual records of fireballs have been obtained. Thus a Perseid fireball of August 12, 1909, appeared at 9 hrs. 42 min., and gave a flash like lightning. It was seen by the writer, and thirty-five reports came in from other observers, among them being a good photographic impression of the trail (*English Mechanic*, 1909, September 10).

Another fireball of lesser note was photographed at Chester on September 6 by Mr. Longbottom, while he had a plate exposed in the region of Andromeda; and should another observation come to hand, the real path of the meteor could be accurately found. But for the registry of normal meteors and ordinary star showers, this method does not apply in the present state of our means; this is regrettable, for photographs would at once ensure accuracy in a field where large errors are often unavoidable.

But after all, these errors chiefly affect inexperienced observers. My opinion is that after a man has carefully recorded 2,000 meteors he has attained the practice necessary to ensure skill in the work. Meteors like the Leonids, Perseids, and Orionids, which leave streaks along their tracks, can be noted almost with critical exactness by a man who has a natural aptitude for the work. For meteoric observers, like poets, are born, not made. Some labourers in this field have been as accurate as men could well be; others have been terribly wild and unsuited for the work—unfortunately the latter class have formed the majority. No amount of training will ensure precision in some instances, because the observer lacks the special qualities essential to success.

During the last few years the adherents of meteoric astronomy have not exhibited much enthusiasm. This department seems quiet, and it needs a splendid shower from Halley's comet in May to revive it and excite the ardour of observers. It ever offers a fine field of work and will always occupy a high place among astronomers.

The spectroscope has been applied to meteors; but these transient bodies are rather intractable to interrogations of this nature. Mr. Lockyer, in the Bakerian lecture (1888), said that "the vapours given out by the meteorites as the sun is approached are in an approximate order: slight hydrogen, slight carbon compounds, magnesium, sodium, manganese, lead, and iron." Of course the meteorites which have fallen to the earth can be subjected to proper analysis. Mr. Fletcher says that they evolve hydrogen, nitrogen, marsh gas and the carbonic oxides when heated. No entirely novel elementary body has been discovered in meteorites, nor can an organic origin be claimed for anything found in them. Browning and Konkoly, from observations of meteors in luminous flight, inferred the presence of the sodium line; and in the fireball of October 13, 1873, Konkoly remarked bands which he identified with the spectrum of a hydro-carbon. In 1908 there were 580 meteorites or fragments of meteorites in the British Museum, and the collection is increasing every year.

There was a great advance in our knowledge of meteors and their attendant phenomena between 1800 and 1900. At the former period relatively nothing was known of these phenomena, and the most crude and erroneous ideas prevailed concerning them. Now everything is changed. Meteors have but acquired their right in being acknowledged to rank with the planetary bodies of our solar system. Our knowledge of them will continue to increase, and it is quite possible they will grow in importance. In the future master minds will arise to amend our theories consistently with observation, and the materials we have been accumulating may form the framework of a cosmogony differing much from present ideas.

With regard to meteors coming from known comets, we can hardly expect, when all the conditions are considered, many cases of well-assured character. There are very few comets with orbits passing near the earth's path. Prof. Herschel carefully examined the orbits of eighty comets which appeared between 1872 and 1892, and found that only two of these made relatively near approaches to the earth, viz. Denning's comet of 1881, with path at one point only 3 millions of miles distant, and Finlay's comet of 1886, about 4½ millions of miles distant. Whether comets at this distance are capable of inducing a meteoric shower is uncertain; but it is a fact that some of these bodies spread their material over vast tracts of the sky. The tail of a large comet may be 50 millions of miles in length, or even more. As these enormous appendages are wheeled rapidly round during a comet's circum-solar flight, the material capable of producing shooting stars may be distributed over a large area. In fact, though the nucleus of a comet may have a perihelion distance of only 50 millions of miles, the extremities of the tail may spread out sufficiently far to envelop the earth, and possibly may give rise to shooting stars. We cannot therefore limit the operations of a comet in this respect, because the material of certain of these bodies extends out indefinitely to almost inconceivable distances, and may influence regions of the solar system apparently far removed from the position of a comet's head. And observations and calculations of cometary positions are always made with special reference to the head or nucleus. Having been engaged for some years in collecting and collating records of meteors, in comparing radiants, and in making observations, I may perhaps venture to express an opinion as to the quality of existing materials. I regret that it does not appear to me to be satisfactory. The majority of the radiants at present determined are of very doubtful accuracy. In catalogues of radiants the results of bad, good, and indifferent observers are mixed up together; and thus it happens that really reliable positions are spoiled by combination with erroneous ones. The observers who have made mistakes can well plead the difficulties of the work in extenuation of their wild efforts. But I think that in some individual instances a fair degree of accuracy has been attained; in fact, when the conditions are allowed for, it is surprising that the results are so correct. It would be well if photographic data could be obtained to displace the faulty naked-eye records of flight; but failing a better method, observers have certainly done their best. In preparing my general catalogue of metoric radiants (published in 1898) I included all the positions I could find; and there are many very doubtful among such a miscellaneous collection. I felt that it was not for me to sit in judgment upon my colleagues in this field, and altogether expunge some of their contributions. But there is no doubt that while some of the observers could not derive radiants to within 7° or 8° of probable error, others could and did do so within 2° or 3° of error!

With regard to the observation of meteors, it is a most inviting branch of research and one holding out the prospect of new and interesting discoveries. It undoubtedly requires patience, a keen eye and good judgment; but no instruments are required save a good celestial globe or star chart. The writer has engaged in several lines of astronomical work but has found watching for meteors the most entertaining of all. This is especially the case on Autumn mornings, when meteors fall frequently, and the observer has to use great expedition to register them. When a man has acquired experience in this department, and a thorough knowledge of the principal showers, the work becomes very interesting. In fact, to realise the extent of this a person must have deeply engaged in the research for many years; he will then understand its details and appreciate its many attractions. To be successful the observer should watch during the whole night, for many showers will otherwise escape recognition on account of their feebleness. In England about 3,000 or 4,000 meteors could be registered in a year by a fairly energetic student; in a finer climate, however, probably three or four times that number could be recorded. But whatever is done in this branch must be done well, or at least as well as the adverse conditions allow. Every accurate observation is valuable; but an incorrect record is worse than useless; and whenever there is a suspicion of error, that record should be rejected. Unreliable data if combined with good observations will only destroy the utility of them all.

SEX AND SEXUAL CHARACTERS

By J. T. CUNNINGHAM, M.A.

In his paper on the Determination of Sex in the July number of this Journal, Mr. Doncaster has given an account of recent researches and speculations from the Mendelian point of view; but there are other lines of research equally (perhaps even more) important to which he has not referred. To him and to others who are absorbed in Mendelian investigations, a Mendelian hypothesis which agrees with the results of certain experiments in cross-breeding seems to have the validity of a complete explanation; whereas a little consideration will show that there are many other facts which cannot be brought within the Mendelian formula—some because they are inconsistent with it, others because they are altogether beyond its scope.

According to the Mendelian view, sex is a property of the gametes—that is to say, each gamete bears one sex only; it has the property of developing either into a female or into a male, but not into both. Thus an ovum may be either male or female, and similarly a spermatozoon may carry either sex. The latest Mendelian theory of sex, the exposition of which is the main object of Mr. Doncaster's paper, and is also explained and illustrated in Bateson's *Mendel's Principles of Heredity*, 1909, is that male individuals in certain animals are homozygous with regard to sex, being of the constitution ♂♂ and producing only male-bearing spermatozoa, while female individuals are heterozygous, being of the constitution ♀♂ and producing male-bearing and female-bearing ova in equal numbers. Thus at fertilisation the ovum may be male or female, but the sperm is always male; and when the ovum is female it develops into a female animal, "femaleness" being always dominant. This hypothesis seems to explain satisfactorily the curious heredity of certain characters in Lepidoptera and some birds; but when it is stated that confirmatory evidence of its truth is afforded by the effects of castration in Vertebrates, while the most important recent discovery concerning those effects is not mentioned, the statement requires careful examination.

Prof. Bateson and Mr. Doncaster assert that castration of the male in Vertebrates may prevent the appearance of the male characters, but does not cause the appearance of characters proper to the female; while, on the other hand, removal or atrophy of the ovaries may bring about the development of characters normal in the male. They consider that this proves that the female possesses the male sex in a latent condition, but that the female sex is entirely wanting in the male. Thus there are two questions to be considered: firstly, whether their statement is in accordance with the facts at present known, and secondly, what is the relation of the secondary sexual characters to sex? It is evident that the above statement refers to secondary sexual characters, such as the antlers of stags, or the male plumage of birds; whereas the only primary and essential difference between the sexes is that the male produces spermatozoa, and the female produces ova. It has long been known that the normal development of secondary characters in male Vertebrates depends on the normal condition and functional activity of the testes, and that after castration these characters fail more or less completely to develop. It has recently been discovered that the connection between the primary organ and the secondary characters is chemical; that the normal development of the secondary character requires the stimulus of a chemical secretion or hormone present in the blood, and derived from the gonad.

Of the various experimental investigations which have demonstrated this important fact in different Vertebrates, it will be sufficient to mention the following. In 1905 Nussbaum (10) first castrated male specimens of the frog, *Rana fusca*, and found that the "brunstorgane," or nuptial organs—that is to say, the swelling on the inner side of the fore-foot and the enlargement of the muscles of the fore-leg, which are present in the breeding-season—failed to develop. He then inserted in the dorsal lymph-sac of a castrated frog pieces of the testis of another frog, and found that the nuptial organs developed to the same degree as in the entire male. The pieces of testis introduced were absorbed—no vascular or nervous connections were formed; therefore the result must have been due to chemical influence—some part of the substance absorbed from the testis must have had the power to stimulate the tissues of the fore-leg and cause the enlargement of the integument on the fore-foot to develop. It had been proved that the development and functional activity of the mammary glands in mammals took place after their connections with the nervous system had been severed, and Prof. Starling and Miss Lane Claypon (9) showed that some development of the glands was caused in virgin rabbits by the injection of extract of fœtuses from other rabbits; some special chemical compound must therefore be present in the fœtal tissue which, when absorbed into the blood, acts as a stimulus to the development of the gland. Shattock and Seligmann (3) found in their experiments on castration that some cocks on which the operation had been performed developed the male secondary characters—especially the comb, wattles and hackle feathers—to the same degree as normal specimens, and on dissection found that in these cases fragments of the testes, detached in the operation, had been left in the abdominal cavity, and had grafted themselves to the peritoneum in some new position, even sometimes on the outer surface of the intestine. In these grafts spermatogenesis took place, and ripe spermatozoa were present. Here again the original nervous connections were all severed, and the only reasonable conclusion is that some specific chemical substance is produced in the process of spermatogenesis which acts as a stimulus for the development of the somatic structures characteristic of the male individual. This remarkable peculiarity of secondary or somatic sexual characters is entirely beyond the scope of Mendelian conceptions and Mendelian experiments. Mendelism regards all characters as represented by certain units in the gametes. These units may be segregated in the formation of the gametes, and may be combined in the union of the gametes in the process of fertilisation. Some units may have no effect in the development of the organism, remaining latent or recessive, because others are dominant; but Mendelism deals only with the characters determined at fertilisation—in fact, with heredity—while the development of secondary sex-characters (although of course partly due to heredity) is not determined at fertilisation, but at a later stage of life by a chemical stimulus.

From the terms of Mr. Doncaster's statement on the subject it might be supposed that the development of male characters was a regular or frequent consequence of the removal of the ovaries in the female, and that the appearance of the female characters would be expected after castration in the male. Bateson is more guarded in his words—referring only to injury or disease of the ovaries, not removal. There is certainly no obvious reason why removal of the gonads in either sex should cause the appearance of the characters of the opposite sex. If this were the case, castration of both sexes would cause the complete reversal and interchange of their secondary characters: the entire male would have the male secondary characters, the castrated male would have the secondary characters of the female, and *vice versâ*. This would be in direct contradiction of the fact that the development of the secondary characters is due to the sexual hormones. A female deprived of the ovaries may be regarded as consisting of soma alone. If this neutral soma regularly developed the male secondary characters, there would be no reason why the castrated male—which is also a neutral soma—should not do the same. Thus, it would be impossible—if the statement of the Mendelians were correct—to understand the absence of the secondary male characters in the castrated male.

The evidence on the subject at present available does not bear the interpretation put upon it by Bateson and Doncaster. It is true that cases frequently occur of the female assuming, more or less completely, the secondary characters of the male; but this does not occur in castrated females, but in exceptional individuals whose ovaries have not been removed. The effects of removal of the ovaries have not been so fully studied as those of removal of the testes. Mr. Shattock informs me that he found it practically impossible to remove the ovary in birds entirely. It is so closely adherent to the vena cava, that after the operation a portion was always left—and from this the organ grew again. In the human species removal of both ovaries is sometimes necessary, and has been carried out; but I have not been able to ascertain that in such cases any marked development of male secondary characters has been observed. In old women, after the reproductive organs have become functionless, a certain development of beard and moustache usually occurs—showing that the character is latent in the female; but I have found no record of such development as a consequence of ovariotomy. Spaying is frequently carried out in pigs; but I know of no evidence that male characters are developed as the result. There is some evidence that the effect of removal of the gonads in the female is (as in the male) the suppression of the secondary characters proper to the sex. Thus Mr. Shattock tells me that in the cow the pelvis is larger than in the bull, and that in a cow which was spayed at an early age the enlargement of the pelvis did not take place.

We are not justified then in assuming that the development of male characters which occurs occasionally in females is due merely to the suppression of the ovarian function. It is true that in several cases dissection has shown that the ovary was reduced to a small vestige. Two such cases are described by Shattock and Seligmann (2): one that of a gold pheasant, the other of a wild duck. But on the other hand, male-plumaged hens are not always sterile: Homelie records a case of a cock-feathered hen which continued to lay eggs and hatch chickens, and at the fourth moult became hen-feathered again; and Shattock and Seligmann had a male-feathered duck which laid eggs. It has been found that in Passerine birds male-plumaged hens occur, and that the condition is not associated with sterility or atrophy of the ovary, dissection having shown that the ovary was large and functional. It is evident that the condition is not the constant result of old age or sterility or removal of the ovaries, but is an abnormality occurring in a few individuals. Two explanations of this abnormality are possible: we may suppose that in these cases a mutation has occurred which makes the male characters develop without the stimulus of the hormone from the gonad, or we may suppose that the individuals are really hermaphrodite—that testicular tissue is really present, and produces the hormone or internal secretion which causes the development of the external male characters. With regard to the first hypothesis, we have good reason to believe that such a variation has frequently occurred both in nature and under domestication, as in the case of the antlers of the female reindeer and the large comb and wattles and even spurs in the hens of many breeds of fowl. The second view is that put forward by Shattock and Seligmann—namely, that the female birds which assume the male plumage are really hermaphrodite: either the ovary being bisexual, or the male element being displaced and included within a neighbouring viscus, such as the adrenal or the kidney—and this view derives support from the fact that females in such a condition often exhibit male sexual instincts. For all we know, the sterile or even the atrophied ovary in such cases may produce the male hormone.

It may be urged, however, that whatever may be the explanation of the occasional assumption of male characters by the female, a corresponding development of female characters in the male does not occur. There are several reasons why the corresponding change should be less conspicuous in the male. In the first place, the female characters in Vertebrates are for the most part negative—that is to say, the female adult does not develop new characters, but remains in the same condition as the immature young; and thus the assumption of the female character would be the same thing as the absence of the male character. Another difference is that in the male there is not a definite limit to the functional activity of the gonad—it may continue (though with diminished vigour) to extreme old age. Thus, if in a particular individual there was a female tendency present, it might not show itself because held in check by the functional male gonad. Where, however, positive female characters do exist, we find evidence of their occasional development in the male. The best example is afforded by the mammary glands. The fact that these are normally present in a rudimentary condition in male mammals is to my mind sufficient to disprove the theory of the Mendelians; for the suggestion of Darwin that they were originally functional in both sexes seems to me contrary to all probability. Many cases are on record of the mammary glands becoming functional in males, both in the human species and in other mammals; and, as in corresponding cases in females, the abnormal development is not common to all castrated individuals, but occurs exceptionally in both entire and castrated males. The wide pelvis is another positive female character in the human species, and in others—such as the cow—where a single large young is produced at birth; but there is little trustworthy evidence of the development of this character in males. It has been sometimes assumed that this last character is developed in castrated human males; but the evidence is not definite, as hitherto little attention has been paid to the distinction between the absence of male and presence of female characters after castration. In some cases it is difficult to decide which of these two descriptions applies to an abnormality. For instance, Shattock and

Seligmann record a case of a male pheasant in which the proximal half of the tail feathers exhibited the female coloration, the distal half the normal markings of the male. The female coloration consists of irregular dark specks on a light-yellowish ground colour between the transverse bars, while in the male part of the feather between the dark bars there is a uniform reddish colour without specks. This looks like the assumption of the female character in the latter half of the feather's growth; but on the other hand, it may be regarded as the absence of the male character. As the bird was not castrated, the former interpretation is the more probable.

The effects of castration on the male of the domestic fowl are somewhat puzzling, and the evidence of different writers is to some extent contradictory. Shattock and Seligmann state that they have never seen a capon without spurs, even in cases where dissection has shown that no remnant of the testes has been left in the body. They also found that the sickle feathers were longer in the capon than in the hen, although not so erect as in the cock. The comb and wattles and the rest of the male plumage were reduced to a condition similar to that of the hen. Darwin, on the other hand, states that the spurs are reduced—which would be expected. Probably the results differ according to the age at which the operation is carried out. There seems no doubt, however, that capons will incubate and hatch chickens and rear them; and this is evidence against the Mendelian view, for it indicates that the female instincts are latent in the male. Shattock and Seligmann (2) record a case of a cock pheasant not castrated which became broody, drove the sitting hen from a clutch of eggs and sat on them himself. There is then positive evidence of the appearance of female characters in males. It seems to me that in both sexes the result of castration in either sex is usually the suppression of the positive characters of that sex; but there may be also some slight degree of development of those of the opposite sex, while the marked development of male characters which occasionally occurs in females is exceptional.

So far as secondary characters are concerned, the fact that those of each sex are latent in the other seems to me to be proved most conclusively by their presence in many cases as rudiments. I have already referred to the universal occurrence of rudimentary mammæ in male mammals. The transmission of the organs proper to each sex to the other is remarkably exhibited in the zebu, as can be seen in the pair of these animals now in the Zoological Gardens. In the bull there are four well-developed teats at the side of the scrotum, while in the cow there is a vertical flap of skin resembling in form and position the penis of the male. In Marsupials there is another instance similar to that of the mammæ: the pouch of these animals, functional only in the female, is present in rudimentary form in the male. These cases are in all respects similar to those in which a male character is transmitted in rudimentary form to the female; for example, the spur and comb of the cock, the horns of many ruminants, the elongated fin-rays of some fishes. It is well known that the accessory organs of reproduction in each sex are present in rudiment in the other. The clitoris in the female is paralleled by the rudimentary uterus masculinus in male mammals, and in lower Vertebrates rudimentary oviducts are present in the male.

So far we have considered only secondary characters, on which the argument for the Mendelian theory is founded; but this is only indirect evidence of sex. The primary and essential difference between male and female is that the former produces spermatozoa, the latter ova. All other differences are secondary, and there are numbers of species in which no other differences exist. Considering that the fertilised ovum, or zygote, consists of a spermatozoon and ovum united, it is difficult to understand how it should be capable of giving rise to spermatozoa only, as the Mendelian theory of sex supposes. The male is supposed to be homozygous in sex; which means, if we leave the soma out of consideration, that all the gametes descended from the fertilised ovum are spermatozoa, and are incapable of transmitting the power of producing ova. This would be to deny to half the ova which are fertilised the property of heredity altogether with respect to the cellular characters of the ovum itself. To say that an ovum is male, means that it may contain all the characters of the soma developed from it, but has no power to give rise by simple cell-division to any other ova. I am inclined to think that, even if the Mendelian theory were true, a male ovum would be inconceivable in the present state of knowledge—quite as inconceivable as is, according to the Mendelians, the heredity of acquired characters. It is more important, however, to consider whether the theory is in accordance with the evidence.

In the lower Vertebrates there is direct evidence that the primary female character is present in the males. Years ago I showed (4) that in Myxine the gonad is hermaphrodite. Schreiner (5) has recently maintained that even here the male and female elements do not come to maturity in the same individual; but there is not the slightest doubt that ova are present in large numbers in the functional males. It has also been shown that ova occur frequently in the testes of Petromyzon, many Teleosts, and many Amphibia. In toads an organ attached to the anterior end of the testes, and known as Bidder's organ, is present in all males, and contains ova which do not come to maturity. It sometimes contains spermatozoa also, and it occurs also in females: but here we are considering only the evidence of the presence of ova in males. Perhaps it will be suggested that there is no evidence of this kind with regard to reptiles, birds, and mammals, and that in these the female sex is absent in the males. It is true that the gonads seem more completely differentiated in these higher Vertebrates; but this applies to males and females equally—so that we ought to conclude that both sexes are sexually pure. But it is at least possible that even in the higher classes of Vertebrates each gonad contains reproductive cells of the opposite sex. In both ovary and testis there are peculiar cells, known as the "interstitial cells," which resemble epithelial cells, and differ from the elements of the stroma. According to the researches of Miss Lane Claypon, these interstitial cells are derived from the germinal epithelium, and are therefore reproductive cells, or gametes which never come to maturity; and it is possible that they represent the gametes of the opposite sex—a rudimentary ovary in the male, a rudimentary testis in the female. The condition of the lower Vertebrates suggests that these, and perhaps all animals—like plants—were originally hermaphrodite, and that the differentiation of sex was evolved by the reduction of one set of gametes to the rudimentary condition.

Where the evidence is obviously opposed to the conclusion that the males are homozygous in sex, Bateson and Doncaster suggest that the female is homozygous and the male heterozygous, the conditions being different in different animals. They accept the conclusion of Geoffrey Smith, from his observations on Sacculina, that the effect of parasitic castration on the male crab is the assumption of the characters of the female, while the same influence on the female produces only a reduction of the female characters. The facts do not seem to me to justify this conclusion, or to indicate that the relations of the sex-characters in Brachyura are in any way different from those which obtain in Vertebrates. It is true that the crab infected with Sacculina has the appearance of a female, and at one time it was supposed that only females were attacked by the parasite; but it is now known that the Sacculina sterilises the testes, and that as a consequence the male characters are not developed. The chief result is the reduction of the size of the chelæ, which are normally much larger in the male than in the female. This is no more the assumption of a female character than the suppression of the antlers in castrated stags. The breadth of the tail, however, is increased; and this is the point which Smith regards as showing the development of the female character, because the tail is normally broader in the female in adaptation to its function of bearing the eggs. But the narrowness of the tail in the male is a secondary character, and therefore suppression of this character means the broadening of the tail; while in the female the special egg-bearing character of the tail is reduced, so that the structure is brought to a similar condition in both sexes. When the male recovers from the attack of the parasite, eggs are found in the gonad; so that there is no doubt that the female sex is latent in the male. I do not dispute this conclusion, but merely maintain that it is equally true of Vertebrates. I consider, however, that the evidence for the absence of the male sex from the female crab is insufficient.

Mr. Doncaster claims that the Mendelian hypothesis which he describes "not only explains the cases which led up to it, and such facts as the effects of castration, but also accounts for the phenomena of sexual dimorphism and the inheritance of some structures by one sex only." It is sometimes maintained that Mendelism deals only with the facts of heredity. It is perfectly true that it deals with some facts of heredity; and the present writer has no desire to deny the facts proved by Mendelian experiment, or to depreciate their importance. But Mendelism does not confine itself to facts—it consists very largely of theory. Setting out from the simple inference that the separation of the members of a pair of characters in the offspring of hybrid parents is due to segregation of those characters in the gametes, it proceeds to invent a new formula for the results of every experiment of a more complicated kind. Its experts are continually revising these formulæ, and in their most recent improvements they make use largely of purely theoretical and imaginary characters, such as colour factors and inhibiting or epistatic factors which at present have no more perceptual existence than the ether. In fact, many of the Mendelian formulæ given in Bateson's recent book are not merely theoretical, but metaphysical—the qualities of a thing are separated from the thing itself. We cannot see the redness of a rose without the rose itself, although in metaphysics we may distinguish an object from its qualities. But the Mendelian finds no difficulty in discussing the inheritance of the bifidity of the comb of the fowl in one gamete and the comb itself in another, or the density of colour in one gamete and the colour itself in another—until we are reminded of Lewis Carrol's description of the smile of the Cheshire cat, which remained behind after the cat had vanished. All this may be perfectly legitimate, if only it is admitted that it is all pure theory—the endeavour which all science makes to arrive at concepts which make the percepts intelligible. But Mendelism makes pretensions beyond all this which are not legitimate, and against which it is time to protest. Mendelism deals only with characters which exist. It is true that it shows how new *varieties* may arise from the omission or addition of existing characters, as the result of crossing; but it does not maintain that new characters arose in this way. It assumes that all new characters arose as mutations in the gametes; and there is nothing in the methods or doctrine of Mendelism to afford any evidence of the truth of this assumption. In the passage above, quoted from Mr. Doncaster, we have an example of the unwarrantable pretensions of Mendelians. To assert that the Mendelian theory of sex which he adopts explains the effects of castration is absurd. On the Mendelian view, there is no reason whatever why castration should have any effect at all. This Mendelian formula merely supposes that both the gametes which unite to give rise to a male organism, bear the male sex-character. If we suppose that the latter term includes the male secondary characters, it follows that these develop by heredity in the male organism, and that the female sex-characters are entirely wanting, and cannot develop. But there is absolutely no connection between this conception and the fact that the secondary characters fail to develop when the gonads are removed. This is not a fact or phenomenon of heredity, but of physiology; and no facts or theories of heredity can explain why the development of the antlers of the stag or the comb of the cock should depend on the chemical secretion produced by the testes. This fact was discovered by the physiologists and by physiological methods, and could never have been discovered by Mendelian experiments. It is equally erroneous to assert that the Mendelian theory of sex explains the phenomena of sexual dimorphism in ordinary cases, for a similar reason: the theory admits that the male secondary characters are present in both sexes, and their development in the male and absence in the female (according to the Mendelian's own account) is not due to the constitution of the zygote, but to the influence of the gonads in the developed organism.

It is quite possible, however, that there are sex-limited characters which are not dependent on hormones, but whose development is entirely due to the constitution of the zygotes. In fact, in the present state of knowledge we are forced to the conclusion that there are two kinds of sexual dimorphism, When I wrote my paper (8) on Hormones and Heredity, I was puzzled by the apparent difficulty of reconciling my conclusions concerning the significance of the effects of castration with the entire absence of such effects in certain Lepidoptera. Oudemans Kellog, and Meisenheimer destroyed or removed the gonads in the caterpillars of *Bombyx mori* and *Ocneria dispar*, and found that the sexual characters of the moths reared from the larvæ were unaltered. There is the same difficulty in the case of congenital defects limited to one sex in man. Prof. Bateson has overlooked the possibility of the distinction which I am here pointing out. He evidently regards all sex-limited characters as belonging to the same category, for he remarks (*Mendel's Principles of Heredity*, 1909, p. 174): "Just as disease or removal of the ovaries may lead to the appearance of male characters, we should expect that such disease might lead to the occasional appearance of colour-blindness in females. I do not, however, know of such a case." Now we do not know anything of the effect of castration in preventing the development of colour-blindness in men; but we know one fact which indicates that this defect, although usually confined to the male sex, is not a sexual character of the same kind as the beard. Colour-blindness is present in young boys at an early age. It does not, like the beard, develop only at puberty; and the same is true of hæmophilia, which shows itself even in infancy. It seems evident then that there are two kinds of sexually-limited characters: those which develop only at sexual maturity, and are affected by castration, and (on the other hand) those which are confined to one sex, but are independent of the presence and functional activity of the gonads. The sexual dimorphism due to the second kind may be explained on Mendelian principles, and its origin may be that which is assumed by Mendelians for all characters—that is to say, such sex-limited characters may arise as mutations which (for reasons yet unknown) are coupled with the primary sex-characters. The other kind of sexual dimorphism cannot be explained on Mendelian principles, and the special relation of the characters to which it is due to the functional activity of the gonads can only be explained on the hypothesis that these characters are of somatic origin.[1]

Bateson's theory of sex appears to have been originally proposed in order to account for the heredity of the character of the lacticolor variety of *Abraxas grossulariata*. The fact that in nature this variety occurs only in the female sex indicates that the colour-character is connected with that sex; but it is scarcely a secondary sexual character, because in artificial breeding experiments male lacticolor were produced. Bateson's theory is not the only one on which the peculiar facts of this case can be explained; for Doncaster himself previously explained them by another, which seems more complicated, but is less open to objection, in my opinion, than Bateson's. In Doncaster's theory there are four assumptions:

(1) Each sex gives off male-bearing and female-bearing gametes in equal numbers.

(2) In heterozygous females lacticolor is coupled with the female character and grossulariata with the male, but in heterozygous males there is no coupling.

[1] Another distinguishing characteristic of secondary sex-characters which are dependent on the sexual hormones is that in the course of evolution they tend to emancipate themselves from this restriction, and then develop equally in both sexes. Examples of this change are afforded by the reindeer and by numbers of ruminants in which the horns are developed in both sexes, and also by many breeds of domesticated fowls in which the comb and wattles of the hens are no longer rudimentary, as in the original species; in some even the spurs are beginning to appear in the hens. This shows that there is no close connection or coupling of the character with the primary sex-character in the gamete.

(3) Fertilisation takes place only between gametes of opposite sex.

(4) The sex which is brought into the zygote by the egg is dominant.

To illustrate the sufficiency of this theory, we will take the result of mating the male lacticolor produced by previous crosses with the female grossulariata, which is heterozygous. The offspring of this mating are grossulariata all males, lacticolor all females. The gametes of the male parent are Lm and Lf in equal numbers. The gametes of the female parent are Gm, Lf, in equal numbers. Thus the zygotes are Gm Lf, and Lf Lm in equal numbers; and as the sex is determined by the ovum and grossulariata is dominant, the visible character of the offspring are grossulariata all males and lacticolor all females. But this is not all: it can be shown that Doncaster's assumptions are not so arbitrary as they seem, but are really in harmony with the views I have maintained in this paper. If we suppose, as I have urged, that ova are necessarily female and sperms necessarily male, there is no segregation of sex between the gametes of a single parent; but we may further suppose that one-half of the ova are strong and one-half weaker—and similarly for the sperms. We may then suppose that there is selective mating between the gametes—a strong ovum attracting a weak sperm and *vice versâ*, and that the stronger gamete determines the sex of the zygote. The result is then the same as on Doncaster's assumption, that dominance attaches to the sex which is brought into the zygote by the ovum. For the weak ovum will be equivalent to what Doncaster calls the "male-bearing ovum"; and to say that this ovum makes the zygote male is the same as saying that in this case the sperm is dominant. On this view the terms of the theory have a real meaning; whereas to say that the ovum bears the male character is to use words which do not represent any conceivable reality, and is a self-contradiction. There is this difference, however, between my interpretation and Doncaster's hypothesis: that if we make the weak ovum correspond to the male-bearing ovum, the selective union is between gametes of the same sex, not of opposite sexes—the weak ovum unites with the strong sperm, and the latter is dominant. Thus the sex is not determined by the ova alone, or by the sperms alone, but by whichever is stronger—that is more vigorous or dominant. My hypothesis further explains why the recessive or lacticolor character appears to be coupled with the female sex-character in the female grossulariata and not in the heterozygous male. In the former we may suppose the lacticolor character when it segregates to go into the stronger ova, because of its affinity for the female gamete; while in the latter (or male individual, all the gametes being spermatozoa) the strong or dominant sperms can carry either lacticolor or grossulariata.

It is easy to see that my view of the nature of sex and of the relations of the gametes applies equally well to the other cases of sex-limited heredity discussed by Bateson; namely, that of the pink eye in cinnamon canaries, and of the pigmentation of the silky fowl. For example, the matings given by Bateson for the case of pink eyes in cinnamon canaries would be interpreted by me as follows. The allelomorphs are B, b, presence and absence of the factor for black eye, b being coupled with the female sex-character. Instead of using the ordinary symbols for male and female, it is necessary on my hypothesis to use M, m, for dominant and recessive sperms, and F, f, for dominant and recessive ova. Then the matings become—

1. Cinnamon pink-eyed female with black-eyed male
   Composition bbFm BBMf
   gametes bF bf, BM, Bm
   result, black-eyed hens and black-eyed cocks
   BbFm BbMf
2. Black-eyed female with cinnamon pink-eyed male
   Composition BbFm bbMf
   gametes bF, Bf bM, bm
   result, pink-eyed hens, black-eyed cocks
   bbFm BbMf
3. Pink-eyed female with black-eyed male from 1
   Composition bbFm BbMf
   gametes bF, bf BM, bM, Bm, bm
   result, black-eyed hens, black-eyed cocks
   BbFm BbMf
   pink-eyed hens, pink-eyed cocks
   bbFm bbMf
4. Black-eyed female with black-eyed male from 1
   Composition BbFm BbMf
   gametes bF, Bf BM, Bm bM, bm
   result, black-eyed hens, black-eyed cocks
   BbFm BBMf
   pink-eyed hens, black-eyed cocks
   bbFm BbMf

In all these cases the facts ascertained can be explained by one assumption; namely, that the character in question is coupled or associated with the female sex-character; but this coupling is not complete or absolute—the character can be present in dominant spermatozoa, and so appear in males. In Lepidoptera such as *Ocneria dispar*, on the other hand, we may assume that the coupling is complete, and therefore when the spermatozoon is dominant the male character only develops in the males, and conversely the female character in the females. It is not, however, proved at present that all secondary characters in Lepidoptera are determined at fertilisation, and are unaffected by castration. It must be remembered that the characters and organs which are dependent on the sexual hormones in Vertebrates and Crustacea have some function in the relations of the sexes, and in Lepidoptera such functions as these—connected with fighting, courtship, and care of the young—seem to be generally absent. It would be interesting to ascertain whether the highly developed special organs in male beetles, which are certainly sexual in function as well as in limitation—such as the mandibles of the male stag-beetle—were also independent of the function of the gonads. Their varying development in different individuals indicates very forcibly that they are not. Even in Lepidoptera, the absence of wings in the female only in some species is probably due to their disuse in this sex only; and in this case it would be expected that the wings would be more developed if the ovaries were removed in the caterpillar. There is much room for further investigation.

Thus it seems to me we arrive in considering sex-limited characters at the same distinction which I have insisted upon in other characters, a distinction which is persistently ignored by the Mendelians. This is in general the distinction between adaptive and non-adaptive characters—which is, I believe, the same thing as the distinction between continuous and discontinuous variations. Mendelians assume that all characters arose as mutations—that all hereditary variations are discontinuous. To me it seems impossible to maintain that the organs which distinguish the frog from the tadpole—the amphibian from the fish—arose as mutations, since they develop continuously in the individual. The lungs develop gradually *pari passu* with the change from aquatic to aerial respiration. The same gradual development (corresponding with a change of external conditions) is seen in the metamorphosis of the flat-fish and of the hermit-crab; while in such cases as the degeneration of the eyes in the blind-fish of the Kentucky caves, or the divided eyes of Anableps—the fish which swims with its eyes half out of water—although there is no actual change of conditions in the individual lifetime, the original condition of the organ is more or less completely developed first, and then the new adaptive modification gradually takes place. On the other hand, we have characters which obviously arose as mutations—the characters the Mendelians investigate—which develop directly from the ovum, and have nothing to do with adaptation, or if they are ever of use to the possessor, are so only by accident. Examples of these are seen in the doubling of flowers, in albinism, in the extra toe in certain breeds of fowls and other cases of polydactylism, in the rose, pea, and walnut combs of fowls, and in all the numerous characters which distinguish the varieties of animals and plants under domestication, which (according to my views) are of the same kind as the characters which distinguish species in nature.

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RECENT HYDROBIOLOGICAL INVESTIGATIONS

THE GULF STREAM—AND CLIMATE AND CROPS IN NORTHERN EUROPE

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The exploration of the seas of Northern Europe, which has been carried on by most of the fisheries authorities of northern countries for the last half-dozen years, is now affording results of considerable value. There are still gaps in the area investigated; thus parts of the North Atlantic and Norwegian Sea, and some small areas in British seas require fuller study. The length of time, moreover, during which the work has been in progress is not sufficient to enable us to detect the larger periods in the physical changes of the sea-water. For these reasons it would be premature to attempt any general summary of the results of the investigations. Nevertheless, there are some recent memoirs of very great general interest, in particular the work of the Norwegian hydrographers.

Introductory

The main facts of the water-circulation of the North Atlantic have long been known, in a general sort of way, but it may be convenient to refer to them here in outline. Premising that there is comparatively little recent work with direct relation to this part of the ocean,[1] and that details still await further investigation, the following account will be sufficient for our present purpose. The impelling force of the water-movements in the North Atlantic is that of the North and South Equatorial Streams. A current produced by the junction of parts of these bodies of water enters the Caribbean Sea and

[1] But see Matthews, "The Surface Waters of the North Atlantic Ocean, South of 60° N. Latitude, September, 1904, to September, 1905," *Second Report* (*Southern Area*) on *Fishery and Hydrographical Investigations in the North Sea and Adjacent Waters*, 1904-5, part i. (Cd. 3837), 1907, pp. 269-348.

Gulf of Mexico, and issues from the latter as the well-known Gulf Stream. This ocean current is joined by a current which flows outside the West Indian Islands, and the stream so formed enters the North Atlantic and flows to the north-east. It is, at the outset, an "Ocean river" only a few miles wide, and possessing a velocity of about a hundred miles a day. Coming from a region of high temperature and rapid evaporation, the water is relatively warm and dense, having a temperature of about 20° C., and a salinity of over 36 per thousand. Soon after issuing from the Straits of Florida the Gulf Stream loses its individuality and fans out into the North Atlantic in about latitudes 40° N. to 45° N. and longitudes about 41° to 42° W. At about latitude 41° N. and longitude 50° W. it encounters the waters of the Labrador Stream coming down from Baffin Bay and Davis Straits, and the two currents interfere to some extent, the waters of the colder pushing through and underneath those of the warmer. All the time, however, the tendency is for the waters of the Labrador Stream to be pushed to the west, and for those of the Gulf Stream to flow to the east. After this collision the Gulf Stream becomes diffuse, and splits up into three badly defined "drifts." One of these turns round to the east and south, as the Canaries Current, and, ultimately rejoining the North Equatorial Stream, completes the gigantic anti-cyclonic eddy of the North Atlantic Ocean which encloses the Sargasso Sea. A second drift flows to the north-east and enters the region of the Bay of Biscay and English Channel. The third drift flows on to the north-east, to the west of the British Isles, as the Norwegian Branch of the European Stream.

These facts have long been known, but it is important to remember them when discussing the details of the circulation in the more northern regions. It is also necessary to remember that the strength and volume of the Gulf Stream itself are periodic, varying throughout the year in a definite manner. With regard to this annual periodicity, however, much still remains to be investigated. The Gulf Stream circulation also varies in strength from year to year. The fact that water of sub-tropical origin reaches the northerly coasts of Europe is, as is well known, of the utmost importance to the climate of the British Islands and that of the Scandinavian countries. It is not so well known that the periodicity in volume of the

Gulf Stream drifts, both the annual and the larger periodicities, are also of the greatest importance as causes of the fluctuations in the yield of the harvests and fisheries of certain parts of North Europe.

The English Branch of the Gulf Stream drift enters both the English and St. George's Channels. Its general distribution is variable from season to season and from year to year. February to April is about the time of the annual maximum. The year 1903 was also a maximum year, but the length of this longer period is not known exactly. Generally speaking, the axial portion of the English Channel is filled with water of truly Atlantic origin, possessing a salinity of about 35 to 35·2 per thousand. A narrow tongue of this water passes through the Straits of Dover into the North Sea. The extent of the 35·2 water and that of the North Sea prolongation of the Channel water are very variable.[1] Doubtless the Atlantic water in the Channel is pushed in, to some extent, by the impelling force of the Gulf Stream drift in the North Atlantic, but it is difficult to resist the suggestion conveyed by a glance at fig. 1, that this water is actually *sucked* through the Channel by the action of the cyclonic system of circulation of water in the North Sea. The volume of Atlantic water entering St. George's Channel and the Irish Sea is much less than that entering the English Channel, possibly because an outflow of relatively fresh water from the Bristol Channel sets up a cyclonic circulation off the mouth of this estuary. But, however this may be, we find that the Atlantic water of 35 per mille. salinity does not penetrate farther into the Channel than some distance south of the Fishguard-Rosslare line, as is indicated in fig. 1, where the further limit of this water is shown by the thick black line.

The Norwegian Branch of the Gulf Stream drift flows on along the west coasts of Ireland and Scotland. Fig. 1 shows that the isohalines lie closely together and very near to the west coast of Ireland, but that there is an area of low-salinity water off the north coast of Ireland and the south-west coasts of

[1] Detailed studies of the variability of the Gulf Stream flow into, and through the English Channel, and in the North Sea, in its southern part, will be found in the papers of Matthews. See *Reports on Fishery and Hydrographical Investigations in the North Sea and Adjacent Waters* (*Southern Area*). Bluebooks Cd. 2670, 1905; and Cd. 4641, 1909.

Scotland, probably because of the outflow of relatively fresh water from the Clyde and Irish Sea, and also because of the shoaling of the sea. We have seen that comparatively little

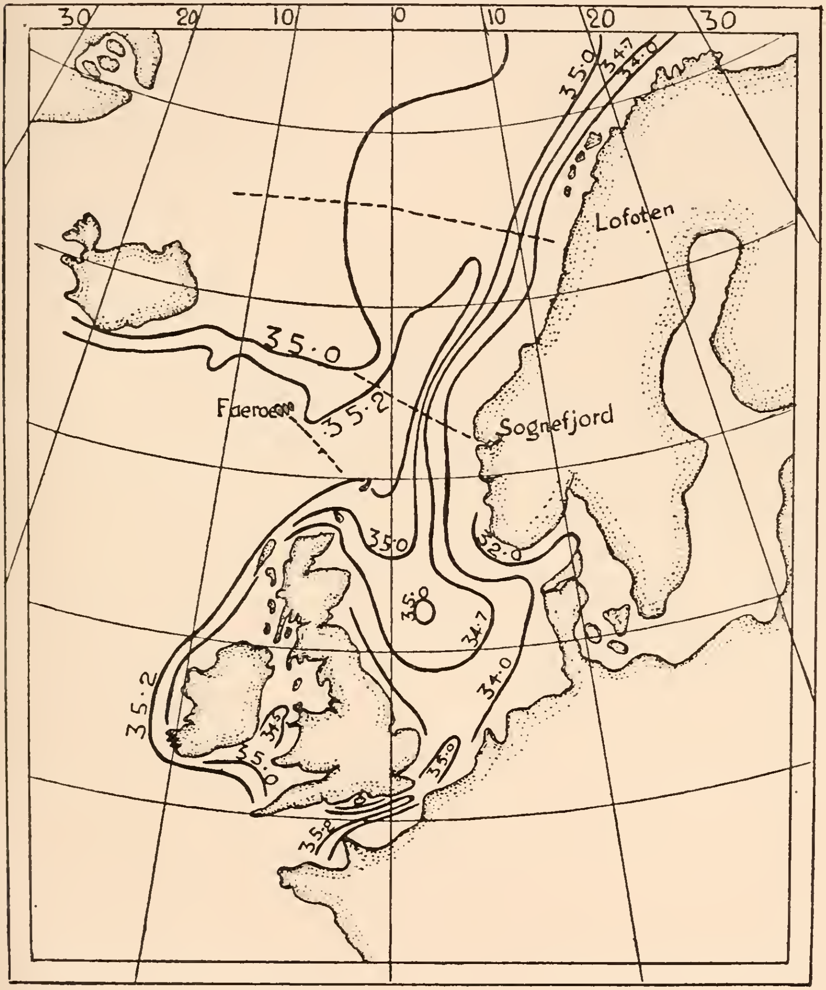

FIG. 1.—The isohalines in the British and Norwegian Seas.

The values represent the salinity (grams of total salts per 1,000 grams of water). The thin broken lines indicate the approximate positions of the main hydrographic sections. (From *Rapports et Procès-Verbaux*, International Council for the Exploration of the Sea, vol. x. 1909.)

Atlantic water enters St. George's Channel from the south, and this is still more so with regard to the northern entrance into the Irish Sea. The channel between Scotland and Ireland is a very narrow one, and though the depth is the greatest in the British sea-area, very little of a true current sets in through this channel. The sea-bottom is shallow between Islay and the north coast of Ireland, and forms a "barrier" which impedes the flow of the Atlantic water. The Irish Sea is therefore a relatively low-salinity area, and at no place does the proportion of total dissolved salts exceed about 34·5 per thousand. Further, there is a comparatively high rainfall off the coast of Ireland and the north-west coasts of England. Nevertheless, the pulse of the Gulf Stream periodicity can be felt distinctly in this part of the sea; and it has been possible to trace variations in the salinity of the Irish Sea, along a line between Holyhead and the Isle of Man, which are to be referred to the annual variations in the strength of the Gulf Stream drift. The salinity of the water is at a maximum here about February to May.[1]

The Norwegian Stream flows round the north of Scotland, mainly between the Shetland Islands and the Faeroes. The inflow to the North Sea between the Shetlands and the Scottish mainland may be neglected. The drift afterwards turns round to the south, flowing down the east coast of Scotland, and then tends to the east and across to the Continental coast into the Bight of Heligoland. Then, reinforced by a flow of coastal water, and by an outflow from the Baltic, it turns to the north and west again. Thus there is a cyclonic circulation in the northern part of the North Sea—as is to be seen from fig. 2. The figure shows that by far the greatest part of the water entering the North Sea comes in from the north, and that the contribution from the English Channel is so small that it may usually be neglected. Tongues of truly Atlantic water may project into the North Sea from both the north and south, but the greatest part of this sea-area is covered with water of 34·5 to 34·7 salinity.

We can hardly speak of these water-movements in the shallow seas as "currents" in the true sense of the word. Movements of water in these areas are mainly produced by

[1] The salinity changes in the water of the Irish Sea have been investigated by the Irish Department of Agriculture and Technical Instruction, and by the Lancashire Sea Fisheries Committee. See Bassett, *Annual Reports of the Lancashire Sea Fisheries Laboratory* for 1907 and 1908; and Herdman & Scott, *ibid.* 1908. The Irish results are published in the *Bulletins* of the International Council for the Exploration of the Sea.

the strong tidal streams, and are oscillatory ones, the water surging backwards and forwards along, or at an angle to the

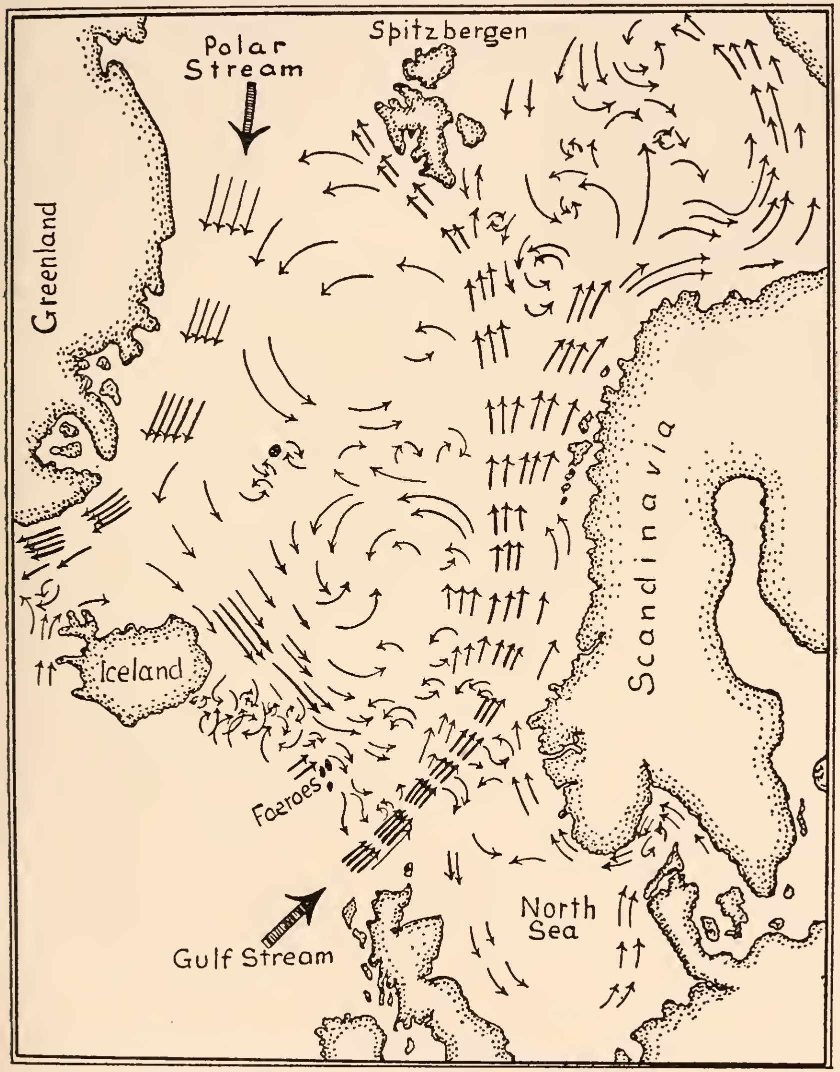

FIG. 2.—The Gulf Stream drift in high northern latitudes.

The arrows indicate the direction of drift of the surface waters. The crowding together of the arrows is roughly proportional to the intensity of flow. (From Helland-Hansen and Nansen, "The Norwegian Sea," *Report on Norwegian Fishery and Marine Investigations*, vol. ii. part i. Bergen, 1909.)

land. The flow of water due to the Atlantic drifts may only be detected by very precise current-measurements, and as resultants when the influence of the tidal streams is eliminated; or by experiments and observations of the course of floating objects and wreckage. When the tidal streams are ignored it is seen that there is a tendency for water to flow (1) round the north of the Shetlands and down the east coast, and across the North Sea, issuing from the latter as a stream which flows along the Jutland coast in a northerly direction; (2) through the English Channel into the North Sea; and (3) through St. George's Channel and the Irish Sea from south to north. The direction of both the isotherms and the isohalines shows that these water-movements do really occur.

Fig. 1, which represents the course of the isohaline lines in northern seas,[1] shows that the 35 per thousand isohaline does not actually touch the shores of the British Islands, but curves round the north of Scotland into the North Sea. It also penetrates through the English Channel into the latter area. The greater part of the North Sea itself is filled with water which is less than 35 per mille. salinity, and there is usually an indication of an "island" of truly Atlantic water in its central part. (See fig. 1.)

Fig. 2 shows the further course of the Norwegian Stream in high latitudes. The water-circulation in these parts of the northern ocean is very complex, because of the existence of a multitude of large and small eddies, mainly cyclonic in direction. The factors governing it are (1) the relatively strong north-easterly Norwegian Stream, and (2) the southerly flowing East-Icelandic Polar Stream. A number of submarine banks extend across from the British Plateau to the coast of Iceland, and the Faeroese Islands are the most elevated points of these. Between the latter islands and the Shetlands is a relatively deep channel, interrupted by a rather narrow elevation, the well-known Wyville-Thompson Ridge: over this ridge the water is from 500 to 600 metres in depth, and on either side of it the sea-bottom slopes down to about 3,000 metres in a comparatively short distance. The Norwegian Stream flows through this channel and over the ridge, scouring the surface of the latter free from any sediment.

The main course of the southerly-flowing Polar Stream

[1] For a detailed account of the salinity and temperature of Northern Seas see *Bulletin Trimestriel, Cons. Perm. Internat. Explor. Mer, Partie Supplementaire.* Copenhague, 1909.

is along the east coast of Greenland, but it spreads across over the Icelandic-Faeroe banks. It may interfere with the Norwegian Stream in the neighbourhood of the latter islands, and may sometimes enter the extreme northern part of the North Sea, carrying Arctic planktonic organisms into the Skagerak. All along the shoals between the Faeroes and Iceland the two streams interfere, causing small anti-cyclonic eddies; and large cyclonic movements of surface water are set up in the Norwegian Sea, and north and south of Jan Mayen, from the same cause. But the main drift of Atlantic water—"an integral part of the Gulf Stream"—is along the west coast of Norway; and the relatively crowded position of the isohalines shows how concentrated the Stream is in this region. South of the Faeroe Islands the Stream sends an offshoot towards Iceland, and at some times of the year there may be a large volume of relatively warm Atlantic water on the coasts of this island. In the neighbourhood of the Lofoten Islands the Stream also splits and a branch passes towards Spitzbergen, giving rise to the more northern of the two cyclonic systems referred to above. The remainder of the Stream passes north, rounding North Cape and entering the Barentz Sea. Both in the neighbourhood of Iceland and in the Barentz Sea this periodic flooding with Atlantic water is of momentous consequence for the local fisheries. By the time the Stream has penetrated into the remote Barentz Sea the temperature of the water has fallen greatly, and it then sinks, because of its greater density, and flows on as an under-current. Although this Atlantic water has cooled greatly its temperature is still higher than that of the Polar water normally present in the Barentz Sea, and it thus happens that the "summer" of the bottom water here is in November, when the Atlantic flow has culminated, while the "winter" of the water is in June.

But for this remote offshoot of the Gulf Stream circulation Norway, as an industrial and fishing country, would hardly exist, and in its place we should have a land with a climate resembling that of Greenland. The mean temperature of the Lofoten Islands in January is some 25° C. higher than the mean of the zone of latitude in which these islands are situated, the greatest temperature-anomaly on the face of the earth. Variations in the strength of the Gulf Stream drift in the neighbourhood of Norway are therefore of very great importance. To Otto Pettersson belongs the credit of first attempting to demonstrate the intimate relationship between the hydrographic conditions in the sea off the coasts of Norway, and the climatic conditions in that country. He showed that an unusually strong Gulf Stream flow led to an accumulation of relatively warm water in the Norwegian Sea. As this cooled it gave up its heat to the atmosphere, setting up ascending air-currents, which gave rise to cyclonic atmospheric disturbances, with all the consequences resulting from these eddies. He also showed that there was an intimate relationship between the air-temperature over Norway and the sea-temperature near the land; and that the earlier or later ploughing or sowing seasons might be associated with earlier or later maxima of the Gulf Stream flow. Pettersson's suggestions have had the greatest possible influence on methods of oceanographical and fishery research: indeed, it is largely owing to them that the International Fishery Investigations were initiated.

The Gulf Stream in High Latitudes [1]

The dotted lines on fig. 1 show the lines along which the hydrographic observations are being systematically taken. Along each of these lines there is a series of "stations," and at each station the temperature of the sea is determined, by means of insulated, or reversing thermometers, at the surface and sea-bottom, and at a number of intermediate levels; while, at the same time, samples of the water are taken for the purpose of salinity determinations, or of analyses of gas-contents, etc. A "hydrographic section" of the sea under the line of observations is constructed by drawing isohalines and isotherms, representing the condition of the water, as regards the distribution of temperature and salt-contents, by means of contour lines. Such a section shows that the water nearer the land is usually low-salinity water, while that near

[1] The most recent and authoritative work on the oceanography of the Norwegian Sea is that by Bjorn Helland-Hansen and Fridjof Nansen, "The Norwegian Sea: its Physical Oceanography, based on the Norwegian researches 1900-1904," *Report on Norwegian Fishery and Marine Investigations*, vol. ii., pt. 1, No. 2, pp. i.-xx. + 1-390; tables and 28 plates. Kristiania, 1909.

the middle of the line is much denser. As a rule the water near the sea-bottom is both colder and denser than that at or near to the surface. In the open sea off the coasts of Britain and Norway practically all the water at or beneath the surface will have a salinity of over 35 per thousand: in any hydrographic section, then, the proportion of the whole area covered with water of this salinity measures directly the strength of the Gulf Stream drift.

It would be possible to measure the area beneath the lines of observation filled with truly Atlantic water, and then, by measuring the velocity of the drift and ascertaining the mean temperature, to find the quantity of heat conveyed per unit of time into any of these northern regions of sea. But the velocity of the drift is not exactly known, and such a calculation would, in the meantime, be a very rough one. It is sufficient to take such a section as that running out from Sognefjord, the more southerly of the two Norwegian lines shown on the chart (fig. 1), and to estimate the area of the section containing water of over 35 per mille. salinity, and multiply this value by the temperature. Relative measures of the amount of heat transported by the drift are so obtained, and these values are also measures of the strength of the Norwegian Stream; or, more precisely, they are functions of both this and the strength of the East-Icelandic Polar Stream, supposing that the latter were a factor in the year considered. Now the study of the Sognefjord line of observations in this manner gives the following series of values:[1]

Product of the sectional area, beneath the Sognefjord line, filled with water of 35 per mille. salinity or over, into the mean temperature.

May, 1901 .	. 918	May, 1904 .	. 950
,, 1902 .	. 1122	,, 1905 .	. 1249
,, 1903 .	. 1077		

Thus there are significant variations in the figures so obtained in this series of years.

[1] Nansen and Helland-Hansen, "Die jährlichen Schwankungen der Wassermassen im Norwegischen Nordmeer ihrer Beziehung zudem Schwankungen der Meteorologische Verhältnisse, der Ernteerträge und der Fischereiergebnisse in Norwegen," *Internat. Revue Gesamt. Hydrobiologie und Hydrographic*, Bd. ii., Nr. 3, 1909, pp. 337–61.

Now fig. 2 shows that the water which passes through the Faeroe-Iceland Channel also passes over the Sognefjord line, and then over the Lofoten line and so into the Barentz Sea. But large masses of sea-water change in temperature and salinity very slowly, and we should expect to find that the periodicity which is indicated above would also be shown in the more northerly hydrographic sections. But if we plot series of values obtained in this way on the same diagram, superposing the data for the same years, nothing but confusion appears to result. If, however, maximum and minimum be superposed on each other it is at once seen that the periodicity

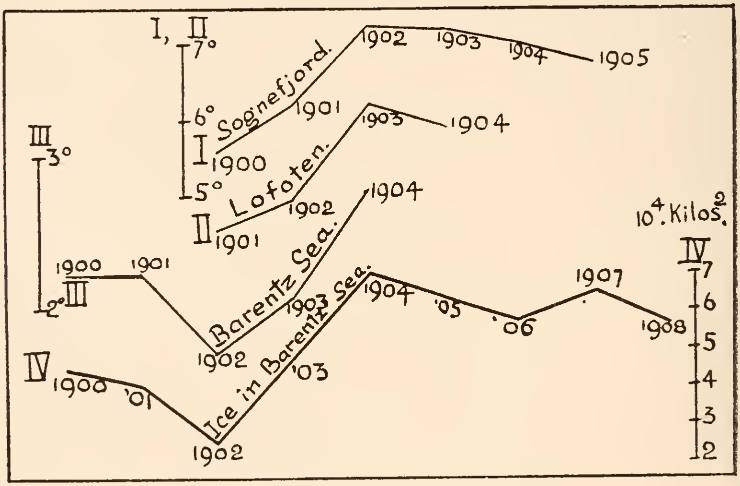

FIG. 3.—Sea-temperatures, etc., in the Norwegian Sea.

Curve I. The average temperature of the Atlantic water beneath the surface at the Sognefjord section.
,, II. The same for the Lofoten section.
,, III. Average sea-temperatures at depths from 100 to 200 metres in the Barentz Sea.
,, IV. The area of ice-free sea-surface in the Barentz Sea. The values are 10^4 kilometres^2.
All curves represent the conditions in May. (From Helland-Hansen and Nansen, *loc. cit.* p. 344.)

is the same. This has been done in the above diagram, prepared by Helland-Hansen and Nansen. Curve I. shows the average temperature of the sea beneath the Sognefjord section, II. the same for the Lofoten section, and III. the same for three Russian stations in the Barentz Sea. Curve IV. shows the area of ice-free sea-surface in the Barentz in hundreds of thousands of square kilometres.

Now, there can be no doubt that the nature of the physical changes in the sea in each of these regions is the same, and that the periodicity is also the same. But the dates of occurrence of the maxima and minima differ. The maximum at Sognefjord occurred in 1902, at Lofoten in 1903, and at the Barentz Sea stations in 1904. The conclusion is that the water passing the more southerly of the three lines—the Sognefjord one—in 1902 traversed the Lofoten line in 1903, and arrived in the Barentz Sea in 1904. Thus the observations give us a rough measure of the velocity of the Gulf Stream drift in these latitudes. It is impossible from the existing data to speak more precisely than this. The observations are made only at intervals of three months, and determinations which are strictly comparable with each other are available for the period of 1902–8. It is also difficult at the present time to allow accurately for the influence of the Polar Stream. One must not expect that an exact value for the velocity can be obtained, nor that the correspondence in the case of the three curves would be very minute. Nevertheless, it seems to be close enough to justify the conclusions.

Climate and Hydrography

The area of ice-free surface in the Barentz Sea is shown graphically in fig. 3. The estimations have been made by Helland-Hansen and Nansen from planimetric measurements on the charts published by the Danish Meteorological Office, for the years 1900–8. The correlation between the curves of sea-temperature and extent of sea covered by ice is extraordinarily good. This is as might be expected, for the amount of ice in the sea must obviously vary as the temperature, allowing, of course, for disturbances due to a variable drift from the Polar seas. But, admitting the incompleteness of the data, it seems also that there is a close correspondence between the quantity of ice in the Barentz Sea and the amount of heat in the water of the Gulf Stream drift in the latitudes of Southern and Middle Norway two and three years previously. That is, a strong Gulf Stream flow corresponds to a minimum ice year in the Barentz Sea, and *vice versa*. Not only so, but it can also be shown that there is an intimate correspondence between the volume of the Gulf Stream and the air-temperature in Norway. Fig. 4 is taken from Helland-Hansen's and Nansen's memoir (p. 345) and shows this relationship. The line I. shows the variations in the amount of heat contained in the water beneath the Sognefjord section: it represents the area of water of salinity of 35 per mille. and over, multiplied by the mean temperature. The line II. represents the mean temperature-anomaly in Norway. The mean air-temperature has been determined from the records of 22 meteorological stations distributed over the whole of the country and is based on thirty years' records. The annual deviations from this mean are then calculated for the same 22 stations. The water-temperature values are those obtained during May, and the air-temperatures are for the period November to April of the following winter. We see

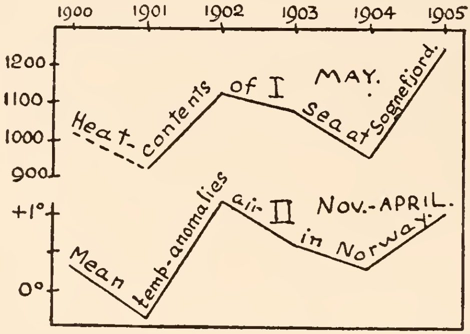

FIG. 4.—Air and sea-water temperatures at Norway.

Curve I. represents the heat-contents in the Atlantic water beneath the Sognefjord section in May.
,, II. represents the average temperature-anomalies in Norway in the following November–May period.
(From Helland-Hansen and Nansen, *loc. cit.* p. 345.)

at once that a maximum Gulf Stream flow in May is followed by a maximum air-temperature in the following winter.

The temperature of the air over the land is therefore a function of the amount of heat contained in the water of the adjacent sea-area. It would not do to attempt to compare the mean surface temperature of the sea with the land-temperature. In the winter half of the year the surface waters of the sea are warmer than the overlying atmosphere, and heat is radiated from the sea to the air. If this liberation of heat is rapid, ascending air-currents are produced, with accompanying cyclonic atmospheric disturbances. Obviously the latter will affect the adjacent land-areas, but how exactly will depend on a variety of conditions. When the surface waters of the sea become cooled by radiation they become denser and sink to the lower levels, setting up vertical convection currents which are of the greatest importance with regard to the distribution of the marine plankton. In the spring of the year the air is warmed, and for a short period there is thermic equilibrium between the sea and the atmosphere. Then the atmosphere becomes the warmer, and heat is communicated from it to the surface layers of the sea, with the result that there is usually, at this season, a warmer surface stratum overlying colder and denser water. The temperature at the surface of the sea is not therefore a good measure of the relative amount of heat conveyed by the oceanic drifts, except in the case of coastal waters of less than about one hundred fathoms in depth. There we usually find a strong mixture of the water, which is due to tidal streams, and the whole body of water may be practically homogeneous as regards temperature and salinity. The best measure of the varying influence of the Gulf Stream drift is the amount of heat contained in that part of the water which is still unmixed with other water of different origin.

The observations under review thus prove—what has indeed long been known in a general kind of way—that the climates of Norway and Sweden, and to a less extent that of the British Isles, are much milder than would be the case if the Gulf Stream did not reach those shores. Really the observations do more than this, for they indicate the probability that the character of the seasons in the Scandinavian countries *may be predicted about six months to a year ahead; while that of the sea off the Murman coast might be foretold about two years in advance.* The water which fills the Iceland-Faeroe Channel flows on and fills the sea off the coasts of Northern Norway about a year and a half later, and the basin of the Barentz Sea about two and a half years later. If, then, we know the exact physical conditions of the water at such a "strategic point" as the Faeroe Channel, from month to month, we might be able to foretell the character of the seasons in the Northern countries some considerable time in advance. This is the conclusion of great practical importance which emerges from the results of the hydrographic study of the seas of Northern Europe. It does not seem that we shall ever be able to predict the exact nature of what we term the "weather" from such data. "Weather" is mainly the product of the cyclonic disturbances originating in the sea some distance from the land. In our own case these disturbances apparently arise in the North Atlantic, no doubt as the products of the irregular heating of the atmosphere over the sea by masses of relatively warm and cooling water. Such disturbances originating in the Iceland area also have an appreciable effect on the weather of the British area. But, unfortunately, there seems to be little likelihood of the hydrographic investigation of the North Atlantic from this point of view.

Hydrographic Conditions and the Harvests

It was shown by Otto Pettersson in 1896 that a close connection between the hydrographic condition of the sea and

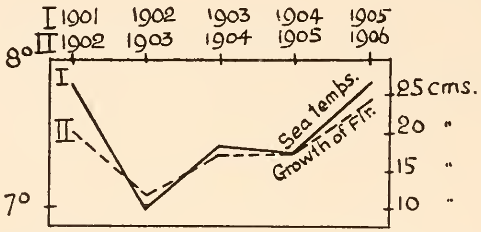

Fig. 5.—Correspondence between the temperature of the surface waters of the Sognefjord section in May, and the growth of the Fir in Norway.

(From Helland-Hansen and Nansen, *loc. cit.* p. 348.)

vegetation phases on the adjacent land was *a priori* probable. He showed, for instance, that the earlier or later flowering of the Coltsfoot (*Tussilago Farfara*) was to be associated with the variations from the mean in the sea-temperature off Norway in the February of the same year. But the coastal sea-temperatures do not, as we have seen, afford a very good expression for the hydrographic changes taking place in the sea in general, and Pettersson's suggestions have only been fully borne out since the data accumulated during the last few years became available.

Holmboe (quoted by Helland-Hansen and Nansen) showed that there was a close correspondence between the growth of the Fir (*Pinus sylvestris*) in Norway and the meteorological conditions during the previous year—when the buds were formed. His measurements of the growth of the trees were made at eleven places in eastern Norway, and are represented graphically by Helland-Hansen and Nansen. I reproduce their diagram as fig. 5. The thick line represents the mean surface temperature of the sea over the Sognefjord line, and the thin line the mean summer growth of the Fir in eastern Norway. The degree of correlation is so good that one cannot doubt that the two series of events are associated.

Meinardus associated two groups of conditions:

A. WEAK ATLANTIC CIRCULATION (August to February).

Low sea-temperature off European coast (November to April).
Low air-temperature in Middle Europe (February to April).
Not much ice round Newfoundland (Spring).
Much ice round Iceland (Spring).
Bad wheat and rye crops in Western Europe and Northern Germany.

B. STRONG ATLANTIC CIRCULATION (August to February).

High sea-temperature off European coast (November to April).
High air-temperature in Middle Europe (February to April).
Much ice round Newfoundland (Spring).
Not much ice round Iceland (Spring).
Good wheat and rye crops in Western Europe and Northern Germany.

A similar parallelism between the physical conditions in the sea and the yield of the land crops has been demonstrated for Norway by Helland-Hansen and Nansen. This is shown in fig. 6, where the thick continuous line I. represents the surface temperature of the Atlantic water on the Sognefjord line in the May of each of the years considered; and the thin line II. represents the mean annual deviation of the air-temperature of June of the same year from the mean of the preceding thirty years. The crop of peas, lentils, and beans of the same year is represented by the line III.; that of cereals in general by the line IV.; potatoes by the line V.; and hay by the line VI.

When we remember that local conditions, such as rainfall, or the amount of cloud, must also affect the yield of these crops, it must be concluded that the correspondence between the various productivities and the intensity of the Gulf Stream drift, as represented by the surface-temperature of the sea, is surprisingly close. Probably the growth of these crops is determined to a great extent during the few months immediately following the time of sowing. During this period the upper layers of the ground are affected by the temperature of

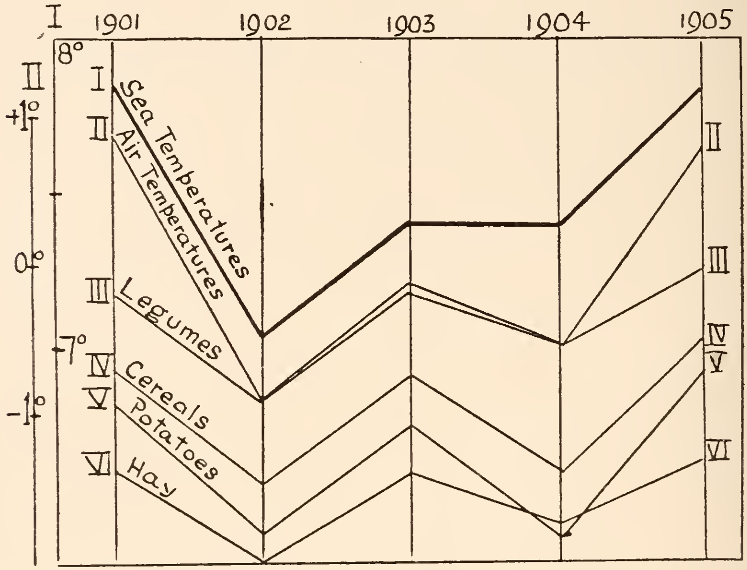

FIG. 6.—Correspondence between the average sea-temperature beneath the Sognefjord section, the average air-temperature in Norway, and the yields of various harvests in Norway.

(From Helland-Hansen and Nansen, *loc. cit.* p. 349.)

the air in contact with it; by the more or less rapid movement of the latter; and by the amount of sunshine and cloud. But these are just the factors which lead to the heating of the surface layers of the sea. The warmer the air, the greater the amount of solar radiation, and the less the amount of cloud-covered sky, the warmer will be the surface layers of the sea in comparison with those beneath. Now, if the temperature of the stratum of sea immediately beneath the surface be compared with that at 200 metres depth, we obtain a measure of the intensity of the factors causing the heating-up of the surface layers, for the water at depths of 200 metres is no longer affected by solar radiation to an appreciable extent. If the difference between the surface temperature and that at a depth of 200 metres be plotted on the same diagram as the yield of barley, peas, lentils, and beans, it will be found that the similarity of the various curves is remarkably close, particularly for the years 1902 to 1905; and the conclusion appears to me to be inevitable that the yield of the land-crops depends on the temperature of the sea and that it should be possible, by making the fullest use of hydrographic data, to estimate some months in advance, with some accuracy, the productivity of the harvests in relation to those of previous years.

(To be continued)

HALLEY'S COMET FROM THE NORMAN POINT OF VIEW[1]

By W. B. BRODRICK

For the past few months Halley's comet has been subjected to a regular literary siege. It has been attacked from the scientific point of view, the semi-scientific point of view, the historical point of view, the popular point of view, and the sensational point of view; but so far as I can discover no one has dealt with it in the manner here proposed—that is, from the Norman point of view. The subject appears to lend itself quite naturally to the following divisions: I. The Introduction—slightly astronomical; II. The Review—mainly historical; III. The Conclusion—partly speculative.

I. Introduction—Slightly Astronomical

"The expected return of Halley's comet. . . ." Such was the properly guarded, if somewhat sanguine, language used until recently by astronomers when speaking of the forthcoming periodical visit of this member of the solar system to perihelion, or the neighbourhood of the sun—a visit instinct with interest to every astronomer and student of the fascinating mysteries of the heavens, and of peculiar, personal, even vital interest to all who claim to be of Norman blood.

Comets are mysterious bodies, and although the resources of modern science and the patient investigations of those who have made a special study of this branch of astronomy have added to the knowledge of their movements and their nature, astronomers to-day are compelled to admit that what they know about comets bears but a very small proportion to what remains unknown.

It has now been ascertained that many comets, of which Halley's comet is one, are periodic and members of the solar system—members, that is, of our sun's family. The rest of the family consists of the steady-going planets, who maintain the best

[1] An address delivered to the Jersey Society in London on December 14, 1909.

traditions of the respectable solar family circle and pursue the even tenor of their way round their parent the sun (be it shy Mercury; Venus, now bashful and modest; ourselves, the Earth; ruddy Mars, with his puzzling canals; giant Jupiter; Saturn, the gem of the family; or the latest arrivals, those slow-coaches Uranus and Neptune). Compared with these steady old files, comets are wild, impetuous fellows who lead restless, irregular, roving lives. For them the humdrum family existence has no attraction; the spirit of adventure fascinates them and impels them to explore the starry depths of the universe. They may be compared with the prodigal son, for this life of dissipation wastes their substance. Like him they return home, but in their case it is only a flying visit, as they immediately hurry off again on a fresh voyage of adventure, to return again and again after approximately the same period of absence. Each visit shows that the life they lead is telling on them: they are not so bright; their clothes show signs of wear and tear; still they continue to pursue their reckless, roving life until, utterly exhausted, their dissolution comes about, and if they do revisit the family they are so shrunk, changed, and disintegrated as to be no longer recognisable, though their tombstones are placed in the heavens in the shape of meteoric swarms which serve to mark out the paths of their individual journeys.

This life-history of a periodic comet was unknown two hundred years ago. It would have been hardly possible then for any but astronomers to have mentioned the word "comet" without experiencing a thrill or a shudder. In all ages comets have been superstitiously regarded as heralds, and even as the actual causes, of strange or disastrous events, plague, famine, war, or the death of princes. In the opening scene of *King Henry VI.*, Shakespeare makes the Duke of Bedford, Regent of France, say in reference to the death of Henry V.:

> "Hung be the heavens with black, yield day to night!
> Comets, importing change of times and states,
> Brandish your crystal tresses in the sky."

Again, in *Julius Cæsar* occur the well-known lines:

> "When beggars die there are no comets seen,
> The heavens themselves blaze forth the death of princes."

A quaint old book, published in 1549, called *The Complaynt of Scotland*, speaking of "the Star called a Comet," says:

"Siklyik there is diuerse other sternis of ane euyl constellation quhille pronosticatis future euyl accidentis. there is ane sterne that aperis nocht oft in our hemispere, callit ane comeit. quhen it is sene, ther ocurris haistyly eftir it sum grit myschief. it aperis oft in the northt, it aperis oft in the quhyt circle called circulus lacteus, the quhille the marynalis callis vatlant streit.[1] Sum tyme it vil apeir lyik lang bludy hayr, sum tyme lyik ane dart, sum tyme lyik ane bludy spyr. it aperit in the lyft[2] lyik ane sourd befor the detht of Julius cesar, and alse it aperit lyik ane trumpet quhen the kyng of perse straik ane battel contrar the Grecians. Sum tyme it hes aperit lyik tua gait buckis[3] iustand[4] contrar vthirs."[5]

I will give one more quotation from an old work entitled *Defensative against the Poison of Supposed Prophecies*, by Henry Howard, Earl of Northampton, published in 1583. It concerns an anecdote of Queen Elizabeth, and runs :

"I can affirm thus much as a present witnesse by mine own experience that when dyvers upon greater scrupulosity than cause went about to dissuade her majestye (lying then at Richmonde) from looking on the comet which appeared last: with a courage answerable to her state shee caused the windowe to be sette open and cast out this word, 'Jacta est alea,' the dice are thrown. . . ."

Quotations to the same or similar effect might be multiplied almost indefinitely; it is sufficient to note that, *inter alios*, Pope, Milton, Josephus, Virgil, and Homer all refer to the dire portent or evil influence of the comet.

This superstitious dread well exemplifies the truth of the words of Tacitus :

"Omne ignotum pro magnifico,"

which may be rendered, "The tendency of the human mind is to exaggerate the possibilities of the unknown." The "ignotum" is apt to become a bogey.

Concerning the present reappearance of Halley's comet—for it was discovered by photography so as to be identified on September 11 last, though it will not be visible to the naked eye until next April—it is interesting to inquire how it

[1] Watling Street.
[2] Lyft = sky.
[3] Gait buckis = he-goats.
[4] Iustand = jousting, fencing.
[5] Contrar vthirs = each other, one another.

came by its name. Speaking generally, all great comets of comparatively recent years bear, popularly, the name of their discoverer, but Halley's comet is an exception to this rule. Edmund Halley was Astronomer-Royal from 1720 to 1742, and was a friend and contemporary of the great Isaac Newton, who died in 1727. Newton had propounded the theory that comets were members of the solar system and as such circled round the sun. Halley, whose admiration for Newton almost amounted to worship, was attracted by and determined to investigate this theory (it is said that Newton was not fond of "doing sums"; he had a mind above such trivialities), so Halley applied it to a comet which appeared in 1682. The result of his labours in this direction—labours which entailed patient and prolonged research and investigation of records of comets—was to prove the correctness of Newton's theory, at least so far as this particular comet was concerned. Halley obtained an elliptic orbit for this rover, and predicted its return in 1758 or 1759. This was perhaps the boldest step ever taken by an astronomer in the matter of prophecy. Whether the knowledge in his case that in all human probability he could not live to see his prediction fulfilled or falsified—he would have been well over 100 in 1758—stimulated his courage to this unusual extent would be idle to speculate, and, in view of what happened, to the last degree ungenerous to suggest.

The predicted return of this comet was anxiously awaited, and when, on Christmas Day, 1758, it reappeared and was recognised, Halley's boldness was vindicated; since then the visitor has always been associated with his name and known as Halley's comet.

Halley found that the period of this comet—that is, the time occupied in making the circuit or round trip from the sun to the utmost limits of the solar system beyond Neptune and back again—was not constant, but varied from 75 to 76 years, and he identified this comet with others which had appeared in the years 1456, 1531 and 1607. Since his time further researches have been made and much has been learnt as to the causes of these variations. The period appears to vary from 74 to 79 years, with an average of about 76¾ years; the variation is ascribed to perturbation due to the attraction of the planets. Exhaustive investigations and examinations of old records were made 50 or 60 years ago by Mr. J. R. Hind, whose work has quite recently been most admirably revised, corrected and extended by Mr. P. H. Cowell and Mr. A. C. Crommelin, of the Royal Observatory, Greenwich. These gentlemen have carried back the identification of this comet, mainly from Chinese records, to the year 240 B.C., which makes the present its twenty-ninth recorded appearance. One of these appearances was in 1066, one of the first dates Normans learnt at school and probably the last they are likely to forget. Mr. Hind's work was not undertaken until after the last appearance of this comet in 1835. He then *suspected* that it was the same body as that which appeared in 1066, but it has been left to the two gentlemen just named definitely to establish its identity; its present appearance, therefore, is the first since proof has been obtained that, in the words of the Astronomer-Royal, "the comet of April 1066 was certainly Halley's comet." It is that appearance which is considered of sufficient interest to Normans to deserve special notice, although it is difficult to find language capable of doing justice to the importance of the occasion.

II. Review—Mainly Historical

The comet appeared, as has been mentioned, in April 1066. The records show that it was an unusually brilliant object and that its appearance produced a profound impression on all who saw it.

Let us now carry our minds back to that period and recall the happenings, in England and Normandy, of the three months preceding that April.

Edward the Confessor died on January 5, 1066. He died childless. It seems that, some years before, he had promised William, Duke of Normandy, that he should succeed to the crown of England on his death. During the previous year, 1065 (or possibly this may have been in 1064), Harold, then undoubtedly the most powerful man in England, while the (involuntary) guest of William in Normandy, had recognised and, so far as he could be said to do such a thing, confirmed this promise; furthermore he had sworn upon a boxful of holy relics to support William's claim to the English throne after Edward's death.

When Edward was at the point of death, Harold prevailed on him to recall his promise to William, and to nominate him (Harold) as his successor, or at least to allow the barons to elect him king; on the day following Edward's death, Harold was crowned King of England.

The news was carried to William, who despatched messengers to Harold demanding the crown and reminding him of his oath. Harold refused point-blank to relinquish the position to which he had been elected, and William decided upon the invasion.

It was not without anxiety that he took this step and started to plan his preparations. He knew that he had nothing like a sufficient quantity of ships for such an expedition, and, what was of far more importance, he knew also the disinclination of his barons and retainers to cross the sea in his service; but his determination to avenge himself and pursue his right was not shaken. William was not only determined, he was energetic and resourceful. Ably seconded in one direction by the bold and fearless Fitz-Osbern, and in the other by the wily Lanfranc (destined later to become Archbishop of Canterbury), he left no stone unturned in furthering the success of the projected invasion. While Fitz-Osbern with lavish promises rallied and encouraged the hesitating barons, and Lanfranc secured the recognition by the Pope of the justice of the Duke's claim, William busied himself in obtaining promises of co-operation, or, where that was impossible, of strict neutrality, from the rulers of the adjoining states and kingdoms. The blessing of the Pope, accompanied as it was by the present of a sacred gonfalon (or banner) and a ring containing a hair of the holy St. Peter, had a tremendous effect in swelling the number of William's followers. The expedition now assumed the character of a crusade and attracted volunteers and mercenaries from the greater part of Western Europe.

This, we may assume, was the position of affairs in the middle of April, when on the 18th (or, according to some chroniclers, the 24th) a magnificent comet, now known to be Halley's, suddenly blazed forth in the heavens with remarkable splendour. It appears certain that this was regarded in Normandy and on the continent generally as a presage of William's success, and it is not unreasonable to assume that it served to supplement the additions to William's forces which the action of the Pope had already so materially influenced.

To complete the historical narrative it is now necessary to quote (1) from the two historians, Freeman and Thierry, whose works on the Norman Conquest, one in English, the other in French, stand out by themselves; and (2) from the Chronicles of some of the chroniclers of the period.

Freeman, at p. 72 of vol. iii., after mentioning the appearance of the comet, says:

"No man in any land ventured to deem that such a token came without its mission. As of old the stars in their courses fought against Sisera, so now that wondrous star was doubtless sent to fight against some one among the great ones of the earth. . . . The sign was indeed one of awe and warning. . . . Some great event was doubtless portended; some mighty ruler was soon to meet with his overthrow; but who could tell whether the fiery sword which hung over the world was drawn on behalf of Harold or on behalf of William?"

And further on in the same volume, at p. 640, he says:

"This comet evidently made the deepest impression in every part of Europe. It is recorded in nearly every chronicle everywhere, and it is generally, even by men who have no special connexion either with England or Normandy, accepted as a presage of the conquest of England."

And Thierry, at p. 308, says:

"L'apparition d'une comète visible en Angleterre pendant près d'un mois produisit sur les esprits une impression extraordinaire d'étonnement et d'effroi. Le peuple s'attroupait dans les rues et sur les places des villes et des villages pour considérer ce phénomène qu'on regardait comme la confirmation des pressentiments nationaux."

He then quotes a *déclamation poétique* of a monk at Malmesbury which will appear later.

Further on in his history he says:

"Ce grand événement (le couronnement de Harold) est suivi d'un autre dont tous les historiens ont fait mention; j'entends parler de la comète qui parut dans le mois d'Avril de cette même année 1066. . . . Les spéculatifs du temps ne manquèrent pas d'attribuer à ce phénomène le changement que l'expédition de Guillaume en Angleterre y causa peu de temps après."

Turning now to the chronicles, it is well to take first the description in the *Roman de Rou* by Master Wace, who is not the credulous Herodotus of the age, but is accounted the most trustworthy of the chroniclers. From the Norman point of view he also enjoys the distinction of having been a Jerseyman. He says, at line 11460:

> "El terme ke ço estre dut
> Une esteile grant apparut,
> E quatorze jors resplendi.
> Od tres lons rais deverz midi;
> Tele esteile solt l'en veir
> Quant novel rei deit regne aveir.
> Asez vi homes ki la virent,
> Ki ainz e poiz lunges veskirent;
> Comete la deit apeler
> Ki des esteiles volt parler."

Translation by Taylor.—"Now while these things were doing a great star appeared shining for fourteen nights with three long rays streaming towards the south; such a star as is wont to be seen when a kingdom is about to change its king. I have seen many men who saw it, men of full age at the time and who lived many years after. Those who discourse of the stars would call it a comet."

Let us next take the chronicle of Geoffrey Gaimar. He says, at line 5145:

> "Après lur mort une comète,
> Une estelle, dont li prophète
> Et li bon astronomien,
> Sievent q'espeant mal ou bien,
> Se demustra el firmament;
> Assez la virent meinte gent.
> La nuit de Letanie majour
> Fist tel clarté cum se fust jour.
> Moult plusours homes l'esgardèrent;
> En maint endreit en deuinerent.
> Chascuns disoit sa divinaille;
> Mès tost seurent la grant contraille,
> E la grant tribulacion
> Qe prius avint à la region."

Translation by Martin in the Rolls Series:

> "After their death [1] a comet
> (A star that is of which soothsayers
> And good astronomers
> Know that it portends either good or ill)
> Showed itself in the firmament.
> Many people saw it well.

[1] Death of Edward and death of Queen.

On the night of Litania Major[1]
It made as much brightness as if it were day.
Many men looked at it;
In many places men foretold from it.
Each man said his guess;
But soon followed the great strife
And the great tribulation
Which afterwards came to the country."

The next extract is from the chronicle of Benoit de Ste. More. He says, at line 36772:

" Dunc en ces jorz si faitement
Aparut sus, el firmament,
Une clartez e un planete,
Une resplendisanz comete,
Dunt en eisseint trei grant rai.
Ce lis e truis e vei e sai
Que quinze nuiz durèrent bien.
Si distrent astrenomien
Que c'est de regnes muemenz
Ou de reis ou de hautes gens."

Translation by W. L. de Gruchy:

" Then in those days there likewise appeared above in the firmament a brightness, a planet, a brilliant comet, from which issued three great rays. This I read and discover and see and know, that they lasted quite fifteen nights. Then said the astronomers, There be changes of kingdoms or of kings or of great persons."

The last extract is from the chronicle of William of Malmesbury, and includes the *déclamation poétique* mentioned by Thierry. He says, at p. 212:

"Soon after a comet, denoting, as they say, change in kingdoms, appeared trailing its extended and fiery train along the sky; wherefore a certain monk of our monastery, by name Eilmer, bowing down with terror at the sight of the brilliant star, wisely exclaimed, 'Thou art come! a matter of lamentation to many a mother art thou come; I have seen thee long since; but I now behold thee much more terrible, threatening to hurl destruction on this country.' "

The next few lines, though not pertinent to the subject of this article, are worth quoting; they seem to show that the monk Eilmer lived a little in advance of his times. They run:

[1] viii. Kal. Mai.—*i.e.* April 24.

Halley's Comet as represented in the Bayeux Tapestry.

[p. 501

"He was a man of good learning for those times; of mature age; and, in his early youth, had hazarded an attempt of singular temerity; he had by some contrivance fastened wings to his hands and feet, in order that, looking upon the fable as true, he might fly like Daedalus, and collecting the air on the summit of a tower, had flown for more than the distance of a furlong; but agitated by the violence of the wind and a current of air, as well as by the consciousness of his rash attempt, he fell and broke his legs, and was lame ever after. He used to relate as the cause of his failure that he had forgotten to provide himself with a tail."

The remaining contemporary evidence is to be found in the Bayeux tapestry, that marvellous piece of work which has been aptly termed "a priceless relic of antiquity." Here, after portraying the death and burial of Edward, the designer (see fig.) pictures some men in an attitude of amazement looking and pointing at a wondrous star, with the legend over the picture "ISTI MIRANT STELLĀ" ("These men marvel at the star"). The artist has produced in the sky a marvellous and fearsome object which is intended to represent the comet. Although it bears little resemblance to our notions of the figure of a comet, yet there are to be seen photographs of comets which appeared as recently as 1905, 1907 and 1908 which, except in the matter of detail, are not very unlike the figure in the tapestry. As to the "three long rays," perhaps considerations of space have compelled the artist to curtail these, and by way of compensation he has presented us with eight short ones.[1]

Objection has been taken to the form of the legend, and it is pointed out that this should run "ISTI MIRANTUR STELLAM." With regard to "stellam," an examination of the original tapestry, or, failing that, of the photograph of the tapestry in the South Kensington Museum, discloses the existence of a line signifying abbreviation over the A of STELLA, and determining the reading as "stellam." Unfortunately, the English artist who, early in the last century, was commissioned to make a copy of the tapestry, failed to reproduce this abbreviation sign; and nearly, if not quite, all of the reproductions of this scene

[1] The church of St. John, Horselydown, plainly visible on the river side of the railway about half a mile out of London Bridge Station, boasts of a weathercock probably unique in design. It is not unlike the figure in the tapestry, and must undoubtedly be intended to represent a comet (perhaps even Halley's comet), though the pew-opener inclines to the opinion that it represents a *louse*.

given in astronomical works appear to have been taken from his copy of the tapestry, and so this error has been perpetuated. MIRANTUR would doubtless be more classical than MIRANT; but on consulting the Lexicon of Facciolatus one finds authority for the use of "miro" in place of the more usual "miror," and it may have been customary in Normandy in the eleventh century. Thierry, in dealing with this matter, makes a curious mistake; he speaks not only of the mark of abbreviation over the A of STELLA, but also of a similar mark over the T of MIRANT (signifying an abbreviation for "mirantur"), which does not exist in the original. A correct reproduction of the legend on this part of the tapestry is to be found in Mr. Fowke's book.

This representation of the comet in the Bayeux tapestry is the earliest pictorial representation of Halley's comet.

For the purposes of the conclusion there now remains the statement of the fact that the battle of Hastings was a hard-fought fight. All the records show that the success on either side was nearly balanced, and that the issue was in doubt practically throughout the day. It was no easy victory for William. Wace says:

> "From nine o'oclock in the morning till three in the afternoon the battle was up and down, this way and that, and no one knew who would conquer and win the land. Both sides stood firm and fought so well that no one could guess which would prevail."

III. CONCLUSION—PARTLY SPECULATIVE

Summing up the foregoing facts and inferences we find—

(i) that the appearance of this comet at a critical period of William's preparations for the invasion of England was probably responsible for a welcome addition to his force in point of numbers;

(ii) that the comet certainly inspired the Normans with confidence and courage, while

(iii) there is evidence that it struck terror into the hearts of the English; and

(iv) that William's victory was gained by only a narrow margin.

Some modern writers are good enough to allow that the comet may not have been without its influence; but surely we are entitled to go further and claim that the natural, obvious and only reasonable conclusion from these premises is that the comet of 1066 was a potent factor in determining the issue of that historic engagement, the Battle of Hastings.

What language can convey the full significance of this conclusion? For what does it mean? Let those who can picture to themselves the consequences to-day of William's *defeat* at Hastings. England, geographically, no doubt would stand where she does; but politically?—nationally?—socially? can any one venture a suggestion? And those Englishmen, whose name is legion, through whose veins courses Norman blood—would any and which of them have been born? These are far-reaching questions which the present visit of this comet has suggested, and are more than sufficient to invest that event with that peculiar and vital interest to all Normans to which allusion was made in the opening sentences.

As this is the first reappearance of Halley's comet since its identification as the comet which inspired and stimulated the Normans in 1066, is it not natural that their descendants in 1909 should hail its return? and should they not regard it with a feeling of almost reverent affection? For, apart and distinct from the immense responsibilities which I have laid at its door, it remains the one silent living witness which links them with those ancestors of theirs who nearly eight hundred and fifty years ago under William, Duke of Normandy, added a glorious chapter to history—not to the history of an empire, but to the history of the civilised world—a chapter entitled The Norman Conquest.

THE PRODUCTIVITY OF WOODLAND SOIL

By J. NISBET, D.ŒC.

In countries where the woodlands have been under more or less systematic management for generations, experience long since showed that the sowing or planting of new crops usually succeeds better on soil that has already been under a tree-crop, and that in sandy localities the growing and trenching-in of lupines is one of the best ways of restoring the exhausted productivity of fallowed portions of nurseries. Experience had also long shown that, as regards natural regeneration of mature timber-crops, the larger and the denser the mature crop, the greater the ease and success with which a new self-sown crop might be raised, owing to the protection thus afforded to the soil against deterioration by atmospheric influences and by a rank growth of herbage and weeds. Thus, while in agriculture large field-crops necessitate crop-rotation and manuring, in forestry close-grown and heavy timber-crops enrich the soil and add to its potential productivity for a following crop of the same species, without any crop-rotation being required in the agricultural sense.

Originally it was thought (Sprengel's theory) that leaf-mould was the chief source of carbon for plant-food; but the conclusion drawn about the middle of last century by chemists (Liebig and others) from these facts of experience was that the dead foliage annually shed by the trees gives back to the soil, in the form of humus or leaf-mould when decomposed, larger supplies of carbon, withdrawn from the air and assimilated and utilised for elaboration of the sap, than had previously been withdrawn from the soil along with the sap. And as the greater portion of the mineral salts contained in the plant-food are then also returned in the dead foliage, the decomposition of the leaves assists in preparing larger supplies of mineral salts in a soluble form, while the leaf-mould at the same time tends to improve the general quality of the soil by adding to its depth, by making clay less stiff and sand more cohesive, and by tending to improve the physical properties of the soil generally, whilst also protecting it mechanically. It was known, too, that humus played an important part in supplying the nitrogen required for forming the albuminoid substances necessary for plant-life; and it was believed that the small quantities of nitrogen usually found in woodland soil became during the leaf-decomposition transformed into ammonia, and subjected to a process of nitrification producing nitric acid.

About forty years ago the researches of Schloesing and Müntz drew attention to the fact that the soil was a laboratory in which myriads of living organisms prepared food for field and woodland crops; and since then the process of nitrification has been rendered more intelligible than previously was the case, though the discovery by Hellriegel and Wilfarth, in 1886, of the *Bacillus radicicola* forming nodules on the roots of leguminous plants, and rendering symbiotic aid in withdrawing nitrogen from the air circulating in the soil and in storing it up within these root-nodules in a form available for enriching the soil and increasing the supply of plant-food, when a sufficiency of nitrogen is not already present in the soil. Among forest trees the production of root-nodules is not confined to leguminous kinds like Robinia and Laburnum, but is also very highly characteristic of the Common and the White Alder, and the Sand-buckthorn shrub. Since Hellriegel and Wilfarth's discovery in 1886, the problem of the fixation of nitrogen in the soil has been occupying the careful attention of a large number of agricultural chemists in England, and amongst the most thorough investigators have been those whose names are so closely connected with the Rothamsted investigations—Lawes, Gilbert, Warrington, and Hall. The investigations made in Britain, however, only deal with purely agricultural and horticultural conditions, and as yet there are no laboratories where special investigations are being made with regard to woodland soil. At present, therefore, we must look for information concerning the productivity of woodland soil chiefly to Continental countries in which the forests form a great and carefully protected national asset; in particular to Germany, with its many well-equipped chairs of forest soil-science at Universities, and Forest Academies ranking as Universities.

As long ago as 1863 it was estimated by Krutzsch of Tharandt, that Beech, Spruce, and Pine required from 30 to 44 kilogrammes of nitrogen per hectare for foliage-production, and that 11 kilos. were utilised in wood-production, while the total amount of nitrogen contained in the atmospheric precipitations amounted only to 11 kilos., or merely enough for the wood-production. The deduction drawn from this was that the leaf-mould was, directly or indirectly, instrumental in providing the further supply needed; and that therefore, as long as the wood was kept close and the dead foliage retained on the soil, there would be no fear of the necessary supply of nitrogen running short. Further light was thrown on this subject by the researches of Frank, Möller, Wollny, Henry, and others, showing that on the roots of many woodland trees, and especially the Cupuliferæ and Coniferæ, a growth of fungi (*mycorhiza*) takes place, which enables the humus, and particularly the nitrogen, to be utilised as food not otherwise available. While the humus is in process of decomposition, carbon dioxide, water, ammonia, and free nitrogen are set free and assume forms making them available as plant-food.

According to the stage of decomposition at which it has arrived, the cast foliage in woodlands may be classed as (1) raw (non-decomposing) humus, (2) coarse (partially decomposed) leaf-mould, and (3) fine (decomposed) leaf-mould, the first being formed under unfavourable conditions, and the other two under favourable conditions—though all of them are formed under conditions different from those obtaining on unwooded moors or peat-bogs, where the dead portions of the plants growing there form a peaty layer of greater or less depth, owing to want of aeration in the soil. But it is now generally accepted as fully proved that the decomposition of dead foliage into leaf-mould is not purely a chemical, but a chemico-physiological process, in which micro-organisms play a very important part; for without their aid neither the withdrawal of carbon dioxide from the decomposing leaves, nor the formation of ammonia, nor nitrification, nor denitrification is possible. But the composition and the physical and chemical properties of humus are more or less dependent on the stage to which decomposition has advanced under the given conditions obtaining throughout the soil; and in course of time the humus gradually disappears on its organic substance being dissolved into carbon dioxide, water, and ammonia, transformable into nitric acid, while the ultimate mineral constituents are left in the soil in a form easily available as plant-food. Hence, under conditions favourable to complete decomposition, there is no danger of good leaf-mould being present in too large an amount. It is only when conditions are unfavourable to decomposition that such a danger exists, and that the presence of raw humus may be unfavourable to the growth of timber-crops.

About thirty or forty years ago Ebermayer's investigations showed the favourable influence which well-decomposed leaf-mould exerts as regards soil-moisture and soil-temperature, while more recent researches have been directed to ascertain its influence on the porosity or the cohesiveness of the soil-particles. Raw (non-decomposing) humus has been found to be always injurious in its action, while fine (decomposed) leaf-mould is always beneficial; and the finest mould is formed by beech-foliage, while a soil-covering of moss not underlain by raw humus is favourable both to porosity and to soil-moisture. But in Beech-woods from which the dead foliage is annually removed the porosity diminishes and the soil becomes hardened and less productive; and the researches of Prussian experimenters showed a loss in twenty-five years of 25 per cent. of the basal increment of the stems while the dead foliage was being annually removed from Beech-woods seventy to one hundred years old.

In a light, porous soil good leaf-mould tends both to retain the soil-moisture holding plant-food in solution, and to produce a larger supply of mineral food held in solution; but when large masses of raw humus are present in the woods, there is always a tendency towards the formation of a subsoil-pan or hard, impermeable layer, which acts unfavourably, as the acid solutions thus formed (especially under heather and on water-logged spots covered with moss) are injurious to the growth of timber-crops by bringing into solution compounds of iron that are both toxic in their effect and also tend to the formation of moorpan. The thicker the layer of raw humus, the greater is the production of these injurious acids (humic, ulmic, geic, etc.), which have themselves a strong affinity for the ammonia that might otherwise become utilised in the nourishment of the tree-crops.

The injurious result of removing dead foliage from woodlands is of course greater and more rapidly apparent on poor than on fertile soil. On poor land from which the foliage is taken annually, Beech-woods will become so open and backward in growth, and the soil will become so top-hardened and overgrown with unfavourable weeds, as to make natural regeneration impossible; while on better soil natural regeneration is made much more difficult, and less likely to be satisfactory as regards the young crop raised. The direct effect of the removal of dead foliage is less apparent in Pine or Spruce woods on good soil, but on poor soil it soon shows its effect in a very considerably diminished rate of growth. Its indirect effect is even greater, however, when such tracts are cleared and replanted; and the absence of humus altogether, as on old fields or pastures and poor waste lands, is even more marked; for, though the foliage of young four-year-old crops of Scots Pine only need from 3 to 8 kilos. of lime, potash, and phosphoric acid for the production of their foliage, yet they wilt through want of the nitrogen that can be supplied by humus. Such sickly plantations are then likely to be attacked by insects and fungoid diseases. On the other hand, of course, any mossy or peaty raw humus must hinder natural regeneration wherever it is so thick as to prevent the roots of seedlings from getting down into a layer that can supply it with the necessary plant-food; while any deep layer of such raw humus has an unfavourable physical influence on the development of the plants, even though the necessary substances for providing plant-food may be abundantly contained in such a soil—though not in a form making them available for imbibition by the root-system. That peaty raw humus can even, under changed conditions as to the soil, become of considerable manurial value has been shown by the successful results obtained in Prussia through manuring sandy nurseries for Scots Pine with peat.

The best method of ensuring a humus favourable to soil-productivity is to grow timber-crops in mixed woods consisting of kinds of trees suitable to the soil and situation, and capable of protecting the soil against the deteriorating effects of sun and wind; and in such mixed woods the Beech has on the Continent been found to possess the most valuable properties regarding the conservation and the increase of soil-productivity. Towards this end, too, caution should be exercised in making thinnings; and when light-demanding crops, like Oak, Larch, or

Pine, become open while maturing, they should be underplanted to protect the soil—for diminution of its productivity means diminution of its capital value, as well as a decrease in the annual return it is then capable of making through timber-production. And if it does not pay to underplant such a crop, it is worth considering whether it is not then better to realise the existing crop (even if not yet fully mature), and raise a fresh crop of a more profitable kind. In the Beech-woods that are about to be regenerated naturally, it is always advantageous to expedite the decomposition of the top layer of raw humus by tearing up the soil with strong roller-harrows, and thus preparing it also for the reception of the seed. Treated thus, the dead foliage is brought within better reach of the earth-worms, saprophytic fungi, and other organisms assisting in the process of decomposition. But in the replanting of areas covered with heathery raw humus, the heathery crust of about eight inches is broken through with a plough to mix the soil and the subsoil, or strips are cleared of heather and deep pits are bored with conical spades for planting.

A different problem, however, has to be solved when the question deals with the planting of poor waste lands, where there is a total absence of the good leaf-mould so favourable to soil-productivity in timber-growing. Even though wet or water-logged moorland be drained so as to render it plantable, yet the nitrogen it may contain may be inert and not available for the purposes of tree-growth owing to the absence of the micro-organisms abounding in soil fertilised by good leaf-mould. And it is probably also mainly due to this same reason—the want of any admixture of good humus—that Pine and Spruce are so apt, almost so certain, to become liable to fungous root-diseases when planted on fields thrown out of arable cultivation. Except in the case of trees like Robinia and Alders, which collect nitrogen in their root-nodules, there can be no sufficient supply of nitrogen without humus; and without an ample supply of nitrogen a soil cannot be so productive as it otherwise might be, judging merely from its mineral composition.

Strikingly successful results have been obtained on the heathery, sandy waste of Lüneburg (Prussia), both by fertilising the poor sand with peat, and by sowing 1 kilo. of Robinia seed per hectare along with 4 kilos. of Scots Pine and $1\frac{1}{2}$ of Spruce seed. Or else the Robinia is planted in advance, and the

Conifers are afterwards planted among it. And on poor, dry, limy soil thrown out of cultivation near Jena over thirty years ago, good coppices of White Alder have been raised, in which other trees, and chiefly Ash, Maple, and Sycamore, have begun to sow themselves spontaneously. The intermingling of White Alder along with Spruce (about one row in every six) on poor limy soil, or of sowing clover and other papilionaceous plants thriving on lime, effects a marked improvement on the growth of Spruce; while on sandy soil equally beneficial results are obtained by sowing perennial lupine. How such methods of improving the productivity of poor soil converted into woodland, and how far Continental experience has shown that artificial fertilisation of poor soil can be advantageously carried out by the addition of chemical manures, may perhaps form the subject of a subsequent article.

REVIEWS

The Story of the Comets. By G. F. Chambers. [Pp. xvi + 256.] (Oxford: Clarendon Press, 1909. Price 6*s.* net.)

This book appears at an opportune season, when general interest is excited by the return of the famous comet of Halley; and it fills a gap in popular astronomical literature, there being no work in English that confines itself to comets and deals with them so exhaustively. The author has drawn on the stores of his well-known *Handbook of Astronomy* both for text and illustrations; but the matter has been carefully brought up to date, including a beautiful series of photographs of the remarkable comet discovered by Morehouse last year. These show in a striking manner the complicated forms and rapid changes of comets' tails, and give an idea of the intensity of the forces that produce them.

After some general chapters on the physical appearance and tails of comets, and the method of their discovery, the author proceeds to a description of individual comets, dividing them into short-period, long-period, and remarkable comets. In the first category Encke's comet naturally takes the place of honour. Its earlier history is very fully told; but more might have been said about Backlund's remarkable researches on the variations in the rate of acceleration, and his suggestions as to a possible connection with the sunspot-curve. He also quotes with disapproval Miss A. M. Clerke's conjecture that this comet, owing to its frequent returns to the sun, was likely to wear out quickly; this has, however, been already apparently verified by the remarkable faintness of the object in 1908, though it has yet to be seen whether the loss of light is permanent. Brorsen's comet is mentioned as though it were still in existence; but, like Biela's, it is now looked on as a definitely lost comet.

Passing to the comets of long period, of which Halley's is the most famous, it will probably be news to many that there are five others of the same type and about the same period. One of these is Westphal's, found in 1852, for which a period of 60½ years was calculated (Mr. Chambers erroneously gives 67½), so that it is expected back in 1913. Another of them, that of Pons, was quite a conspicuous object in 1884; a drawing of it by Trépied is reproduced. Halley's comet naturally claims a chapter to itself, in which the story of the detection of its periodicity is once more told, and historical anecdotes recounted about the different returns back to B.C. 240, the earliest appearance that can be identified with reasonable confidence. Some account follows of the phenomena exhibited by the comet in 1835, and an anticipation of the circumstances of the present return, including an ephemeris computed by Dr. Smart. As the comet has now been detected by photography, an estimate can be made of the time when it will pass perihelion, which will be on or about April 20 next. As this is only 3½ days later than the date assumed by Dr. Smart, his ephemeris will not be much in error.

The next chapter deals with the remarkable comets of modern times, including the six-tailed one of 1744, and those of 1811, 1843, 1858, 1874, and 1882. There are numerous excellent illustrations, and the letterpress is interesting.

Then we have a chapter on the orbits of comets, in which these are explained in as simple language as the case permits; and, indeed, any one with an elementary knowledge of trigonometry should be able to follow the explanations.

The chapter on the spectroscopic study of comets, contributed by Messrs. E. W. Maunder and W. E. Rolston, deals fully with the methods and results, from the early visual observations to the recent introduction of the prismatic camera, which shows in such a convenient manner the chemical constitution of various parts of the comet. We quote the following paragraph:

"We must be content to look on the comet spectrum as radiation, probably produced by electrical action of some kind, from the particles of the comet itself. That the volatile gases of the carbon compounds should be the first to be excluded is not a matter of wonder, whilst the observation that when the comets attain to lesser distances from the sun, and therefore become more strongly heated both by the solar radiation and by the increased number of collisions among their own particles, sodium and iron are vaporised and rendered incandescent, is but another step in accordance with the law of continuity."

The relation of comets to meteors is discussed, and a chapter on comets in history and poetry will appeal to many readers.

Among several useful appendices is one in which the catalogue of comets in the author's *Handbook* is brought up to date, including the faint comet discovered by Borelly in June, 1909.

The Fundamental Principles of Chemistry. An Introduction to all Text-books of Chemistry. By WILHELM OSTWALD. Authorised Translation by HARRY W. MORSE. [Pp. xii + 349.] (London: Longmans, Green & Co., 1909. Price 7*s*. 6*d*. net.)

PROF. OSTWALD, in his latest volume, has turned his attention to the "Fundamental Principles of Chemistry," and has sought "to work out a chemistry in the form of a rational scientific system without bringing in the properties of individual substances." He describes the work in a sub-title as "An Introduction to all Text-books of Chemistry," but qualifies this in a preface, by adding that he does "not mean that the beginner should absorb the entire contents of this book before he learns about oxygen and chlorine as chemical individuals." He is quite of the opinion that a close personal acquaintance with a considerable number of important and characteristic substances is, and always must be, the fundament of all instruction in chemistry. But when this acquaintanceship has once been obtained, it can be nothing but an advantage to the student to point out to him the great connections by which these separate facts are bound together into a unit. This qualification reflects accurately the character of the book. It is emphatically not a book for the elementary student, and could only be recommended with advantage as a real introduction to the science in the case of a scholar (if such there be) who had mastered the allied subject of physics and wished at a mature age to enter upon the study of the sister science. Only for such a type of beginner would the earlier paragraphs and definitions possess a real and useful significance.

The method of treatment adopted has certain obvious limitations, the most serious of which arise from the fact that it is only in certain branches of chemistry that it is possible to discuss general questions without referring to the properties, and even to the idiosyncrasies, of individual elements and compounds. Thus, while two hundred pages are devoted to the questions of equilibrium which are usually dealt with as illustrations of the "phase rule," the whole of organic chemistry is dismissed in ten pages, and the fundamental principles illustrated by the tetrahedral formula for the carbon atom are entirely passed over. So, too, the periodic system, which constitutes the crowning generalisation of inorganic chemistry, is set aside because it cannot be explained without reference to the properties of individual elements. The hypothetical "beginner" would, therefore, be in danger of getting a very restricted and erroneous view of the science of chemistry if his studies were confined to this volume, and would need to recognise clearly that only a single aspect is presented here.

The essential merit of the book is, however, set forth in the paragraphs which describe its value to the teacher, and through him—by an *indirect* method of transmission—to the student. "Questions concerning fundamental principles meet the teacher at every step, and the mental character of the developing chemist is frequently determined by the way in which they are answered." "Generalisations are the fundamental base of the chemical symphony, and the various separate parts may be varied according to need or desire." "The book . . . may serve to show him how such generalisations are to be handled and how they can be woven into his daily instruction in elementary chemistry." It is in this direction that the author is likely to find recompense for the labour involved in his novel undertaking, and many a teacher will find mental recreation, whilst his students reap substantial benefits, from a perusal of the eleven chapters of which the volume is composed. Attention may be directed specially to the careful distinction between quantities and intensities in Chapter I., to the inclusion of glassy solids and liquid crystals in discussing the "States of Matter" in Chapter II., and to the ingenious classification of mixtures in Chapter III., where mud, clay, fog, and foam appear in due order as examples of liquid-solid and liquid-gas mixtures. The succeeding chapters on "Change of State" and on "Solutions" proceed along familiar lines, and the long Chapter VI. on "Elements and Compounds" is in part familiar to English readers as the author's Faraday lecture to the Chemical Society in 1906. Chapter VIII. deals with "Colligative Properties," Chapter IX. with "Reaction-Velocity and Equilibrium," Chapter X. with "Isomerism" and Chapter XI. with "The Ions." The last chapter is brightened by a definition of a salt as "a substance which has the properties of a pure substance in the undissolved condition, while it exhibits the properties of two substances while it is in solution."

The translation from the German has been admirably carried out, so much so that it is difficult to resist a feeling that the English is an original edition of the author's work. Two unimportant misprints appear on p. 28, line 19, and p. 34, line 28, but the book is excellently presented, and will be read with considerable enjoyment by many who have found but little inspiration in the catalogue of detailed properties of individual elements and compounds which bulks so largely in the routine of chemical instruction.

T. MARTIN LOWRY.

The Vegetable Proteins. By THOMAS B. OSBORNE, PH.D. [Pp. xiii + 125.] (London: Longmans, Green & Co., 1909. Price 3*s*. 6*d*. net.)

THIS last addition to the valuable series of biochemical monographs which Messrs. Longmans are issuing under the editorship of Drs. Aders Plimmer and Hopkins is particularly welcome. It is really the first systematic attempt to deal with an important branch of biochemistry in book form. No one could have been better qualified to fill this gap in scientific literature than Dr. T. B. Osborne, the pioneer in vegetable chemistry, especially as his research work has been so largely occupied with the investigation of the vegetable proteins.

The book, though short, is a veritable mine of information, and the very complete bibliography provided enhances its value to the original investigator and to the senior student. It is dry, solid reading, and so hardly the kind of book one would take as a solace on a railway journey; its proper place is the laboratory, and as a work of reference.

The protein cycle starts in the vegetable cell; for here it is that the complex albuminous molecule is first built up from simple compounds. Animal protein, about which more is known, is secondarily formed from vegetable protein or from its cleavage products. Whether the chemist in his attempts to unravel the secrets of the protein molecule should follow Nature's order is questionable; at any rate that has not hitherto been the method chemists have decided to follow, for curiously enough the simplest members of the protein family, and those therefore most likely to be correctly analysed and successfully synthesised, are not those found in the vegetable world.

The vegetable proteins differ in many particulars from their cousins in the animal cell, but they do not appear to be any less complex than the most complex members of the animal group. These differences are set forth with full details in Dr. Osborne's book, and one can confidently recommend it to all interested in this most important subject.

W. D. HALLIBURTON.

An Introduction to the Geology of Cape Colony. By A. W. ROGERS, D.SC., F.G.S., and A. L. DU TOIT, B.A., F.G.S., of the Geological Survey of Cape Colony, with a chapter on the Fossil Reptiles of the Karroo Formation, by PROF. R. BROOM, M.D., B.SC., C.M.Z.S., of Victoria College, Stellenbosch. [Pp. xii + 491.] (London: Longmans, Green & Co., 1909. Price 9*s*. net.)

THE authors may be congratulated on the production of a greatly improved edition of a book that was of excellent quality in its original form. The recent important work of the Survey on the præ-Cape rocks of the north of the Colony have enabled them to give, at least in this region, a connected account of a long succession of ancient, practically unfossiliferous formations, interrupted at different points by well-marked discordances in the stratification, one of which is believed to include the entire period of the deposition of the great Witwatersrand formation of the Transvaal, as well as of its removal, if it were ever laid down in this area.

It is of interest to find at different horizons in these old rocks important beds of limestone containing, as a rule, a large percentage of magnesia. These exhibit at times oolitic structure, but yield only indistinct and unrecognisable traces of organisms.[1] If, as the authors believe, they were laid down below the sea, they furnish the only evidence that a large portion of South Africa has ever been subjected to marine conditions.

In the Griqua Town Series, which overlies the blue dolomitic limestones of the

[1] They closely resemble an unfossiliferous limestone found at Corumbá and elsewhere in Matto Grosso, Brazil.

Campbell Rand Series, beds of unmistakably glacial character are met with which may be provisionally correlated with the præ-Cambrian glaciation in the Gaisa Series of the Varanger Fiord in Northern Norway, while similar deposits have been described from China and South Australia, which would seem from their position to be also of præ-Cambrian age. The Lower Huronian glaciation of Cobalt and other localties in Northern Ontario would, if its stratigraphical position be correctly determined, be in all probability of still greater antiquity than any of these.

The Matsap Series, the latest of the old unfossiliferous formations of northern Cape Colony, is identified with the Waterberg Sandstone of the Transvaal. It appears to resemble in some respects the Torridonian, and was probably, like it, a continental formation formed to a large extent of sands and conglomerates laid down by streams from mountain slopes. The authors do not accept the correlation of the Waterberg and Matsap Beds with the Table Mountain Sandstone in the south. They believe them to be much older, and thus throw back the unfossiliferous formations of the northern area to a still higher antiquity than that with which they are usually credited.

The Table Mountain Sandstone Series itself is usually considered to be Devonian, since it is overlaid conformably by the Bokkeveld Series, which may be placed at or near the base of the Middle Devonian. The "sudden and exact" (to use the authors' expression) line of division between the sandstones of the former and shales of the latter may, however, represent a considerable lapse of time. It is unfortunate that the only fossils yet discovered in the Table Mountain Sandstone Series, some lamellibranchs in micaceous sandy shales at the base, should be so badly preserved as to be undeterminable. The most remarkable episode in the deposition of these beds, which the authors regard as of fluviatile origin, is represented by the intercalation of some 300 ft. of shales, the lower third of which consists of greenish blue or reddish mudstone without lamination containing scattered pebbles and boulders with flattened and striated surfaces, indicating a second period of intense cold in South Africa. It may be suggested that they represent a period when the waters of the rivers were dammed back by the accumulation of glacier ice to the southward, so that great lakes were formed in much the same way as happened in North America in the Pleistocene glaciation.

The Lower Bokkeveld Beds afford the first unmistakable evidence of the presence of the sea in South Africa, but the marine forms of life that characterised them soon disappeared, and it is probable that for a prolonged period, represented by the Upper Bokkeveld, Witteberg, and the greater part of the Karoo rocks, characterised by the frequent occurrence of remains of terrestrial plants, the geographical conditions in Cape Colony were akin to those that now prevail on the northern shores of Asia, where the gradual recession of the ocean has laid bare a wide tract of low-lying country on which alluvial desposits containing a great quantity of vegetable material are slowly accumulating.[1] The special character of these deposits is usually attributed to the melting of the upper course of the rivers, while the lower portions are still frozen. This may also have been the case in South Africa during a large portion of the period above referred to, but the incoming of the Karroo is marked by evidence of the prevalence of much more

[1] The frequent occurrence of reptile remains in the Karroo Beds may be compared to the numerous entombments of mammoths in the swamps of Northern Siberia.

intense cold, the third distinctly glacial epoch in the long record of the South African rocks—an epoch that is generally believed to be contemporaneous with the glaciation that affected India, New South Wales, Argentina, and the Falkland Islands in late Carboniferous or early Permian times.

Not the least interesting chapter is that on "Tertiary and Recent Deposits," for it is concerned with problems of denudation and accumulation under conditions of a scanty rainfall, which may be paralleled more than once in the geological record of the British Isles.

The authors attribute the enormous denudation that has taken place in South Africa in different geological periods mainly to fluviatile and glacial action, and in spite of the fact that they recognise the work of the wind in local erosion and in the formation of *drei kanters* in the Table Mountain Sandstone, they do not seem to entertain the possibility that it may have been by far the most effective agent in the removal of rock in South Africa, as it certainly has been in many parts of the world, not excluding the United Kingdom, where plains of æolian erosion have probably been of greater importance than those of marine denudation.

Very curious is the occurrence of salt-pans, not only near the coast, but in a tract from the north of Calvinia, along the outcrop of the Dwyka Series, north-eastward to Kimberley and Boshof, as well as on the same beds in Gordonia to the north-west; but they are not confined to this formation, either in Cape Colony or in the Transvaal. The authors believe that they have been excavated through the agency of the prevailing northerly or north-westerly winds, which form accumulations of the resulting sand on the southern and south-eastern margin of the pan, while the north-western rim is in some cases a cliff capped with the calcareous tufa which covers the country. It is remarkable that very often round the edge of a salt-pan, and occasionally even within it, fresh water can be obtained by digging shallow pits. The authors believe that the salt may be derived from the Dwyka formation, but Sir Thomas Holland has shown that the salt of the Sambhar Lake in Rājputana has been blown in the form of impalpable dust more than four hundred miles across the desert from the Arabian Sea by the south-west monsoon winds. There seems no reason why a similar explanation should not hold for the salt-pans of South Africa, though the distance to be traversed by the north-western winds from the Atlantic would be in some instances nearly eight hundred miles.

It is to be regretted that the word laterite is apparently used in the popular sense of superficial hydrated ferruginous deposits, instead of that of a decomposition product of aluminium-bearing silicates, which contains free aluminium hydrate. The latter definition includes, not only the original material to which Buchanan first applied the name more than a century ago, but also what is ordinarily known as laterite in India at the present time. The word has been definitely accepted in this sense in Germany and France.

The book is full of instructive matter for geologists of all countries, and it is not its least merit that it will enable every one to read with keen interest and full understanding the reports of the Cape Geological Commission, some of the most valuable contributions to the geological literature of the present time.

The map is remarkably clear and detailed, but it may be suggested that another on the same scale, showing the orographical features of the country and, if possible, the axes of the principal stratigraphical folds, would prove very useful for comparison. It may be mentioned that in the section on p. 118 the letters E. and W. are transposed, and the direction of that on p. 17 is not stated.

John W. Evans.

Agriculture in the Tropics. By J. C. Willis, M.A., Sc.D. [Pp. xviii + 222.] (Cambridge: University Press, 1909. Price 7*s*. 6*d*. net.)

In dealing with the application and usefulness of this book, it is essential to recognise, as the author carefully points out, that "no attempt has been made to write a book for the practical man to use in connection with his field work. The effort has been to produce a book that may be helpful and thought-stimulating to the student, the administrator or the traveller." Perhaps it would have been well if this special line of treatment had been more completely indicated in the title of the book; for there are two dangers apparent—that the practical man may turn to it for information it does not profess to afford, and that the student, administrator or traveller may not realise the extent to which the book may prove of interest and value to him.

The opinion may safely be expressed that the book is likely to prove a useful and important one, provided that it can secure the attention of the class to whom it appeals. Those engaged in organising agricultural enterprises in the tropics, particularly those whose duties are associated with Botanic Gardens and Experiment Stations and similar efforts connected with Government organisations or the organisations of powerful bodies of associated planters, will be grateful to Dr. Willis for his work.

The tendency is to discuss agricultural problems in a broad, philosophic spirit; questions relating to land and soil, labour, cultivation, finance, education, and kindred matters are dealt with in this manner.

With regard to the crops which constitute the principal cultivations of the tropics, the plan is to discuss first the general history of each crop in relation to the countries where it is chiefly cultivated, to give some statistics indicating its importance and extent, followed by a very brief outline of the normal methods of cultivation and the diseases to which the crop is liable; finally, there are put forward suggestions as to the probable future of the industry, and the lines of thought and action which may be followed to advantage in efforts to improve, develop, or extend the industry in question. While the practical details of cultivating, preparing, and marketing are given only in outline, the suggestions bearing on the various lines of policy calculated to improve or develop each industry are likely to be of considerable interest.

Space does not permit of a critical review of the author's views concerning each individual crop; from the circumstances of his surroundings there is naturally considerable prominence given to matters relating to the Eastern tropics, and particularly to Ceylon, though his range of inquiry covers, in most cases, tropical countries as a whole. Were it not that the desire is to draw particular attention to the broad administrative problems connected with tropical agriculture, and the danger that a discussion of details might obscure this, we might be disappointed that more detailed information is not given concerning each crop—that the treatment is scanty may be judged, for example, from the fact that the three crops coffee, cacao, and kola are dealt with in less than ten pages.

In the early days of European interest in tropical agriculture, efforts were principally directed towards the introduction of new crops, and interesting and important are the results achieved. These efforts necessitated the maintenance of botanic gardens and kindred institutions, which have eventually exerted a most pronounced and beneficial influence on the development of tropical agriculture throughout the world; and it is significant that Dr. Willis can write with conviction that, "without the aid of the Botanic Gardens, Ceylon would have remained a small and unimportant native possession." Other similar instances of usefulness would not be difficult to specify.

This phase of the work is, however, rapidly lessening in importance, for it is obvious that, as time goes on, the introduction and acclimatisation of foreign products in any one colony or possession must decrease in importance, for the simple reason that most of the new products that can possibly be brought in are now introduced and the chance of finding anything of great value becomes less every year. Thus, during the last twenty years the Ceylon Gardens have not been able to introduce anything of much value, though they may have been able to bring in a few minor fruits, shade trees, and other things ; and during the last ten years a great change has come over the organisation of the establishment, which has expanded into a Department of Agriculture to suit the changed needs of the colony.

This gives us the keynote of the situation—the change from efforts to introduce new crops to efforts to organise the methods of dealing with those now established, though it must be remembered that there is no relaxation of efforts in the former direction when favourable opportunities occur.

The various problems which arise under this changed order of things are well stated by Dr. Willis, and all who are in any way responsible for, or associated with, this work will be helped and stimulated by a careful study of the book. Particularly may this be commended to the attention of those in administrative capacities in tropical colonies, for it is evident in many cases that an intelligent comprehension of the agricultural needs of a colony is essential to good government and wise administration. The chapters on organisation and policy are full of suggestions of the highest import, in a form calculated to be of very great service to Government officers throughout the tropics. A careful study of the problems and the suggested lines of treatment will do much to secure that breadth of view and that continuity of policy so essential for the success of the many efforts at present in formative stage, and so capable of being made or marred by administrative officers in whose hands they lie.

Officers in charge of, or associated with, Departments of Agriculture, Botanic Gardens, or Experiment Stations, will find much that will prove of assistance in their work. Part I. will make it plain to them that the agricultural methods that are employed in old tropical countries are the outcome of long experience, and that, although conditions are arising, and have arisen, which necessitate the making of changes, these changes must take place gradually ; while those who are stimulating agricultural progress, and are guiding it into new channels, cannot afford to ignore the lessons that may be taught by the methods that are displaced. By Part II. their appreciation of the extent and variety of tropical cultivations will be increased, and they may be led to do more in the matter of experimental trials with new crops. Though the subject-matter in Part III. has its chief application to tropical countries where agricultural methods, however primitive, have been in vogue for ages, chapters iv. and v. contain material which will be found useful under any and every condition. Lastly, in Part IV. they will find information which will bring them into sympathy with the aims and objects of those who direct their energies, and will enable them to see the way in which their more particularised work can be directed so as to make those aims thoroughly efficient.

To the planter, as well, who is recognising more and more the desire for his co-operative aid on the part of those who have charge of the administrative work of his country, and who knows the great value of such co-operation, a large portion of the book has a direct appeal. He will see, from Parts I. and IV., why it is necessary for him to take a broad view in relation to official agricultural effort; not to be obsessed by the recognition of the more immediate needs of the industry in which he is engaged, but to see that the amelioration of agricultural conditions in a country is a matter of well-directed general policy rather than of temporary and particularised alleviation of hardships. Part II. cannot fail to be interesting to him, and to widen his view as to the diversity of planting interests in the tropics. Finally, from Part III. he will see that his interests and those of the peasant are not antagonistic, and this is the more so in proportion as he is willing to help to improve the latter's position, both in regard to the agricultural methods which he will employ and to his ability, by means of the right kind of education, to see where his best interests lie.

FRANCIS WATTS.

The Making of Species. By DOUGLAS DEWAR and FRANK FINN. [Pp. xix+400.] (London: John Lane, 1909. Price 7*s*. 6*d*. net.)

THIS is another book on Darwinism. A book of this kind to be really valuable must contain either a great many new observations of actual facts, or must be the result of the activity of a far-seeing mind. We cannot bring ourselves to think that there was an urgent need for the book before us. Many of the observations made by the authors have already appeared in two other works: *Ornithological and Other Oddities* by Frank Finn, and *Birds of the Plains* and *Bombay Ducks* by Douglas Dewar. Our sympathy with the observation of living animals is so great, that we cannot but regret that such competent observers as Mr. Dewar and Mr. Finn thought of writing a book about evolution. Many of their general opinions are of great value: such, for instance, as their insistence on the importance of not confounding inference with fact. We cannot think that the qualities possessed by the authors, admirable and indispensable ones in their own sphere, justify them in launching another book on evolution on the market. We are only able to express an opinion on the general philosophical value of the book, for we are not ornithologists, and all the pictures seem to be of birds. No one can realise more than we do the importance of the study of living things by the biologist, whose business is, after all, the elucidation of the nature of living things, and whose practice has been too long limited to the investigation of the structure of carcases.

The ornithological information imparted by the book has, we believe, already been criticised in *Nature*, and we refer the reader for information on this point to a recent number of that journal; but the study of those parts of the book which relate to general questions has not led us to form a very high opinion of the critical faculty or of the philosophical insight of the authors. So many of their statements in the first chapter give the impression of skimming the surface of things, and of not so much failing to get into relation with actuality itself, but of, what is much worse, failing to realise that they are not in this close relation. For instance, "it seems to us that a fatal objection to all these Neo-Lamarckian theories of evolution is that they are based on the assumption that acquired characters are inherited, whereas all the evidence goes to show that such things are not inherited." Any one who uses the expression "the inheritance of acquired characters" without first an apology, and secondly a precise statement as to the exact thesis which this expression is intended to convey, runs great risk of losing the reader's confidence straight away. But that the authors of this book are not in touch with modern theories which most deeply concern the fundamental nature of the process of evolution is evident from the following statement: "For some time after the publication of the *Origin of Species*, Mivart appears to be almost the only man of consequence fully alive to the weak points of the Darwinian theory; the great majority seem to have been dazzled by its brilliancy." What about Samuel Butler?

The authors may reply to this criticism that speculative theories, such as those which are associated chiefly with the names of Samuel Butler, Hans Driesch, Richard Semon, Henri Bergson and others, do not fall within their province. To this objection we should reply that no modern book dealing with evolution can be complete without them; and this brings us again to our original point, namely, that it seems a pity that the extraordinary amount of valuable observation of which these authors are evidently capable should have been curtailed by the expenditure of time and energy which the production of this book must have cost. Again, any modern book on evolution, the authors of which fail to see the significance of the attitude of the biometrical workers to vital phenomena cannot pretend to completeness. There is no reference in the book to the writings of Weldon, and the only reference to the other great exponent of this attitude is contained in the following sentence: "The only Zoologists who had investigated experimentally the question of sexual selection appear to be Karl Pearson and Frank Finn."

Mendel's Principles of Heredity. By W. BATESON, M.A., F.R.S., V.M.H. [Pp. xiv + 396.] (Cambridge University Press, 1909. Price 12*s.* 6*d.* net.)

THE title which Prof. Bateson has given to his latest book cannot be considered to be very appropriate: in the first place it is the same as that of the essay which he published in 1902, although the present work is not merely a second edition of the earlier; in the second place this book is rather a survey of the results of Mendelian investigation up to the present time than a mere exposition of the principles originally formulated by Mendel. It is a mistake not uncommonly made by those who have only a superficial acquaintance with the subject to suppose that Mendelism is limited to the results reached by Mendel or to results of exactly the same kind, which is no more correct than to suppose that modern Lamarckism is identical with the doctrines of Lamarck. The importance of any new doctrine in Biology is to be judged, not by the particular facts on which it was founded or by the exact terms in which it was first expressed, but by its fruitfulness, by the progress to which it leads, the new fields of investigation which it opens up, and the new light it throws on old problems. No one can study Prof. Bateson's book without being impressed with the efficiency of the method and theory of Mendel as means of investigation, the fascinating interest of many of the results obtained, and the promise of still more important discoveries in the future. Those, however, who expect to find in this book new light on the main problems of evolution will be to some extent disappointed. The author tells us that in his original plan it was intended to discuss somewhat fully the bearing of the new facts on general problems, but this intention was abandoned and the book is devoted chiefly to the discussion of the concrete facts. He hopes to publish separately at some future time the lectures he gave at Yale University in 1907, in which he considered the relation of Mendelism to evolution. As far as can be judged from the few remarks in his Introduction, his view is that the only important problem is the problem of species, that Darwin's solution of that problem has proved a failure, and that for the present we do well to confine our attention to the immediate problems of genetic physiology. Indications, however, are not wanting that in thus concentrating his attention Prof. Bateson is running the risk of misunderstanding other phenomena with which his researches bring him into contact.

The essential phenomenon of Mendelism is the segregation of characters in the offspring of hybrids or crosses, a segregation which must be due to the segregation in the gametes of the elements that determine the characters. These elements are therefore regarded as units which may be combined but cannot be divided. When a pair of characters are united one may be dominant, but dominance is not necessary or universal. Modern investigations have gone far beyond the simple cases studied by Mendel, and the mere assumption of units in the gametes corresponding to the visible characters has been found insufficient to explain the results. It has been found necessary to invent purely hypothetical units of whose real nature nothing is known, and which are mere names for elemental factors whose existence is inferred from their visible combinations. Although dominance is not universal, the evidence leads to the general conclusion that allelomorphic characters differ only in the presence or absence of some single factor. For example, the rose comb in fowls is allelomorphic and dominant to the single comb; the interpretation being that the rose comb is due to the presence of a factor which is absent from the single. In segregation the present factor segregates from the absent. By the aid of this method the remarkable results of crossing the different types of combs are elucidated. When pure rose and pure pea are crossed, the type known as walnut is produced; when these are bred together four types are produced, namely, walnut, rose, pea, and single. The extraordinary thing here is the appearance of single when no single occurred in the parentage. The explanation adopted is that the pure rose contains the recessive p, that is, the absence of pea, and the pure pea the absence of rose. When the two absences meet in fertilisation the result is single. Although the theoretical results given by this scheme agree with the observed results, it must be evident to any one but a Mendelian that the explanation is a mere juggling with words and symbols that have no real meaning. In the terms rose and absence of rose, obviously absence of rose means single comb; in fact, the author explicitly says so: "The rose comb is a single comb modified by the presence of R." Similarly, absence of pea means the same thing, namely, single comb. Yet for the purpose of the formula, absence of rose and absence of pea are treated as two different factors. The rose used in the experiment breeds true and never yields single, but for the purpose of the formula it is supposed to contain the recessive p or absence of pea, which separates in the breeding of the hybrid walnut. Either p means nothing at all or it means the factor for single comb, and therefore the rose itself ought to segregate and give single, which it does not. According to Prof. Bateson, the absence of rose is one thing, the absence of pea is another, but absences by themselves are nothing. In mathematics it is legitimate to deal with negative quantities and to add together $-a$ and $-b$, but Prof. Bateson deals with zeros and makes $a-a$ one quantity and $b-b$ another; it might be maintained that absence of rose could mean pea comb as well as single, but then the pea comb would be merely PP instead of Pr, and this would not improve matters.

A great part of the book deals with the heredity of colour characters, and in these chapters we find some extremely interesting results, especially with regard to latent factors. The investigations show in a striking way how different may be the visible characters of the individual and the qualities which it transmits; they carry us far indeed from the old idea that heredity was the transmission of a visible character from parent to offspring. We find that albinos may carry a factor for black which shows itself only when united with another factor for colour. In flowers two whites carrying invisible colour factors may, when crossed, produce a coloured flower: two white sweet peas, for example, differing only in the shapes of their pollen grains, produced, when crossed, a heterozygote with purple flowers like the original *Lathyrus odoratus*, and in F_2 gave three kinds of flowers, purples, reds, and whites. To explain these colours and their numerical proportions three pairs of allelomorphs are assumed, two of which are factors that must occur together to produce any colour at all. Various other factors produce subordinate types. Thus we arrive at the Mendelian interpretation of reversion on crossing, and of variation, or certain cases of variation, in cultivated organisms. Reversion is the union of factors occurring separately in the types crossed, and variation is the omission or addition of a factor. With regard to eye-colour in man and the colours of race-horses, comparisons are made between the Mendelian and biometric methods, much to the advantage of the former. It is obvious that the units of eye-colour required by the biometric method are very difficult to determine, while the Mendelian units, on the other hand, have sometimes the disadvantage of being purely theoretical.

The most unconvincing chapters are those which deal with heredity and sex. Prof. Bateson's treatment of this subject fails because he has not taken into account what is known of sexual dimorphism in nature. He quotes the results of crossing sheep horned in both sexes with another breed hornless in both sexes, the hybrids being horned in the males, hornless in the females. The author merely compares this result with other cases of sex-limited inheritance, ignoring the fact that in the ancestral sheep the females were hornless or nearly so. The discussion of colour-blindness in man is left in complete obscurity. After a number of definite statements and a diagrammatic scheme of the heredity in the original text, a half-page of corrections has been inserted in the volume which cancels all the principal conclusions reached: colour-blind men, we are told, do not have colour-blind sons, so that the descent in this case cannot at present be explained on Mendelian principles. Several other cases of human abnormalities are discussed, and it is shown that many of them behave as dominants and are transmitted in accordance with Mendelian laws.

An impartial account is given of the few cases in which the evidence indicates that no segregation takes place, as in the cross between negro and European. One most remarkable case in which the absence of segregation was observed by Mendel himself, and puzzled him completely, is that of *Hieracium*. It has now been discovered that this plant and several others have the power of producing seeds parthenogenetically, and as no reduction in the nuclei of the gametes takes place there can naturally be no segregation.

Although as mentioned at the beginning of this review, Prof. Bateson does not profess to discuss fully the relation of Mendelism to evolution, he has a short chapter on Biological conceptions in the light of Mendelian discoveries. He suggests that the something which is present in a dominant character may be of the nature of a ferment, but that it is not the ferment which is present in the gamete, but the power to develop the ferment. He does not think, in spite of the apparent correspondence between reduction divisions and segregation, that the chromosomes are the sole effective instruments in heredity. He points out that it follows from Mendelian results that characters, or rather the elements of characters, are indivisible units, and that variation therefore is either the addition or omission of one or more factors. Of the causes of this addition or omission we are still ignorant, but one question arises which Prof. Bateson does not discuss, namely, whether all additional factors are derived from an original unchanging stock. This conclusion would seem to follow from his argument that variation is the consequence of asymmetrical divisions in the germ-cells. It is with his brief remarks on Adaptation that the present writer most emphatically disagrees. He asserts that each new character is formed in some germ-cell of some individual at a particular time. If such a character is beneficial or injurious it will survive or disappear, but that the occurrence of the variation is guided ever so little by the needs of adaptation there is not the smallest sign. It will be time to consider this assertion when Prof. Bateson or any Mendelian has investigated the heredity of any one adaptive character. At present they are unaware of the difference between adaptive and specific characters; in fact they do not understand adaptation. The problem of species which they investigate has nothing to do with, or at least is entirely distinct from, the problem of adaptation.

It must not be supposed from the above criticisms that the present writer fails to appreciate the value of Prof. Bateson's book, which must be acknowledged to be the most important that has appeared for a long time in relation to general Biology. Every one interested in the general problems of the science must be sincerely grateful for such an excellent exposition and survey of the Mendelian doctrine of heredity, the value of which is increased by the full bibliography. No one can study evolution or heredity now without a knowledge of Mendelism, and this book will doubtless take its place among the classics of biological literature.

J. T. CUNNINGHAM.

Outlines of Bacteriology (Technical and Agricultural). By DAVID ELLIS, PH.D., D.SC., Lecturer in Bacteriology and Botany to the Glasgow and West of Scotland Technical College, Glasgow. [Pp. xii + 262.] (London: Longmans, Green & Co., 1909. Price 7*s*. 6*d*. net.)

IN the Preface it is stated that this book is intended to serve as an introduction to Bacteriology in all its branches, though more attention has been bestowed on that aspect of the subject which deals with technical and agricultural bacteriology.

The scheme is an ambitious one, but nowhere are more than the rudiments of the subject dealt with, except perhaps in the first five chapters in which the morphology and general biology of the bacteria are adequately considered. We doubt, therefore, if the *student* will find sufficient for his needs unless he desire merely a superficial survey of the subject.

After a general biological introduction to the bacteria and certain higher forms, chapters are devoted to sterilisation, the bacteria pathogenic to animals and plants, thermogenic, chromogenic, photogenic, and sulphur and iron bacteria, the preservation of foods, nitrification, fermentation and its industrial applications, and lastly sewage and its disposal.

The plan of the book and its sectional contents have been well conceived, but the author would have been well advised to have had some assistance in revision, for a number of errors and slips mar the general utility of the book.

In dealing with disinfectants the old exaggerated views as to the germicidal efficiency of dilute solutions of corrosive sublimate are retained—in fact the figures given for the disinfecting power of disinfectants require complete revision. No mention is made of the action of organic matter in reducing the efficiency of permanganate and chlorine. Ozone is stated to be of no practical value, yet several towns and cities have their water sterilised with it. As regards boracic acid, it is stated that "bandages for wounds are usually sterilised by soaking" in this substance and that "it *has* been employed for preserving tinned meats, milk, etc." (the italic is ours).

Again, the section on pathogenic bacteria contains many erroneous and loose statements, *e.g.* that white rats are susceptible to anthrax, that anthrax bacilli from animal excretions "will probably form spores if they do not at once find a nutritive medium," that mallein is used to protect animals against glanders, whereas it is employed solely for *diagnostic* purposes, and that the *Staphylococcus pyogenes aureus* can withstand half an hour's exposure at 80° C. without injury. All the diagrams of the organisms in this section are on the same scale, and thus *B. tetani*, the cholera spirillum, *B. tuberculosis*, *B. anthracis*, and *B. influenzæ* appear about the same size, and the figures give no indication of some of the finer and more characteristic details in the microscopical appearances of these organisms.

One of the best sections in the book is that on nitrification, but it is not free from error.

In the section on the yeasts no mention is made of ascospores and their importance in the analysis of yeast.

Various other errors could be pointed out in the concluding portion of the book.

The book is written in an interesting manner and is well printed and illustrated, and if expanded somewhat and subjected to thorough revision would fill a useful place among text-books on bacteriology.

R. T. HEWLETT.

Immunity and Specific Therapy. By W. D'ESTE EMERY, M.D., B.Sc., Lond. With Illustrations. [Pp. xiv. + 448.] (London, H. K. Lewis, 1909. Price 12*s.* 6*d.*)

UNDER this title Dr. Emery has written, in the words of his Preface, "a connected and symmetrical outline of the chief facts definitely known with regard to the method in which the body protects itself against infections, and of their applications in the diagnosis, prevention, and treatment of disease." It is essentially a book for those interested in advanced work on the subject. The author follows no particular school, and gives full prominence to the difficulties in the way of the accepted views; indeed it requires a book like this to demonstrate how complex and how little understood are the phenomena of immunity. This work is probably the most complete survey of the present knowledge of the subject which is published in English in reasonably small compass. The chief theories are stated clearly and at length, and references are given where conclusions are cited without full discussion.

After an opening chapter which is fresh and suggestive, the problems of the book are approached according to the following scheme. The first subject discussed is that of toxins, the nature of which is described in an admirably clear chapter, followed by an account of the technique of antitoxin preparation. The interreactions of toxin and antitoxin come next, and make very hard reading ; the chapter leaves a sense of considerable confusion, for none of the many theories satisfactorily explains the observed phenomena. The "side-chain theory" is well summarised, and employed to explain the origin of antitoxin, and immunity to toxins.

The problems of immunity to bacteria as distinct from immunity to toxins are then treated in a very full chapter in which the side-chain theory is given in greater detail. A section on cytolysis, and chapters on the agglutinins and precipitins complete this aspect of the subject.

A long chapter is devoted to phagocytosis, and though the author adopts no partisan view, there is strong internal evidence both here and elsewhere that he has a high opinion of the functions of the leucocyte in all phenomena of immunity. The "tuberculin" test and its modifications is discussed, along with the difficult problem of anaphylaxis, and other highly theoretical matters such as the colloidal theory of antibodies and immunity to bacteria. The book closes with an account of practical applications of these views in diagnosis, and in prophylaxis and treatment by means of vaccines and sera. Excellent additions to the subject-matter are a good glossary and a very well selected bibliography.

There is no criticism to be offered of the subject-matter of this volume. It has the defects of all books which aim at an impartial record of varying views ; the reader comes away from controversial subjects without clear convictions, with no working hypothesis. But as a summary of the important conclusions of different schools it forms a most valuable handbook. The text is mercifully free from the multiplicity of proper names so frequently found in summarised work, only the most important being given, and the book is not overburdened with the history of early research. The order chosen is one in which the subjects are developed in a satisfactory sequence. Some observers will not agree with the author as to all the doses of vaccines suggested, and some will be aware of results which are better than those quoted, as, for instance, in the treatment of tuberculosis.

A study of this book enforces recognition of the unfortunate complexity of the terms used in this difficult subject. For instance, in one passage in this book the word "haemolysin" occurs, signifying a substance which dissolves blood-corpuscles. In the same paragraph is the word "streptocolysin," which should mean a substance dissolving streptococci, but it actually signifies a haemolytic product of the streptococcus. The author is, of course, not responsible for either word, but he writes with sufficient authority to make some stand against terminology so obviously confusing.

D. W. Carmalt Jones.

Physiological and Medical Observations among the Indians of South-western United States and Northern Mexico. By Alex Hrdlicha. (Washington, U.S. : Smithsonian Institution, Bureau of American Ethnology, Bulletin 34.)

This book is a record of the physiological and medical observations made by Dr. Alex Hrdlicha during six expeditions among the Indians living between latitude 38° × 18° N. west of the Rio Grande and east of the Rio Colorado and the Gulf of California. Dr. Hrdlicha points out that one of the main results of his work is to show that "in many points of physiological nature, as well as in those relating to medicine, there is much similarity among all the tribes visited. This likeness extends, so far as can be judged, . . . far beyond the region already outlined. Another point of even greater importance is the growing evidence of similarity, though never reaching full identity, of the vital processes in Indians and whites."

This is perhaps scarcely surprising, for the majority of the Indians investigated appear to have adopted white customs, and to a great extent to have given up their old habits of life, though many tribes still build huts of the patterns made by their forefathers. Agriculture is the staple industry, though some tribes fish extensively and the Otomi are largely labourers and carriers of burdens. These facts and a number of others recorded in the first ten chapters show that the altered environment of the tribes has greatly modified the habits of the members, though there is nothing to show whether adaptation has changed any of the functions of the body or not. Nor does the author give his opinion upon this important matter, though it is certain that the tribes among whom these observations were made cannot be regarded as living their natural life under normal conditions. Under these circumstances it does not seem possible at present to estimate the value of the many tables of measurements which this volume contains. On the other hand, the absence of decisive evidence as to the existence of hardened arteries, although special attention was paid to the study of senility, seems to show that in some matters at least the Indians have retained their primitive physiology. The point here mentioned is of considerable interest, since the writer has examined a considerable number of elderly uncontaminated Papuasians, some of whom were absolutely senile, without finding any trace of arterio-sclerosis. It is noted that the special senses (sight, hearing, smell, and taste) "differ but little, if any, from the same functions in the whites." Although the adult Indian passes somewhat more time in sleep than the civilised white man, the Indian shows greater capacity for enduring loss of sleep without ill effects. Ceremonies and gambling-parties are usually prolonged throughout the night, and often last over several nights and days, yet the usual effects of sleeplessness were never complained of, and seldom noted, in spite of the fact that but little sleep was taken during the day. Dreams, though not often terrifying, appear to be frequent and variable.

C. G. SELIGMANN.

The New Flora of the Volcanic Island of Krakatau.[1] By A. ERNST, Ph.D., translated by A. C. SEWARD, F.R.S. [Pp. 74.] (Cambridge: University Press, 1908. Price 4*s*. net.)

ANY naturalist who visits the tropics, and particularly if he has the good fortune to visit different and preferably widely separated localities in that zone, can scarcely fail to have his attention attracted by the interesting plants which comprise the

[1] Prof. Seward gives reasons for following Prof. Ernst and adopting "Krakatau" in place of the more familiar spelling "Krakatoa."

floras of the coastal districts. Go almost where he may, he finds the sandy shore occupied by a characteristic association of plants several of the members of which are cosmopolitan. The long trailing stems of *Ipomea biloba* (closely related to the *Convolvulus Soldanella* of our own shores), the bean *Canavalia obtusifolia*, the prickly *Cæsalpinia Bonducella*, *Morinda citrifolia*, *Hibiscus tiliaceus*, *Cocos nucifera* (the coco-nut palm), are equally characteristic of sandy beaches in Ceylon, the Malayan region, the West Indies, tropical South America and parts of Africa and Australia.

On a muddy shore and in estuaries, the assemblage characteristic of the sandy beach is usually replaced by another equally well marked plant association, the Mangroves, some of the members of which are similarly widely distributed through the tropics of both hemispheres. The general aspect of the mangrove swamp is surprisingly similar, whether it is developed on a comparatively small scale in the estuaries of Ceylon or the West Indies, or on the grand scale in a West African delta region, where it is possible to steam for miles in an ocean-going liner through typical mangrove forest.

We might expect to find some break in the uniformity on visiting isolated coral atolls or remote oceanic islands which have never been connected with a great land mass. This, however, is not the general case, and the shores of these islands are inhabited by many of the same species of plants. The majority of these coastal plants have fruits or seeds which have considerable buoyancy enabling them to traverse wide areas of ocean. Some germinate and establish themselves on reaching a suitable resting-place. For our knowledge of the methods of dispersal of these interesting plants we are largely indebted to the well-known researches of Schimper and Guppy. The best practical demonstration was afforded, however, by the reorganisation of Krakatau after the famous eruption of August 1883. On that occasion a portion of the island was completely blown away and the remainder covered with lava, to the destruction of all living beings. Treub visited the island in 1886 and recorded the colonists which had already arrived. Prof. Ernst brings our knowledge of events up to date, and by the aid of excellent photographs presents a vivid picture of the state of affairs to-day when "twenty-five years since the complete destruction of all organic life on the group of islands, these are again covered by a mantle of green, the growth being in places so luxuriant that it is necessary to cut one's way laboriously through the vegetation."

All divisions of the plant kingdom are now represented on Krakatau, the total list of species being 137. Of the plants of the shore no less than two-thirds are species ubiquitous on tropical coasts. The author tabulates observations regarding ocean currents, prevalence of winds and other data, and his researches in conjunction with those previously made afford very full knowledge regarding this aspect of the colonisation of an island and of the dispersal of tropical shore plants. Especially interesting is it to find that the coco-nut palm has established itself, as the power of this plant to do so has been denied.

Birds and air currents have also played very important parts in the restocking of the island. Treub in his early visit showed conclusively that an inland and a littoral flora were being formed independently and concurrently. To these observations Prof. Ernst has added considerably; of especial interest to others beside botanists is his account of the manner in which the blue-green algæ (Cyanophyceæ), bacteria and moulds have played their part in the preparation of a favourable substratum for the higher plants. Prof. Schwarz in his article on "The Organisms of the Soil" (SCIENCE PROGRESS, 1909, 4, 153) refers to the fact of Treub finding as early as 1886 that these blue-green algæ were the first to appear on the bare rock of Krakatau. Penzig later made further observations, and now Ernst, especially by his bacteriological records, has gone far to present a complete picture of the microscopic flora of the soil of the island. It is sufficient to note here that the nitrite and nitrate bacteria and the nitrogen-fixing bacteria were all found by him at Krakatau, and the leguminous plants, of which no less than sixteen are present, all showed characteristic root nodules.

The volume is thus of exceeding interest, as affording the most complete account we possess of the colonisation of an isolated, originally entirely uninhabited island, and also as being a very useful contribution to the question of the formation of soil and the preparation of a medium capable of supporting the higher plants.

W. G. Freeman.

THE ETHICS OF FOOD

I.—THE PROGRESS OF SCIENCE AS EXEMPLIFIED BY THE UNITED STATES FOOD AND DRUG ACT OF JUNE 30, 1906

By H. W. WILEY, Ph.D.

Chief of the Bureau of Chemistry, United States Department of Agriculture

[This and the following article under the same general title are the first two of a series dealing with *Food Materials* which it is proposed to publish from time to time. Dr. Wiley may be said to be the leading authority of the day on the subject of pure food, his strenuous efforts as a propagandist having gained for him a world-wide reputation. His attitude towards the question is clearly displayed in his article. Naturally enough, action such as he has taken is greatly resented by the threatened interests, but the general public are beginning to appreciate his efforts on their behalf. At present there is much need of a similar crusade being preached in our country.—Eds.]

The increasing utility of science appears to no better advantage than in its connection with public sanitation and public morals. The fundamental principle of science is truth ; the fundamental guide of the scientific investigator is honesty; when truth is sought for dishonest purposes or when dishonest methods are used in seeking truth, we cannot hope for useful results. The fundamental attitude of the scientific worker is receptivity. We cannot hope that mankind will ever be free from bias and prejudice but we can hope that a man may so rise above bias and prejudice as to be able to see things in their true light. Applied science is dynamic science; research is potential science. All truth is useful no matter how abstract it may be. Every extension of the limits of our knowledge makes in some way for the good of humanity. In this sense all science is applied science.

The welfare of a nation is too often judged merely by the magnitude of its industries and the balance of its imports and exports. The nation that sends away more than it brings in is always becoming gradually exhausted, yet the excess of exports over imports is regarded as a mark of prosperity. It is true that a nation cannot advance and science cannot advance in the midst of starvation; an abundance of food and clothing and other necessities of life are alike indispensable to material, intellectual and spiritual progress; an excess of these good things, however, is almost as injurious as a deficiency.

There is another index of national welfare which appears to me to be more certain and that is morality. That science has an intimate relation to morality may be a new doctrine to preach but it is not new to science. The men who have been great lights in the scientific world have been men of unimpeachable morality; they have been men who could not possibly do a wrong thing knowingly. Hence it was evident that sooner or later scientific investigation would come closely into touch with public morality. This particular tendency of scientific investigation is the one which has been operative in securing the enactment of the food and drug law of the United States. It was by the investigations of scientists that attention was directed to the fraudulent representations made in regard to the characters of foods and drugs in the United States and to the sophistication to which they were subjected. A number of the sciences collaborated in this work; notable among them were chemistry, pharmacy, botany and microscopy. The conditions which obtained in the United States were perhaps no different from those in other countries, hence it will be unnecessary to go into great detail.

At the time the Food and Drug Act became law, on the last day of June, 1906, the following practices prevailed to a large extent in the United States:—

1. It was quite customary to call food and drug products of an inferior character by the name of the superior article.

2. It was quite customary to sell a different article under the name of the real one.

3. It was a common custom to add various substances of the nature of a drug to foods, either to colour them or to preserve them from decay.

4. It was a common practice to abstract, in whole or in part, some of the valuable ingredients of an article without changing its name.

5. It was a common custom to mix together an article of a higher value with one of a lower value and call the mixture by the name of the article of the higher value.

6. It was a common custom to make false and misleading statements concerning the character and origin of articles and to attach to them false and misleading designs or devices.

The general purpose of all such practices was gain. There were few forms of adulteration or misbranding, perhaps none, which were of any benefit whatever to anybody except the manufacturer and dealer. I cannot recall a single case of sophistication of the nature mentioned which was of any advantage to the consumer.

The magnitude of these malpractices, discovered by scientific examination of food materials, was so great that public discussion was excited and public attention so drawn to them that ultimately a sentiment was aroused which made remedial legislation possible.

The attitude of the legislator towards questions of this kind has often been severely criticised. The criticisms may sometimes have been just; on the other hand, it is manifestly improper to impugn the motives of legislators who, year after year, pass by evils so glaring without voting in favour of their removal. This fault is not inherent in the legislator alone. It is the common condition of the people and perhaps a desirable condition. It is not well that people should be unduly excited at all times about matters which relate to their welfare. There is a sentiment of peace, of quietude and patience, which is becoming to a nation and is an element of strength. This condition of insensibility, as it may be called, permits a nation to pursue its ordinary course without undue divagation. It is only when great principles are at stake or intolerable evils are to be removed, that the people rise in their strength and indulge in a general house-cleaning. So it was that a quarter of a century of investigation and discussion of the evils arising from the adulteration of food went on in the country before the final effort was made to correct them by national legislation.

It is true that many of our States were led years ago to pass laws regulating sales of food and drugs. There were also national laws of a partial character but no general Act had been placed upon the statute-books by Congress. All the laws relating to the sale of food and drugs were special and referred to named articles, while in the Act of June 30, 1906, a radical departure was made in passing a law which did not specialise but applied equally to all articles used as foods and drugs.

It may be said, I think with propriety, that the passage of this Act was a distinct indication of the scientific progress made in the United States in applying science to sanitation and public morality. The passage of this law—in which I took an active part—and its enforcement—in which I have played a subordinate part—have called to my attention the different obstacles which had to be overcome, in the first place, in the enactment of the law and which still remain to be overcome to secure its full enforcement. The way of scientific progress will become more obvious if the obstacles which have been and are being encountered are pointed out. The first great obstacle to legislation of this kind was individual and corporate greed. People who were making money by misrepresentation and by the addition of drugs to foods did not at all fancy a public discussion of their methods nor legal regulation of their business. This was true as regards trade in both foods and drugs. It was especially true of that very large class of Americans that makes money by selling drugs to the laity or by practising medicine *in absentia*. There are many people in our country, more perhaps than in any other, without any medical training whatever or only of the most superficial kind, who venture to practise medicine by advertising in the newspapers and magazines and by circulars distributed through the mails to the people at large. These advertisements and circulars depict in the most vivid and horrible manner those symptoms of insidious diseases, those symptoms which perhaps are but the common slight departures from an ordinary state of health. In this way they work upon the fears of the uninformed and easily find access to their pockets. A law which proposed in any way to restrict practices of this kind and to require vendors of the so-called remedies to cease misrepresenting them excited most violent and vigorous opposition from this class of our citizens. Banded together in compact organisation, being also in command of abundance of money and able to patronise the press of the country by advertising liberally in the papers, they held a vantage ground from which it was difficult to dislodge them. Up to the very end, the lobby representing their interests was to be found in Washington; every means known to them of modifying or minimising the requirements to be made of their trade was resorted to up to the last moment.

Another interest which was found blocking the wheels of legislation, unless concessions could be granted, were the compounders of intoxicating drinks. Various forms of distilled beverages were offered to the people of the United States under false and misleading names. Liquors compounded with neutral spirits, flavours and colours were represented as "old, aged in wood" and as being particular kinds of whisky, brandy, rum or gin. The Wholesale Liquor Dealers' Association of the United States kept a keen eye on the progress of legislation and in a circular issued a short time before the food law was passed urged the members to contribute liberally to the funds for watching legislation, on the ground that, in previous Congresses, they had successfully prevented the passage of food and drug bills which did not meet with their approbation.

Other parties who opposed the passage of the Food and Drug Act were the manufacturers who used drugs in their foods. These manufacturers were constantly appearing before committees to urge the legalisation of certain forms of preservatives and colours by specific legislation or a wording of the Act which would enable them to continue their practices after the bill should become law. In fact, at the beginning of the agitation for a national law, it is probable that a large percentage of the trade engaged in the manufacture and sale of food and drug products was either opposed to any legislation whatever or extremely apathetic respecting it. As scientific inquiry proceeded, however, many manufacturers and dealers gradually abandoned the stand they had taken and instead of being opponents of legislation they became its advocates. This only goes to show that the evil which was to be removed was of a nature which, when shown in its full character to the honest manufacturer, led him to join the army of those who were seeking to correct it.

Among the great organisations which were constantly in favour of legislation of this kind should be mentioned particularly the American Medical Association. From the outset the medical fraternity of the country has promoted legislation, both in the State and in the Nation. While it is true that there are many physicians who have either been indifferent to the matter or actively opposed it, they are isolated in number and have no commanding influence in the councils of medical bodies. Both national and state medical associations have stood firmly for the principle of food legislation and for its proper enforcement.

The principles of interpretation which should be placed upon the law have been fully elucidated by the judges of the United States Courts. Mr. Justice McPherson of the federal court of the Western District of Missouri in the beginning of litigation under the Food and Drug Act, in interpreting the method of enforcing the Act, said:

> "Adulteration of goods and false labelling had become so common that it was well-nigh impossible to purchase pure goods or that which was called for. The same was true as to medicines. Congress undertook to remedy it. The one purpose was to prevent the sale of adulterations. The other purpose was to enable a purchaser to obtain what he called for and was willing to pay for. . . . This statute is to protect consumers and not producers. It is a most beneficent and righteous statute . . . and should be enforced. It cannot be enforced if it is to be emasculated, as is sought in the present case."

Mr. Justice Humphrey, of the Southern District of Illinois, in a recent case where the United States proceeded against fifty-two cans of eggs preserved with boric acid, said:

> "When there are two interpretations to be placed upon a law the Court should follow that one which more clearly carries out the purpose and intent of the law. The Food and Drug Act of June 30, 1906, was intended to prevent interstate commerce in adulterated and misbranded goods. To make a ruling which the defendants ask in this case would be to evade the law. The ruling is denied and the case will proceed to trial."

On a trial of the case, the charge of the Government against the eggs was sustained and the order given that they were to be destroyed as containing a "substance which may render them injurious to health."

The same attitude which the courts have thus proclaimed judicially, it seems to me, should characterise all persons who are entrusted with the enforcement of the law. I can only speak for myself in this particular, in so far as the enforcement of the law or any part of it is placed in my hands. I have acted uniformly upon the principle which has been judicially confirmed by the above decisions. Wherever there is any question concerning the meaning of the analysis or investigation which is placed before me and where it is possible to put two constructions upon that analysis, I have invariably put the one on it which protected the consumer. In other words, the man who executes the law or brings the action is neither the judge nor the jury—his duty is simply to see that the intent of the law is carried out and that all cases of apparent violation are brought before the courts. For this reason, I have always ruled that chemical antiseptics which preserve food by destroying germ life are to be excluded in the interests of public health and on that broad principle I have been willing to let the matter go to the courts. In the courts, the defendants have ample opportunity to show to the contrary or the Government has ample opportunity to prove its contention. The utility of science in this respect, of course, is unquestioned; it would be impossible to bring most of the cases which finally reach the courts under the Food and Drug Act without the aid of one or more of the sciences.

What, may be asked, is the attitude of scientific men in such matters? The answer is not a difficult one. The question as to whether a certain substance is injurious to health or not is often an open one and men can honestly arrange themselves on opposite sides. It is not strange, therefore, that in every instance eminent scientific authority is found favouring the use of antiseptics in foods. In such cases the Court not only decides from the preponderating testimony but also judges from the character of the testimony and the reasons for the witnesses being present in court. Unfortunately in the United States, as possibly in other countries, there are many scientific men who are, one might say, professionally engaged as scientific advisers to the defendants in suits of this kind. For instance, there are names in American scientific annals which are found quite frequently in cases where the Food and Drug Act is offended, especially where the defendants can afford to employ high-priced talent. I am not criticising the action of these men but only mentioning the fact to show that the courts do not fail to take them into judicial account. This is well shown in the opinion given by Mr. Justice Humphrey of the Southern District of Illinois in considering affidavits of eminent scientific men respecting the nature of whisky. He said:

"Complainants present sixty-nine of such affidavits and the defendants a lesser number.

"These affidavits are from rectifiers and distillers, members of the wholesale and retail liquor trade and scientists and chemists of high rank. They do not agree. Indeed, it may be said that some of them present diametrically opposite views more or less elaborately stated.

"In brief, the affidavits for complainants tend to support the proposition that a distilled spirit from grain reduced by water to potable strength from which most of the fusel oil has been removed by rectification is whisky and that all distilled spirits from grain are 'like substances,' without reference to differences in their percentage of alcohol or of secondary products present therein.

"The affidavits presented for defendants tend to support the view that whisky is a product made by the proper distilling of a fermented mash of grain with such care and at such low temperature as to retain the congeneric ingredients of the grain, aged under a normal temperature for not less than four years in charred oak casks. Thus broadly in statement do the chemists disagree. They are more or less persuasive to the court according to the soundness of scientific reasoning given in support of their statements."

After a careful review of the arguments and the statements in the affidavits, Mr. Justice Humphrey ruled in favour of the defendants.

It is undoubtedly true that, before the courts in the United States at least, the testimony of the experts who appear for the State, in the capacity of protectors of the public health, carries greater weight and is considered of greater importance than the testimony of even more eminent scientific men on the other side. In fact it is difficult to see how an added substance of the kind I have mentioned is harmless. You might prove that it did not injure a certain number of individuals to whom it had been administered but you could go no further. A positive statement respecting the injurious effects of such added bodies would overcome a vast amount of testimony as to their negative effects.

In the United States at the present time the situation in regard to added antiseptics is as follows:

Certain of them are forbidden, as for instance, boron compounds, formaldehyde and salicylic acid. One antiseptic is permitted by a special regulation, namely benzoic acid. The regulation permitting its use reads as follows:

"It having been determined that benzoate of soda mixed with food is not deleterious or poisonous and is not injurious to health, no objection will be raised under the Food and Drug Act to the use in food of benzoate of soda, provided that each container or package of such food is plainly labelled to show the presence and amount of benzoate of soda."

The question of the harmfulness of sulphites was answered in the affirmative by the investigations of the Bureau of Chemistry. Appeal from this decision has been made and the question is now before the Board of Consulting Scientific Experts.

This Board also has before it for consideration the question of the use of saccharin and of sulphate of copper. Pending the decision of the Referee Board of Consulting Scientific Experts, all these bodies are permitted to be placed in food products in the United States.

The question of using alum has been considered and the recommendation has been made that it be referred also to the Board of Consulting Scientific Experts. This Board is composed of Dr. Ira Remsen, President of Johns Hopkins University, Prof. Russell H. Chittenden, Director of the Sheffield Scientific School of Yale University, C. A. Herter, Professor of Pharmacology and Therapeutics, Columbia University, New York City, J. H. Long, Professor of Chemistry, Medical College of the North-western University of Chicago and Alonzo E. Taylor, Professor of Bacteriology, University of California. This Board has made investigations which have been published on the subject of benzoate of soda, reaching the conclusion that it was not injurious to health and did not lower or injuriously affect the quality of the food product to which it was added.

The summary of the condition, therefore, is this: Benzoate of soda is legalised in the United States. Sulphurous acid and sulphites, saccharin and sulphate of copper are permitted pending a report of the Referee Board. Alum is permitted for the present. Boron compounds, salicylic acid and formaldehyde are forbidden.

It appears from a study of the data of some cases of the use of antiseptics of this kind in foods that other than strictly hygienic considerations have been potent in determining conclusions, as for instance the toleration of boron compounds in England and of copper sulphate in France. This, of course, is a most unfortunate matter; if science is to be appealed to to decide these questions, all considerations other than scientific truth and the welfare of the public should be removed from the problem. There is no doubt in my mind that science will yet in a convincing way answer the questions raised and that her dictates will in the end be followed by all nations. It would not be advisable in this paper, even if space permitted, to enter into a detailed discussion of the various ways in which science is applied to great problems of public sanitation and public morality. There is no doubt that, in the future, answers to to questions of this kind will be largely dictated by the investigations of scientific men.

THE ETHICS OF FOOD

II.—MILK

By RALPH VINCENT, M.D., B.S., M.R.C.P.,
Senior Physician to the Infants Hospital, Westminster

PURE milk is a vital necessity of the young. No other food can take its place, the qualities and properties of milk being peculiar to itself and definitely related to the peculiar digestive powers of the infant and the precise requirements on which structural and functional development depend.

When the mother's milk is not available or is of defective quality, as so frequently happens at the present time, the infant becomes entirely dependent upon the milk of some animal. In the case of young children, milk should always constitute a large proportion of the daily food.

The plentiful supply of cows' milk, pure and of good quality, is an absolute essential to the rearing of a healthy and strong people, properly equipped in regard to physical structure and with those powers of resistance to moderate adversity of environment which constitute the determining factor in relation to health or disease for a very large proportion of the community.

In regard to children, nothing can well be more striking than the secondary part played by pathogenic organisms in the causation of permanent injury or disease. The well-nurtured child attacked by one of the specific infectious diseases, such as scarlet fever or measles, usually makes a good and rapid recovery, so that the mother not infrequently remarks that the child seems "all the better for it." Far otherwise is it in the case of the child suffering from defective structure—the consequence of chronic infantile malnutrition. The recovery is anything but good or rapid: "*sequelæ*" and complications make their appearance. "Middle-ear disease," generalised tuberculosis and other disastrous consequences ensue as the result of the child's inability to repel the invasion of elements hostile to its health.

When, however, we consider the condition of the milk as generally supplied, we are at once confronted with the fact that nothing in town or country is so comparatively rare as milk which is pure, reasonably clean and of good quality. In London it is possible to obtain a pure *fresh* milk produced hygienically and supplied under proper conditions—that is, in sealed bottles. But the milk thus produced and supplied constitutes a minute fraction of the total supply.

The milk-supply of London is drawn from the surrounding country within a radius of some two hundred miles. Soon after the cows are milked, as a rule the milk is reduced in temperature by the use of some form of cooling machinery. This cooling undoubtedly retards bacterial growth but it is seldom carried out with any approach to efficiency. Ice or refrigerating machinery is almost never used; in fact, the temperature of the cooled milk depends upon the temperature of the water-supply at the farm. Hence in winter, when the cooling is least required, the reduction of temperature is most efficient, whilst in summer, when cooling is most urgently required, the reduction of the temperature of the milk is so slight as scarcely to make any appreciable difference. In regard to the details involved in the production, collection and delivery of milk, the conditions are so hopelessly bad, the ignorance and carelessness displayed on all sides are so great, that no serious improvement can be attained without radical alterations.

As a rule the farm where milch cows are kept is exceedingly filthy. The cow-house is dirty: in the great majority of cases it is insanitary in the extreme; the drainage and ventilation arrangements could scarcely be worse. Everything is soiled with cow-dung, urine, dirty fodder, etc. The cows themselves are covered with filth, dried dung being its chief constituent. The floor on which they stand is covered with an oozing mass of excreta and the effluvium baffles description. It need hardly be said that, under such conditions, the cows appear to be far from healthy; they seem, in general, to look ill and out of condition. It is scarcely matter for wonder that tuberculosis is rife—indeed, that this is one of the scourges of farms.

It is characteristic that wholesome food is excluded from the dietary and that brewers' grains, oil-cake and other artificial products having a definitely prejudicial effect on the milk—especially when it is intended for the use of infants—are much used. In the person of the milkers and in all the details of milking no opportunity of contaminating the milk would seem to be lost. Cow-dung is a normal constituent of the milk as supplied. The following is part of an account published by a correspondent in the *British Medical Journal*:

"Against one wall of the shed was banked up a great heap of manure, while on the opposite side all the cinders, old bones and general rubbish of the farm were accumulated. . . . I was horrified to see the filthy state of the milk as it flowed out of his pail. It was discoloured with grit, hairs and manure. 'Look at that!' I said, pointing to a specially large bit of manure. I regretted my zeal, for he dipped his whole hand into the pail and, as he brought it out, said, 'Oh, that ain't nothing; that's only off the cow!'"

In November 1903 Dr. George Newman, then Medical Officer of Health for Finsbury, published an elaborate report on the conditions of the milk-supply in that borough and the facts there collected may be regarded as fairly typical of the conditions generally prevailing.

He found that 90 per cent. of the milk was obtained from country farms and that 95 per cent. of these were situated at a greater distance than 100 miles from London. As a rule, the evidence showed that the cow-sheds from which the milk was derived were ill-lit, overcrowded, badly ventilated and badly drained. Of the milk-shops, 52 per cent. were found to have one or more sanitary defects and 73 per cent. of the vendors failed to keep the milk covered or protected from dust. The average number of bacteria in unpreserved milk was found to be 2,370,000 per cubic centimetre. Pus and dirt were present in numerous cases.

In consequence of the contaminated condition of the milk as commonly supplied, various processes of heating milk have been introduced in order to destroy the numerous micro-organisms and thus protect infants and others from the effects of contamination. These methods have been enthusiastically advocated by many writers, so that the boiling or sterilisation of milk has almost come to be regarded as a desirable practice, typical of sanitary advance and medical progress.

Whatever may be the excuse of expediency, the whole argument is unsound and inconsistent with the principles of scientific procedure. To supply an infant with contaminated milk is certainly far from advisable but milk that has been contaminated remains contaminated whether boiled or unboiled.

Moreover, boiling of the milk does not protect the infant. It irretrievably injures the food of the infant, definitely destroying elements essential to nutrition; whilst the interference with the natural processes of digestion is so great that the infant fed for any considerable period on boiled milk suffers severely from malnutrition directly arising from atrophy of the digestive glands.

Among the poorer classes the boiling of milk plays an important part in relation to the production of the most fatal disease of infancy—zymotic enteritis. This disease is chiefly caused by the putrefactive decomposition of cooked milk. The changes that occur can only be produced when the milk is cooked or its natural characteristics are interfered with, raw milk being protected from these poisonous changes by the action of the lactic organisms. These organisms acting in raw milk produce lactic acid and the acidity thus engendered protects the milk from putrefactive changes which can only occur in milk that is neutral or alkaline.[1]

Recent developments in the milk trade have been distinctly retrogressive. In the summer months it is becoming a common practice for some of the milk companies to pasteurise the milk prior to delivery. This is a matter of serious moment. Milk is a natural article and the business of the milk-vendor is to supply it in its pure and natural condition.

The most pernicious of all practices in connection with milk is the use of "preservatives." The action of these substances on the infant is of the most serious character. At the very beginning of gastric digestion processes essential to the health of the infant are directly interfered with. In consequence of this perversion the chemical changes attending digestion in the intestine are interfered with and atrophic enteritis develops. In the case of an infant suffering from the effects of preservatives in the milk it has consumed, the full extent of the injuries can scarcely be appreciated until the cause has been removed. It is not till then that the harm done is fully realised, the digestion being so injured that the most delicate adjustments

[1] For a fuller discussion of the effects of the boiling of milk, *vide* the writer's *Nutrition of the Infant*, 3rd ed. 1910.

of the diet are required, and it is frequently several months before the infant fully recovers.

In the worst cases, when the infant has received considerable amounts of preservatives, such as boric acid, over a considerable period, the atrophy of the digestive glands may be quite incurable and death ensues after a lingering illness characterised by much pain and suffering.

Such a practice requires to be dealt with by the legislature in no uncertain manner. In the opinion of the writer all such deliberate offences should come under the criminal law. They are offences of commission, not of omission, so that they need to be dealt with in a manner which shall leave offenders in no doubt as to the public view of the nature of their offence. That physicians should be hampered in their treatment of infants and young children because the "fresh" milk, recommended by them and ordered by the parents, contains boric acid is an altogether intolerable state of affairs and should be dealt with accordingly.

Great harm has been done by the persistent advocacy of "boiled" milk. The boiling of milk is quite ineffectual as a means of protection; the advocacy of the practice has done more than anything else to mislead the public and to encourage all sorts of wild expedients which leave the essential problems untouched. There is only one way in which clean milk can be obtained, namely by ensuring cleanliness and sanitation from its production to its delivery and giving special attention to the peculiar hygienic precautions indicated by the peculiar qualities and properties of milk.

The Infants Hospital, Westminster, obtains its milk from a farm directly under the control of the hospital authorities; as the procedures adopted at this farm afford a complete and practical illustration of sanitary milk-production, they may be described in some detail. The milking sheds have been arranged so as to allow of thorough ventilation, ample light, rapid and efficient cleaning. The sheds are provided with hydrants and are thoroughly flushed out twice a day, immediately prior to the cows entering the shed for milking. This flushing ensures that the floor and stalls are wet when the cows enter, so that the raising of dust in the shed at and about the time of milking is reduced to a minimum. No drains are allowed to enter the sheds. The floors are of solid concrete and all excremental and other material is flushed by water along open channels into a culvert external to the sheds. By these means any defect that may arise in the drains is prevented from affecting the sanitation of the milking-sheds. These sheds are used solely for milking, so that they are only occupied by the cows for a short period twice a day. The rest of the day the animals spend elsewhere. In summer and in mild weather they are in the meadows; while, in winter, they are cared for in specially constructed winter quarters. These may be described as large yards roughly covered in to protect the animals from the inclemency of the elements while securing for them plenty of fresh air.

The cows are selected with great care so that a milk may be obtained which is most suitable for the purpose of infant feeding. Jerseys and Guernseys, for example, are strictly excluded, owing to the fat-globules of their milk being excessive in size and insufficiently discrete. Various other factors are carefully considered, such as steadiness of yield, quantity of milk produced per diem, etc., etc. The closest scrutiny is kept upon the actual work done by each cow as a milk-producer. This point is of the greatest importance in relation to both economy and efficiency. A cow, for instance, may yield a good quality of milk as regards purity but one which is low in total solids and the amount of milk she yields per diem is below the proper amount. In such a case, when the cow is in good health, it will generally be noticed that the cow is increasing in weight; in other words she is producing beef instead of milk.

This question, which is one of prime importance, as a matter of fact, is almost entirely neglected by farmers. It has amused the writer on many occasions to hear the farmer lamenting the fact that milk-production yields little or no profit, when the most casual inspection of his herd was sufficient to show that he had hardly a single efficient milk-producer in it. If the average farmer is asked what quality of milk a given cow is producing, *i.e.* what is the amount of fat and of albuminoids in her milk, his only reply is a vacant stare of astonishment. Of course the absolute want of knowledge of essential matters directly connected with proper management has a direct bearing upon the breakdown of the milk-supply. If ignorance be the mother of all evil, then there cannot be much doubt as to the cause of the condition of the general milk-supply—the average milk-farmer is gloriously and magnificently ignorant.

At the farm supplying the hospital, the dieting of the cows is carried out on a systematic and scientific basis. Each cow has to be studied, so as to keep her in prime condition for milk-production. She must not be allowed to put on flesh; as a matter of fact, she should look rather thin. On the other hand, she must be in good health and her coat must be smooth and glossy. The maintenance of this balance so as to keep the cow in good health while securing the fullest yield of milk, rich in fat and total solids, calls for expert management and the dietary needs to be carefully adjusted for each individual animal so as to maintain the "balanced ration." Moreover, when the milk is to be used for infants, special considerations apply and in the adjustment of the diet of the cow the infant has continually to be borne in mind. Most of the foods commonly used, such as oil-cake, brewers' grains, distillery grains, etc., are strictly forbidden on account of their extremely prejudicial effect on the infant. Grass, hay, pea-meal, bean-meal and mangolds are some of the chief articles used, care being exercised to prevent an undue proportion of mangolds or green food, as these are only allowable in very moderate amount—just sufficient to make the food succulent.

The process of milking is conducted as nearly as possible on the principles of aseptic surgery. The milkers thoroughly wash their hands and they wear sterilised overalls; while all the vessels used in milking are sterilised by steam prior to use. The milk from each cow is drawn into a pail of special construction which is designed so as to protect the milk from any dirt or hairs falling from the cow. It is provided with a cover which is placed on the pail immediately the cow is milked. The milk from each cow is successively and *immediately* transferred to the separating and refrigerating rooms, situated in buildings at a distance of some thirty yards from the milking-sheds. As the milk is required for the milk-laboratory, the warm milk is passed through a power-driven separator whence it emerges as (1) fat-free milk, (2) cream. The milk is then immediately reduced by means of refrigerating machinery to a temperature of 38° Fahrenheit.

This stage, of critical importance in regard to the storage and transport of milk, is accomplished very quickly, so that within about five minutes of the milk leaving the milking-sheds it is passed through the separator, reduced to a temperature of six degrees above the freezing point of water and placed in specially constructed churns for transport by rail to the hospital.

The transmission by rail of milk at a temperature much below that of the external air requires special arrangements to protect it from the access of external heat. The churns are constructed throughout with a double wall. Between the outer and inner walls is a layer of enclosed air. Owing to the extremely low specific heat of air, this layer of still air acts as an efficient insulator, preventing any serious rise in the temperature of the contents.

The milking at the farm begins about 5.30 a.m. At about 6.30 the last pail of milk is separated and cooled and the churns are despatched to the railway station, which is at a distance of about one mile from the farm. It leaves the station at 7.30, arrives at Charing Cross at 8.30 and is received at the hospital about 9 a.m., so that the milk is at the hospital within four hours of milking. The churns are sealed before leaving the farm so as to guard against any possible tampering with the contents on the journey. On arrival at the hospital the churns are taken from the cart by means of a crane directly into the receiving room of the milk-laboratory, so that the milk is not exposed until it is protected from the access of the dust and dirt of London. The milk is systematically examined at the hospital, where bacteriological and microscopical tests are carried out, so that a constant and searching check is maintained upon all the processes.

By these means the hospital obtains for its patients a pure milk of good quality and it may not be out of place to refer to the cost of the milk as received at the hospital. Notwithstanding the elaboration of method and the high quality called for as compared with ordinary milk production, the cost of this milk is not appreciably above the ordinary cost of producing very ordinary milk. As a matter of fact, the hospital pays for the milk a price which is twenty-five per cent. less than the ordinary retail price of milk.

The explanation of this lies chiefly in the organisation and in the great economies resulting from the application of scientific method. In other words, the production of milk is dealt with as a technical matter, every detail being carefully studied. The result on the "cost-sheets" is apparent at every stage. There is nothing very remarkable in the fact that great economies are effected as compared with the fatuous and empirical methods of the traditional farmer. Indeed, the first feature that strikes the informed observer on inspecting an ordinary milk-farm is the incompetence of those in charge and the waste and extravagance which are the inevitable accompaniments of incompetent administration. Many instances of this could be cited but a single one may suffice to show the kind of thing that is quite characteristic of the ordinary methods. In a certain herd there was a remarkable cow yielding a milk of fine quality in altogether exceptional amount—nearly twenty quarts per diem. She was, in consequence, a valuable animal worth, according to her milk-yield, over £30. The time came for her to calve. Owing to the neglect of certain simple sanitary precautions she developed puerperal sapræmia, with the result that her milk entirely failed. She had then to be fattened for the butcher and was sold for £12—a dead loss of some £20. It is little matter for wonder if the farmer, in such circumstances, finds milk production expensive and unprofitable.

In connection with infants, however, the provision of pure cows' milk is only one of the important measures necessary for the providing of the infant with adequate food.

Even if it be supposed that cows' milk and human milk contain exactly the same materials and that there is nothing in the one which is not in the other, the proportions are markedly different, as is seen from the following comparison:

Human Milk.	Per cent.	*Cows' Milk.*	Per cent.
Fat	4·00	Fat	4·00
Milk-sugar	7·00	Milk-sugar	4·50
Proteins	1·50	Proteins	3·50
Mineral salts	0·25	Mineral salts	0·75

It will at once be seen that the mere dilution of cows' milk is an entirely inadequate method of adjusting the food; for while this may reduce the proteins to their normal amount, the fat and the lactose are also diminished; and as the fat is present in the same amount as in human milk, whilst there is less milk-sugar, the diminution of these is highly undesirable. But in reality the difficulty is much greater than is represented by these factors.

Not only are the proteins much greater in amount in cows' milk but the "caseinogen," the extremely indigestible curd-forming element of the milk-proteins, is present in great excess; moreover the delicate whey-proteins, which constitute the bulk of the proteins in human milk, are only present in relatively small amount in cows' milk. This will be clearly seen from the proportions in the following table:

PROTEINS IN HUMAN MILK AND IN COWS' MILK

Human Milk.	Per cent.	*Cows' Milk.*	Per cent.
Whey-proteins . . .	1·00	Whey-proteins . . .	1·00
Caseinogen . . .	0·50	Caseinogen . . .	2·50
	1·50		3·50

Thus, in a given quantity of human milk the whey-proteins will be twice as much as the caseinogen, while in cows' milk the caseinogen will be two and a half times as much as the whey-proteins. It must further be borne in mind that human milk is not of constant composition. It varies with the individual mother and with the age of the infant. Consequently, it is necessary, in connection with substitute-feeding, to provide a method which meets the varying requirements.

The procedure carried out in the milk-laboratory of the Infants Hospital illustrates all the essential factors and the measures by means of which the special and varying requirements of the infant are met.

The fundamental principle underlying the work of the milk-laboratory is the adherence to the standard of human milk in its natural condition. No methods of boiling, pasteurising or otherwise cooking milk are allowed. All the sterilisation that is effected is applied *not* to the milk but to the vessels coming in contact with it. Some of the processes to be described are rather more elaborate than may be necessary for the average healthy infant; but it must be remembered that the hospital is dealing with infants who are critically ill and that the dietetic adjustments for them need to be of the most refined character. Nevertheless, the general principles apply to the substitute-feeding of all infants. As soon as the milk is received in the laboratory the cream is tested in order to determine the exact amount of fat it contains. This having been ascertained, a standard cream is prepared by the addition of the proper amount of fat-free milk. Whey is prepared from the fat-free milk. A standard solution of lactose (milk-sugar) is made. Sterile water is obtained by means of a Pasteur-Chamberland filter and lime-water is prepared from pure calcium hydrate. In this way the modifying nurse has six solutions (or ingredients) at her disposal: (1) Cream; (2) Fat-free milk; (3) Lime-water; (4) Lactose; (5) Whey; (6) Sterile water.

These solutions are placed in metal tanks fitted with ice-containing chambers at the back; from these solutions the milk-mixtures prescribed for the infants are prepared. The diet prescription for each infant is sent from the ward to the milk-laboratory in the following form, for example:

Ward I. Infant No. 24.

	Per cent.
Fat	2·00
Lactose	6·50
Whey-proteins	0·75
Caseinogen	0·25
Alkalinity	5·00

Ten tubes each of 4 oz.

In the laboratory the prescription is translated into actual amounts. The following is the translation of the above prescription:

	c.c.
Cream (32 per cent.)	75
Lactose solution (20 per cent.)	121
Whey	858
Fat-free milk	59
Lime-water	60
Water	27

The milk mixture so constructed is then carefully put up into separate bottles, one for each feed. Many thousands of such combinations are in use at the hospital.

Since milk is provided by Nature to be transferred directly from the mother to the offspring, it is essential that the measures adopted for the storage of milk should be of such a character that its natural properties are uninjured. The keeping of milk at ordinary temperatures involves a rapid development of bacteria, with the result that the composition of the milk is altered in consequence of the bacteria living in and upon it. It is therefore necessary to keep the milk at a temperature at which these processes cannot occur, *i.e.* at a temperature not exceeding 40° Fahrenheit.

The maintenance of the low temperatures is accomplished by means of refrigerating machinery and insulated rooms specially designed to meet the precise requirements of the hospital. The general work performed by the machinery is as follows:

(1) The manufacture of ice for use in the milk-laboratory (modifying room). The ice is made so as to fit exactly the tanks previously referred to, thus making for economy as the waste due to breakage is obviated.

(2) The maintenance of the milk in the storage cabinets attached to the modifying room and to each ward at a temperature not exceeding 40° Fahrenheit.

(3) The reduction of all solutions used in the modifying room to a temperature not exceeding 40° Fahrenheit. This applies to the whey, lactose solution and the sterile water.

By these means the food of the infant is protected from all the changes which usually occur in it as a result of storage—from the time of milking onwards the low temperature is consistently maintained. Just before modified milk is given to the infant, it is restored to the normal temperature of milk by warming it to 100° Fahrenheit.

THE INDIAN INDUSTRIAL PROBLEM

By ALFRED CHATTERTON

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The Need of a System of Industrial Training

The publication in 1884 of the Report of the Royal Commission on Technical Education drew the attention of administrators in India to the fact that no adequate provision had been made by the Indian Educational Departments of systematic instruction in the scientific principles underlying industrial processes. The interest of the educated public was languidly excited and vague notions became current that the acknowledged decay of Indian manufactures could be arrested if arrangements were made to remedy the defects in the existing educational machinery. Accordingly, in the course of the next few years, each Province took action in this direction and sanction was accorded to such measures as the local governments considered to be immediately necessary. One result of the application of European ideas on the subject of technical education was the establishment of the Victoria Jubilee Technical Institute in Bombay, where the cotton-spinning industry was already firmly established; as another result the engineering school at Seebpore, near Calcutta, was reorganised and expanded to provide for the needs of Bengal, where the manufacture of jute, coal-mining and mechanical engineering were local industries of considerable and growing importance. Both these institutions are now valuable centres of recruitment for the organised industries of their respective Presidencies; that they have not reached the standard of excellence we are accustomed to expect in similar institutions in Europe and America is due to the fact that Indians do not regard an industrial career with any favour; they only take to it when they are convinced that they have no prospect of success in more congenial occupations.

In other parts of India it was obvious that modern industrial enterprise was too feebly developed to support either specialised technical schools like that devoted to the cotton industry in Bombay or a general engineering school like that at Seebpore. In Madras, however, an original attempt was made to create a demand for technical education by providing facilities for the examination of students in a great variety of technical and industrial subjects. The scheme was modelled on the lines of the examinations of the Science and Art Department and of the City and Guilds of London; it has proved of little value, though it has supplied convenient tests of the training given to pupils in trade and elementary engineering schools.

The only practical outcome of these early attempts was to strengthen the staff and improve the equipment of the existing engineering colleges at Roorkhee, Poona and Madras, where Indians are trained for the various branches of service in the Public Works Department. Unlike Seebpore, where most of the students find employment in the industrial undertakings of Bengal, these institutions are intended to supply the very considerable demands of the provincial Governments, native states and district boards for men to carry on the current engineering work of the country in connection with railways, road and bridges, irrigation, buildings and general municipal work. Mechanical engineering is not entirely neglected but it is regarded as subordinate to civil engineering, hence probably the limited degree of success hitherto attained by Indian engineers in the practice of a profession which calls for an intimate acquaintance with the materials and methods employed in construction. For a long time these colleges were not very popular, notwithstanding the fact that a number of well-paid Government appointments were guaranteed to the students who completed full courses of instruction; of late years there has been a great change, the competition at the entrance examinations being now very keen. Apart from the too early specialisation in favour of civil engineering, the work done in these colleges suffers from the defective previous training of the students; but little improvement can be expected so long as the general education of the country is dominated by the Universities. The reforms which have been introduced, since the report of the Universities Commission, have done something to raise the general tone of Indian education but they have done little or nothing to render it of a practical character. It seems almost certain that another educational system is required that will provide for the industrial needs of the country, entirely independent of the control of the Universities.

For the indigenous industries of the country, which are entirely in the hands of illiterate artisans, it was not deemed possible to make any provision. The first attempts to deal with industrial education were made by missionaries, who started schools for the instruction of orphan boys in their charge in such trades as carpentry, weaving and blacksmiths' work. Subsequently the idea was developed, chiefly by local bodies, and encouraged by Government grants-in-aid. At first the main object of these schools was to break down the exclusiveness of the caste system; later, to improve the hereditary methods of the artisans; the admittedly small measure of success they have achieved is roughly proportionate to the extent to which they have influenced the conservative mind of the Indian hand worker. As schools for the industrial training of boys they have not so far justified their existence but in some instances as demonstration-workshops they have had a beneficial influence on the industrial centres in which they are situated.

At first the cry for technical education in India was but a feeble echo of that raised in England and awakened no response from the educated classes. There was a demand for the services of university graduates and they could readily obtain employment; the rest of the country did not count. All the technically trained men required for Government and for the industrial concerns working on modern lines were obtained from Europe; India was satisfied to see its sons finding congenial careers in the administrative services of the country, in the learned professions and in the educational institutions, which were rapidly expanding. From the early nineties onwards the supply of university graduates began to exceed the demand and year by year the competition has been steadily increasing, with the inevitable result that attention has been turned to other spheres of activity. When it was found that a university training and a university degree were no passports to an industrial career, a genuine demand began to assert itself for technical education and it was soon found that no provision had been made in the country to meet it. A few enterprising youths sought in Europe what they could not obtain at home, to meet only with bitter disappointment on their return. Their education in India was found to be an unsatisfactory preparation for foreign technical schools; they benefited little by their studies and returned to India completely lacking that practical knowledge and experience which are absolutely essential to success in an industrial career. Gradually it has become evident both to the Government and to the educated classes in India that industries must precede technical instruction and that any future industrial development must follow on the lines which have been so successfully pursued in the case of the cotton industry in Western India, the jute and mining industries in Bengal, the leather and cotton trades of Cawnpore and the many miscellaneous industrial undertakings which have been successfully established in every province of India.

The Lack of Native Industrial Leaders

It is now fairly generally accepted that technical colleges in India can only do useful work when they train students for whose services there is a demand in existing industries and that the pioneer work of starting new industries must be undertaken by men who have acquired their skill and experience in other lands where those industries are carried on under favourable conditions. The establishment of technical schools, like the Victoria Technical Institute in Bombay, in other parts of India is now recognised as useless, unless there is a corresponding industrial development to be catered for. Only in Bengal can it be said that this state of things exists; the Seebpore College already makes fairly adequate provision for the needs of that part of India.

The increasing pressure of the educated classes in the more favoured fields of employment can only be relieved by providing new openings for them in other directions and of these by far the most important will be found in the organisation of the immense resources of India for industrial undertakings of many kinds. A great deal has already been done in this direction by European initiative; the reason why the actual benefit to India has not been greater is the fact that Indians have, as a rule, stood aloof. The original impulse, capital and directive energy came from abroad, India having only furnished the raw material and the labour. The profits have been taken out of the country year by year but of greater moment is the fact that there has been no gradual growth of industrial experience, so that to-day, except perhaps in the cotton trade, India lacks native industrial leaders. The men with capital, business acumen, technical knowledge and administrative capacity, who form the backbone of industrial life in Europe and America, are lacking and no preparation has been made to create them. Development in the immediate future, as in the past, must mainly depend on men not born and bred in the country and who will only remain in it for a time, taking with them, when they leave, the experience they have gathered. A change is possible —it may be even said to be inevitable—but it can only be brought about slowly. Indians have begun to appreciate the importance of industrial activity; they have started the Swadeshi movement to encourage it and by degrees they are learning the nature of the problem they have to face. A detailed history of the modern development of the cotton industry in Western India would furnish much useful information to those who are seeking for guidance as to the methods to be pursued to raise India in the scale of nations, to utilise her resources and to provide her people with something more than the bare necessaries of life. There can only be a vigorous and healthy industrial life when it is carried on by the people themselves—that is, they must supply the capital, take the risks, enjoy the profits, bear the losses and, above all, undertake the management and control of the many branches into which it is subdivided.

The Extent of Native Resources

The labour problems in India are not serious; there is plenty of labour, although the standard of efficiency is very low and there is a sad lack of energy and staying power, partly attributable to climatic causes and partly to the low standard of living. The small wages paid for such labour compensates for its disadvantages in a commercial sense and it is certain that as progress is made there will be a corresponding improvement in the condition of the working classes—their output will increase and their wages rise; if education be spread among them, their wants will become more numerous and gradually they will emerge from the thraldom of conservatism and prejudice which dominates them and strangles all aspirations for any higher state of existence than that which they now enjoy.

Of capital there is plenty in the country and year by year it is accumulating; but the people do not know how to use their wealth and it is uselessly hoarded in the form of gold, silver and jewellery. There is a general impression that in India too large a proportion of the population is dependent upon agriculture and that the establishment of new forms of industrial enterprise on modern lines has not compensated for the decay or extinction of indigenous industries. It is suggested that there has been a one-sided development of the natural resources of the country and that in consequence the people are unduly exposed to the perils of famine and scarcity. During the last half-century the indigenous industries have been subjected to ruinous competition with imports from abroad, as a result of which the condition of the artisans has steadily deteriorated. Probably, however, their numbers are actually larger and the amount of their output greater than at any previous time. It is the margin of profit which has almost vanished, with the natural consequence that widespread poverty and destitution have taken the place of a state of comparative affluence. Caste restrictions, combined with ignorance and intense dislike to change of any kind, have kept the artisans to their hereditary methods and in the absence of any external assistance they have only been able to face their difficulties by selling their labour at lower and lower rates, till all they can now obtain is scarcely sufficient to provide for a bare subsistence. On the other hand, during the last seventy years, agriculture has greatly expanded and by the extension of irrigation it has to a large extent become independent of the vicissitudes of the seasons over very considerable areas. The soil of India is rich and when supplied with sufficient moisture and manure yields an abundant harvest. In good years it supports the vast population with ease and yields for export agricultural produce to the value of more than one hundred millions sterling. Some of this is in a manufactured state but the bulk goes out as raw material and it is this enormous quantity of raw material which offers a field of development to those who are interested in the creation of an industrial India.

The charge is often made that British rule in India has brought about an impoverishment of the people and that they are worse off now than they were under the Moguls and their own princes. The charge is easily made and difficult to disprove, as but little is known of the condition of the people before the rise of British power. The standard of living is very low among the great bulk of the population; it is hardly possible that it could have been much lower but the numbers to-day are certainly double, possibly treble, what they were three centuries ago. Famine and plague still devastate the land but their terrors are much diminished and the ravages of war and intestine feuds have entirely ceased. Roads and railways have opened up the country, irrigation works have converted waste, desolate tracts into fertile fields and the *pax Britannica* ensures to every man the enjoyment of his possessions; but the people themselves have not changed—their ruling passion is still to hoard their wealth in a portable form and they still live much as their forefathers did. The main result of British rule has been a startling increase in numbers rather than a marked rise in the standard of living.

A striking commentary on this unproved charge against British administration is that in the five years ending with April 1908 the net imports of bullion into India amounted to £92,287,000, nearly the whole of which has gone to increase native hoards of precious metal, that still represent to the people the most desirable form in which to accumulate wealth. This, it must be remembered, is in addition to the gold raised in India itself, which amounted during the same period to more than ten millions sterling. For all practical purposes these hoards are useless, save as an indication that the material development of India under foreign stimulus is really at a faster rate than that at which the people are deriving benefit from it.

What a capital expenditure of twenty millions a year would effect in India may be inferred from the fact that in a single year it would furnish sufficient capital to establish the whole of the cotton mills of Bombay and of the jute mills of Bengal. In a year and a half it would provide the forty-four crores of rupees which the Irrigation Commission reported could be judiciously expended by Government in bringing a further six and a half million acres under irrigation. It is five times the whole amount annually spent on education—on the education of an empire containing three hundred million people—and it is approximately equal to the land revenue of the whole country and to the total annual expenditure in the military department. Surely, then, it cannot be contended that when so large an amount is put on one side every year and merely hoarded, that the people are becoming poorer? Is it not rather fair to assume that they are accumulating wealth faster than they know how to use it?

Various estimates of the hoarded wealth of India have been made but they are all mere guesses and it would perhaps be unwise to give further currency to them; it suffices for our purpose to assume that the sum-total is very large and that it is enormously greater than any possible demand that can be made for generations to come for capital for the development of the country. From an international point of view this hoarding of gold in India is of great importance in preventing an inconvenient depreciation of the monetary standards of the world; in time to come, when the folly of the practice has been recognised, the dispersal of these hoards may be equally serviceable; in maintaining equilibrium, if the productiveness of the mines should fall short of the demands of an ever-increasing traffic and commerce. This service India renders to the world at large and its people pay the cost not grudgingly but with a cheerful alacrity which is the outcome of extreme simplicity.

It must be remembered that this hoarded wealth is very generally diffused and that it can only be rendered useful by concentration in the hands of a comparatively small number of men who are competent to assume the responsibility of directing the enterprises which can be started by returning it into circulation. This implies the existence of an instinct for co-operative working that at present is but slightly developed; also a knowledge of and desire to participate in the amenities of life which our modern civilisation offers; finally, what is in no way less important than these, an intelligent comprehension of the elementary principles of credit and finance, without which it is impossible to create the feelings of security and confidence which form the basis of commerce and industrial enterprise.

Need of Education

It is only by educating the people that any progress can be made in this direction, and the efforts now being made to extend primary education may be viewed with intense satisfaction by all who are interested in the welfare of India; but much more might be done than has so far been attempted. In the year 1907–8 the total expenditure of British India on education was £4,018,764 or slightly over fourpence per head of the population. This is not extravagant, but in the native states it is even less and if a rational system of education can be devised to meet the requirements of the people, it is certain that it would be wise policy to increase very largely the expenditure under this head, as such expenditure would greatly promote the moral welfare and material well-being of the people. The finances of India are in a flourishing state, the incidence of taxation is light and the natural growth of revenue is equal at any rate to the demands upon it. This is due to the excellence of the administration, which exercises a most careful scrutiny over the spending departments of Government, although it is possible that, in the laudable desire to prevent waste and to keep down taxation, economy has been effected at the expense of national well-being. Any material increase of the grants for education could only be secured by fresh taxation but the necessity for such is now so great that it may well be urged that delay is prejudicial to the best interests of the country. Any form of direct taxation would be extremely unpopular but an increase of fifty per cent. in the very moderate import duties would probably be welcomed and would yield about two millions a year, which would be sufficient to provide for that re-organisation of the educational system which is so urgently needed to prepare the way for a general improvement in the condition of the vast population by teaching them how to make better use of their enormous capacity for labour and how to exploit the natural resources of the soil so that it may yield a return commensurate with its extent and richness.

The suggestion that the increased expenditure which it is advocated should be incurred to remedy the defects of the present educational system may be met by increasing the tariffs on imports naturally raises the question, Why not give India an avowed protection tariff and under the shelter of that tariff build up an industrial system adequate to the needs of the country? That it could be done in this way there is no doubt but the people of the country could not do it and it would have to be done with imported capital and imported brains. The urgency for industrial development in India is mainly due to the limited field that at present exists for the employment of the rapidly increasing educated classes. It is essential that suitable work should be found for them and it is quite certain that if inducements were created to invest capital in India, the investing capitalists would send out their own men to look after and manage their interests. The people of India will be welcomed as "hewers of wood and drawers of water" but in no other capacity. Further, it must not be forgotten that the ultimate authority on the government of India is the British democracy, whose opinions on fiscal matters are very unstable. If the erection of a tariff wall were sanctioned by one Parliament, it is by no means unlikely that it would be pulled down or materially altered by some later Parliament. With a tariff wall there would always be some uncertainty as to the continuance of the protection which it would afford, and in proportion to the intensity of the feeling of uncertainty this would militate against its efficiency as a factor in creating industries in India. The conditions in India are such that State intervention is necessary to bring about the economic changes under discussion but it should be directed to assisting the growth of private enterprise in the country rather than to the maintenance of an artificial barrier to the free exchange of commodities with the rest of the world.

By far the most important matter for the State to deal with at the outset is the establishment of an educational system which, from the primary stages upwards, will be practical rather than literary. Every Indian boy grows up in a certain environment and the education given to him should have reference to that environment and should aim at making him master of it. Hand and eye training, the cultivation of the powers of observation, the co-ordination of the various faculties in the service of their possessor—these should be the objects of educational processes, not merely the development of the mental powers along comparatively narrow lines. The present system of education has failed lamentably to produce men of action, with balanced judgment and sound constructive faculties. The memory rather than the imagination controls thought and in the absence of experience responsibility is declined. It has turned out good if not great lawyers, excellent judges, a few engineers but no original investigators or deep thinkers.

The Lack of Individualism

It must, however, be admitted that it is not the education system alone that is at fault. In India the vitalising force of nationality is almost entirely absent and centuries of subjection to a foreign yoke or to the endurance of an almost continuous state of internal discord and anarchy have deprived the people of that individualism which finds its highest expression in collective effort. Social customs and caste restrictions militate against progress and the general prevalence of early marriages handicaps the race, not only by imposing the cares of domestic life upon students and even upon children who ought to be at school but also because such immature unions result in offspring deficient in physical vigour and lacking force of character. These are deeply rooted obstacles which cannot easily be removed. Emancipation from the tyranny of a grotesque and unique social code has begun and the movement for greater individual freedom of action will be accelerated by the increasing tendency of Indians to travel in other parts of the world. Climate again is a factor which must be taken into account—it induces indolence on the one hand and renders existence easy with but a moderate degree of exertion on the other. The position is one of extraordinary difficulty and complexity; the future well-being of India demands, in fact, a careful consideration of the various elements before any policy is finally framed to guide the administrator through the years of rapid change which lie before us. Educated Indians want work—there is work for them to do but it is work they dislike and their education has not removed their prejudices or rendered the task any easier by training them for it.

The Revival of Native Industries

The educational methods can be changed but it will take a generation to show any result; in the meantime, the evils arising from the lack of suitable employment must be checked and a system of industrial development devised to deal with the existing state of things. Enterprise on a grand scale can be left to grow in the manner it has done during the last half-century and at present need not concern us. Our attention should be concentrated on the decaying indigenous industries: hand-weaving, working in metals, tanning and leather manufactures, on all the petty industries which supply the simple needs of the people. Labour must be trained to work more efficiently—there must be less of brute force and more of skill, the primitive tools of the artisan must be superseded by better implements; subdivision of labour must be introduced and from the crude simplicity of each family as a unit of productive effort strong combinations must be evolved either by co-operative working or by the concentration of manufacture in small factories. That this can be done there is not the least reason for doubt. Every well-directed effort that has been made on these lines has met with success and if so far the sum total of the results is insignificant compared with what has to be done, it is because the experimental stage has only just been passed through. Individuals scattered over India have attacked the problem according to their lights and, whilst many have failed, some have succeeded. A critical review of the circumstances of each case leads to the general conclusion that success has invariably been due to the application of scientific methods and practical experience; that the failures might in most cases have been predicted from the outset, as essential elements to success were neglected and more zeal than discretion displayed in dealing with the difficulties that had to be overcome.

It would serve no useful purpose to cite instances of misdirected enterprise the failure of which has engendered in Indian minds a deep-seated distrust of the tools and appliances which in modern times have so enormously reduced the amount of human labour to be expended in converting raw materials into a form suited to the needs of man. The poverty of India measured by European standards is undeniable but the requirements of the people are extraordinarily small and, except in times of famine, there is but little of the destitution and misery which are to be found in the great centres of civilisation. There are signs, however, that a struggle for existence is beginning to be felt, due to the increasing pressure of the population on the soil, to the expanding needs of the educated classes and to the growing inequality in the distribution of wealth. Within the last few years there has been a marked rise in the price of food grains, which presses severely on the landless labourers in the villages and upon the artisans and workers in the towns. The old order of things is changing, and India is being steadily drawn into the stream along which the nations of Europe and America are being hurried to a by no means clearly discerned destination.

There is in the country much unrest which is far from being of political origin. The problem for the statesmen who will have to control the administration of India is to provide outlets for this newly awakened energy and to direct it in such a manner as to satisfy the growing aspirations of the vast population. Hitherto, the intellectual classes of the country have held almost entirely aloof from the rest of the people, whom they have looked down upon and despised. They have left the working classes to face the growing difficulties of their position, careless of everything outside the range of their own immediate interests; now that they are forced by internal competition to take a broader outlook, they find themselves incompetent to deal with the practical problems which await solution; to bring about a healthier state of things, it is necessary that means should be devised whereby they may be associated with the artisans and workers of the country to their mutual advantage. The future progress of India largely depends on the proper appreciation of her greatest asset—abundant cheap labour—labour at present not without some measure of skill but almost entirely untrained and unorganised.

The Need of Studying Local Conditions

Our work is to show the educated classes how they can find useful careers, honourable and remunerative employment, work that will benefit both themselves and the whole community in supplementing the deficiencies of the workers, in dispelling their ignorance and softening their conservatism.

First we must train them in our schools and colleges, then in our workshops and laboratories and finally we must start them in life, giving them practical work to do under competent supervision until they get accustomed to the new atmosphere and surroundings and are able to launch forth by themselves. But we ourselves have to discover how this may best be done; we must call to our aid all the resources of science and obtain the services of experienced men to study the local conditions. It will be for them to train our students, make surveys of the existing industries, take stock of the natural advantages, search for hidden resources and suggest new lines of work and innovations which may be introduced.

In regard to matters purely agricultural, this procedure has already been adopted by the Government of India and by all the Provincial Governments. At Pusa an Imperial College of Agriculture has been started, a staff of highly competent scientific and practical experts appointed, an experimental farm has been laid out and for some years now the many problems of Indian agriculture have been the subject of close study and unremitting investigation. Valuable results have already been obtained. Each Province has been provided with an Agricultural Department on similar lines, the officers of which deal with the special problems of the Province and by demonstration farms, by direct teaching and by personal intercourse with the people on the land make them acquainted with new discoveries, new crops, new implements and the advantages of adopting improved methods of cultivation. The great primary industry of India is well provided for and in the years to come the country at large cannot but greatly benefit by the thorough and patient way in which the capabilities of the soil are being examined.

The lengthy discussions on the methods by which the industrial problems are to be solved have not yet crystallised into the form of a comprehensive declaration of policy on the part of the Government of India and the Secretary of State. The various Provinces have examined the question, have submitted proposals and in some cases have tentatively embarked upon active measures; but no clear line of action has been marked out as in the case of agriculture. In the education departments, the need of improved science teaching has been admitted and, through the munificence of the late Mr. Tata and his sons, an Imperial Institute of Science has been established at Bangalore for post-graduate work and research which should in time do a great deal to attract the highest intellect of the country to practical pursuits.

The subtle mind of the Hindu delights in philosophic speculations and in unravelling the intricacies of legal enactments; it is possible that the same qualities applied to scientific investigation would afford their possessors equal gratification in probing the hidden mysteries of natural phenomena. That the practical aspects of such inquiries would appeal to them is less certain but, whether or not, their work will be insensibly influenced by the growing need of the country for scientific help in solving the problems which the increased activity of the people will force upon public attention.

The important principle is gradually meeting with acceptance that scientific education must precede attempts at technical instruction and that the latter can only be usefully provided to meet the requirements of existing industries. So long as the great organised industries in the country are mainly controlled by Europeans, so long will the technical assistants be obtained from Europe, and Indians must go there for training and to acquire experience if they want to take a part in such work. This is tacitly admitted by the increasing numbers who year by year leave India to seek such instruction in countries more favourably situated for supplying it. The unfortunate feature in this movement is that the majority of the students who go abroad are inadequately prepared in the way of preliminary education to avail themselves of the facilities which they find placed at their disposal and they are in almost every case quite unable to supplement the purely college courses of technology by practical experience in workshops and manufactories, without which their whole training is imperfect and useless. Not till Indian capital finances Indian industries will the people gradually be able to acquire that experience which it is necessary they should possess if they are ever to manage their own enterprises successfully. The fact that this has to a large extent been accomplished in the cotton trade in some degree accounts for the remarkable progress of that industry.

The cotton and jute industries and mining for coal in Bengal and gold in Mysore have developed because of certain natural facilities or because of the existence of easy markets in which the products were in demand, but the bulk of the industrial work of India is languishing in face of the competition with imports. The external trade of the country has grown at the expense of the internal, resulting in an unhealthy and one-sided development of the country's resources. Roads, railways, telegraphs, the construction of the Suez Canal, every improvement in the means of transport both by sea and land has contributed to the difficulties and, in many cases, to the ultimate discomfiture of the Indian artisan. The attention of Government has been almost entirely directed to the opening up of the land, to the provision of irrigation; assistance has in more than one case been given directly to the efforts of English manufacturers to exploit Indian markets, whilst the industrious artisan has been left severely alone to combat as best he can the growing difficulties of his position. That he has survived so long may be taken as evidence of the possession of certain elements of vitality and as affording justification for the hope that a permanent place may be found for him in the industrial future of India. What we have to do is to supply the artisan with all those factors that contribute so largely to industrial success in which he is so conspicuously deficient. He lacks capital and organisation, his tools and implements are primitive and imperfect, he has no commercial knowledge and in his dealings with the outside world he is almost always in the hands of money-lenders and petty traders, who make their profit out of his helplessness and strenuously resist any attempts to improve his position that would render him independent of their aid. He is industrious and would be intelligent were it not that his faculties are undeveloped owing to the narrow field in which there is scope for exercising them. His technical knowledge is a negligible quantity and of improved trade processes and methods he has but a slight acquaintance.

It would however be far from the truth to say that he has remained entirely uninfluenced by the progress made during the last century. A few typical illustrations will serve to indicate one of the directions in which we must look for advance. (1) The ryot, who grows sugar cane, has entirely discarded the old wooden mills in favour of those made of cast iron, with the result that the work is done with less labour and a higher percentage of juice is extracted. (2) In many parts of the South of India the weavers prepare their warps on rotary mills and in some places the advantage of subdivision of labour is so far recognised that the preparation of warps on these mills has become a distinct business. (3) The extraction of oil from seeds is largely done in screw presses worked by hand in place of the old-fashioned rotary wooden mill. (4) The fly-shuttle loom has been substituted for the native hand loom among the weavers of certain districts of Bengal, with the result that their speed in weaving has been doubled. (5) Wood and metal workers almost invariably use some tools of European manufacture. (6) Singer's sewing machines are to be found in almost every tailor's shop in the country and, although these machines are somewhat delicate and complicated pieces of mechanism, the facilities for the repair or renewal of parts have been so widely diffused that the tailors find no difficulty in keeping them in working order.

It would be easy to multiply illustrations of this kind, especially in regard to agriculture and its dependent trades and those industries which have been influenced by the workshops and factories to be found in the centres of modern industrial activity. We may rest assured that there will be no opposition to the introduction of improved tools or improved methods of working if it can be clearly shown that they are real improvements. The reputation that Indians are averse from all change and are obstinately wedded to the antiquated ways of their forefathers is not justly deserved. They are conservative but they know their own business fairly well and many of the so-called improvements which they have rejected were really unsuitable innovations.

The Development of Small-Scale Industrial Enterprises

India offers a great problem to the civilised world. It has abundance of cheap labour which, if properly trained, would be skilled; it needs to be shown how to apply this labour to the best advantage. The whole trend of modern progress has been to replace the man by the machine, to replace the individual by the factory and the isolated factory by the organised trust. Where labour is dear this system has developed most largely and human ingenuity is ever exercised in extending the scale of operations. We have introduced the system into India but it has not yet taken root. We may either regard it as inevitable that it should ultimately be established or we may adopt an alternative and apply the resources of science, engineering and commercial experience to a great attempt to raise the worker and pit his skill, ingenuity and adaptability against the monstrous growths produced by the abnormal development of the mechanical arts. The problem ever before the modern industrial world is to devise means of dispensing with labour, to cheapen production by making it more automatic. The success has been remarkable but it has been purchased somewhat expensively; it is possible that we might now with advantage turn our attention to developing the function of the man rather than the power of the machine, to evolving a system the object of which should be to employ human labour to the greatest extent possible and in the way most advantageous to the individual man.

The conditions in India are suitable for such an experiment.

It has not yet accepted the factory system nor will it do so willingly, the undivided family has to be reckoned with and the extreme subdivision of property renders productive effort on a large scale difficult. Comfort rather than luxury, a moderate rather than a vast fortune—these are the ideals of enlightened Indians. It would be foolish to imagine that as India now stands in relation to the British Empire and to the rest of the world it could disregard the external influences to which it must always be subjected but there is no reason why it should not strive to move forward to a goal more in harmony with its own traditions than is that presented by Western civilisation.

In England, America and Australia there is a widespread movement in favour of small holdings instead of large farms and much evidence is now available to show that where the conditions are suitable this method of cultivation tends to the more general diffusion of prosperity and contentment. In India small holdings are universal. Industrial operations, except in so far as they have been changed by the advent of Europeans, have also been carried on by men of small means and they have survived to the present day mainly because of the inherent vitality of such a system. There is no necessity to abandon this way of working but we must improve it and bring the status of Indian artisans to the same level as in other countries which have in recent years made so much progress.

There are greater prospects of the small manufacturer being able to compete with the big than there were a few years ago, as recent progress in science and the mechanical arts has done much to raise the efficiency of working on a small scale. Not by any means in all directions but in some and those more particularly which are likely to flourish in India. The cost of power has been enormously reduced especially in the case of very small plants, so that the small user of power is in a much better position to compete with the large user than was possible only a few years ago. There is in consequence a perceptible reaction against production on a large scale and a tendency to make greater use of the elasticity which allows small works more readily to adapt themselves to changes and fluctuations in trade, cyclical or otherwise.

Again it is evident even in the most highly developed industrial countries that the human factor is becoming more important and in the distribution of profits between capital and labour the latter is demanding a larger share. It must not be imagined that the great primary industries are materially affected in this way—they are not and it might even be contended that the ever-increasing perfection of mechanical appliances is rendering the labour question one of constantly diminishing importance. With this phase of industrialism we are not at present concerned. It may be fully trusted to look after itself, but there is no likelihood that it will ever be greatly developed in India excepting in certain localities. The main reason for this is that over the greater part of the country there are no special natural resources.

There is no doubt that the various castes and groups of artisans in India maintain themselves against the present competition of European industrialism and that, although they may have suffered severely, they have not succumbed. Equally it is certain that much could be done to render their work more effective both by improving their methods and by supplying their trades with a commercial organisation that would bring their proceeds into the markets where the demand is greatest. Obviously Government is the only agency by which such a change can be brought about; the greatest difficulties will probably arise from the opposition of the artisans themselves, who care little about education and are averse from abandoning the free and improvident life they have always led. In framing a policy the provision for a suitable education must come first. It must be of a simple character and have a direct bearing upon their future prospects. It must appeal to the people and attract them by its direct reference to their everyday life and, above all, it must not be regarded as the first rung of a ladder which will elevate a few above their fellows; its object should be to raise the mass from their lethargy and ignorance to a higher level, whence in due time a fresh start may be made. For the present, possibly for a long time to come, we must look to the educated classes, as we now understand that term, to furnish the men who will lead the industrial groups and bands which it should be a primary duty to organise.

(To be continued)

THE CHROMOSOMES IN RELATION TO THE DETERMINATION OF SEX[1]

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Introduction

As Mr. Doncaster has recently indicated in this journal,[2] the view that sex is determined by conditions external to the germ has in recent years slowly given way to the belief that it is primarily established by an automatic mechanism within the germ-cells. The change of front in regard to this time-honoured and most interesting problem has been due to the convergence of evidence from two widely different lines of inquiry, one relating to the facts of sex-heredity, the other to the cytological constitution of the germ-cells. It is the purpose of this review to consider especially the second of these; but the bearing of the data will be more clearly apparent if an account of them is prefaced by a brief mention of a few of the more important facts derived from other sources.

Though many of the earlier experiments seemed to show that sex may be determined or influenced by conditions of nutrition and the like, that act upon the fertilised egg or the embryo during its development, this conclusion has received little or no support from more recent investigation. It is true that in the case of hermaphrodites—such as Hydra, or the prothallia of ferns—the organs of one sex or the other may be delayed in development, or altogether suppressed, by particular conditions of the environment. It seems also clearly to be proved that the change from parthenogenetic to sexual development in diœcious forms (which involves the appearance of males and of sexual females) may likewise be influenced

[1] The following review, prepared at the invitation of Prof. Farmer, follows the same general lines as an address on the subject given by the author before the American Association for the Advancement of Science at the Baltimore meeting in December 1908, and printed in the issue of *Science* for January 8, 1909. The results of a number of important contributions to the subject, since published, are here incorporated.

[2] Science Progress, July 1909, IV. pp. 90-104.

or induced by external factors; and in at least one case of sexual reproduction (that of *Dinophilus apatris*, according to Malsen) the ratio between male and female eggs may thus be somewhat modified. Nevertheless, most of the exact researches of recent years strongly indicate, if they do not fully prove, that in diœcious organisms, when once the egg is fertilised its sex is already fixed; and they show, further, that in some cases even the gametes can be distinguished as male-producing and female-producing (I do not say male or female) before their union.

Some of these facts have long been known. In the parthenogenetic reproduction of certain phylloxerans and rotifers there are large and small eggs, which produce respectively females and males. The same is true of the sexual eggs of *Dinophilus apatris*, and has been asserted of several other cases. More recently, researches by the Marchals, Strasburger and Blakeslee demonstrate that the spores of diœcious mosses and liverworts (Blakeslee finds analogous phenomena in some of the fungi) are already predestined as male-producing and female-producing, probably in equal numbers; while the work of Correns and Noll indicates that the same is true of the pollen-grains of certain diœcious flowering plants. Apart from this direct evidence, there is a considerable and increasing body of data—such as the similar sex of double monsters and of multiple embryos derived from the same egg—to show that even when the sex of the germ-cell escapes observation by the eye, it is already determined at the beginning of development. In the famous case of the honey-bee the eggs produce females if fertilised, males if unfertilised, and the same appears to be true, or at least the rule, in the wasps, ants and certain other Hymenoptera.[1]

On the experimental side it has been made known by crossing experiments that the sex-characters are in certain cases transmitted according to a definite law that is independent of external conditions and is at least nearly akin to Mendelian heredity. I need not review a subject that has been so recently and ably discussed by Bateson.[2] It need only be recalled that

[1] The evidence in the case of the bee seems overwhelmingly strong, despite the fact that the Dzierzon theory has been disputed. In the ants and wasps the case is not so clear.

[2] *Mendel's Principles of Heredity*, 1909.

the cross between male and female gives typically a result (approximate equality of the two parental forms) that is the same as that of the cross between a heterozygote and a homozygote recessive. That this is something more than a mere coincidence is rendered probable by direct experiment, notably by Correns's remarkable results with reciprocal crosses between different species of diœcious plants, which lead him to conclude that one sex is in fact homozygous in respect to the sexual characters, the other heterozygous.

All this, and much more in the same direction, unmistakably indicates that sex is determined by a mechanism that pre-exists in the germ-cells. The most direct and obvious evidence of this is given by the cytological observations to which attention will be directed in this review. These observations establish the fact that in certain animals (including many air-breathing arthropods, and one or two echinoderms and nematodes) the sexes differ visibly in respect to the constitution of the cell-nuclei. They show, further, that these nuclear differences must be established at the time the eggs are fertilised, and are traceable to original differences in the gamete-nuclei before fertilisation takes place. The somewhat complicated data on which this conclusion rests may be more readily understood after a brief preliminary statement of the most essential facts, beginning with the group best known in this regard, the insects.

In many species of insects there are two kinds of spermatozoa, equal in number, which in the early stages of their development differ visibly in respect to nuclear constitution; while there is but one kind of egg, which is in nuclear type identical with one of the classes of spermatozoa. That is to say, if the two kinds of spermatozoa be designated as the "X-class" and the "Y-class," respectively, the eggs are all of the X-class. The male may, accordingly, be designated as the *heterogametic* sex, the female as the *homogametic*. The evidence demonstrates (though at one point it still remains indirect) that *fertilisation of the egg by the X-class of sperm produces females, by the Y-class males.* General formulas for sex-production in these animals may accordingly be written:

$$X + X = \text{Female and } X + Y = \text{Male},$$

which is identical in form with the Mendelian formulas

$$R + R = RR \text{ and } D + R = DR,$$

The evidence of this is most complete in certain representatives (now including nearly a hundred species) of the Hemiptera, Coleoptera, Orthoptera, Diptera and Odonata; but the same principle no doubt applies to the myriapods and arachnids, some of which are also known to produce two classes of spermatozoa exactly corresponding to those of the insects. A precisely similar relation has very recently been discovered by Boveri and Gulick in the nematode *Heterakis*[1]; and Boveri believes that the facts reported by Boring in the classical object, *Ascaris megalocephala*[2] are open to the same interpretation. On the other hand, it has recently been demonstrated by Baltzer[3] that in certain sea-urchins the above relation is in a general sense reversed, there being two kinds of eggs and but one kind of spermatozoa; in other words, in these animals it is the female that is the heterogametic sex, while the male is homogametic. It seems probable that the same condition exists in *Dinophilus apatris*, where two kinds of eggs have long been known. The experimental evidence suggests that this is true also of certain other animals of which cytological data are not yet at hand. As will be seen, the results obtained on the sexual forms of insects are fully confirmed by investigations on some of the parthenogenetic species in which all the fertilised eggs produce females (aphids and phylloxerans).

The Cytological Facts

In examining the cytological phenomena more in detail (beginning with the insects) we have before us the following two principal questions:

(1) How does the nuclear dimorphism of the spermatozoa arise?

(2) What is the evidence that the two classes of spermatozoa are respectively male-producing and female-producing?

It is necessary at the outset to bear clearly in mind certain familiar general relations of the chromosomes. In the sexually produced organism the somatic cells and earlier generations of germ-cells possess a double or *diploid* group of chromosomes. At a certain period in the life-cycle the diploid number undergoes reduction to one-half in those cells that are to produce

[1] *Arch. f. Zellforsch.* 1909, 4, 1. [2] *Ibid.* [3] *Ibid.* 2.

the gametes; and this reduced or *haploid* number is delivered to the latter when ready for their union. Fertilisation restores the diploid number by bringing together two haploid groups; and the diploid number thereafter persists until reduction again occurs. Reduction takes place in the process of maturation, which culminates in two *maturation-divisions*. In all higher animals these divisions are the last two preceding the formation of the gametes. In higher plants the corresponding divisions occur in the sporophyte, or asexual generation and give rise to asexual spores which only produce the gametes after the formation of a sexual generation or gametophyte.

(I) The sexual dimorphism of the spermatozoa in insects results from an asymmetrical distribution of the chromosomes that occurs in *one* of the maturation-divisions (spermatocyte-divisions)—in some cases the first (Orthoptera, Coleoptera, Diptera), in other cases the second (many Hemiptera). Even closely related species may differ in the order of division; for example, in the family of Coreidæ the differential division is the second in most species (*e.g.*, *Anasa*, *Protenor*), but in a few cases is the first (*Archimerus*, *Pachylis*). In either case the result is to produce two exactly equal classes of spermatids and of spermatozoa.

The details of the differential division vary considerably in different species, but all are reducible to a common plan. The most familiar case is that originally discovered by Henking in the hemipteran genus *Pyrrhocoris*,[1] and subsequently found by other observers in many other insects and in a few myriapods and arachnids. In this type a single chromosome fails to divide with the others, and passes undivided to one pole. Half the spermatozoa thus receive one more chromosome than the other half. Examples of the actual numbers are: *Pyrrhocoris* 12 and 11, *Anasa*, *Euthoctha*, *Narnia* 11 and 10, *Protenor* and *Alydus* 7 and 6, *Brachystola* and many other Acrididæ 12 and 11, *Anax* 14 and 13. Close study leaves no doubt that the chromosome which thus fails to divide is always the same particular one, at least in some species, where it may positively be identified by its size; in *Pyrrhocoris* or *Protenor*, for example, it is much the largest of all the original chromosomes. Half the spermatozoa are thus characterised by the presence of a particular chromosome that is absent in the other half; and, as will presently be

[1] *Zeitschr. f. Wiss. Zool.* 1891, 51.

shown, those in which it is present are the female-producing or "X-class." The chromosome in question is variously known as the "accessory chromosome" (McClung), "odd chromosome," "heterochromosome" (Montgomery), "heterotropic chromosome," "idiochromosome," "X-chromosome" (Wilson), etc.[1]

Fig. 1, *a* shows the differential division of *Protenor* (second

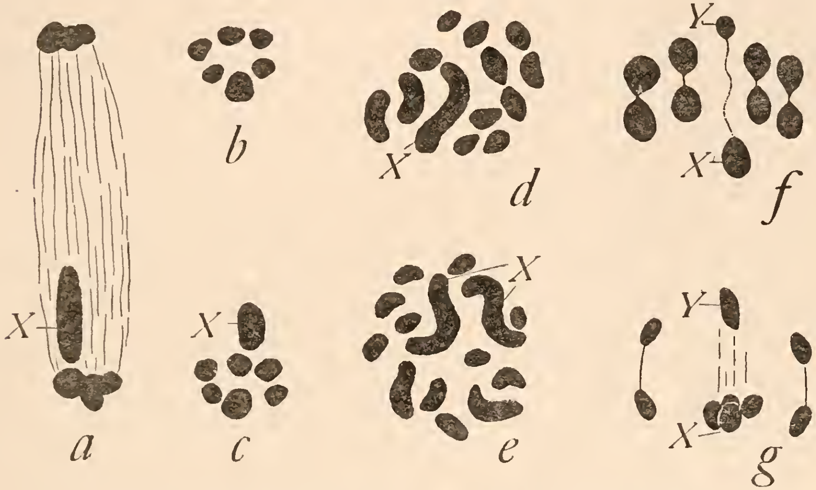

FIG. 1.—Exact drawings of the chromosomes in *Protenor* (*a–e*), *Euschistus* (*f*), and *Gelastocoris* (*g*); those of *Protenor* and *Gelastocoris* from photographs.

a, side-view of the differential division (second spermatocyte) in *Protenor*, showing the large X-chromosome approaching one pole; *b* and *c* the two daughter chromosome-groups, both from the same spindle, of a stage like *a*, seen from one pole, showing all the chromosomes, the large X-chromosome (somewhat foreshortened) at one pole; *d*, diploid (spermatogonial) group of the male; *e*, diploid group of the female.

f, differential division of *Euschistus* in side-view, X and Y separating.

g, corresponding view of differential division of *Gelastocoris* (from Payne), at a slightly later stage.

spermatocyte-division) in side view, with the large X-chromosome nearly at one pole; *b* and *c* show the two poles (from

[1] A fact of importance for the practical study of the X-chromosome is that in the differential division it often lags behind the others. This is, however, by no means invariable, even in the same group. Thus, in *Anasa* or *Protenor* it always lags, but in the nearly related *Syromastes* it always passes to the pole in advance of the others and never even enters the equatorial plate. The latter case is the rule in the Orthoptera. In the dragon-fly, *Anax*, it sometimes lags, sometimes precedes the others. Another interesting and perhaps important detail is that the X-chromosome differs from the others in behaviour during the growth-period of the spermatocytes, always having throughout this period a compact consistency and rounded form like a nucleolus, while the ordinary chromosomes are in a diffuse and lightly staining condition. In the spermatogonia, too, it often differs from the others in certain definite ways.

one spindle) in end view, all the chromosomes being in evidence, six of them at one pole and seven at the other. (Cf. fig. 2.)

In a second type the X-chromosome is double, consisting of two components which pass together to one pole. Examples of this are *Syromastes* (Gross), *Phylloxera* (Morgan), and *Agalena* (Wallace). The X-spermatozoa receive in these cases two more chromosomes than the Y-class (fig. 2).

In a third and very common type, widely distributed among the insects, the X-chromosome is accompanied by a synaptic "mate" or "partner" (usually smaller than X) which passes into the Y-class of spermatozoa, and may therefore be called the Y-chromosome or "Y-element."[1] In one of the maturation-divisions (the first in Coleoptera and Diptera, the second in Hemiptera) X and Y are found in close contact, but as the division proceeds they separate and pass to opposite poles, while all the other chromosomes divide equally. The final result is that all the spermatozoa possess the same number of chromosomes, but half contain X and half Y (*Lygæus*, fig. 2; *Euschistus*, fig. 1, *f*). Here again it is certain in some species, and extremely probable in all, that the X- and Y-chromosomes are of constant identity. Comparative studies have made it very probable that from this type, by the disappearance of the Y-chromosome, has arisen the first type (with an odd or "accessory" chromosome); for various intermediate stages have been found in different species between those in which Y is almost as large as X (*Benacus*, *Mineus*) and those in which it is very minute (as in many Coleoptera and Diptera). Its final disappearance would leave the X-chromosome without a mate. On the other hand, there are some forms (*e.g.*, *Nezara*, *Oncopeltus*, fig. 2) in which X and Y are equal in size, and cannot be distinguished by the eye; but these two chromosomes agree precisely in all other respects with the X- and Y-chromosomes of other species. The possibility is thus suggested that there may be many cases in which the X- and Y-chromosomes may be present, though not visibly different from the others.[2]

From the types just described, with both X- and Y-chromosomes, an interesting series may be traced in another direction,

[1] In this type X and Y are identical respectively with the chromosomes which have been called the large and the small "idiochromosomes."

[2] See Wilson, *Journ. Exp. Zool.* 1905–6.

for a knowledge of which we are indebted mainly to Payne.[1] In *Fitchia* and *Thyanta* X is double, in *Prionidus* and *Sinea* it is triple, and in *Gelastocoris* quadruple, forming with the Y-chromosome in these respective cases triad, tetrad, or pentad elements (fig. 2; fig. 1, *g*). In every case the compound element behaves in maturation like a single XY pair, the single Y-chromosome passing to one pole and the compound X-element to the other. All these cases are evidently reducible to the same fundamental type. The X-chromosomes, though separate in the diploid groups, behave in maturation like a single element and always pass together to one pole. It is a plausible view, advanced

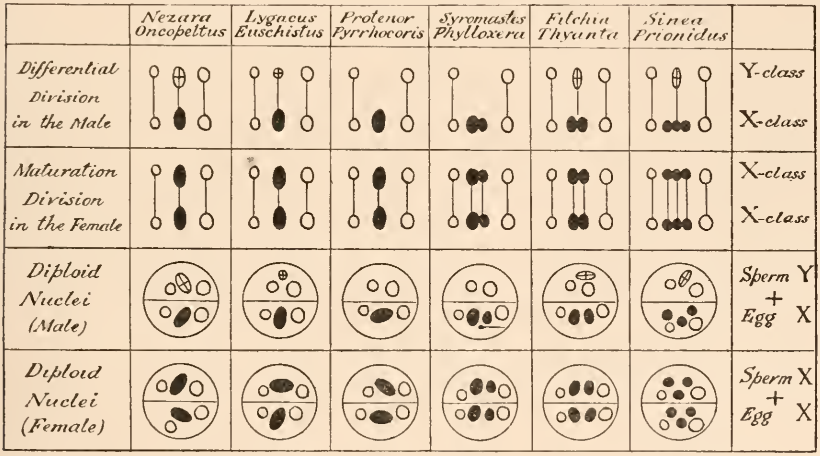

FIG. 2.—Diagrams of different types of the chromosome-relations in the Hemiptera.

None of the figures show either the exact number or size-relations of the chromosomes. In all cases the ordinary chromosomes are in outline, the X-chromosomes in black, and the Y-chromosome with cross bar. The maturation-divisions in the females of the types illustrated have actually been worked out only in *Protenor* (Morrill).

by Payne, that the compound X-element in these cases was originally a single chromosome (large idiochromosome) which has secondarily become divided into a number of components.

(II) The foregoing are the most essential facts regarding the dimorphism of the spermatozoa. What, now, is the evidence that this dimorphism is connected with sex-production? Immediate ocular evidence of the fact has not yet been obtained; for no one has yet been able to trace completely the history of the two kinds of spermatozoa in the fertilisation of the insect egg. Nevertheless, that the two kinds must be respectively

[1] *Biol. Bull.* 1909, xvi.

male-producing and female-producing is indicated by a chain of evidence so strong and detailed as apparently to leave no escape from the conclusion. The crucial evidence is given by a comparison of the diploid chromosome-groups of adults and embryos of the two sexes. These groups are found to show certain characteristic and constant differences which are precisely such as would result from fertilisation of the eggs by the two respective classes of spermatozoa. In all forms such as *Pyrrhocoris* or *Protenor*, which have a single X-chromosome in the male, the female diploid groups differ from the male in the presence of one additional chromosome (as was first shown by the writer in certain Hemiptera). Further, the additional chromosome in the female is a duplicate of the unpaired X-chromosome of the male, as is particularly well shown in *Pyrrhocoris* or *Protenor*, where the X-chromosome is at once recognisable by its size in both sexes. The female groups are otherwise identical with those of the male (figs. 1*d*, *e*; fig. 2). These results, first reached in the case of dividing cells of the testis and ovary, have now been extended to cells from other organs and tissues, and to embryonic stages (Morgan, Stevens, Morrill), so that the sex of the embryo may at once be distinguished, even in the earliest stages of cleavage, by mere inspection of the chromosome-groups.[1]

Such a diploid group as that seen in the female gives rise, upon reduction, to a haploid group that contains but one X-chromosome (fig. 2); and each mature egg, as well as each polar body, contains such a group. That reduction in the female should produce such a haploid group is evident from the composition of the diploid groups, and the fact has now been directly observed by Morrill[2] in certain insects (*Anasa*, *Protenor*, *Archimerus*, *Chelinidea*), and by Boveri and Gulick[3] in the nematode *Heterakis*, which possesses a typical single "accessory" or "X-chromosome" in the male. Reduction in the male, on the other hand, results in the presence of the X-element in but half the spermatozoa, while half are without it. In *Heterakis*, for example, all the mature eggs receive five chromosomes, while half the spermatozoa receive five and half but four. A moment's consideration shows that the characteristic diploid combination of the male will arise upon fertilisation of the egg by a spermatozoön of the Y-class

[1] Morrill, *Science*, December 31, 1909. [2] *Op. cit.* [3] *Op. cit.*

(lacking X), the female combination upon fertilisation by a spermatozoön of the X-class. Fig. 3 shows this in diagram, and the result may be formulated as follows:

Egg X + Spermatozoön no X = Zygote X (male),
Egg X + Spermatozoön X = Zygote XX (female);

the nuclei being otherwise of identical composition.

In the type represented by *Lygæus* or *Euschistus*, where a Y-element ("small idiochromosome") accompanies the X ("large idiochromosome") the same conclusion is shown even more clearly; for in many species the Y-element is the

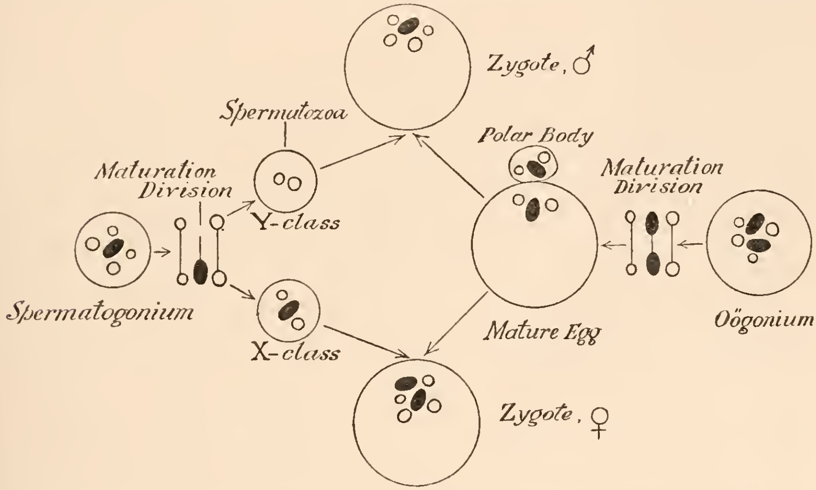

FIG. 3.—Diagram to show the relation of the two classes of spermatozoa to fertilisation. The formation of gametes in the male is shown at the left, in the female at the right; fertilisation, producing the male or female zygote, in the middle.

smallest of all the chromosomes and is therefore readily recognisable. In all such cases—as was first shown by Miss Stevens in the Coleoptera and the writer in the Hemiptera—this chromosome is confined to the male, while in the female its place is taken by a second X-chromosome ("large idiochromosome"). The female diploid groups contain accordingly XX, the male XY (being otherwise identical); and upon reduction each mature egg contains one X, while half the spermatozoa contain X and half Y. Obviously, therefore, the same fertilisation-formulas apply as in the former case, if Y be inserted in its proper place, *i.e.*,

Egg X + Spermatozoön Y = XY, Male,
Egg X + Spermatozoön X = XX, Female.

This result is still more strikingly shown in cases where the X-element consists of two or more components. Whatever be their number in the diploid groups of the male—two in *Syromastes* or *Fitchia*, three in *Prionidus* or *Sinea*, four in *Gelastocoris*—twice this number appear in the female groups. Thus the general formulas X + X = Female and X + Y = Male (in which Y may = zero) are applicable to the chromosome-combinations characteristic of the two sexes throughout this whole series of species, whatever be the composition of the X-element (fig. 2).

This is hardly the place to trace in detail the history of the subject or to enter upon the controversies to which it has given rise. It may, however, be recalled that the first basis for the cytological results was laid by Henking, in the work already cited, by his fundamental discovery of the dimorphism of the spermatozoa in *Pyrrhocoris*, though he did not recognise the meaning of the phenomenon. The results of this remarkably able investigator, long neglected, were first confirmed by Paulmier in *Anasa*,[1] and later by many others. The fruitful suggestion that the two sexes arise upon fertilisation of the eggs by the two respective classes of spermatozoa is due to McClung,[2] who ably supported his view by a series of interesting arguments. Unfortunately, however, no evidence was at that time available in regard to the female chromosome-groups, and McClung's conclusions therefore necessarily remained of a somewhat hypothetical character. As the event showed, his view was correct in principle; but from *à priori* considerations he was led (naturally enough) to regard the X-class of spermatozoa as male-producing. The direct and decisive evidence, first produced by Stevens and Wilson in 1905–6[3] by a detailed comparison of the male and female diploid chromosome-groups, showed this view to be the reverse of the fact; and the results of these authors have since been confirmed by many others, among whom may be mentioned Montgomery, Boring, Lefevre and McGill, Wassilieff, Gutherz, Morgan, von Baehr, Jordan, Davis and Morse. Boveri's recent extension of the result to the nematodes has been mentioned

[1] *Journ. Morph.* 1899, **15**, Suppl.

[2] *Biol. Bull.* 1902, **3**.

[3] See a series of papers by each of these authors in the *Journ. Exp. Zool.* with literature there cited.

above. On the other hand, some observers have sought to discredit these results, including even so unmistakable a fact as the differential division and the resulting dimorphism of the spermatozoa. I think it may fairly be said, however, that steadily accumulating evidence from many sources has now placed the essential facts upon too firm and broad a basis to admit of doubt. Scepticism has in some cases arisen from insufficient observation or unsuitable technique, in others from a confusing of different types or the examination of unfavourable material. It has doubtless been due in some cases also to the observation of forms in which two kinds of spermatozoa cannot be distinguished, either because the female is the heterogametic sex, or because the X- and Y-elements are of equal size.

Among the most recent discoveries that have confirmed and extended the general result none possess a greater interest and importance than those of Morgan, von Baehr and Stevens on the phylloxerans and aphids. These animals, like the daphnids, rotifers and bees, are well adapted to afford a crucial test of the matter; for in them, as has long been known, all the fertilised eggs produce females. This seems at first sight to present a formidable difficulty; but the detailed study of the phylloxerans and aphids converts this apparent difficulty into a remarkable confirmation. Morgan[1] discovered in *Phylloxera*, and von Baehr[2] independently in *Aphis*, that half the secondary spermatocytes are much smaller than the others and degenerate without producing spermatozoa. Further —and this is the crucial point—those spermatocytes, and those alone, which produce the functional spermatozoa receive the accessory chromosome or X-element (in *Aphis* a single chromosome, in *Phylloxera* a double element). In these animals, accordingly, *only the X-class, or female-producing spermatozoa come to maturity, while the Y-class, or male-producing spermatozoa, are abortive.* All the fertilised eggs therefore produce females. A long-standing riddle thus receives a simple and elegant solution, and one that is fully in accordance with the facts seen in the ordinary sexual forms. The bees and ants exhibit phenomena that are evidently of somewhat similar type, though the work of Meves and others still leaves us in the dark regarding the details of the chromosomes. It seems probable,

[1] *Journ. Exp. Zool.* 1909, 7. [2] *Arch. f. Zellforsch.* 1909., 3.

however, that the same principle will be found applicable to these animals, and a similar expectation is created in case of the rotifers and daphnids.

These observations establish another and most important point. It is a familiar fact that the parthenogenetic egg of the aphid or phylloxeran (like that of the daphnid) whether male-producing or female-producing, forms but one polar body, and undergoes no general reduction of the chromosomes. Both Morgan and von Baehr find that the males (produced parthenogenetically) nevertheless differ from the females in the absence of one X-element from the diploid groups; and Morgan's work shows, almost with certainty, that this element must be extruded from the egg at the time the polar body forms. This remarkable discovery throws a new light on the whole question of sex-production in parthenogenesis, and indirectly demonstrates that it is not fertilisation *per se* that determines sex but a constellation of factors that may be established by other means as well.

We turn now to Baltzer's no less important work on the sea-urchin egg,[1] which deals directly with the fertilisation-stages. His results are based on the investigation not alone of normally fertilised eggs but also of dispermic, merogonic and hybrid fertilisations; and the consistent results from these various sources give the strongest grounds for their acceptance. They show that in these animals it is the *female* that is the heterogametic sex, while the male is homogametic.

The fertilised eggs or zygotes and early cleavage-stages all contain the same number of chromosomes (36), but are of two types, in one of which all the chromosomes may be equally paired, while in the other there is one pair that consists of quite unlike members. These may provisionally be called "F" and "G." Study of the gamete-nuclei (in the fertilisation-stages) proves that F is always derived from the egg-nucleus, and never appears in any of the sperm-nuclei. F is however present in only half the egg-nuclei, its place being taken in the other half by G; while all the sperm-nuclei alike contain, in place of F a chromosome indistinguishable from G. None of the eggs were reared to maturity, nor was the history of maturation followed out. It nevertheless seems certain that fertilised eggs containing F must be females (since F is confined

[1] *Op. cit.*

to the egg-nucleus, and hence to the female line), and that those which lack F are males. Sex-production in these animals must therefore conform to the formulas

Egg F + Spermatozoön G = FG, Female,
Egg G + Spermatozoön G = GG, Male.

It is not known what is the exact relation between these sex-chromosomes and those of the insects and nematodes. It may be that F corresponds to X and G to Y—*i.e.*, that the female combination is XY and the male YY; but there are other possibilities. However this may be, it is evident that the same principle appears in both cases. In both, one sex is heterogametic, the other homogametic. In both, accordingly, one-fourth of all the gametes are of different nuclear constitution from the others; but in the insects these gametes (Y-class) are confined to the male line, and are male-producing, while in the sea-urchin the reverse relation exists.[1]

It is, of course, impossible to say in what measure the foregoing conclusions will be found to hold true in other animals and in plants; but the convergence of cytological and experimental results from widely separated groups leads us to believe that the principle that they display is of wide validity. The cytological evidence is now drawn from groups as far apart as the insects, nematodes, and echinoderms; and there is some evidence that it may extend to the vertebrates on the one hand (Vejdovský, Guyer), and possibly even to the Protozoa on the other (Schellack, Léger, and Duboscq), though both these cases are doubtful. A wide and inviting field here lies open to further observation; and we may hope to see phenomena of more or less similar type demonstrated in other groups where they have hitherto escaped detection. It is probable, however, that cytological differences as marked as those appearing in the insects are exceptional, and that in many forms no visible differences exist. Even in this case, however, the same principle may rule, as is pointed out below.

[1] Beard, in his interesting paper on the determination of sex (in *Zool. Jahrb. Anat. Ont.*, 1902, **16**), advanced the speculative view that the gametes were primitively fourfold, including two kinds of eggs and two kinds of spermatozoa. He assumed, however, that in all existing Metazoa one kind of spermatozoa has become non-functional, and that sex is now determined solely by the egg. "The spermatozoön has, and can have, absolutely no influence in determining the sex of the offspring." This particular assumption was wrong, as is now apparent; but we should recognise the essential nucleus of truth in his conception.

Some Theoretical Aspects of Sex-production

Thus far we have held closely to the observed facts, leaving aside all matters of theory; for the conclusion that the two classes of gametes in the heterogametic sex are respectively male-producing and female-producing is not in any sense a theoretical construction, but a direct and apparently unavoidable conclusion from accurately known data. Now, however, we may be allowed to consider the theoretical aspect of the problem. In making some inquiry into the causal relation between sex and the chromosomes, I wish not to appear as a special pleader for any particular theory. We still know too little of the physiological meaning of the chromosomes to commit ourselves unreservedly to any fixed theory or interpretation regarding the part that they play in sex-production. My intention is only to analyse briefly the possibilities suggested by the facts. If certain of these possibilities are characterised as more probable than others, it is with entire readiness to accept a different view as soon as new facts may change the aspect of the problem.

The obvious view is that the chromosomes—specifically the X-chromosomes—are actual sex-determining factors; that the egg becomes male or female according as it is fertilised by one kind of spermatozoön or the other; and that it is purely a matter of chance which of these kinds enters the egg. I incline to believe that this obvious view is the correct one, though it requires some qualification. It is impossible to admit that the definite relation between sex and the chromosomes can be a merely casual one that is devoid of all significance in sex-production. The most that can be urged against the obvious view is that the chromosome combination may be not the cause of sex but one of its results. There are indeed some facts that may, if we choose, be interpreted in this sense. For instance, it is possible that in the differential division, which s-nucleus, the X-class from the Y, the two poles of the dividing ce [illegible] be already predestined as male-producing and female-producing, irrespective of the chromosomes—*i.e.* that it is not a matter of chance to which pole the X-chromosome wanders. We find, it is true, no hint of such predestination in the ordinary forms of spermatogenesis; but in the phylloxerans and aphids, as Morgan and von Baehr have shown, the differential division is unequal, and *the X-chromosome always passes into the larger* *cell.* It is, of course, possible to assume that the inequality is itself determined by the asymmetrical distribution of the X-chromosome; but on the whole it seems more natural to conclude that the X-pole is already predestined before the X-element passes into it. Again, as Morgan discovered in *Phylloxera*, the males, produced *parthenogenetically*, possess but one X-element instead of two (thus conforming to the general rule), one of the X-elements being eliminated in the maturation of the male-producing egg; but this egg is already distinguishable as male-producing, by its smaller size, *before the elimination takes place.* It thus becomes rather a nice question whether the egg should be regarded as actually male before the characteristic male chromosome-combination has been established. We may, I think, reasonably take the epigenetic view that sex is first really established or determined when all the essential sex-producing factors have been brought together in the egg. The chromosome-combination may be taken as the culmination of this process, and hence as a convenient point of departure. The following discussion is based upon the assumption (there is no complete proof of the fact) that the X-chromosomes are one of the essential factors; and if the foregoing qualifications are not lost sight of, we shall not, I think, be seriously misled by the further assumption that they are the decisive factor. With this explanation let us examine some of the possibilities that present themselves.[1]

Our first impression is that the X-chromosomes must be the bearers of specific and opposing factors (such as different chemical substances) that are *per se* male-determining or female-determining; but this view speedily involves us in a maze of difficulties, of which only a few will here be considered. For instance, under this view the single X-chromosome of the male must be a male-determining factor (since in the males of many species it is the only sex-chromosome present); yet its introduction into the egg establishes the *female* condition. We are therefore driven into the following additional assumptions: (1)

[1] The discussion of these complex possibilities may be simplified if for the present we leave the Y-element out of consideration. I do not mean to imply that this element is of no importance. On the contrary, until its significance has been made clear, no statement of results can be regarded as more than provisional. Since, however, the Y-element may be either present or absent, even in different individuals of the same species (*Metapodius*), we must perforce for the present lay the question on the table.

that in the egg which produces a female this male-determining X-element meets a dominant female-determiner represented by the X-chromosome of the egg-nucleus; (2) that after maturation there are two kinds of eggs—half containing the female-determiner (X_1) and half the male-determiner (X_2); (3) that the former kind are fertilised only by the X-class of spermatozoa, the latter kind only by the Y-class ("selective fertilisation").[1] These assumptions are in essential agreement with an earlier hypothesis of Castle, based on more general grounds.[2] They form the first of two alternative interpretations offered (not advocated) by the writer in 1906[3] as *possible* solutions of the problem. This interpretation seems to me, however, improbable in itself; and when we look further afield the improbability becomes still greater. For example, in the bee, where females arise from fertilised eggs and males from unfertilised, the following assumptions must apparently be made: (1) the fertilised egg contains, as usual, a dominant female-determiner (X_1) and a recessive male-determiner (X_2); (2) in maturation of the egg the female-determiner is eliminated, leaving the mature egg with only the male-determiner; (3) in fertilisation a dominant female-determiner is introduced by the spermatozoön. But this is an utter absurdity; for, by the assumption, the mature unfertilised egg from which the male (and hence the spermatozoön) arises has eliminated the female-determiner (!).[4] A multitude of difficulties akin to this arise under any form of the assumption that specifically distinct and opposing male and female "tendencies" or "deter-

[1] Such a selective fertilisation was suggested on other grounds, independently and almost simultaneously, by several writers. Beard, in the work already cited (1902), postulated this as the primitive mode of fertilisation in the original types having fourfold gametes, but did not apply it to existing Metazoa. It was suggested by McClung in the same year as a possibility in insects having an accessory chromosone (*op. cit.*). The same assumption formed an essential part of Castle's earlier Mendelian theory of sex (1903) since abandoned.

[2] *Bull. Mus. Comp. Zool.* 1903, **27**.

[3] *Journ. Exp. Zool.* **3**.

[4] This difficulty (first pointed out by Castle) might be met by the further assumption, which has in fact been made by Beard and others, that there may be two kinds of unfertilised eggs, male and female, of which only the female are capable of fertilisation. There is, however, no evidence of this in the bee; while in Hydatina the results of Maupas, which have been confirmed by Whitney (*Journ. Exp. Zool.* 1909), show with at least probability that the egg, which if unfertilised produces a male, produces a female if fertilised.

miners" are borne by different chromosomes or different gametes; and this remains true whether we assign these "determiners" to the chromosomes or to something else.

We are thus led, or rather driven, to seek a different kind of interpretation. Could we regard the sexual differentiation as due primarily to factors of a quantitative rather than a qualitative nature, most of these difficulties would disappear; and such a conception would be in harmony both with the cytological facts and with the experimental evidence regarding sex-heredity. Several views of this general type have in fact been suggested, some of them quite independently of the facts here under consideration. For example, Richard Hertwig advanced in 1905[1] the view that sex depends on the "Kernplasma-relation," or ratio of nuclear to protoplasmic mass, the female being characterised by a relatively smaller nuclear mass than the male. In support of this view, Prof. Hertwig has contributed many interesting experiments, and has suggestively discussed the facts in several successive papers. Again, T. H. Morgan suggested in 1903[2] that the determinative factor in the bee may be quantitative, the sex of the egg depending merely on whether the cleavage-nucleus is made up of two germ-nuclei or one. The same view appears in his *Experimental Zoology* (1907), where he also suggests that the sperms containing the X-element produce females simply because of "the greater amount of chromatin brought into the egg by the sperm." This view approaches one of my own suggestions, made in the preceding year,[3] but differs from it in one very important respect.[4]

The conditions seen in such forms as *Nezara*, and the remarkable facts observed in *Metapodius*, make one sceptical of any hypothesis which postulates only a quantitative difference of nuclear or chromatin mass taken as a whole. The case is different if we limit the quantitative relation to a particular *kind* of chromatin, namely, to that contained in the X-element. Except for the fact that the Y-element is here left out of account (for the reasons already stated), this is in all essentials identical

[1] *Verh. Deutsch. Zool. Ges.*

[2] *Pop. Sci. Monthly*, December.

[3] *Op. cit.*

[4] An extended critique of the subject is given in Morgan's later work on *Phylloxera* (1909) already cited.

with the second interpretation that I suggested in 1906 as an alternative to the qualitative conception, and further developed in 1909.[1] As was pointed out in these papers, many of the difficulties raised by the qualitative conception disappear if we accept the "naïve view," directly suggested by the cytological facts, that in the insects the presence of one X-element means *per se* the male condition, while the addition of a second element *of the same kind* produces the female condition. The puzzle of the bee is now solved, and we at once comprehend how the elimination of one X-element by the parthenogenetic egg (as in *Phylloxera*) produces the male condition.

"In ordinary sexual reproduction all the unfertilised eggs should after maturation bear the male tendency, because one X-element is left in the egg after reduction. If capable of parthenogenesis with the reduced or haploid number of chromosomes, such eggs should produce males (as appears to be actually the case in the bees and ants). If fertilised by a spermatozoön that lacks the X-element, the egg still produces a male for the same reason. If fertilised by a spermatozoön that contains this element, the egg produces a female because of the introduction, not of a dominant 'female tendency,' but of a second X-element."[2]

Accepting this view we may designate the female as the plus sex, the male as the minus. In current Mendelian terms, the female *Protenor* or *Anasa* is cytologically "homozygous," arising by the union of like plus gametes (each containing X) and again producing gametes of this type only; while the male is "heterozygous," arising by the union of a plus gamete and a minus, and producing these two classes in equal number.[3] To employ a more recent and perhaps in this case preferable terminology, in respect to the X-element the female is of "duplex" constitution, the male of "simplex."

This view equally accounts for all the facts in the sexual reproduction of the forms under consideration, and for many

[1] *Science*, January 8.

[1] Wilson, *op. cit.*

[3] This use of the terms "homozygous" and "heterozygous" is the reverse of the one employed by Morgan in the discussion appended to his paper on *Phylloxera*, already cited. It seems to me to be justified both by the etymology of the word and by Bateson's definition, given at p. 16 of Mendel's *Principles*. Morgan had in mind the fact that the parthenogenetic female may produce males as well as females; but this is, I think, equally consistent with the terminology here employed for the cytological conditions.

of the facts of parthenogenesis, without postulating specifically different male and female "determiners"; and it avoids the necessity for assuming selective fertilisation, which is such a stumbling-block in the way of the qualitative interpretation. Further, the quantitative view, as thus stated, can readily be applied to cases like that of *Metapodius*, where the total chromatin-mass varies in the same species, independently of sex,[1] or of *Nezara* and *Oncopeltus*, where the total chromatin-mass is the same in both sexes. Here we perceive the possibility of extending the interpretation over a much larger series of forms than those in which a dimorphism of the spermatozoa or eggs is visible to the eye. It may be urged against this that visible sexual differences in the chromosomes are exceptional (which is probably the fact) and that we have no right to transfer the conclusions based on these exceptional cases to the more usual ones in which such differences (apparently) do not exist. There are at least two replies to this. One is the fact, already mentioned, that all gradations have been found between species which do, and species which do not, show such differences. Another is that experiment demonstrates a quite analogous sexual predestination in germ-cells where observation has not thus far demonstrated any visible differences, in the chromosomes or otherwise. It will, I think, be found difficult to escape the conclusion that the same principle applies to all these cases; though such a conclusion must of course be held subject to revision.

In attempting a broader development of the foregoing view we at once become aware that the particular formulas thus far employed—XX = Female and X (or XY) = Male—cannot hold true of all forms. It obviously does not apply to cases where females as well as males are produced from germ-cells that develop with the haploid number of chromosomes, *e.g.*, the spores of diœcious plants such as mosses and liverworts. It will not, apparently, apply to the case of the sea-urchin; and, as Castle has shown,[2] it seems inadmissible in case of the remarkable results of crosses between *Abraxas grossulariata* and *A. lacticolor*, as worked out by Doncaster and Raynor. In all cases, indeed, where the female is the heterozygous or heterogametic sex, this formulation seems to be excluded. It nevertheless remains possible, as I earlier suggested in the case

[1] Wilson, *Journ. Exp. Zool.* 1909, vi. [2] *Science*, March 5, 1909.

of plant spores, that the male-producing cells "may be characterised by the absence of some element that is present in the female-producing ones.[1] If this be correct, a common principle may apply to all cases, whether the germ-cell develops with the haploid or the diploid number of chromosomes, and in the latter case whether the heterogametic sex be the male or the female.

In accordance with these considerations Castle, abandoning the earlier form of his hypothesis, has endeavoured to show how the facts may be given a general formulation in terms of the "presence and absence" hypothesis of Mendelian heredity.[2] His suggestion is that the female is in all cases characterised by the presence of something that is absent in the male, but two general cases must be distinguished as follows:

A. In the first case, where the male is heterogametic, the presence of two X-elements in the zygote means the female condition, the absence of one of them the male.

B. In the second case the presence of one X-element means the female condition, its absence the male. This assumption may apply to cases where the female is the heterogametic sex; and it will explain such cases as the diœcious plants, where the asexual spores may produce either males or females.

The postulation of two such classes, while it gives a formal explanation of the facts, is of course a speculative construction *ad hoc*, and as such can for the present only be taken in a very provisional way. It should not be forgotten, however, that the cytological evidence establishes the actual existence of two cases, one of which (insects) certainly corresponds to Castle's Class A, while the other (sea-urchins) *may* correspond to Class B. In any event the assumption offers some very interesting suggestions for further inquiry, especially on the cytological side. It seems at first contradictory to assume that in one case a single X-element stands for the male condition, in the other case for the female; but this is not out of harmony with known facts of heredity. For example, in the alternative heredity of coloured forms when crossed with white, the presence of colour usually "dominates" its absence, but the reverse case is not uncommon. According to Shull[3] this may be interpreted to mean that in the first case the presence in the heterozygote of one colour-producing factor or "gene" is sufficient to produce the colour, but in the second case colour only appears when

[1] *Op. cit.*, *Science*, 1909. [2] *Op. cit.*, 1909. [3] *Am. Nat.*, July 1909.

doubly represented (*i.e.*, in the pure or homozygous condition). Another example is given by the heredity of horns in the cross between Suffolk hornless sheep and Dorset horned, as reported by Wood.[1] Among the descendants of such a cross the females are horned only when both parents have this characteristic, while the males are horned if only one parent be horned. Conversely, the males are without horns only if the deficiency be received from both parents. In other words, if the factor for the horned character be called "H," in the female the presence of HH means the horned condition, and of H alone the hornless; while in the male the presence of H means the horned condition, its absence the hornless.

It would not, I think, be profitable further to pursue the theoretical side of the question at this time, or to take up the question as to what rôle, precisely, is played by the chromosomes in sex-production. One possible misconception must, however, be guarded against. The statement that the female is the "plus sex," characterised by the presence of something that is absent in the male, of course does not mean that the female is a more highly developed male. It only expresses the assumption that the presence of a particular factor so affects the germ-cell as to turn the balance of development to the female side, while its absence has the reverse effect. The primary cause is quantitative, but not the effect, or not in the same sense. *How* the original quantitative factor operates we do not in the least know.

It will be sufficiently evident that the quantitative hypothesis of sex-determination seems to the writer to give the more intelligible view of the problem, in so far as it relates to diœcious organisms; and this is also the position taken by Morgan in the work on *Phylloxera* already cited. Further, the principle that is revealed alike by the cytological and the experimental evidence, seems so natural and simple that we may at least hope to see its application much more widely extended than at present. There is a certain temptation in the thought that the sexual differentiation may be rooted in a simple principle of plus and minus that holds true of all sexual organisms, from the lowest to the highest, and may be an expression of a fundamental principle of metabolism. But such a view, however tempting, cannot be accepted, or only with great reserve,

[1] See Bateson, *op. cit.*

until some of the difficulties in its way have been surmounted. One of the most conspicuous of these, as Prof. Correns has pointed out, is offered by the facts of hermaphroditism; and they seem indeed to give basis for a serious objection to the quantitative interpretation. The puzzle is, however, not more baffling than that presented by the heredity of such characters as the mottled or piebald types of colour-patterns under the presence and absence theory; and possibly an explanation of the latter case will give a clue to the nature of the former.

But space forbids further discussion of this point. We return from our excursion into the theoretical field compelled to admit that the data for an adequate or definitive general interpretation of sex are not yet at our command. Cytological research has nevertheless made a substantial advance towards the solution of the problem. The cytological phenomena that have been described have already led to a better understanding of sex-production; and the concrete results of observation must not be confused with any of the theoretical interpretations that we may place upon them. Phenomena of this kind seem likely to throw further light on the mechanism of Mendelian heredity as well as of sex-production, for they demonstrate a disjunction of different elements in the formation of the gametes; and this again is a fact, not a theory.

THE SCIENCE AND PRACTICE OF PARA-RUBBER CULTIVATION

THE NEW TROPICAL INDUSTRY OF THE EAST[1]

PART II.—RUBBER PREPARATION.

BY JOHN PARKIN, M.A., F.L.S.

THE simplest method of obtaining solid rubber is to allow the latex to dry naturally. There are several objections to this plan. Impurities, such as particles of bark and soil, are apt to be incorporated, lowering considerably the quality of the rubber produced. An undesirable amount of water is also liable to be enclosed in the caoutchouc-mass. Further, if the latex be allowed to dry in the open air, it may become exposed to direct sunlight, and the heat from the sun's rays in the tropics is sufficient to cause permanent stickiness, rendering the rubber of little value. Moreover, many latices, notably those of Hevea and Manihot, putrefy on standing, and an evil-smelling caoutchouc of inferior grade is the result. Even if the latex be strained to remove foreign particles, and a preservative be added to prevent putrefaction, ordinary drying is a very slow and tedious process, especially as the latex usually requires some dilution to facilitate the straining. Consequently speedier methods of preparation have been devised. These depend for the most part on taking advantage of the phenomenon known as coagulation. But before treating of this important subject, the separation of the rubber by centrifugal force requires consideration.

CENTRIFUGALISATION

Centrifugal force was first applied to the separation of the caoutchouc globules from the latex by Prof. Biffen.[2] In 1897

[1] Continued from p. 416. As this article is appearing in two parts, it may be well to point out that the first part dealt principally with the Extraction of the Latex from the Para Rubber Tree (*Hevea brasiliensis*), the chief interest lying in the phenomenon known as wound-response. This, the concluding part, deals mainly with the Preparation of the Rubber from the Latex with coagulation as the central feature.

[2] Biffen, R. H., *Annals of Botany*, 1898, **12**, 165.

he accompanied as scientific adviser a small rubber-exploiting expedition to tropical America, and when there experimented on various latices with a cream separator. He found that the machine answered admirably for Castilloa and claimed also that he effected the separation of the caoutchouc of Hevea latex by this means. The advantage of the centrifugal method lies in the fact that the rubber can be extracted from the latex in a state of great purity.

The success of the centrifugal separator in the case of Castilloa has been repeatedly confirmed. This latex, indeed, creams readily on standing and good rubber can be prepared in this way. With Hevea, however, confirmation has not been forthcoming. No one in the East, as yet, has been able to effect the separation of the caoutchouc globules of this latex by centrifugal force, even with a speed of 11,000 revolutions per minute. This latex, further, never shows the least sign of creaming. It remains homogeneous as long as the slightest acidity is prevented from arising. The difference in the behaviour of the two latices is probably due to the size of the caoutchouc globules. These are most likely much larger in Castilloa latex than in that of Hevea.

Biffen's ingenious application appeared most promising twelve years ago as a handy means of preparing first-class commercial rubber. Since it has been proved to be impracticable with the latex of cultivated Para rubber trees in the East, it is now of minor importance.

Centrifugally separated caoutchouc is no doubt the purest, as it is practically free from albuminous matter (protein) which forms a small percentage of all other raw rubbers. Recent investigation[1] suggests, however, that the small quantity of cured protein contained in Para rubber is not without an advantageous influence. The texture of the raw rubber appears to depend to a large extent on the manner of coagulation—a phenomenon depending upon the protein present.

Coagulation

To Biffen[2] is also due the first adequate explanation of the nature of latex-coagulation. His view has been challenged

[1] Spence, D., *Quart. Journ. Liverpool Instit. Commercial Research*, 1908, **3**, 47.

[2] Biffen, *loc. cit.* p. 170.

since, but in the writer's opinion the adverse criticisms are due to the confusing of distinct phenomena;[1] in fact, the experiments brought forward in refutation of the theory, in reality, amplify it.

The explanation he propounded is as follows. When the protein dissolved in the latex comes out of solution it forms a delicate network throughout the latex, entangling the caoutchouc globules in the meshes. This network shrinks upon itself, forming a spongy clot, which can be removed from the mother liquid and compressed into a solid mass of rubber.

Latices behave differently as regards the means to be employed to bring about coagulation. Manihot latex is easily clotted by boiling; not so that of Hevea. A trace of acid, however, coagulates the latter. In the first case Biffen considered the protein to belong to the globulin class and in the second instance to the albumins. He applied his theory to the explanation of the means employed in the Amazon region of preparing Para rubber by smoking and gave strong reasons for believing that this excellent empirical native method is due to coagulation. The smoke arising from the smouldering fire made of palm-nuts contains both acetic acid and creosote. The former coagulates the latex as it is passed over the rotating wooden paddle held in the smoke; the latter impregnates the rubber as it is formed layer upon layer, thus preventing any subsequent moulding or putrefaction. The wet system of coagulation, now in use on the Hevea estates of the Eastern Tropics, was largely suggested by Biffen's explanation of the Amazon smoking method.

My attention was early directed, in the Ceylon experiments of 1898–9,[2] to this question of coagulation as it affects Hevea latex. Several acids and some salts were tried, with the result that acetic acid was recommended as the best reagent to be used on the estates to bring about coagulation, and so for the preparation of the rubber from the latex. This acid is now generally employed.

The following table gives the weight required of the different reagents to coagulate completely 100 cubic centimetres

[1] Coagulation and coalescence. The latter term can be used for the fusing together of the rubber globules through drying or by means of pressure.

[2] Parkin, *Circular R.B.G.*, *Ceylon*, 1899, Series 1, Nos. **12**, **13**, **14**, 148.

of Hevea latex. Since these Ceylon experiments, a few other coagulants have been recommended, and these are appended below the horizontal line:

	Grams per 100 c.c. latex.
Sulphuric acid	0·1
Hydrochloric acid	0·1
Nitric acid	0·3
Acetic "	0·95
Oxalic "	0·2
Tartaric "	0·25
Citric "	0·5
Mercuric chloride	0·8
Formic acid [1]	0·45
Hydrofluoric acid [2]	—
Acid potassium tartrate [3]	0·16

In the first place with respect to the acids, an excess has to be avoided, otherwise the coagulation ceases to be complete and rubber globules are left in the mother liquid. The range for complete coagulation is very small with all the acids tested, except acetic. With sulphuric, for example, the amount can hardly be doubled without interfering with the coagulation; whereas with acetic it can be increased some four times before the residual liquid shows turbidity. In fact one quarter or nine times the necessary amount of acetic acid may be added with very little waste of rubber—a very considerable range within the limits of which it is easy to keep in practice. For this reason acetic has much the advantage over the others, even though a greater weight is required than in the case of any of the other acids.

In the second place the quantity of acid needed depends only upon the amount of pure latex, and is independent of its dilution. A definite weight of acid is required to coagulate completely 100 c.c. of latex, no matter whether this be diluted to five or ten times its volume with water. The latex of Hevea can thus be diluted to any extent, and yet its particles of caoutchouc remain capable of being drawn together into a clot by the addition of the requisite quantity of acid. This was even done for latex diluted two thousand times.

[1] Spence, D., *India-Rubber Journal*, 1908, vol. xxxv. p. 425.

[2] A weak solution of this acid has been introduced as a coagulating and curing agent under the fancy name of "Purub." Little information is yet to hand as to its use on estates.

[3] Bird, J. A., *Official Account*, *Ceylon Rubber Exhibition*, 1906, p. 145.

The following chain of reasoning was put forward by way of explaining this acid coagulation.[1] "The latex is slightly alkaline. The proteid is of such a nature as to be insoluble in neutral solution, but soluble in alkaline or acid media, *i.e.* it is an alkali-albumen. When the alkalinity is neutralised by the necessary amount of acid, the proteid comes out of solution and produces with the globules of caoutchouc the clots of rubber. If excess of acid be added, then the proteid remains in solution, being now in an acid medium. The acid required for coagulation bears a definite ratio to the quantity of pure latex only, no matter what its dilution may be, because the alkalinity is not altered in amount by this dilution. Acetic, being a weaker acid than the others, does not bring about the changes so rapidly."

The coagulating power of several salts was tried on the latex of Hevea. None of them were very satisfactory, excepting mercuric chloride (corrosive sublimate). They either gave an incomplete coagulation, or else a large quantity of the reagent was required. A solution of mercuric chloride, however, rapidly separates out the caoutchouc and very little is needed. This is not surprising as the salt is one of the strongest precipitants for proteins. Magnesium sulphate was, of the salts tried, the next best coagulant. Mercuric chloride has not been experimented with on a large scale. One serious objection to its use is its very poisonous nature. It would, however, be interesting to have a large sample of such rubber reported on by a manufacturer. It furnishes a very tough kind of caoutchouc, which might possibly have a special use. Such a coagulant has one advantage over acetic acid. It preserves the rubber, whereas that made by means of the acid will mould and rot if kept damp, unless an antiseptic be added.

Victor Henri's[2] fairly recent researches on the coagulation of latex deserve mention. He employed for his experiments latex which had been submitted to dialysis, until no appreciable quantity of saline matter remained in it. Such dialysed latex behaves differently in regard to coagulation. Alcohol, for example, which is a strong coagulant of ordinary latex, has no effect on the dialysed variety, but after the addition of a salt coagulation sets in, hence the presence or absence of salts has much influence on the clotting of latex.

[1] Parkin, *Annals of Botany*, 1900, **14**, 197.
[2] Henri, V., *Compt. Rend. Soc. Biol.* 1906, **60**, 700.

Henri makes no mention of the albuminous matter present in the latex, and so it is to be inferred that he regards the coagulating reagents as acting directly on the caoutchouc globules, causing them to agglutinate. But his facts are readily explained otherwise. The dialysis will only remove the salts and not the proteins. Now the clotting or separating out of colloids, such as proteins, is influenced by the presence of salts. When these latter are removed the proteins in solution will behave quite differently towards the various reagents employed for separation; hence the coagulation of the latex, when dialysed, will not be able to be effected in the same manner as that of the fresh latex; at the same time the clotting will be due none the less to the protein present. On such reasoning the coagulation of latex will be brought into line with the facts now known respecting the so-called "strength" in wheat flour and the behaviour of proteins generally in regard to precipitation.

Two matters of great importance from a practical point of view deserve to be noticed in regard to coagulation. In the first place the reagent employed has in all probability a specific influence on the quality of the rubber produced and especially on its behaviour in vulcanisation.

Secondly, coagulation alone is not sufficient. It should be combined with curing. Rubber prepared by acetic acid will mould and deteriorate if not kept dry. Hence in the Ceylon experiments creosote was used and recommended as a preservative. This, however, does not seem to be generally practised. Much of the plantation rubber now on the market is not cured, but being produced in thin sheets it dries rapidly and so reaches London, as a rule, in good condition.

There is no doubt, however, that cured plantation rubber will in the future gain the favour of the market. It is significant now that what is known as "smoked sheet" commands the highest price. It remains to be seen whether a convenient wet method of coagulation can be introduced which will produce a satisfactorily cured rubber. If the coagulant and preservative can be combined in one reagent, so much the better. Formic acid, mercuric chloride and "Purub" have this double property, if any of them may prove satisfactory otherwise. If acetic acid be adhered to, then some antiseptic must be supplied in addition.

This apparent necessity of curing the rubber has focussed attention once more on the Amazon smoking method. Wickham[1] for one strongly advocates this practice, and has invented an apparatus for the purpose.

Some of the Eastern estates have met the difficulty half way by first preparing the rubber in thin sheets by means of acid coagulation and then submitting these to the action of smoke from burning wood. The rubber is thus surface-cured only. It has just been pointed out that rubber so treated (smoked sheets) fetches at present the top price in the market. Previously pale crêpe was most in favour, largely on account of its light colour. A cured rubber, retaining its pale tint, would seem to be the desideratum, and this would be difficult to obtain by any smoking method. Consequently it would seem likely that a wet system both of coagulation and curing will ultimately be adopted in Para-rubber cultivation; especially as a watery or much diluted latex can more easily be dealt with by wet coagulation than by means of smoke.

Electrical Method of Separation

Cockerell,[2] of the Ceylon Technical College, has recently shown that rubber can be separated from Hevea latex electrically. The caoutchouc is deposited on the anode. Few details are to hand yet. In any case it is doubtful if such a method would be practicable on estates. Nevertheless it is of considerable scientific interest.

Composition of the Latex and Rubber

A moderate number of analyses have been made of Hevea latex and rubber; several are by Mr. Kelway Bamber, chemist to the Ceylon Government. Below are given in round figures the average percentage composition of the latex drawn from the tree-trunk (after wound-response has commenced) and also that of the rubber prepared therefrom.

	Hevea Brasiliensis percentage composition.	
	Latex.	Rubber.
Water	55 to 63	0·5
Caoutchouc	40 to 32	94·0
Resin	2·1	3·0
Protein	2·1	2·5
Ash	0·4	0·5
Sugar, etc.	0·4	—

[1] Wickham, *The Plantation, Cultivation and Curing of Para India Rubber*, p. 29.

[2] Cockerell, *India-Rubber Journal*, 1909, **37**, 331.

The percentage of water and caoutchouc will naturally vary inversely. Para rubber is remarkable for the low proportion of resin—a point much in its favour. Other rubbers usually have more. The comparatively large amount of protein in the latex is noteworthy; a considerable part of it naturally becomes incorporated with the rubber through coagulation.

Lack of Strength in Rubber from Young Stems

The possibility of preparing commercial rubber from the young stems and leaves of caoutchouc-plants has often presented itself in the past; but in respect to Hevea and some other rubber trees as well, there is one fatal objection to this project. The latex from these parts of the plant produces a "rubber" somewhat adhesive, with little elasticity and strength.[1]

This is a peculiar point and has not yet been clearly explained. The writer has suggested that perhaps the latex formed in primary growth has a different composition from that produced in the secondary tissues by the cambium. The difference does not seem to be wholly due to a higher percentage of resin in the latex from the young organs, though in some cases this may occur. A sticky weak caoutchouc may show on analysis a chemical composition almost identical with that of a standard product.

The explanation evidently lies deeper, and may be bound up in the mysteries surrounding "tackiness" in rubber, which is receiving attention from Dr. Spence.[2] Plantation rubber occasionally arrives on the market in a sticky state, though despatched from the tropics in good condition. This change and great deterioration have been laid to the charge of bacteria. Spence shows reasons for thinking that the alteration is not directly due to bacterial growth but rather to a change in the physical nature of the rubber particles themselves, which ordinary chemical analysis fails to reveal. There may be some physical relationship between the weak adhesive caoutchouc from young plant organs, the so-called "tacky" rubber and that rendered permanently sticky by heat.

Although the globules in the latex of rubber-yielding plants

[1] Parkin, *Annals of Botany*, 1900, **14**, 203.

[2] Spence, *India-Rubber Journal*, Quart. Century No. 1909, 43.

are often spoken of as composed of caoutchouc, it does not necessarily follow that they are so. There is some evidence for the view that the globules contain a liquid rather than a solid and, that in the formation of rubber from the latex, this liquid polymerises to true caoutchouc. Preyer[1] first suggested that the globules might hold a mobile liquid. Weber[2] has shown that they are soluble in ether, whereas the caoutchouc prepared from them is not.

Caoutchouc is generally regarded chemically as a high polymer of isoprene (C_5H_8), a hydrocarbon of the terpene series. The latex globules may contain a low polymer. Polyprenes with double and four times the molecular weight of isoprene are known and are liquids.

Darkening of Rubber and Oxidising Ferments

The darkening of latex, as it issues from the tree, is a fairly frequent occurrence. The oxygen of the air gaining access to the latex, oxidises, with the help of special ferments known as oxidases, certain substances occurring in solution, and from them are produced dark-coloured bodies. Castilloa latex markedly exhibits this peculiarity. If this latex dries naturally, a dark brown, almost black, rubber is produced. As the deeply coloured substance is in solution, the creaming or centrifugal method permits the preparation of an almost colourless rubber.

The latex of Hevea, such as comes from the trunk or branches of the tree, does not darken on exposure to the air. That, however, from the green wall of the unripe capsule (fruit) changes rapidly from a white to a black colour. The latex caused to exude from young shoots sometimes darkens, but not always, whereas the blackening of that from the capsule is without exception in the writer's experience.[3]

The rubber samples prepared during the Ceylon experiments of 1898-9 from Hevea latex by the acetic acid process have permanently retained their pale colour. Much, however, of the plantation rubber made in this way appears on the market quite dark in colour. A gradation of tints from pale amber to black can be observed in the sale rooms. Kelway Bamber has turned his attention to the matter, and considers

[1] Preyer, *Beihefte zum Tropenpflanzer*, 1900, **1**, 29.

[2] Weber, *India-Rubber Journal*, 1903, vol. xxvi. 373.

[3] Parkin, *Annals of Botany*, 1900, **14**, 199.

that oxidases are really responsible for this deep brown appearance. This explanation is not without difficulties, when the fact that the latex does not darken on exposure to the air is taken into consideration. Perhaps the action is slow in taking place and only at times are all the substances necessary to cause darkening present in the latex.

The reason why no colour change was observed in the early experiments may be partly accounted for by the fact that the samples were, as a rule, prepared by the hot acid process; any oxidase present would naturally be destroyed by the heat. The cold process, on the other hand, is the one generally practised on the estates.

Considering that pale rubber (unknown in the market before the advent of plantation) finds much favour with the manufacturers, being advantageous for certain purposes, the knowledge of the conditions regulating its production is of considerable importance. Bamber recommends the plunging of the rubber clots, just after their formation, into hot water, at a temperature sufficiently high to kill the oxidases. Such rubber should then remain permanently pale in colour.

Dr. Spence has proved the presence of oxidases in Para rubber, and also in Hevea latex itself. He writes: "These observations prove conclusively that the darkening in colour of raw rubber is due to an oxidase which is associated with the protein or the so-called insoluble constituent of the rubber."[1]

Why, it may be asked, does not Hevea latex itself darken on exposure to the air, and why, further, do some samples of rubber prepared from it darken and others remain light in colour?

Procedure on Estates

The "biscuit" was the earliest form of plantation rubber to appear on the market. The diluted latex, after careful straining to free it from all foreign particles, and after the addition of the necessary amount of acetic acid, is poured into flat circular dishes. Coagulation gradually sets in, and after twenty-four hours the caoutchouc from each vessel is capable of being removed in the form of a spongy clot. This, on being passed through a mangle, is compressed into a thin circular

[1] Spence, *Biochemical Journal*, 1908, **3**, 179.

piece of opaque white rubber, which on drying turns to a lightly coloured translucent biscuit.

Biscuits prepared in some such fashion are still made on the smaller properties, as no machinery is required, but on large estates more economical ways of procedure have been devised.

Coagulation can be brought about much more speedily by the use of a rotating machine, known as a coagulator, and acting after the manner of a churn. The latex with the requisite amount of acid is poured into the coagulator, and after a few minutes' rotation the clots are formed. These are then passed through a washing mill. Water plays on the rubber, as it is masticated and stretched in its passage through the corrugated rollers, clearing out the acid and impurities generally. The rubber finally emerges in a continuous broad ribbon, resembling crêpe in texture, which is readily dried. If smooth rollers be used then "sheet" rubber is made. Sometimes the clots are cut up by a special machine into small pieces, and such rubber is known as "worms."

Caoutchouc prepared in these different forms is not fit for shipment till it is dry, *i.e.* until the opaque whiteness changes to a translucent pale amber colour. Methods, therefore, of rapidly drying the wet rubber have been introduced. The quickest way is by means of the vacuum dryer. The removal of moisture by this apparatus entails a fairly high temperature, which appears to have a somewhat detrimental effect on the quality of the rubber. The best means of drying the newly formed rubber is still undecided, and the problem is to some extent bound up with that of curing.

One estate is turning out a variety of rubber known as "block," which is much appreciated. It is made by compressing in a special manner dry sheets or other thin forms. There are distinct points in favour of marketing plantation rubber in this shape. It is convenient to handle, and less surface is exposed to oxidising influences. If the blocks be made not more than an inch in thickness, their homogeneity can easily be verified.

If blocks be made direct from the clots, then the difficulty is to dry them in short enough time to prevent moulding and putrification. Here is shown the value of a preservative. Willis and Bamber[1] a few years ago turned their attention to

[1] Willis and Bamber, *Circ. and Agric. Journ., R.B.G. Ceylon*, 1907, 4. No. 1.

the feasibility of making "wet" block by means of acid-coagulation in the presence of creosote, as an antiseptic. They argued that a certain percentage of moisture in raw rubber had a beneficial effect on its keeping qualities. Brazilian Para always holds about ten per cent. of water. Their wet block was prepared directly from the clots by pressure; no trouble need be taken afterwards to thoroughly dry it, since it is permanently cured by the creosote. So far this idea has not been put into practice by the planters, neither have buyers evinced a desire for this kind of rubber. The chief difficulty would be to keep the percentage of moisture constant. Manufacturers naturally want to know how much pure caoutchouc they are buying in the raw article. At present they know that dry plantation rubber contains practically no extra weight in the way of moisture. As some one remarked, "Why pay freight on moisture? We can get enough of it in Britain for all our needs!"

A word as to cleanliness. Planters now see the great importance of paying strict attention to this in the preparation of raw rubber. Metal collecting vessels, on account of their liability to rust and so to stain the latex, have been discarded in favour of glass. In fact, as in butter and cheese making, metal utensils and appliances should be avoided as much as possible in the manipulation of the latex. There is much similarity between a rubber factory and a modern dairy.

Yield

The rubber-producing capacity of cultivated Heveas has in the past been generally under-estimated, and even now the full extent to which it may ultimately reach, as the trees mature, cannot be said to have been gauged with any degree of accuracy.

An average of one and a half pounds per annum for a tree twelve years old was the original calculation for Ceylon, but this was before wound-response had been taken into account. In Malaya six-year-old plantations are now giving 10 oz. to 1 lb. per tree, an amount gradually advancing to 3 lb. as the trees reach an age of ten years; higher subsequent yields are expected. A few old, well-developed trees have given 12 to 25 lb. each per annum. Eight seventeen-year-old Heveas in the Perak State, of an average girth of 55 inches, have supplied $28\frac{1}{2}$ lb. of dry rubber per tree.[1]

[1] *India-Rubber Journal*, 1909, **38**. 239.

The Cicely Estate, one of the older Malay companies, obtained an average of 6 lb. per tree from 9,000 which were regularly tapped in 1908. The age of these trees varied from about ten to five years, but a third of them were of the latter age, undergoing their first tapping; consequently some of the older trees must have yielded well over 6 lb. of rubber per annum.

From a financial or economic point of view a better method of calculating the yield is by the acre. As the planting distance varies so much, the average per tree is no clear indication of the producing capacity of an estate. It is now generally assumed that an acre of Para rubber, when it comes first into bearing at the age of five or six years, will yield 100 lb. of rubber per annum; in the tenth year three or four times this amount at least may be expected. Beyond this there is little data available. The producing power, for example, of a twenty-year-old plantation, which has been regularly tapped, can be merely guesswork at the present time. The future may have unforeseen drawbacks in store or it may furnish still more agreeable surprises.

The Quality of Plantation Rubber

The rivalry which is now commencing between plantation Para rubber and the wild product of Brazil will be keenly felt in the near future. The latter has been the standard caoutchouc for a long period, and buyers can rely on its uniform, excellent qualities. Manufacturers have their machinery especially adapted for its manipulation. Its requirements as regards vulcanisation are known exactly. It is the specified brand to be employed in a number of Government and other contracts. Little wonder then that plantation rubber should have met with some little opposition at the outset. The surprise rather is that it has come to the front so quickly. This early success is largely no doubt to be attributed to the general shortage in the raw rubber supply, but is partly also due to the great purity of the plantation article. It can be used directly for making rubber solution and is largely bought up for this purpose. Wild Para has first to undergo the laborious process of cleaning.

If plantation rubber had appeared in quantity ten or fifteen years ago, it would most likely have had a harder uphill fight to find a good market. The supply of Brazilian Para relative to the world's demand was then much greater. Manufacturers would have been chary about risking their money and reputation on an untried raw material. For the planting community, then, it would seem that cultivated rubber has arisen at a most opportune time. Manufacturers are obliged to turn their attention to it, and by doing so must hasten on improvements in its preparation, so that ultimately it will take a place in the rubber market second to none.

Though the best grades of plantation rubber have almost invariably received a higher price per pound than fine Brazilian Para, yet the buyer is in reality purchasing the cultivated caoutchouc at a rather cheaper rate, for the wild rubber suffers a loss of ten to fifteen per cent. of its weight in washing, whereas the plantation product loses hardly one per cent. Rubber planters will not be content to rest till their article fetches a relatively higher price than fine Para.

The influences above mentioned no doubt keep the value of plantation rubber intrinsically rather lower than that of the Brazilian export; but at the same time there is a general impression that the former lacks to some extent the strength and elasticity of the latter. This is at present a much disputed point. But taking into account both the general bias of manufacturers for the well-tried wild article and also the variety in shape and quality of the cultivated rubber now on the market, there would seem to be little ground for regarding the best grades of plantation as inferior to fine hard Para. A fair amount of badly prepared and "tacky" rubber from the East has reached Mincing Lane from time to time, and this must tend to damage the reputation of plantation Para as a whole. It may be claimed, however, that previous to the arrival of cultivated Hevea rubber from the East, no raw caoutchouc so free from impurity and moisture and so pale in colour had ever been put on the market.

The youthfulness of the trees from which the majority of plantation rubber is at present obtained has been blamed for this supposed lack of strength. The tapping of cultivated Heveas is begun when their stems, at a height of three feet from the ground, have attained a girth of about twenty inches. They reach this size under favourable conditions of growth in five or six years from the time of planting. The rubber in the forests of the Amazon is collected from much older trees. Then it is an undoubted fact that rubber from quite young trees or twigs of Hevea is very deficient in elasticity. There has consequently been much opinion expressed to the effect that the latex takes some time to mature and so naturally it is argued that the rubber from old trees must be better than from young ones. But the botanical fact is lost sight of that new laticiferous elements are continually being added by the cambium to the bast, no matter what age the tree may be. These must take time to mature. Previous to their full development they are not likely to yield an appreciable quantity of latex. Hence, unless the latex alters its character as the tree grows older, there is no reason for thinking it is less mature in a six or ten-year-old tree than in a fifteen or twenty-year-old one; both will have immature laticiferous tubes as well as fully functional ones.

The reason why the latex from young stems and shoots yields an inferior rubber may be associated with the fact that this latex is contained chiefly in the tubes formed in primary growth. These may quite well differ in their contents from those produced in the so-called secondary growth, which is due to the activity of the cambium and by which the tree increases its girth. If there be any truth in this supposition, then this will account for the fact that the rubber from Hevea trees under four years old, and especially of Castilloas of a similar age, is midway in strength between that from the shoots and that from older trees. In such young trees the primary laticiferous tubes will still be yielding some latex, which will mingle with that from the secondary tubes, giving an intermediate product. Later the primary ones will become wholly compressed by the growth in thickness, and cease to give any latex.

Further, direct testing of the rubber seems now to be dispelling this notion of an inferiority in the caoutchouc from six to ten-year-old trees, as compared with that from older ones. Beadle and Stevens[1] have carried out interesting vulcanisation tests with plantation rubber and fine Para. They argue rightly that, as almost all rubber is vulcanised before use, the trials of comparison should be made after, and not before, vulcanisation. Their results are distinctly favourable to plantation rubber. Tests for tensile strength and elongation

[1] Beadle and Stevens, *Chem. News*, 1907, **96**, 37, 187.

at the moment of rupture gave results equal, if not superior, to those of fine Para. They consider therefore that the statement that plantation rubber is wanting in "nerve" is not justified, and conclude that the new product will turn out to be at least as good as, if not superior to, Brazilian fine Para. The variation in the quality of plantation rubber which is to be observed at times should be attributed rather to differences in the method of treating the latex than to the age of the trees.

Brief reference has already been made to Spence's work[1] on the protein in rubber. By using suitable staining reagents he was enabled to demonstrate a fibrous reticular structure in raw Para rubber, due to the distribution of "cured" protein throughout the mass. He considers that it most likely plays an important part in the quality of the rubber, adding notably to its strength, and thus is a desirable adjunct. To militate somewhat against this view is the fact that in the processes of mastication and vulcanisation such structure must most likely disappear. However, as raw rubber is sold on its strength, whatever may add to this deserves consideration. Protein in the uncured state is no doubt a disadvantage at times, because such rubber, if kept damp, will mould and deteriorate. Even if protein be undesirable or inert, it does not seem to be practicable at the present time to prepare raw Para rubber without it. Hevea latex will not submit to separation by centrifugal force, otherwise a caoutchouc free from protein could be readily prepared. All methods of coagulation must result in the incorporation of albuminous matter in the rubber mass. Perhaps, by means of filtration or by the electrical method of separation, caoutchouc free from protein might be prepared on a laboratory scale and then compared with the article obtained by coagulation.

Further, the question arises are all caoutchoucs when pure, *i.e.* free from resin, protein, etc., identical in physical properties? Is, for example, that of Castilloa, Manihot, or Ficus equal in every way to that of Hevea? They possibly are, but there seems a probability that they are not. In Hevea it has been fairly well proved by Bamber that the rubber from four-year-old trees, though inferior to that from older trees, has the same chemical composition; and further the product from two-year stems, though sticky and without strength, showed little

[1] Spence, *Quart. Journ. Liverpool Inst. Com. Research*, 1908, **3**, 58.

difference in analysis, the slight increase in protein and resin being too little to account for the great difference in physical properties. Thus the gross chemical composition, as revealed by the ordinary methods of analysis, is no criterion as to the physical properties of caoutchouc. There may be many varieties of this substance, differing in elasticity and strength, but identical as far as their chemistry can be pushed.

Synthetic Rubber

The possibility of the production of a commercial synthetic caoutchouc to compete with the natural article has at times perturbed the rubber-planter. A few years ago the forthcoming of an artificially prepared product looked more hopeful than it does now. In the first place a distinction must be drawn between a laboratory prepared and a commercial synthetic rubber. The former has been an accomplished fact[1] for a number of years, and credit is due to Prof. Tilden[2] for his work in this direction; no one since apparently has advanced further than he did. A synthesis of caoutchouc occurred in his laboratory by accident. Engaged at one time in researches on the terpene series of hydrocarbons, he noticed that some liquid isoprene which had been laid aside in bottles for several years had formed clots of solid substance which had the composition and properties of india-rubber. He set to work to investigate the matter and found that isoprene could be changed into caoutchouc in two ways: either by very slow polymerisation in the presence of a trace of acid, such as had occurred in his laboratory by chance, or by bringing isoprene into contact with strong aqueous or moist gaseous hydrochloric acid. The first method is not a practical one on account of the long period required, and the second could not be made a commercial success, as the caoutchouc is merely a small by-product in the formation of isoprene hydrochloride; and further the yield of isoprene from turpentine—the starting point of the synthesis—does not probably exceed ten per cent. under favourable conditions. Tilden confesses that after two years' experimentation he had to reluctantly abandon the sub-

[1] Bouchardat as far back as 1878 had noticed that a tough elastic solid, resembling india-rubber, was produced by the action of strong acids on isoprene. (*Compt. Rend.* 1878, **87**, 654, and **89**, 361 and 1117.)

[2] Tilden, *Proc. Birmingham Phil. Soc.* 1892, **8**, 182; *Report Brit. Association* [York], 1906, 525; *India-Rubber Journal*, 1908, **38**, 321.

ject, seeing no way of making synthetical rubber commercially possible.

Even if future research should result in the production of artificial caoutchouc in quantity, it is very doubtful if it could ultimately compete with natural rubber, especially the plantation variety, as this most likely could be sold with a fair profit at a price of 3*s.* or even 2*s.* 6*d.* per lb. The raw material required for the synthesised product might cost nearly as much. Then again, though the artificial rubber might appear, as far as chemical analysis could show, identical with the natural article, it might be lacking in the essential physical properties. The synthesis of a colloid like caoutchouc, presumably of high molecular weight, is a problem of a different order from that of such comparatively simple crystallisable bodies as vanillin or even indigo.

However, at the present price of rubber, a synthetic commercial rubber of passable physical properties would not only be a boon, but a lucrative discovery. Patents have been taken out, and even companies floated for the production of synthetic rubber, but nothing visible has appeared as yet!

It is important also here to draw a clear distinction between a true synthetic caoutchouc and the so-called artificial rubbers. These latter are merely substitutes or adulterants, and would be discarded if raw rubber were cheaper. They are prepared chiefly from oils, linseed being considered the best.

It is, of course, not the purpose of this paper, even if the writer had the necessary knowledge, to deal with the chemistry of caoutchouc. This part of the subject has already received full treatment in the pages of Science Progress.[1] Suffice it here to say that through the important researches of Prof. Harries[2] attention is now being directed towards the synthesis of caoutchouc from carbohydrates. This investigator has shown good reasons for regarding caoutchouc as related to the pentoses, and so it is suggested that in the plant it may be derived from such sources.

Concluding Remarks

This new industry then appears to have a most hopeful future before it. The time, however, has by no means arrived

[1] Pickles, S. S., Science Progress, 1907, **1**, 497.

[2] Harries, *Ber. Deut. Chem. Ges.* 1905, **38**, 87 and 1195.

when managers of estates can content themselves with any rule-of-thumb methods. Eastern planters seem fortunately well alive to this, and now recognise the value of true scientific help. A manager of a well-known estate has recently put in print some admirable "Conclusions" on rubber cultivation. One of these reads: "That text-books on rubber-planting should only be regarded as historical works"—a maxim, I venture to say, of wider application.

Everything connected with this novel cultivation is still largely in the experimental stage. It is a pleasing sign to see directors of companies deliberating upon the advisability of employing scientific experts on their estates. Considering that such enormous profits are now being made by the older companies, a small fraction of their receipts might well be spent in this way. Planters should not be content with the scientific assistance rendered by the Government alone. A superintendent of an estate has not the time at his disposal, nor probably the necessary training, for carrying out laboratory experiments, or for keeping a sharp look-out for the initial stages of disease—a vital point. Joint experts for several neighbouring estates might well be employed. Money so spent upon plant sanitation should be regarded in the light of insurance.

It is also gratifying to note that this new tropical industry is almost wholly of British origin. The seeds were collected in Brazil and transhipped by an Englishman. Kew raised the young plants and sent them to the Middle East. The Botanic Garden Departments there took charge of the trees and made the first tests upon them, bringing their cultivation to the notice of the planting community. The planters, once realising the possibilities of this new undertaking, took it up with their characteristic energy and daring and have already brought it to a surprisingly successful issue with bright prospects opening ahead. Thus as a nation we have taken the lead in this new cultivation. May we not lose our hold upon it through paying too much heed to immediate gains, and too little thought to the more distant future!

The subject has further an Imperial aspect. The foundations have now been truly laid for making the British Empire before long self-supporting in regard to this valuable raw material.

RECENT HYDROBIOLOGICAL INVESTIGATIONS[1]

THE GULF STREAM—AND SEA FISHERIES OF NORTHERN EUROPE

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The Marine Fisheries and Hydrographic Research

The evidence that the productivity of the marine fisheries varies according to the physical conditions of the sea during the same, or preceding, years is now fairly strong. Apparently there are various ways in which physical changes in the sea-water may affect the abundance of fishes. (1) There are optimum conditions of temperature and salinity for many species of fish, and these conditions restrict their distribution. But changes due to the periodicity of the Gulf Stream drift, or the annual sea-temperature-wave itself may enlarge or restrict a sea-area exhibiting uniform, or approximately uniform, conditions as regards temperature and salinity. With the variation of this sea-area so will the distribution of certain species of fishes be affected. The annual summer migrations into the Irish Sea of the Bass (*Labrax lupus*), the Garfish (*Belone vulgaris*), and the Mackerel are probably instances of this kind of fish-migration. These fishes normally inhabit a sea-area to the south of the Irish Sea, and as the temperature in the latter rises during the summer they migrate to the north. (2) Certain conditions of temperature and salinity appear to favour the spawning acts of some fishes, and when the annual spawning period approaches, such species migrate in order to reach the sea-areas characterised by these physical conditions. Cod, for instance, are present to some extent in the Irish Sea throughout the entire year, but they enter this sea in greater numbers in

[1] The preceding portion of this article, "The Gulf Stream—and Climate and Crops in Northern Europe," appeared in Science Progress, January 1910, **4**, 474–91.

the winter and early spring in order to reproduce in the shallow relatively fresh, and cold water covering the coastal banks and slopes. Possibly the shoaling movements of the herring are reactions to such hydrographic changes. No doubt there are herring in the North Sea off the east coasts of Great Britain during the entire year, for it would appear to be the case that the various herring caught there are local in their habitat. But the fish only shoal, or become segregated, at a certain time in the year, and it is only then that a fishery becomes possible. (3) There are small local movements of fishes which lead to an occasional abundance in certain restricted parts of the sea, and in many cases these strictly local migrations are caused by a local abundance of some animal on which the fishes feed. Bottom-living lamellibranchs, for instance, such as the mussel, are a favourite food of species of flat-fishes, and the distribution of these molluscs is variable from time to time in any one place.

It is always very difficult clearly to demonstrate the connection between the abundance of fishes in a restricted sea-area, and some set of physical conditions in the sea. This is partly due to the difficulty of obtaining full and accurate information with regard to the hydrographic changes, and also with regard to the abundance of fishes. The periodic hydrographic cruises of the last half-dozen years are now enabling us to supply the first kind of information. These cruises are made every three months, or, at the most often, once a month, but the development of methods of interpolation, such as, for instance, the use of the sine-function in obtaining values of the sea-temperature for intervals intermediate between the dates of the observations, enables us to make a continuous picture of the state of the sea. Measures of the abundance of fishes in a part of the sea are obtained from the commercial statistics of fish landed from the fishing-grounds; and during late years the methods of collection of the fishery statistics in Great Britain, and other North European countries, has been greatly improved. As a rule the more perfect these methods of acquiring information become, the closer is the connection between biological and physical events in the sea. Nevertheless, there is great need of strictly scientific methods of investigation with regard to the distribution and rate of metabolism of the marine fishes.

The Lofoten Cod-fishery

So far this appears to be the best example of a periodic sea-fishery, the fluctuations of which depend on the fluctuations of the Gulf Stream drift in northern latitudes. The great Lofoten cod-fishery usually begins in January and ends in April, but the variations from these dates are very notable. Generally the maximum catches are made in March, but in some years the fishery is only beginning in that month while in other years it is then practically at an end. Fortunately the Norwegian fishery authorities have collected statistics which give not only the quantity of cod taken from week to week during the progress of the fishery, but also the quantity of cod-roe, and cod-liver obtained, these latter being commercial products of great importance. The fishery records also include statements of the air-temperature at the Lofoten Islands, and during the last few years there are very precise data with regard to the intensity and variations in the periodicity of the Gulf Stream flow in those latitudes. It will be seen from fig. 2[1] that these islands lie in the direct core of the Gulf Stream drift.

I reproduce here the figures prepared by Helland-Hansen and Nansen in order to show the variations in the productivity of this fishery and the associated intensity in the volume of the Gulf Stream drift. An arbitrary date—March 15—has been selected, and the numbers of cod taken before that day have been calculated and expressed as percentages of the total catch of the whole fishery season. The greater the percentage of fish taken before March 15, the earlier is the appearance of the shoals, and *vice versa*. This percentage is shown in fig. 7 as a thin broken line (III.). It was maximal in 1902, minimal in 1903–4, and has decreased since the latter year. The same figure shows the air-temperature at Lofoten as a thin line (II.), and this was minimal in 1902, maximal in 1903–4, and then decreased. The thick continuous line (I.) shows the average temperature of the sea beneath the surface at the Sognefjord line. This value varies similarly to that of the air-temperature at Lofoten, but the phase is different; that is, the sea-temperature at Sognefjord is a year earlier than the air-temperature—and, therefore, the surface sea-temperature—at Lofoten.

There is, therefore, an inverse relationship between the con-

[1] See Pt. I. of this article, Science Progress, January 1910, p. 479.

dition of the cod-fishery and the temperature of the sea. The higher the latter, the later in the year do the cod-shoals approach the fishing-grounds: that is to say, a strong Gulf Stream flow corresponds to a late fishery, and generally a rather poor fishery; and, conversely, a weak Gulf Stream flow corresponds to an early, and generally a good fishery. Further—and this is a point of the greatest practical importance—it is possible to predict the approximate date at which the fish may be expected to reach the fishing-grounds, the date of the maximum fishery, as well as the probable yield of the season's operations; for a

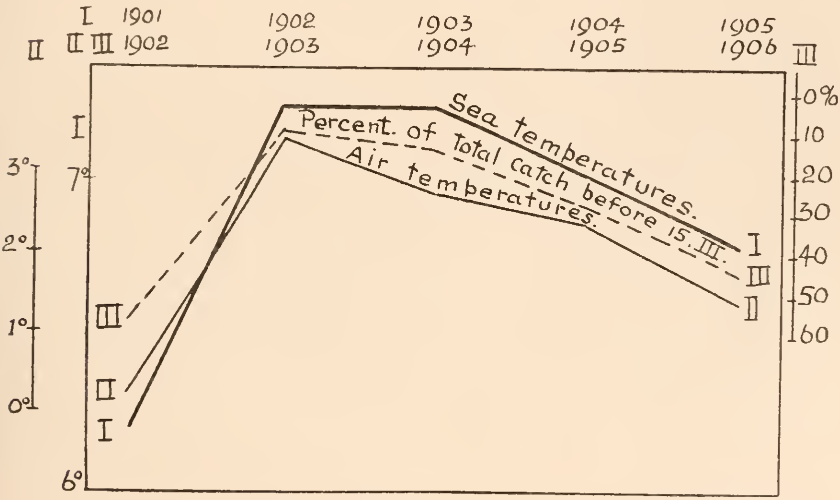

FIG. 7.—Sea-temperature at Sognefjord in May, air-temperature at Lofoten, and phase of cod-fishery at Lofoten in the following year.

(From Helland-Hansen and Nansen, *loc. cit.* p. 351.)

high average sea-temperature at Sognefjord in May corresponds to a high sea-temperature at Lofoten about a year later. These conclusions are based, it is true, on only four or five years' observations, the period during which the International Hydrographic Investigations have been in progress, but other corroborative evidence is available. The total catch of the season and the air-temperatures are recorded since 1880, and it is seen that a fairly close parallelism holds for this period of years: the higher the air-temperature at Lofoten—that is, the stronger the Gulf Stream flow—the lower is the proportion of cod caught before March 15 of the same year.

Influence of Physical Conditions on the Metabolism of Fishes

The shoaling movements which lead to the Lofoten cod-fisheries are spawning and not feeding migrations. The fishes do not come into shallow waters (in this particular case) to feed. It is rather the ripening of the reproductive organs which determines the period at which they come to the shore. No doubt this annual spawning migration is a habit which is deeply stamped on the fish by heredity. For some reason or other—probably because the prevalent drift of surface water in this locality leads to a distribution of the pelagic eggs and larvæ which is favourable to the species—the adult fishes approach the shallow coastal waters in order to deposit their eggs. But the ripening of the reproductive organs is the immediate stimulus which determines at what precise time the shoaling of the fish takes place.

Is a late fishery to be associated with a retardation of the annual maturation of the eggs or spermatozoa? It is with regard to such points that the commercial statistics fail, and precise scientific observations become necessary. There is comparatively little in the records of the Lofoten, or, indeed, any other great fishery, which throws light on this point. Systematic and very abundant observations of the progress of the maturation of the gonads in marine edible fishes, also systematic quantitative plankton experiments, carried out with the object of ascertaining the relative numbers of eggs and larvæ present in the sea during the spawning period, would be necessary for the settlement of this point. Some observations made by Hjort[1] do, however, help us. The cod-fishery at Lofoten was quite abnormal in 1903. Only about 5 per cent. of the total season's catch was made in that year before March 15. Week after week passed without the appearance of the fishes on their usual spawning-grounds, and when they did appear it was seen that their condition was wretched ("ganz elend"). The ovaries were only imperfectly matured, and the size of the ova (the best criterion of the stage of maturity) was as small in March as would have been the case in the December of a

[1] Hjort, *Some Results of the International Ocean Researches*, published by the Scottish Oceanographical Laboratory, Edinburgh, 1908 (translated from *Aarsberetning vedk. Norges Fiskerier for* 1907. Bergen).

good fishery year. The livers of the fishes were also small and heavy; and the condition of this organ is so good a test of the general condition of the fish as regards nutrition that fishermen always observe it. The year 1903 was, therefore, one in which the late appearance of the shoals was associated with a state of nutrition of the fish far below the normal; and in that year the temperature of the sea at Lofoten was higher than usual—that is, it was a year of exceptionally strong Gulf Stream flow.

The fishery statistics do, however, show that there is a relationship between the physical conditions of the sea and the biological conditions, though it is, of course, necessary to be cautious and not to strain these data too much. The roes—that is, the imperfectly matured ovaries of the cod caught—and the livers are both important articles of commerce, and the quantities of both products exported from Lofoten have been recorded for many years by the Norwegian fishery authorities. The roes are used as food, and the livers are made into oil. The figures for the take of roe are rather imperfect, and, further, the total quantity of roe taken depends on the proportion of fish which have not yet spawned—a proportion which falls off as the season progresses. The take of liver will be in direct proportion to the take of fish. If, however, the quantities of roe and liver be expressed as numbers of hectolitres per thousand fish caught, we obtain a measure of the degree of nutrition of the fish, as indicated by the grade of development of these organs. When these ratios are calculated for a number of years, and compared with the hydrographical conditions of the sea, it is seen that minimal takes of roe and liver are to be associated with sea-temperatures at Lofoten which are above the normal; conversely, maximal takes of these commodities are associated with sea-temperatures which are below the normal. It is not possible to compare the sea-temperature, and the quantity of roe and liver taken, for a long period; but the air-temperature can be used for this purpose, and the latter rises and falls with the temperature of the sea at the surface in the neighbourhood of the Lofoten Islands.

Fig. 8 has been constructed by Helland-Hansen and Nansen to show this parallelism. The air-temperature values are expressed as "anomalies," that is deviations above and below the mean temperature of a great number of years. These values are represented by the thick line I. in the figure. The quantities of liver taken are hectolitres per thousand cod caught; the quantities of roe are litres per thousand fish caught, and the latter figures have been "smoothed" by application of the well-known statistical formula, mean value of $b = \frac{a+2b+c}{4}$, such a mean value being substituted for each of the original values. It is at once seen that there is a direct relationship between quantities of roe and liver taken, and the variations from the mean of air-temperature. That is, the lower is the temperature of the air, the higher is the quantity of those products obtained. The correspondence is not particularly good, but it serves to support the conclusion already drawn, that the more intense

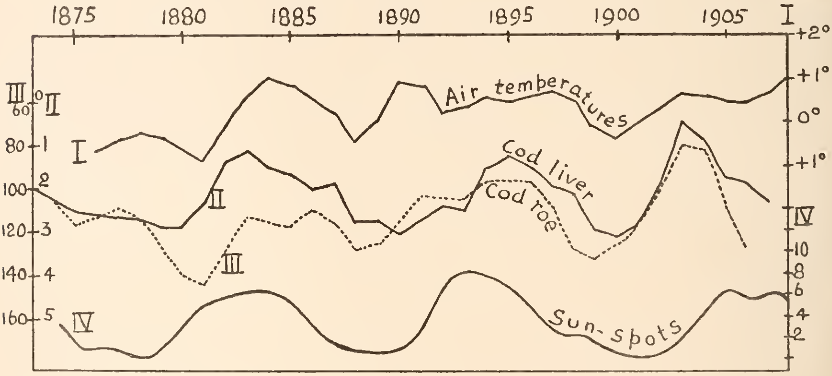

FIG. 8.—Productivity of the Lofoten cod-fishery compared with the air-temperature.

Scale I. is °C.; II. represents hectolitres of liver per thousand fishes caught; III. litres of cod-roe per thousand fishes caught; and IV. relative numbers of sunspots. (From Helland-Hansen and Nansen, *loc. cit.* p. 355.)

the Gulf Stream flow, the lower is the state of nutrition of the fish; for we have seen that the air-temperature varies directly as the temperature of the surface water of the sea.

The cod which reach the fishing-grounds at Lofoten are almost all sexually mature fishes, and their age varies from five to seven years. A certain proportion of these fishes have no doubt already made the shoreward migration and have spawned at least once before in their lives, but the majority are probably fishes which are sexually mature for the first time in their lives—that is, they represent the brood of some one particular year. If this is so, we ought to be able to refer them to some spawning-period about seven years prior to the date of their appearance at Lofoten. It would follow from this argument that a season characterised by an unusually large number of eggs in the sea would be followed by a season, some seven years later, during which an unusually large number of adult cod would be caught. There are no determinations of the number of cod-eggs per unit of sea surface, but the mass of roe taken in the course of a fishery is a rough measure of the fecundity of the fishes, for only a proportion of all the cod actually present on the fishing-grounds are caught by the fishermen. It is interesting then to note that, on the whole, curves showing the quantity of roe taken and the total numbers of cod taken can be superposed so that they show similar phases, if the curve of fish taken is pushed forward seven years in advance of the curve of roe taken. There are obviously many sources of error in such a comparison, but the relation is distinctly indicated by the statistics available.

Summary and Conclusions

Certain conclusions appear to emerge from a consideration of the data referring to the fisheries and harvests of Norway, when compared with the records of sea-temperatures and salinities in extreme northern seas. The air-temperatures on the land, and other climatic phenomena; phenological events, such as the dates of flowering of certain plants; the relative growth of certain trees; and the productivity of some harvest crops,—are directly related to certain physical conditions in the sea. A strong Gulf Stream flow in northern latitudes is to be associated with mild and boisterous weather; with high sea-temperatures, and a relative freedom from ice in Polar waters; and also with an earlier seed-time, and a more abundant harvest than usual. Further, the flooding of northern seas with the relatively warm water of the Gulf Stream drift is a somewhat tardy process: this water streams to the north-east very slowly, so that it can be foretold a year or two in advance what the conditions of the sea off the coasts of Scandinavia are likely to be. The effect of the sea-temperature on the fisheries appears to be the opposite to that produced in the case of the harvests. In the latter case a strong Gulf Stream flow leads to a good harvest; but a strong Gulf Stream flow is apparently productive of a bad fishery. This applies not only to the Lofoten cod-fisheries, but also to the fishery for haddock in the northern part of the North Sea. The latter relation seems to be an anomalous one, for it might be argued on *a priori* grounds that a high sea-temperature would lead to increased metabolism and more perfect nutrition.

It seems to be established that a high sea-temperature off the coast of Norway is to be associated with imperfect nutrition of the cod-shoals visiting that part of the ocean; and the cause is perhaps to be sought in some food-supply factor. Several such factors might be suggested, and that indicated by Helland-Hansen and Nansen is as probable as any. The temperature of the sea off the coast of Norway, and in the northern part of the North Sea is a function, not only of the intensity of flow of the Gulf Stream, but also of that of the southerly-flowing East-Icelandic Polar Stream, which consists of cold water. The ultimate food-stuff in the sea consists of the inorganic compounds of nitrogen and carbon on which diatoms and other protophyta feed. These constituents of the plankton are in their turn the food of small animals, such as copepods and other micro-crustacea, upon which the fishes feed, either directly or indirectly. The abundance of these inorganic food-stuffs is therefore the factor governing the production of organised living substance in the sea; and the grade of nutrition of the cod, and other fishes, depends ultimately upon the abundance of these compounds.

Water rich in these substances flows into the Polar Sea from the Siberian and other rivers. But a large part of the surface of this sea is covered with ice, and, since sunlight is unable to penetrate this covering in sufficient intensity to be utilised by the vegetable plankton, it may well be the case that the greater mass of this store of food-stuff is not utilised. Probably the water beneath the ice in the Polar Sea is relatively rich in the inorganic food-stuffs of the vegetable plankton; and it is this water that flows south as the East-Icelandic Polar Stream, and mixes with the Gulf Stream water in the sea between Iceland and the coast of Scotland. If the strength of this stream is unusually great in one year, the temperature of the Gulf Stream drift off the coasts of Norway, or in the higher part of the North Sea, will be unusually low. The Gulf Stream water itself appears to contain comparatively little inorganic food-stuff, or plankton, and it is where the water from this current mixes with water flowing from the north, or from the coastal regions, that the richest plankton is to be found. In those years in which the intensity of the Gulf Stream is least, there is probably a more extensive mixture than usual of truly Atlantic and Polar water. If the hypothesis suggested by Helland-Hansen and Nansen is borne out, it is therefore the abundant food-stuff contained in this Polar water which is the factor favourable to the nutrition of fishes. It is conceivable that the low temperature is itself a factor leading to increased nutrition, but the latter effect is most probably to be traced to several causes. But, however this may be, the relation of a strong Gulf Stream flow, as evidenced by a high temperature and salinity, to a relatively poor fishery, is one which is likely to be of very great practical importance.

It is impossible to avoid speculating on the causes of this larger periodicity of the Gulf Stream flow. The latter waxes and wanes throughout the year, attaining a minimum in our latitudes during the autumn, and rising again to a maximum in the spring. But a larger periodicity is superposed upon this yearly one, a period of how many years we do not yet know, for the International Hydrographical Investigations have not been carried on sufficiently long. But if we admit that the larger fluctuations in air-temperature in these countries, which are influenced predominantly by the Gulf Stream drift, are to be traced to the variations in the strength of the latter, then a cause suggests itself. In fig. 8 the curve of relative numbers of sunspots is represented, and it will be seen that the fluctuations in the numbers of sunspots observed during the period 1874–96 are roughly similar to the fluctuations in the productivity of the Lofoten cod-fisheries.

Sooner or later a connection between the sunspots and the abundance of fishes in a part of the sea was certain to be suggested, and one is prepared to be sceptical as to the value of such a comparison. But it appears to me that the data quoted by Helland-Hansen and Nansen afford good grounds for regarding the hypothesis as a working one. A fairly large volume of evidence has been collated by Arrhenius to show that many phenological, climatic, and other phenomena are to be associated with these paroxysms in solar activity. On the one hand, it appears impossible to seek for any other cause of the greater variations in the intensity of the Gulf Stream flow than some cosmic one; and variations in the intensity of solar radiation seem to be an extremely probable cause. On the other hand, it is just in such events as the variations in the intensity of the Gulf Stream flow that one would seek for the manifestations of variations in the intensity of solar radiation. The current in northern latitudes is an extremely stable one, and we can hardly imagine it to vary except under the impress of some powerful force; while such variations as would be produced would be likely to be well marked, and would persist for some time.

THE PHYLOGENY AND INTER-RELATIONSHIPS OF THE GREEN ALGÆ

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In any attempt to trace the evolution of the vegetable kingdom a considerable amount of interest must naturally centre about its simplest representatives, which may justly be regarded as having some resemblance to the primitive ancestral forms from which the higher types have sprung. The lowest forms of plants are at the present day placed in the class of the Thallophyta, which embraces the two large divisions of the Algæ and Fungi. These are separated largely on physiological grounds, the latter being devoid of the chlorophyll, possessed by the Algæ and the remaining classes of the vegetable kingdom. The absence of chlorophyll in the Fungi and the consequent inability to manufacture their own food-substances in the way characteristic of the majority of plants have led to far-reaching peculiarities. As our knowledge of the Fungi improves and a better insight into these peculiarities is obtained, it is, however, becoming increasingly clear that we shall soon have to abolish the artificial distinction between Algæ and Fungi, and recognise the fact that the latter are merely parasitic or saprophytic degenerates of diverse lines of algal evolution (*cf.* especially Davis 19,[1] Lagerheim 45, Lotsy 46, Stevens 65). In any case, however, the Fungi must be regarded as a side-line, and it is unnecessary to consider them in connection with any theory as to the mode of evolution of the higher green plant, which no doubt arose directly from algal ancestors.

The Algæ were formerly classified in four groups according to the colour of the pigment in their chloroplasts: the green Algæ or Chlorophyceæ, the brown Algæ or Phæophyceæ, the red Algæ or Rhodophyceæ, and the blue-green Algæ or Cyanophyceæ. Of these the green and blue-green forms are

[1] References to all the memoirs quoted will be found in the bibliography at end of the second part of this article.

largely freshwater or subaerial, while the two remaining groups are almost entirely marine. It is obviously only among the green Algæ (in which the chlorophyll in the plastid is not obscured by the deposition of a strange pigment, as is the case in the other three groups) that we can expect to find hints as to the ancestry of the higher representatives of the vegetable kingdom, and it is interesting to note in this connection that they include a far larger number of what may be regarded as simple and primitive forms than the remaining three groups of the Algæ. It is therefore with the green Algæ alone that we shall be concerned in the present article.

The investigations of the last fifteen years have very considerably modified the conceptions of the green Algæ which existed when they were all classed together as Chlorophyceæ. In particular two points have become evident—viz. that we must distinguish at least two perfectly distinct classes (Isokontæ and Heterokontæ) among them, and that they are linked by intermediate forms to the group of the Flagellates, at present still a rather motley assemblage of incompletely known forms, which show a curious mixture of plant and animal characteristics and are probably the survivors of the simplest groups of organisms from which both kingdoms have arisen. It is the purpose of this article to indicate briefly the diverse lines of evolution that can at present be traced among the green Algæ and to consider their connection with the Flagellates. Before turning our attention to these matters it will be well, however, to indicate briefly the salient features in the structure and life-history of the forms under consideration.

The green Algæ comprise unicellular and colonial forms (fig. 1, A, fig. 2, B) among their simpler representatives, while the higher forms are either branched or unbranched chains of cells (so-called filaments, fig. 3, G), flat plates of cells or (among the Siphonales) complicated developments of multinucleate cells (so-called cœnocytes). In the simpler forms each cell usually contains but a single chloroplast, which is of large size and variable shape, although generally conforming to a common ground plan within one and the same group. In many cases the chloroplasts contain one or more pyrenoids (fig. 1, A, *p*), *i.e.* specialised bodies, which are lodged within the substance of the chloroplast and are apparently of the nature of a store of nitrogenous reserve-material; under normal conditions, however, they are remarkably persistent and serve as centres for the formation of starch during assimilation. They are not found in those forms which do not produce starch as the result of photosynthesis. Except in the case of reproductive elements, the algal cell is always enveloped by a well-defined cell-wall, which often has a characteristic shape. A certain number of the simpler unicellular forms of the green Algæ are motile during the greater portion of their life-history; the movement is effected by the lashing to and fro of two or more delicate protrusions (so-called cilia) of the protoplasm of the cell or of each cell of the colonial forms, these cilia generally arising close together (fig. 1, A). The cells of these motile forms are generally also provided with a reddish eye-spot (fig. 1, A, *s*) which is situated in diverse positions, but always in contact with the chloroplast, and seems to serve as an organ for the perception of light-stimuli. Two peculiar vacuoles (so-called contractile vacuoles, fig 1, A, *v*), which are constant in position and are characterised by their rhythmic alternate expansion and contraction, are also found side by side just beneath the point of attachment of the cilia of the motile forms.

The reproductive processes of the green Algæ show much diversity, as will become evident from the subsequent considerations, but at this point only a general outline will be given. At the outset it is well to realise that all the different methods of reproduction peculiar to each form depend on the realisation of certain outside conditions (Klebs 40) and not on any inherent tendency; Klebs has shown in a considerable number of cases that any particular method of reproduction can be called forth at will by the introduction of certain appropriate external conditions. The most important methods of reproduction are the asexual and the sexual. In asexual reproduction the contents of certain cells (which are known as zoosporangia, and may or may not differ in shape and appearance from the ordinary vegetative cells) contract away from the wall and are liberated with or without previous division as motile cells, which are known as zoospores (fig. 3A). The latter are generally more or less pear-shaped, are provided with two or more cilia at the pointed front end, and are naked (*i.e.* devoid of a cell-wall).

After a period of movement, which is of varying duration (generally several hours), the zoospores come to rest and the cilia are drawn in; they become attached to some dark object by their pointed front end, a cell-wall is excreted and cell-division sets in, whereby a new organism is produced. In the simplest unicellular green Algæ, which are motile during most of their life-history, the zoospore does not differ except in size from the mother-cell, and becomes enveloped by a cell-wall before it is liberated from the latter (*cf.* the description of *Chlamydomonas* on p. 628). In the higher forms it often happens—either as a normal phenomenon or under certain exceptional conditions—that after contraction and division of the contents of the zoosporangium has taken place, these contents are not liberated as zoospores, but immediately become enveloped by (generally thick) cell-walls. The reproductive cells thus formed are obviously a mere modification of the ordinary zoospore and are known as aplanospores; they are generally capable of passing through a prolonged resting-period before germination takes place.

In the sexual reproduction of the green Algæ we have to distinguish between cases in which the sexual cells (gametes) are alike (isogamy) and cases in which they are unlike (anisogamy). In the isogamous forms the sexual cells are quite similar to the zoospores except that they are of smaller size and are consequently produced by a larger number of divisions in the cell-contents of the mother-cells (so-called gametangia) than is the case in zoospore-production. The perfectly identical gametes of these isogamous forms meet in pairs during their movement (fig. 1, F) and fuse together to form a resting spore, which becomes enveloped by a thick membrane and is known as a zygospore. In some of the forms showing this method of sexual reproduction there are slight differences in size and power of movement between the two fusing gametes; these slight indications of anisogamy are much more pronounced in other forms, and thus we can trace nearly all stages from typical isogamy to that extreme of anisogamy which goes under the name of oogamy and is found only in the highest types of each series of algal forms. Here we have to distinguish between a female cell or ovum, which is generally a large more or less spherical and deeply coloured motionless cell, mostly remaining in the female organ or oogonium until liberated from the latter by its decay, and a many times smaller almost colourless or frequently yellowish male cell or spermatozoid, which is actively motile and obtains access to the ovum within the oogonium by a special aperture, which is formed in the wall of the latter. The oogonium is generally a somewhat enlarged cell, characterised by its ventricose appearance, and as a rule the whole of the contents contract away from the wall to form but a single ovum. The spermatozoids, which generally show some resemblance to the zoospores of the same Alga, but are much smaller and have at the best but a minute chloroplast, are formed generally in considerable numbers in the male organ or antheridium. The fertilised ovum, after becoming surrounded by a thick wall, constitutes a resting oospore. Zygospores and oospores may on germination either give rise to a new organism straight away, or may first form a number of zoospores, which on coming to rest produce the mother-plant.

The brief outline given above will suffice to indicate the general features of the group, the discussion of whose phylogeny will occupy us in the following pages.

The subject cannot be more worthily opened than by a brief consideration of the genus *Chlamydomonas* (Goroschankin 29 and 30, Dill 21, Dangeard 16 and 18, Klebs 40), which may without exaggeration be said to have played the most important part in the advances of the last fifteen years. The genus comprises a large number of species, most of which inhabit small stagnant pools or larger areas of fresh water, while a few are marine. The individuals (fig. 1, A) are unicellular, more or less spherical or ovoid in shape, and are in many cases provided with a pronounced kinoplasmic protrusion (Membranwarze) at the front end, from which the two equal cilia arise (four cilia in the allied genus *Carteria*). The cell-contents include the very characteristic bell- or basin-shaped chloroplast, the thickened floor of which contains one (sometimes two) spherical or sausage-shaped pyrenoids (*p*; the allied genus *Chloromonas* (Wille 69) differs in lacking pyrenoids), a central nucleus, and two contractile vacuoles (*v*), which pulsate alternately, and are situated beneath the point of attachment of the cilia. Abundant starch is formed as the result of photosynthesis. A dot-like or linear eye-spot (*s*) is situated laterally, but generally nearer the front end of the cell, and is apposed to the chloroplast;

only a thin layer of protoplasm separates this eye-spot from the cell-wall, which forms the external boundary of the *Chlamydomonas*-individual and is provided with two delicate pores at the front end for the protrusion of the cilia. Under normal conditions the *Chlamydomonas*-individual moves actively through the water with the ciliated end directed foremost, while the body at the same time rotates on its own axis. It shows marked chemotactic and phototactic sensitiveness, being, for instance, attracted by light not exceeding a certain degree of intensity.

Asexual (or vegetative) reproduction is effected simply by subdivision of the individual (fig. 1, B–D). The cilia are drawn in or cast off, the nucleus divides, the pyrenoid becomes doubled, and thereupon the cell-contents undergo gradual constriction into two halves. One or two further divisions generally take place in planes at right angles to the first. The daughter-individuals, while still lying within the mother-cell, become enveloped by new walls of their own, cilia appear, and ultimately the products of division (comparable to the zoospores of the higher types) are liberated as motile elements identical, except as regards their smaller size, with the mother-individual. The direction of the first division-wall is not the same in all the species of *Chlamydomonas*, and constitutes a point of some importance (Dill 21). In certain species of the genus (e.g. *C. angulosa*, *C. gigantea*, also in the allied genera *Chloromonas* and *Carteria*), the first division is longitudinal (fig. 1, B), being effected by a constriction appearing both at the front and back end of the cell; in these cases the second division is generally also longitudinal. In the majority of species (e.g. *C. Reinhardi*), however, the first division-wall is at right angles to the longitudinal axis of the cell. These two cases are connected by intermediate forms; thus in *C. longistigma* the first division commences in a direction parallel to the longitudinal axis, but before the process of constriction is complete rotation to the transverse plane (fig. 1, C, D) takes place (in from thirty to forty minutes), so that the division comes to resemble that of the second type. Oltmanns (53, p. 143) has suggested that the first division may in all cases be a longitudinal one, but that in many species rotation to the transverse plane takes place at such an early stage that it is not recognisable; this view is based on the consideration that symmetrical halving of the chloroplast is impossible except by a longitudinal division (cf. also Klebs 40, p. 426). In every case the four or eight products of division ultimately become ranged parallel to each other and to the longitudinal axis within the mother-cell.

Under certain, not yet fully recognised, conditions the products of such asexual division are not immediately liberated from one another but remain within the mother-cell and continue

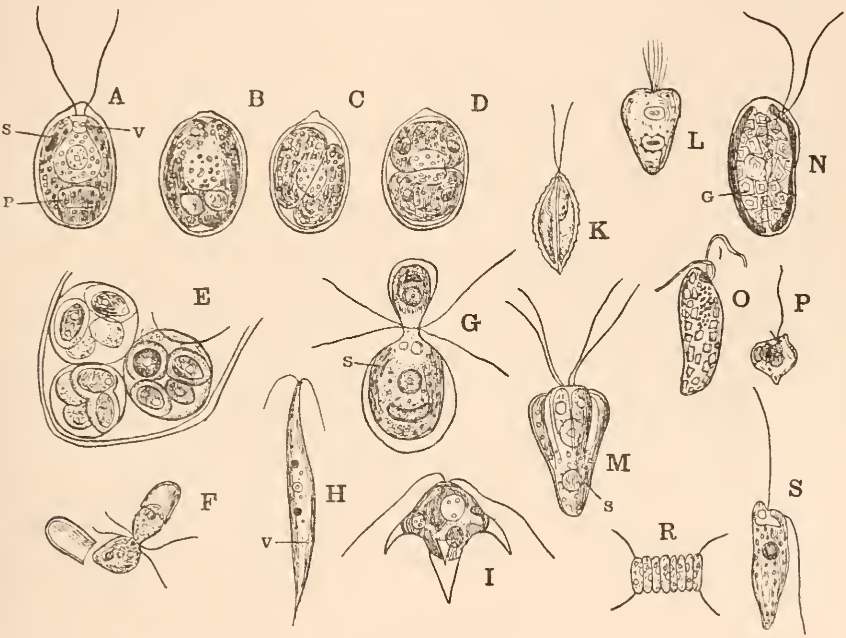


FIG. I.

A–B, *Chlamydomonas angulosa*, Dill (A, vegetative individual; B, dividing). C–D, *C. longistigma*, Dill (stages in division). E, *C. Braunii*, Gor. (Palmella-stage). F, *C. media*, Klebs (conjugation). G, *C. Braunii*, Gor. (conjugation). H, *Chlorogonium euchlorum*, Ehrbg. I, *Brachiomonas submarina*, Bohlin. K, *Phacotus lenticularis*, Stein (side-view). L, *Polyblepharis singularis*, Dang. M, *Pyramimonas tetrarhynchus*, Schmarda. N, *Cryptomonas erosa*, Ehrbg. O, *Chilomonas Paramœcium*, Ehrbg. P, *Chloramœba heteromorpha*, Bohlin. R, *Scenedesmus quadricauda* (Turp.), Bréb. S, *Vacuolaria flagellata*, Senn. (A–D, and M after Dill; E and G after Goroschankin; F after Klebs; H and K after Stein; I and P after Bohlin; L after Dangeard; N and O after Senn; R after Nägeli; S after Stokes).

to divide, the walls of the successive mother-cells becoming highly mucilaginous. In this way extensive gelatinous clumps are formed, which are known as *Palmella*-stages (fig. I, E). With the return of normal conditions each cell acquires cilia, and, slipping out of the enveloping mucilage, commences an independent existence. Whereas in the majority of species of *Chlamydomonas* the Palmella-stage is quite a subsidiary phase in the life-history, there are certain forms (e.g. *C. Kleinii*) in which this stage is much more prolonged and even dominant (Schmidle 59).

In its sexual reproduction *Chlamydomonas* shows many noteworthy peculiarities. The majority of species are isogamous, the gametes being formed by a similar process of subdivision of the cell-contents as obtains in the case of asexual reproduction, with the important difference that a larger number of divisions occur (16 or 32 gametes are the rule) and that the products of division ordinarily remain naked and conjugate in this condition. A point of much importance, however, lies in the fact that in those species of *Chlamydomonas* in which the first division-wall is longitudinal the gametes are provided with an enveloping cell-wall, which is cast off just prior to or during conjugation (fig. 1, F). This is no doubt to be regarded as a primitive feature, which coincides well with the probably primitive nature of longitudinal divisions in these species. The only anisogamous form is the well-known *C. Braunii* (Goroschankin 29); here there is a constant difference in size between the (clothed) gametes derived from different mother-cells, and conjugation always takes place between a large gamete, which has come to rest, and a much smaller motile one (fig. 1, G). The zygospores, which are capable of passing through a prolonged resting-period, are provided with a thick stratified membrane and generally contain a quantity of red oil. On germination the oil is absorbed, the contents become green, and subdivision of the latter takes place, resulting in the formation of a number of ordinary *Chlamydomonas*-individuals. In a few species (e.g. *C. gigantea*), resting-stages are formed in a different way; the contents of an ordinary individual slip out from the cell-membrane and, after performing metabolic movements for a little time, become rounded off and excrete a thick membrane (constituting a so-called cyst).

Apart from the three very closely allied genera *Chlamydomonas*, *Chloromonas* (Wille 69), and *Carteria*, the Chlamydomonadaceæ include a number of forms, which show more considerable differences. It is unnecessary, however, to deal with these in any detail, but one or two points of interest may be indicated. The genera *Chlorogonium* and *Hæmatococcus* (Wille 69, Wollenweber 71) are peculiar in having a considerable number of contractile vacuoles (v) distributed through the protoplasm of the cell, and this has been looked upon as a primitive feature. The individuals of *Chlorogonium* (fig. 1, H) are fusiform, have a thin, closely adherent cell-wall, and a rather ill-defined chloroplast with a considerable number of pyrenoids. *Hæmatococcus* (*Sphærella*) is distinguished by the marked separation of the outer portion of the cell-wall from the cell-contents, probably due to the mucilaginous character of the inner layers of the wall; the space between contents and wall is traversed by branched protoplasmic strands, which have been regarded as pseudopodia by Wille (69), or, perhaps better, as branched pits by Oltmanns (53, p. 141). The genus *Brachiomonas* (Bohlin 7, West 66), lastly, is interesting owing to its peculiar shape (fig. 1, I) and to the fact that the daughter-individuals acquire their mature form before they are liberated from the mother-cell (autospore-development).

In addition to this main series we have two interesting side-lines, the colourless Polytomaceæ (Franzé 26) and the bivalved Phacotaceæ. The former are an almost exact replica of the Chlamydomonadaceæ, but lack even the remains of a chloroplast, although large amounts of starch-grains are formed in the cells. The two genera of the order are incompletely known, but *Polytoma* is like a colourless *Chlamydomonas*, while *Chlamydoblepharis* in its offstanding wall resembles a *Hæmatococcus*. These genera call for comparison with the colourless forms of *Euglena*, and like these they are no doubt saprophytic degenerates of green ancestors.

The chief respect in which the Phacotaceæ (Dangeard 16 and 17) differ from the Chlamydomonadaceæ lies in the fact that the coarse wall consists of two halves, which fit together by their thickened rims like two watch-glasses (fig. 1, K). In the formation of daughter-cells the two halves of the wall move apart from one another, but remain connected by mucilage, until the daughter-cells have attained their mature structure.

Before attempting to trace out the numerous lines of algal descent that emanate from *Chlamydomonas* and its allies, we may for a moment turn our attention to the Flagellate ancestry of the forms hitherto described. The first genus that calls for notice in this connection is Dangeard's *Polyblepharis* (Dangeard 16), a unicellular organism, having a simple ovoid shape, closely resembling a *Chlamydomonas*, but bearing from six to eight cilia, and devoid of a cell-wall (fig. 1, L). The front end of the cell is generally somewhat truncated, while towards the back it narrows down slightly. The place of the cell-wall is taken by a denser peripheral layer of protoplasm, which, however, allows of a limited change of shape (metaboly). A nucleus occupies the centre of the cell, while the bell- or goblet-shaped chloroplast, which contains a single pyrenoid, is situated at the back end, and bears a red eye-spot at its margin. One or two contractile vacuoles lie near the bases of the cilia. The only method of reproduction observed is by means of *longitudinal* division of the contents after the cilia have been drawn in. The organism also possesses the power of encystment, a single individual being formed from the cyst on germination. The genus *Pyramimonas* (Dill 21, Griffiths 31) is a close ally and agrees in many respects with *Polyblepharis*, but the cell is characteristically pyramidal and has four wing-like protrusions: there are, further, only four cilia, and the eye-spot (*s*) is situated posteriorly (fig. 1, M). A sexual process has not been observed, and reproduction appears to be effected solely by longitudinal division, the process of constriction commencing at the back end of the cell and generally taking place during the motile stage. A genus has been recently described under the name of *Dunaliella* (Teodoresco 65a) which appears to be closely allied to the two forms just considered. Like them it lacks a cell-wall, has a bell-shaped chloroplast, longitudinal division, etc., but its particular interest to us lies in the fact that a sexual process between isogamous gametes has been established in this case.

The most essential peculiarities of these three organisms, as contrasted with *Chlamydomonas* and its allies, lie in the absence of a cell-wall and the consequent power of change of shape, in the prevalence of longitudinal division and in the power of forming cysts. These are all features which are characteristic of the Flagellata, as we shall see. On the other hand, in their motile unicellular character, in the characteristic chloroplast, in the formation of starch during assimilation and in the general distribution of the cell-contents, *Polyblepharis*, *Pyramimonas* and *Dunaliella* show a very marked resemblance to *Chlamydomonas* and the allied genera. This resemblance is heightened when we recollect that certain species of *Chlamydomonas* show longitudinal division and have the faculty of encystment (cf. pp. 628 and 630). The genera *Polyblepharis*, *Pyramimonas* and *Dunaliella*, which with one or two other, not very well known, forms constitute the Polyblepharidaceæ, thus unquestionably show a very marked mixture of Chlamydomonad and Flagellate characteristics. They are therefore regarded as very important links in the chain connecting the green Algæ with the Flagellata (Dill 21). It seems probable that *Polyblepharis* with its relatively simple shape and more numerous cilia is a more primitive form than *Pyramimonas*, while *Dunaliella* appears to be most specialised.

The next step carries us back into the group of the Flagellata (Klebs 38, Senn 63), and we have now to seek for a possible connection among the organisms of this group with the Polyblepharidaceæ. Senn subdivides the Flagellata[1] into seven groups, three of which are colourless, while four, which may be styled the algal Flagellates (Chrysomonadineæ, Cryptomonadineæ, Chloromonadineæ, Euglenineæ), are possessed of pigments, whereby holophytic nutrition is possible. It is obviously among these four groups that we must look for the allies of the Polyblepharidaceæ. Only two of them, the Cryptomonadineæ and Euglenineæ, have a green pigment of the nature of chlorophyll. Of these the Euglenineæ may be dismissed at once, since their organisation is too complex to admit of a close comparison with the green Algæ, and since starch, one of the great characteristics of the algal forms hitherto considered, is not the product of assimilation. We are thus left with the Cryptomonadineæ (Senn 63), which, as far as our present knowledge goes, include rather a diverse collection of forms. Of these the genera *Cryptomonas* and *Chilomonas* are of the most interest from our point of view. *Cryptomonas* (fig. 1, N) occurs both in fresh and salt water, is actively motile by means of two cilia arising in a depression a little way behind the obliquely truncated front end of the somewhat flattened ovoid cell, and is provided with two parietal chloroplasts, which have the shape of a shallow watch-glass and may be green or yellow or otherwise coloured. Numerous hexagonal starch-grains (*g*) are apposed to the inner surfaces of the chloroplasts. A nucleus is situated posteriorly, and there are two contractile vacuoles at the front

[1] The most essential characters of the Flagellata lie in their motile condition (with an often pronounced sedentary tendency), in the absence of a definite cell-wall and the consequent more or less pronounced power of change of shape, in the longitudinal division, the scarcity of sexual reproduction, and the faculty of encystment (cf. Klebs 38).

end of the cell. *Cryptomonas* is capable both of holozoic and holophytic nutrition. Reproduction takes place by longitudinal division—in most cases apparently while the organism is in a kind of *Palmella*-stage and enveloped by stratified mucilage, which sometimes shows the reactions of cellulose. Oval or rounded resting-cysts, having a coarse cellulose-membrane, are formed in response to unfavourable conditions. The genus *Chilomonas* (fig. 1, o) differs principally in being colourless; the chloroplasts are replaced by leucoplasts, numerous starch-grains generally occurring in the protoplasm. The method of nutrition is saprophytic. Both genera agree with other Flagellates in lacking a cell-wall, but they do not possess as marked a power of changing their shape as do other members of the group. The prevalence of the motile phase in the life-history is also a rather striking feature.

In their possession of chlorophyll and in forming starch the Cryptomonadineæ show significant analogies to the green Algæ, and there are certain other points of resemblance to which attention may be drawn (cf. also Scherffel 58). The group includes both holophytic forms and saprophytic forms producing starch (cf. Polytomaceæ, p. 631). The two parietal chloroplasts of *Cryptomonas* are readily derivable from a basin-shaped chloroplast, like that of the Chlamydomonads, by imagining splitting to have taken place, so that both Cryptomonadineæ and Chlamydomonadineæ (*i.e.* in the wide sense, including Polyblepharidaceæ, etc.) may be derived from a common type with a basin-shaped chloroplast. In view of the fact that the Chlamydomonadineæ, although otherwise obviously of a predominant algal character, have the motile phase as the dominating one in the life-history, we may expect the same to be the case in the Flagellate ancestors, so that it is quite in accord with the theory we are endeavouring to support that the Cryptomonads show little of that sedentary tendency which is sharply marked in certain other Flagellate groups (*e.g.* Chrysomonadineæ or Chloromonadineæ, Senn 63). Other minor features that lend support to a relationship between Cryptomonadineæ and Chlamydomonadineæ are the limited power of change of shape in the former group (cf. Polyblepharidaceæ), the two contractile vacuoles at the front end of the cell, the two cilia of equal length (characteristic of the Isokontæ among the green Algæ, (cf. the second part of this article), and the occurrence of mucilage and cyst-membranes, giving the reactions of cellulose in the Cryptomonadineæ. On the other hand, there are some marked differences (*e.g.* the origin of the cilia from within a depression and the obliquity of the cell in the Cryptomonadineæ) which point to the relationship not being too close. On the basis of the existing evidence we may perhaps assume a hypothetical ancestral group of Flagellates (having several equal cilia, a basin-shaped chloroplast, starch as the product of assimilation, no pyrenoid and a limited power of change of shape), giving rise on the one hand to the Cryptomonadineæ and on the other hand to the Polyblepharidaceæ, the Chlamydomonadaceæ branching off during the evolution of the latter. Unfortunately it is as yet impossible to trace the origin of *Chlamydomonas* and its allies more completely than we have done, for *Cryptomonas* is, of course, in some respects more highly differentiated than the Polyblepharidaceæ, which I should prefer to regard as still being Flagellates. The Flagellate origin of the Isokontan series (of which *Chlamydomonas* is the starting-point) must, however, be regarded as fully established. The most serious obstacle lies in the absence of sexual reproduction in the Cryptomonadineæ, but in view of the recent discovery of a process of conjugation rather similar to that of the Chlamydomonadaceæ in the Euglenineæ (Dobell 22) this difficulty may very probably be removed when our knowledge of these groups improves. Sexuality has only quite recently been established in a member of the Polyblepharidaceæ (viz. *Dunaliella*).

We may now once more return to the Chlamydomonadaceæ and advance from them along the lines of evolution that are by slow degrees becoming manifest. One very obvious line has long been recognised; the forms that constitute it are classified as the Volvocaceæ, and may be looked upon as a glorification of the motile *Chlamydomonas*-individual. The series is so well known (cf. Oltmanns 53) and has so frequently been described that it will suffice to indicate certain salient features. It would seem that this evolution in the colonial direction had started from two or three different points, for in *Spondylomorum* (Stein 64) (fig. 2, A) we have a colonial *Carteria*, in *Stephanosphæra* (Hieronymus 33) a colonial *Hæmatococcus*, while the familiar *Gonium* (Migula 50) (fig. 2, B) is a colonial *Chlamydomonas*. Only one of these attempts at colony-formation has, however, proved successful, namely that of *Gonium*, from which we can trace a very striking series through *Pandorina*, *Eudorina*, and *Pleodorina* to that marvel of colonial perfection, the genus *Volvox* (Klein 41). This series affords a very striking example of the recapitulation of phylogeny by ontogeny in the invariable recurrence of the flat *Gonium*-stage in the early phases of the development of the spherical colonies and in the antheridial colonies of *Eudorina* and *Volvox*, but this is only one of the innumerable interesting points that a study of this series brings to light. That a form very much like *Chlamydomonas* was the

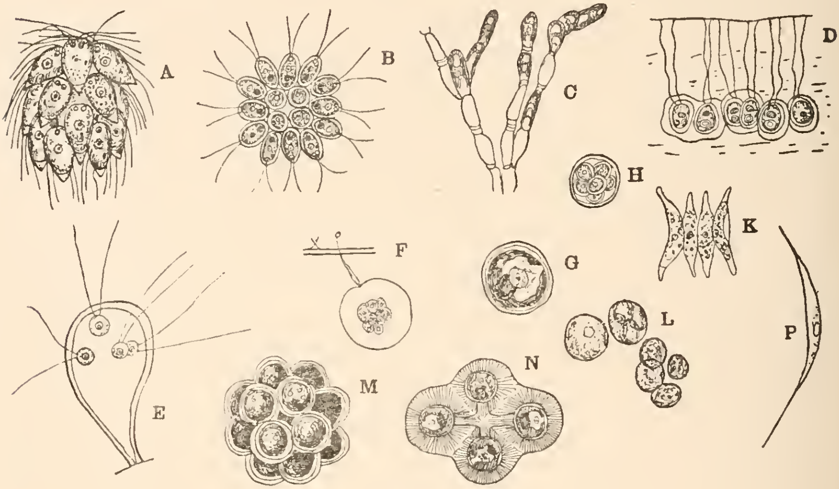


FIG. 2.

A, *Spondylomorum quaternarium*, Ehrb. B, *Gonium pectorale*, Müll. C, *Chlorodendron subsalsum* (small portion of a colony). D, *Tetraspora* (with pseudocilia). F, *Apiocystis Brauniana*, Näg. (pseudocilia). F, *Physocytium confervicola*, Borzi. G, *Cœlastrum proboscideum*, Bohlin (single cell). H, *Chlorella conglomerata* (Artari), Oltmanns. K, *Scenedesmus acutus*, Meyen. L, *S. acutus* (spherical cells formed in presence of much oxygen). M, *Cœlastrum microporum*, Näg. N, *Dictyosphærium pulchellum*, Wood. P, *Schröderia setigera*, Lemm. (A and B after Stein; C after Davis; D, E and P after Chodat; F after Borzi; G, M, N after Senn; H after Oltmanns; K, L after Beyerinck)

ancestor of the group is obvious when the complete accord in cell-structure between this genus and the different members of the Volvocaceæ is recognised; an isolated cell from a colony of one of the latter is in some cases almost indistinguishable from a *Chlamydomonas*-individual. As one advances along the series a tendency towards division of labour among the cells of the colony becomes more and more manifest. Already in *Pandorina* (Pringsheim 57) we find that the cells at that end of the colony which is directed forwards during motion have larger eye-spots than the others, and this feature is most strikingly developed in *Volvox*, where the cells at the posterior end of the colony are practically destitute of eye-spots. In *Pleodorina* (Kofoid 42, Merton 49) and *Volvox* again we have the relegation of asexual reproduction to certain larger cells at the back end of the hollow spherical colony, these cells being devoid of cilia in the case of the latter genus. In *Pleodorina californica* half the cells of the colony are thus destined for reproductive purposes, while in *P. illinoisensis* and *Volvox* the number is far smaller. This distinction of somatic and vegetative cells is one of the most important steps in the evolution of the Volvocaceous series, for with it comes the phenomenon of death, a certain portion of each colony ceasing to exist after its life-cycle is accomplished. In *Volvox* we also have the restriction of the sexual function to certain cells of the colony, a marked step in advance of *Pleodorina* and *Eudorina*, in which every cell has the power of forming sexual elements. Hand in hand with the division of labour just noticed goes a gradual evolution of oogamy, commencing with traces of anisogamy in *Pandorina* (Pringsheim 57), becoming much more pronounced in *Pleodorina* (Merton 49) and *Eudorina* (Goebel 28), and culminating in the ova and spermatozoids of *Volvox*. In this latter genus we have therefore arrived at a stage of differentiation which is only found among the highest representatives of other lines of algal descent. It was perhaps the conservative tendency shown by the vegetative structure of the Volvocaceous colony that precluded further development along these lines, for it is difficult to conceive how *Volvox* could be further elaborated. It is interesting to notice that the *Gonium*-series is the successful one at the present day as regards abundant occurrence, for both *Spondylomorum* and *Stephanosphæra* are very rare forms as compared with the others.

Returning again to *Chlamydomonas*, we have now to follow up evolutionary series in which the power of independent movement becomes restricted. This evidently took place along various lines, of which we shall only be able to notice the more important. We first have what we may call the *dendroid* series, constituted by Oltmann's family, the Chlorodendraceæ (Oltmanns 53, p. 136). The genus *Chlorodendron* (fig. 2, c) consists of elongated cells, situated at the ends of hyaline stalks and united to form a branched colony. The cells have the usual basin-shaped chloroplast and a large eye-spot, which is present both in the resting and motile cells. The latter have four cilia (cf. *Pyramimonas*), and after a period of movement become attached by their ciliate end, which gradually produces a long stalk at the lower extremity of the cell. By oblique longitudinal division of the cells the branched colony is gradually produced. To this family we may perhaps also refer the genus *Chlorangium*,[1] which Oltmanns places among the Chlamydomonadaceæ and is of interest because the cells have two lateral chloroplasts (Stein 64). Both genera are readily derivable from the *Chlamydomonas*-type, and are chiefly interesting because they are the algal analogues of various Flagellate forms (such as *Dinobryon*, *Colacium*, etc.), although themselves not much beyond the Flagellate boundary.

The next series, that of the Tetrasporaceæ (so called because of the prevalent arrangement of the cells in fours), is more important. The typical representatives of this family (i.e. *Tetraspora* and *Apiocystis*) are easy to derive from *Chlamydomonas* by assuming a prolongation of the palmelloid phase, such as is well shown by *C. Kleinii* (Schmidle 59); indeed, in *C. apiocystiformis* (Artari 2, p. 39) we even have a *Palmella*-stage very closely resembling the Tetrasporaceous genus *Apiocystis*. Both in this genus and *Tetraspora* (Chodat 13) we have cells, showing all the characteristic features of a *Chlamydomonas* except for the eye-spot and the cilia, embedded in considerable numbers in an attached mass of mucilage, which in *Tetraspora* (fig. 2, D) is more or less irregular in shape, while in *Apiocystis* (fig. 2, E) it has a characteristic pear-shape. When the cells are liberated as motile elements in asexual reproduction it is very hard to distinguish them from *Chlamydomonas*-individuals. Apart from this, one of the most interesting points about the Tetrasporaceæ are their *pseudocilia*, which in *Tetraspora* (Schröder 61) extend only up to the surface of the mucilage enveloping the colony (fig. 2, D), while in *Apiocystis* (Correns 15) they project some way beyond it (fig. 2, E), the projecting portion being provided with a special mucilage-sheath (not shown in the figure). These

[1] It seems a little questionable whether the affinity between *Chlorangium*, *Chlorodendron* and *Chlamydomonas* is really close enough to warrant placing them in the same order. The first of these genera seems more referable to the Cryptomonad line with its two lateral chloroplasts than to the Chlamydomonad line, to which *Chlorodendron* undoubtedly belongs.

pseudocilia consist of protoplasmic processes, and one is of course tempted to look upon them as remnants of cilia, which have become useless for their ordinary function—a view which has fallen into disrepute of late years, chiefly because the pseudocilia are not used for purposes of locomotion. There would, however, seem to be some ground for regarding them as derived from cilia, and this for the following reasons. In *Chlamydomonas Kleinii* (Schmidle 59) we have a good example of undoubted cilia persisting within the mucilage of a *Palmella*-stage. In some of the Chlamydomonadineæ (e.g. *Hæmatococcus*, *Polytoma*, and *Chlamydoblepharis*) the cilia of the mother-cell persist up to the time of the liberation of the daughter-cells. In a *Tetraspora* or *Apiocystis* this persistence of the cilia would obtain throughout the whole colony derived from the germination of a motile zoospore, and the cilia would only be discarded when a new motile element was to be formed. In the division of a *Tetraspora* or *Apiocystis* cell within the mucilage each daughter-cell is stated to receive one of the two pseudocilia and forms the other *de novo.* Keeping these facts in mind, it seems not unlikely that in the pseudocilia of the Tetrasporaceæ we have the remains of actual cilia, which have become modified, perhaps to serve some other function (respiration ?). This view also receives some support from a study of the genus *Physocytium*, which was discovered as an epiphyte on fresh-water Algæ by Borzi (10) but has not since been observed. The organism (fig. 2, F) consists of globular colonies, which are attached to the substratum by two delicate threads and contain a few irregularly arranged cells. These ultimately acquire two short cilia and become liberated as zoospores. After swarming about for some time the latter become attached by the apices of their cilia, a membrane appears around them, and they subdivide to form a new colony. Here, therefore, the cilia appear to serve directly as organs of attachment at the termination of the motile phase—a function which they perform in many other cases, although *Physocytium* is peculiar in their subsequent persistence.

Many authorities (Wille 68, Blackman and Tansley 6) include a number of filamentous forms, generally more or less enveloped by mucilage, among the Tetrasporaceæ, and have suggested that these forms indicate the direction of evolution of the higher filamentous Algæ. This is, however, to say the least, doubtful.

Some of the genera here included (e.g. *Hormospora*) are no doubt peculiar stages of the common filamentous Alga *Ulothrix*, while others (such as *Hormotila* and *Hauckia*) diverge widely from the typical Tetrasporaceæ in the presence of several chloroplasts in the cells and in the liberation of a large number of zoospores from each cell; it is not impossible that these forms are again merely stages in the life-history of some of the higher Algæ, but in any case our knowledge of them is so imperfect that it is not possible to classify them at present. The higher filamentous Algæ, as we shall see, probably arose from another source (cf. p. 645, and the second part of this article).[1]

Passing over a large number of more or less imperfectly known forms (*Glœocystis*, *Palmella*, *Botryococcus*, *Palmodactylon*, etc.), which all represent mere modifications of the palmelloid tendency, we may turn our attention to a new line of evolution, the Protococcales of Oltmanns, which is one of the most important and instructive of the series derivable from the *Chlamydomonas*-stock. We owe our present clear insight into this series to Senn (62) and Oltmanns, who have done more to cast light on this difficult assemblage of forms than any one in the last twenty years. There are two distinct tendencies represented in this series of the Protococcales, and the forms involved are placed by Oltmanns (53, p. 169) in the Scenedesmaceæ and the Protococcaceæ. The simplest form of the Scenedesmaceæ is the genus *Chlorella* (including a considerable number of forms referred by Artari (2) to *Pleurococcus*). This Alga (fig. 2, H) consists of rounded cells with an almost spherical chloroplast, which is interrupted only by an aperture on one side of the cell, a pyrenoid being embedded in the chloroplast at the point opposite to the aperture. This chloroplast is of course to be regarded as an enlargement of the *Chlamydomonas* chloroplast. The reproductive process is very simple, the contents subdividing into two or four parts, each of which acquires a cell-wall (fig. 2, H), and is then liberated by the rupture of the membrane of the mother-cell. The daughter-cells may either separate or cohere to form small groups. Such a form is very readily derivable from the *Chlamydomonas*-type; it is only necessary to imagine that an enlargement of the chloroplast took place and that the products of division lost their power of free movement. Incidentally we may also notice that if the products of division cohere it is only necessary for gelatinisation of the walls to take place to give rise to a palmelloid form like *Glœocystis*, etc.

The first beginning of this *Chlorella*-tendency, as we may perhaps call it, was probably the drawing-in of the cilia of the *Chlamydomonas*-individual prior to division. There is some latitude in this respect in the different species, and we need only imagine an increasing prolongation of the interval between the drawing-in of the cilia and the process of division to arrive ultimately at the state of affairs obtaining in *Chlorella*. Looking upon *Chlorella* in this way, we can derive from it a considerable number of genera in which the daughter-cells cohere to form colonies in diverse ways (viz. *Scenedesmus*, *Cœlastrum*, *Dictyosphærium*). At first sight these forms do not appear to have very much in common with *Chlorella*, but we owe to Senn's (62) researches the discovery of the fact that the *Chlorella*-type may appear in them under certain special conditions. These colonial forms differ from the loose colonies of *Chlorella* in having their cells joined together by local development of their outer mucilage-envelopes. When there is an abundance of oxygen in the surrounding medium, however, the development of the connecting processes between the individual cells does not take place. After a number of generations the cells in many cases become rounded off, and in this condition are practically indistinguishable from individuals of *Chlorella* (fig. 2, G, L); as long as the influencing conditions remain the same, these cells, moreover, continue to divide just as in *Chlorella*, and the products of division are of the same type. This reversion to the *Chlorella* condition is certainly of phylogenetic significance, and warrants our placing this genus at the commencement of the present series.

In the genus *Scenedesmus*, which forms short chains of four or eight cells cohering by their lateral surfaces, two species have been studied from this point of view. In *S. acutus* the mucilage-envelope is particularly prominent between the flat surfaces of the cells, thus cementing them together, but it also forms the hyaline knob-like thickenings at the two ends of each cell (fig. 2, K). In *S. caudatus* (*S. quadricauda*) the cells cohere by little bridges of mucilage extending between the free tips of adjoining cells, the mucilage also forming the four characteristic processes to which the species owes its name (fig. 1, R). In *S. acutus* the *Chlorella*-stage is fairly easily obtained (fig. 2, L), but in *S. caudatus*, although the mucilage becomes uniformly developed in the presence of abundant oxygen, the cells cohere together as cylindrical structures and do not become rounded off.

In the genus *Cœlastrum*, in which the colonies consist of sixteen or thirty-two cells joined to form a hollow spherical reticulum, the species show similar differences in this respect. In *C. microporum* (fig. 2, M) the colonial tendency is little pronounced, and free cells are almost as frequent as colonies; in the latter the cells are connected by little round cushions of mucilage, which correspond in adjacent cells. The same is the case in *C. reticulatum*, but here the connections between adjoining cells are more pronounced, and colonies are the rule. In both these species the isolated cells are practically identical in shape with those in the colonies. This is, however, not the case in a third species—*C. proboscideum*—in which the cells of the colonies are polygonal, while the isolated ones are rounded; with this change there goes hand in hand a very pronounced colonial tendency, isolated cells being obtained only with difficulty. In both *Scenedesmus* and *Cœlastrum*, therefore, we can distinguish between species in which the colonial tendency is not yet very pronounced and species in which colonies are the rule. With the latter we may place the genus *Dictyosphærium*, in which isolated cells are only formed when there is a great excess of oxygen present. Each cell of the colony in *Dictyosphærium* is provided with a thick mucilage-envelope, showing a radiate structure, while the cell-contents are like those of a *Chlorella*. In division the cells nearly always divide into four daughter-cells, which become free by the rupture of the wall of the mother-cell into four basally connected lobes; a daughter-cell loosely coheres with the apex of each of these lobes, the whole being held together by the enveloping mucilage (fig. 2, N). As this process is repeated, we obtain colonies of quite considerable size. *Dictyosphærium*[1] is thus merely a special kind of colony, derived from the *Chlorella*-type of individual, and the same applies to other genera of the Scenedesmaceæ (*Oocystis* (Wille 70), *Schizochlamys*, *Raphidium*, *Kirchneriella*, *Sorastrum*, etc.), which space does not permit of considering here. Numerous allied genera, classified by Blackman and Tansley (6) as the Phyteliaceæ,

[1] *Dictyosphærium* has been stated to reproduce also by zoospores, but this awaits confirmation.

have become adapted as Plankton-organisms (fig. 2, P), but the differences between them and certain members of the Scenedesmaceæ are so insignificant that a separate family is scarcely warranted. A peculiarity of all these forms (Scenedesmaceæ and Phyteliaceæ) is that the daughter-individuals are only liberated from the mother-cell, when they have attained their complete differentiation. This has been styled *autospore-development*, and finds an interesting parallel among the Chlamydomonadaceæ in the genus *Brachiomonas* referred to above.

If we derive *Chlorella* and the other genera above discussed from a Chlamydomonad ancestry, it is only natural to expect that there will be forms, which have not gone to the extreme of giving up all power of adopting the motile condition but, whilst sedentary during the greater part of their life-cycle, have retained the power of reproducing by motile cells (zoospores). This is the case with Oltmann's Protococcaceæ, which occupy an intermediate position (but not in the phylogenetic sense) between the Chlamydomonadaceæ and Scenedesmaceæ. The genus *Chlorococcum* (Artari 2, p. 11) is regarded by Oltmanns as the simplest form of the Protococcaceæ. It consists of rounded and mostly isolated cells of rather large size, although otherwise having the same structure as those of a *Chlorella*. These cells, however, reproduce by successive subdivision of the contents to form a large number of naked zoospores (fig. 3, A), which, on coming to rest, become enveloped by a cell-membrane and form new *Chlorococcum*-individuals. When grown in concentrated nutritive solutions, the products of division of the cell-contents are not liberated as zoospores, but become enveloped by cell-walls, while still within the mother-cell; on the rupture of the membrane of the latter the contained cells (so-called *aplanospores*) give rise each to a new individual, this modification of the reproductive process obviously bringing *Chlorella* completely into line with *Chlorococcum*. In fact, we might call the former a *Chlorococcum*, which has permanently acquired the property of aplanospore-formation.

The young cells of a *Chlorococcum* are uninucleate, but as the cell becomes older the nucleus divides and so a multinucleate condition is attained. This nuclear division is here merely a preliminary to zoospore-formation, and in all probability the subsequent fission of the cell-contents leads to the formation of as many zoospores as there are nuclei. The fact remains, however, that for a short period during its life-cycle, *Chlorococcum* affords us the first example of the cœnocytic habit, paramount throughout the series of the Siphonales. This incipient cœnocytism is found in all the different members of the Protococcaceæ, which can, however, only be dealt with very briefly (Klebs 37). The genus *Chlorochytrium* is nothing more than a *Chlorococcum* which has become modified as a space-parasite,

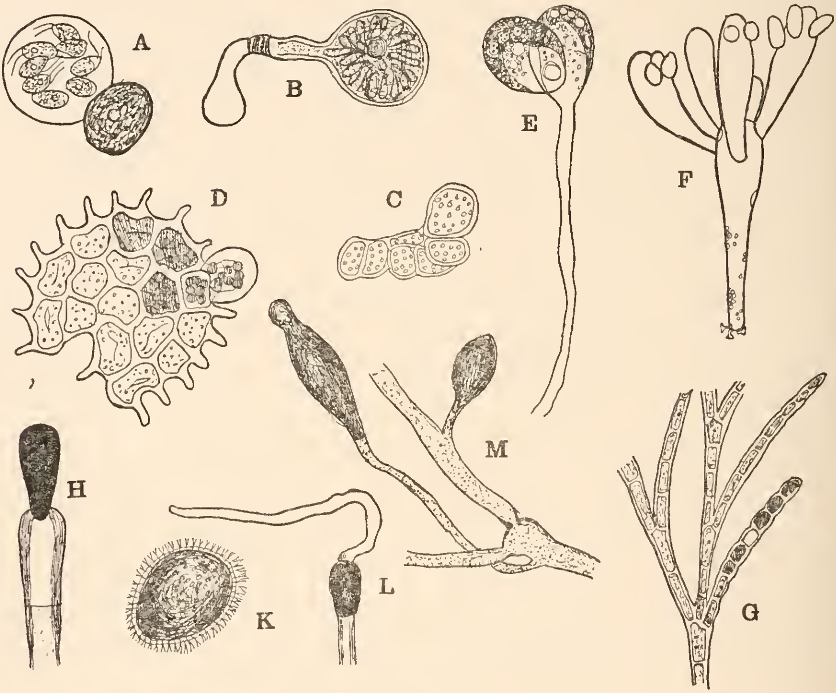


FIG. 3.

A, *Chlorococcum limicolum* (Beyerinck), Oltmanns (liberating zoospores). B, *Phyllobium dimorphum*, Klebs. C, *Chlorosphæra consociata*, Klebs. D, *Pediastrum Boryanum* (Turp.), Menegh. E, *Protosiphon botryoides* (Kütz.), Klebs. F, *Valonia utricularis* (Roth.), Ag. G, *Cladophora* spec. (small portion of a filament). H, L, *Vaucheria piloboloides*, Thur. (aplanospores). K, *Vaucheria repens*, Hass. (zoospore). M, *Dichotomosiphon tuberosus* (A. Br.), Ernst. (with gemmæ). (A after Beyerinck; B and E after Klebs; C after Artari; D after A. Braun; F after Schmitz; G after Oltmanns; H, L and M after Ernst; K after Goetz).

the large resting-cells of the different species occupying the intercellular spaces of diverse higher plants. It is possible that the *Chlorochytrium* receives nitrogenous compounds from its host, for the species of *Chlorococcum* and other allied genera have the faculty of making use of organic nitrogen, and often abound, as a consequence, in putrid solutions, avoided by other forms. There is altogether a marked saprophytic or parasitic tendency in the members of this group. *Endosphæra* (a space-parasite within dead leaves of *Potamogeton*) differs appreciably from *Chlorochytrium* only in its method of reproduction (see below), but in *Phyllobium* (occurring in the intercellular spaces of the leaves of diverse marsh-plants) we have a form showing a branched siphoneous structure, strongly recalling that of certain Siphoneæ (fig. 3, B). A final representative of this group, the genus *Rhodochytrium* of Lagerheim (45), has completely given up an independent existence, and lives as a colourless parasite in the tissues of *Spilanthes Lundii* in Ecuador; the genus owes its name to the blood-red colour of its gametangia, which liberate isogametes, capable also of germination without sexual fusion. The series we have been following up, thus, apart from its siphoneous tendency, also indicates evolution in the direction of parasitism, and it is probable that from some such ancestry a portion at least of the lower Chytridineæ may have arisen. The resemblance between *Endosphæra* and certain species of *Synchytrium* is certainly astonishing (cf. Lotsy 46, pp. 38 and 117).

All the different members of the Protococcaceæ just referred to agree with *Chlorococcum* in lacking the power of ordinary vegetative division, but there is a (probably) close ally of this genus, which is characterised by the possession of this faculty. This is *Chlorosphæra* (Artari 2, p. 35), the species of which occur in freshwater or as space-parasites in water-plants, and have a chloroplast very similar to that of a *Chlorococcum*, which they closely resemble in outward appearance. Like the latter they also reproduce by subdivision of the contents to form zoospores. At the same time, however, the cells of *Chlorosphæra* can multiply by ordinary vegetative division, *i.e.* a division in which the daughter-cells are not provided with a perfectly new membrane (as is the case in *Chlorella*), but in which only that part of the membrane is new which serves to separate the two daughter protoplasts from one another. It can hardly be doubted that the development of this method of division was the beginning of the filamentous tendency (also of the flat plate, seen in *Ulva*, etc.); in *Chlorosphæra consociata*, Klebs (fig. 3, C), very short threads of cells are sometimes actually formed. It is, of course, not necessary that the higher filamentous Algæ (Ulotrichales) took their origin from a form exactly of the *Chlorosphæra*-type, but it seems very probable that the ancestor combined the faculty of zoospore-formation with that of ordinary vegetative division. In *Pleurococcus* we have another example of a simple organism of this type, but, as will be explained below, it is probably more correct to regard this genus as a reduced than as a primitive form. There are, therefore, among these Protococcaceæ, forms capable both of zoospore-formation and vegetative division, and others able only to form zoospores. It is interesting that the third possibility (that of an organism capable only of vegetative division) is realised in the genus *Eremosphæra* (Moore 51), a large spherical form very abundant in the small pools of peat-bogs. *Eremosphæra*, however, differs from the forms previously considered in having numerous discoid chloroplasts, and its correct systematic position yet remains to be settled.

The methods of reproduction found in the Protococcaceæ (Klebs 37) are worthy of a brief consideration, as they give an idea as to the possible origin of another line of algal descent. In *Chlorococcum* the motile elements formed as a result of the subdivision of the cell-contents appear to be purely asexual in character, although it is not impossible that a sexual tendency may yet be discovered in view of the behaviour of the motile elements in the species of *Chlorochytrium*. In some of these the motile elements are again asexual zoospores, but in others they fuse in pairs, while still within the bladder which surrounds them after liberation from the mother-cell (fig. 3A). This is no doubt a very primitive type of sexual process, since gametes from the same mother-cell are concerned in copulation, but it is also interesting because there is practically no swarming-stage prior to sexual fusion, although the zygozoospore subsequently becomes liberated and moves actively to a new host-plant. This restriction of swarming-power is much more obvious in *Endosphæra*, in which the reproductive process is a little more complicated. As in *Chlorochytrium* or *Chlorococcum* the cell-contents undergo subdivision, but the products become enveloped by cell-membranes even before cilia are formed and without being liberated from the mother-cell, thus giving rise to a multicellular individual. Each cell of the latter subsequently liberates a number of isogametes. The chief interest of the reproductive process of *Endosphæra* lies in the co-operation of all the zoospores, formed from a single mother-cell, to produce a multicellular individual at the sacrifice of the motile stage. This is the main characteristic of the peculiar group known as the Hydrodictyaceæ, the two best-known genera of which are *Pediastrum* (Askenasy 3) and *Hydrodictyon* (Klebs 39). In the former generally sixteen or thirty-two cells of more or less irregular shape are arranged to form a flat plate, the outermost cells of the colony being provided with characteristic blunt processes (fig. 3, D). In *Hydrodictyon*, on the other hand, we have large cells united in great numbers to form a very characteristic and often extensive network with wide-open meshes. In both genera asexual reproduction takes place by subdivision of the contents of any or all of the cells to form a number of biciliate zoospores (fig. 3, D). These exhibit a slight jerking movement within the mother-cell, but this soon ceases; the zoospores then become arranged in the way that is characteristic of the genus and acquire cell-membranes, and thus a new daughter-colony may be produced from each cell of the mother-colony. The analogy with *Endosphæra* is obvious, but it seems to me that it can be carried further. Asexual reproduction of the type just described may go on for many generations in the Hydrodictyaceæ, but under certain conditions gametes are produced. Their formation is identical with that of the zoospores, but unlike the latter they become completely free from the mother-cell and after swarming about for a time fuse in pairs to form a zygospore, which is somewhat angular and has a thick wall. After a period of rest a limited number (generally two to four) of large zoospores are produced from this zygospore; but these soon cease to move, and, assuming a somewhat angular outline, become enveloped by a thick membrane. These so-called *polyhedra* go on increasing in size for some time until ultimately their contents subdivide to form numerous zoospores, which, without being liberated, acquire cell-membranes and arrange themselves as a new *Pediastrum*- or *Hydrodictyon*-colony. Apart from complications, the resting-cells of these Hydrodictyaceæ thus give rise to a multicellular colony just as do the resting-cells of *Endosphæra*. The cells of this colony are capable of reproducing daughter-colonies in the same way through numerous generations; but this is the only essential respect in which they differ from *Endosphæra*, for sooner or later the cells of the *Pediastrum*- or *Hydrodictyon*-colony become gametangia, just as do those of the *Endosphæra*-colony. In both *Endosphæra* and the Hydrodictyaceæ the fusion of the gametes leads to the production of a resting-cell, which sooner or later gives rise to a new colony. It seems probable, therefore, that the Hydrodictyaceæ are a side-line of evolution from the stock of the Protococcales, and that this line possibly branched off from the Protococcaceous series. The relationship to the Scenedesmaceæ is, however, also very close, for there is obviously not very much difference between the mode of formation of the daughter-colonies in a *Cœlastrum* (for instance) and that in the Hydrodictyaceæ. The polyhedron-stage of the latter, moreover, bears an astonishing resemblance to the genus *Tetraëdron*,[1] which is a member of the Scenedesmaceæ. Lastly, we may note that in the multinucleate cells of their colonies the Hydrodictyaceæ again approach the Protococcaceæ, while it has been shown that in the very young cells of *Hydrodictyon* the chloroplast is a simple plate (Artari 1), quite easy to bring into line with that of a *Chlorococcum*, etc., although in the mature condition it is a complicated reticulated structure with numerous pyrenoids.

We have now completed our survey of the numerous unicellular and colonial forms found among the lower green Algæ, and subsequently will turn our attention to the higher filamentous types.

[1] In this connection it is of some interest also that Wille (70) has recently shown that the genus *Oocystis* (Scenedesmaceæ) forms similar polyhedra as resting-stages.

(To be continued)

VERTEBRATE PALÆONTOLOGY IN 1909

By R. LYDEKKER

In this review I make, as usual, no pretence to give a complete survey of the work accomplished last year in vertebrate palæontology, but merely notice the more interesting and important papers which have come under my own personal observation. Among these papers, special interest attaches to the one by Mr. Gidley on the skull and skeleton of the American Tertiary mammal *Ptilodus*, a near relative of the English Jurassic *Plagiaulax*, in which it is shown that both genera are diprotodont marsupials. From a distributional point of view the discovery in the Pliocene of North America of remains of a peacock and of antelopes of an African type is as unexpected as it is interesting; while from the morphological standpoint a surprise is afforded by the discovery of the "rodent-goat" in a cave in Majorca. Among many other papers of importance, special attention may be directed to Dr. Hay's account of the extinct turtles of the genera *Archelon* and *Protostega* and their bearing on the origin and relationship of the modern luth or leathery turtle.

As a work dealing very largely with vertebrate palæontology in general, a translation of Prof. C. Depéret's "Les Transformations du Monde Animal" published in the *International Scientific Series* under the title of the "Transformations of the Animal World" claims first mention. Unfortunately the English version is sadly marred by the incompetence of the translator, as is exemplified, among other instances, by his use of the word "square" when he means the quadrate bone.[1]

In connection with the above may be noticed a paper by Dr. R. F. Scharff, published in the *American Naturalist* for September, on the evidence in favour of the existence of a land-bridge between North and South America during the early portion of the Tertiary period. In the author's opinion, the two continents were in communication at the epoch in question by means of a strip of land joining Western North America

[1] See review in SCIENCE PROGRESS, 1909, 3, 709.

with Chili, at which date the present Central America and the northern portion of South America were submerged. Many lines of evidence, it is argued, support the existence of such a narrow land-bridge and without this the author finds it difficult to account for the presence of primitive armadillos in the North American Eocene and likewise the intimate relationship believed to exist between the genus *Stiromys* of the Santa Cruz Miocene and the modern Canadian porcupine (*Erethizon*).

In a second paper, published in vol. xxviii. of the *Proceedings* of the Royal Irish Academy, the same author reviews the evidence in favour of a former land-connection between North America and Northern Europe. The author, who believes the connection to have existed during the pre-Glacial epoch, cites evidence derived both from soundings and from certain elements of the fauna of the two continents, laying special stress on the fact that the distributional area of the fresh-water members of the perch family includes Central and Eastern North America and stretches from the British Isles to Eastern Siberia, but stops short of the Bering Strait district and Kamchatka.

Turning to papers of a general faunistic character, attention may be directed to Dr. L. Mayet's review of the mammaliferous horizons of the typical Faluns of the Touraine, issued in the medical section of the *Annales* of the University of Lyons, fascicule No. 26. Among other specimens described and figured, special interest attaches to a newly discovered fragment of the lower jaw of the Tertiary gibbon (*Pliopithecus àntiquus*).

As it arrived too late for notice in the article on the palæontological work of 1908, I may also refer to a much more bulky memoir by the same author, issued in the same serial (fascicule No. 24, 1908), on the Miocene mammals of the Sands of the Orléanais and one portion of the Faluns of the Touraine. This includes a detailed account of all the previously known species, with a description of several regarded as new, and likewise full information with regard to their topographical distribution.

Pleistocene mammal remains from Gesprengberg, in the Kronstadt district of Siebengebürge, form the subject of a paper by Dr. Towler, published in vol. lix. pp. 575, 614 of the *Jahrbuch der k. k. Geol. Reichanstalt* at Vienna. Several of the animals from this deposit are identified with existing species, but a wolf is described as new and the same is the case with a rhinoceros of the *etruscus* group, for which the name *Rhinoceros kronstadtensis* is proposed. The molars are stated to be in a considerable degree intermediate between those of *R. etruscus* and *R. mercki*.

Nearly connected with the above is a paper by Messrs. E. Harlé and H. Stehlin, in the *Bull. Soc. Géol. France*, ser. 4, vol. ix. pp. 39 *et seq.*, on the existence of a late Tertiary mammalian fauna on the Quercy plateau of Central France, so well known for its Oligocene land-vertebrates.

Tertiary mammal horizons of Western America form the subject of an important paper by Prof. H. F. Osborn, with faunal lists by Dr. W. D. Matthew, published in *Bulletin* No. 361 of the U.S. Geological Survey.

Among a few subfossil remains of existing vertebrates from superficial deposits in Sweden, described by Dr. Einar Lönnberg in vol. vi. No. 3 of *Arkiv für Zoologi*, special interest attaches to the occurrence of a skeleton of the ringed seal (*Phoca fœtida*) in a bed of clay in the parish of Trönö, a few miles to the south-west of Söderhamn. The specimen was buried 3 ft. in the clay, at an elevation of about 14 ft. above sea-level, and the associated diatoms indicate that the animal lived at the comparatively recent epoch when the Baltic was a fresh-water lake. Although the skeleton is that of a full-grown male, the bones are much smaller than those of an ordinary ringed seal; so that the fresh-water Baltic race, like the one now inhabiting Lake Baikal, was evidently small. Two other skeletons, probably of about the same age, of the ringed seal, were found at the village of Rutvik, a short distance from the town of Lulea, at the head of the Gulf of Bothnia. The bed in which these lay, as indicated by the buried diatoms, appears to have been deposited in a fresh-water lagoon; and the spot where the find was made seems formerly to have been the termination of a system of long and narrow fresh-water sounds and fjords. Dr. Lönnberg suggests that these seals, which are likewise smaller than the ordinary ringed seal, entered the fresh-water at a time when the depth was greater than usual and were unable to find their way out or become shut in when the water-level fell. At any rate, they show that the ringed seal has for a long period been in the habit of entering fresh-water, and that when permanently cut off from the sea it becomes reduced in size.

In the United States Messrs. W. D. Matthew and H. J. Cook (*Bull. Amer. Mus. Nat. Hist.* vol. xxvi. pp. 361–414) announce the discovery of a new Pliocene mammalian fauna in Western Nebraska, corresponding in age with the well-known Loup Fork beds. The new forms include a very large species of camel of the genus *Pliauchenia* (*P. gigas*) and likewise a giraffe-camel (*Alticamelus procerus*), together with an antelope allied to the modern African *Hippotragus*.

In this place reference may be made to two papers in vol. xi. of the *Proceedings* of the Washington Academy on the age of the Ceratops beds of Montana and Wyoming, which have generally been regarded as representing the Laramie or Upper Cretaceous. In the first of the two papers Dr. F. H. Knowlton, from both stratigraphical and palæontological evidence, correlates, however, these beds with the Fort Union series—an identification which, if justified, would indicate the survival of dinosaurs into the Eocene. On the other hand, in the second article, Mr. T. W. Stanton denies the existence of evidence for the proposed change.

Turning to systematic work, it may be mentioned, in the first place, that in the year 1884 Dr. F. Ameghino proposed the name *Diprothomo* for one of the presumed ancestors of the human species without allocating any type to the new genus. Recently he obtained from a superficial stratum in Buenos Aires, regarded as of Lower Pliocene age, a calvarium of low type, which in his opinion is generically distinct from *Homo*. For this supposed new genus it is proposed to adopt the name *Diprothomo* with the affix *platensis*. "*Diprothomo*" is, however, a *nomen nudum*, and cannot be employed in a new sense. Dr. Ameghino concludes that the skull affords further evidence of the view that South America is the cradle of the human race. Additional testimony in favour of this opinion is stated to be afforded by the lower jaw of a child with the angle inflected in marsupial fashion. The Tertiary South American genus *Microbiotherium* is regarded as the ancestral type of most mammals, and from this sprang *Clenialites*, the ancestor of the Primates. Dr. Ameghino's paper is published in vol. xix. of the *An. Mus. Nac. de Buenos Aires*.

A more satisfactory piece of anthropological work relates to the skeleton discovered in 1908 in the cavern of Le Moustier, in the Dordogne, in a stratum lying about 30 feet below the one worked by Messrs. Lartet and Christy. The latter stratum is assigned to the Neanderthal period, but the skeleton, from the evidence of associated implements, is identified with the earlier Acheuleën or St. Acheul epoch. An age of about 400,000 years is assigned to this deposit, which belongs to the penultimate inter-Glacial epoch, the Le Moustier hunter, for whom the name *Homo mousteriensis hauseri* has been proposed by Prof. Klaatsch, representing the oldest known human skeleton. Although greatly damaged when discovered, the skull has been pieced together, and the bones of the skeleton have been freed from matrix and placed in their proper positions. The remains indicate a male between sixteen and eighteen years of age. The limb-bones are relatively short and thick, the cranial portion of the skull is receding and the jaws protrude, after a fashion occasionally met with among modern Australians. Associated with the ape-like muzzle is a powerful dentition, the teeth having stouter roots and more distinct enamel-folds than in any living race. Among other characters of the skull may be mentioned the large size and wide separation of the orbits, and the broad and deeply sunk root of the nasals, the latter feature indicating a wide and flattened nose, with the nostrils directed mainly forwards. A popular illustrated account of this interesting discovery is given by Dr. L. Reinhardt in *Naturwissenschaftliche Wochenschrift* for May 23, 1909.

Passing to the Carnivora, the first paper for mention is one by Prof. S. H. Reynolds on the British Pleistocene *Canidæ*, issued among the Palæontographical Society's Monographs for 1909. The author, who includes the fox in the typical genus *Canis*, apparently does not recognise subspecies, as he makes no attempt to identify any of the early British remains of the wolf with the various forms which have been named by continental palæontologists. The jaw on which the so-called *Lycaon anglicus* was named by the present writer is regarded by Prof. Reynolds as that of an aberrant wolf.

From the Pleistocene asphalt-deposits of California Dr. J. C. Merriam (*Univ. Calif. Pub., Bull. Dep. Geol.* vol. v. pp. 291–304) has described the skull of a large cat regarded as representing a local race of *Felis atrox*. Special interest attaches to the discovery, owing to the fact that remains of *Smilodon* occur in the same deposit, thus confirming the opinion that *Felis* and *Smilodon* differed widely in habits.

One of the most important and elaborate palæontological memoirs published during the year is that by Mr. W. D. Matthew on the Carnivora and Insectivora of the Bridger Middle Eocene Basin, forming part 6 of the ninth volume of the *Memoirs* of the American Museum of Natural History. In a valuable review of the developmental history of the Carnivora the author refers to an important cranial difference between progressive and non-progressive types; the cranial region in the former being elongate, whereas in the latter it

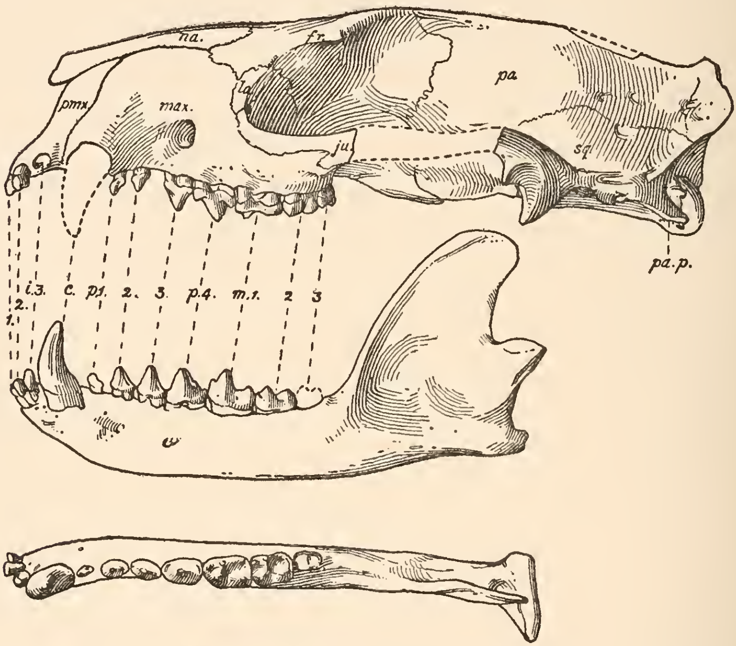


FIG. 1.—Skull of *Vulpavus profectus*, an "adaptive" Creodont. Natural size. From Matthew, *Mem. Amer. Mus. Nat. Hist.*

is short. This may be illustrated by comparing the skull of a primitive placental carnivore like *Viverravus* with that of a carnivorous marsupial of similar size. In the placental there is ample room for brain-expansion in both the longitudinal and the transverse direction, without undue encroachment on other parts and organs; whereas in the short-skulled type brain-growth is greatly hampered by the necessity of much rearrangement of other parts of the skull and its organs in order to provide sufficient space.

In regard to the relationships of Carnivora, Dr. Matthew disputes the alleged origin of the whale-like zeuglodonts from the creodont division of the order, arguing that the creodont resemblances of the zeuglodont *Protocetus* are restricted to certain features shared with *Hyænodon*, a specialised Oligocene cursorial type of the Creodontia; such features being absent in the primitive Eocene members of the latter group, such as *Sinopa*. The presence of these features "in a marine mammal of Middle Eocene time cannot be due to relationship, for if it were it would be shown more or less clearly in *Sinopa*, the early Eocene ancestor of *Hyænodon*. On the other hand, the resemblance between *Protocetus* and the more generalised creodonts does not indicate any very near relationship, except such as we should expect a primitive cetacean to show to the primitive members of other orders."

The last sentence is noteworthy on account of its containing an admission that, in the author's opinion, the zeuglodonts are members of the Cetacea; this view not being shared by all American palæontologists.

After discussing the classification of the order, Dr. Matthew proceeds to observe that the Cretaceous ancestors of the Carnivora appear to have been small arboreal mammals resembling the North American opossum in size and habits, but more nearly related to the primitive Insectivora. The angle of the jaw was not inflected and the cheek-teeth were of the opossum-type, although their cusps were at first conical, and did not acquire the notched shear-like form characteristic of Insectivora and opossums till a later date.

In discussing a suggested relationship between the Creodont Carnivora and the Marsupialia, the author remarks that although the carnivorous representatives of the latter are in many respects of a very archaic type, yet they differ in so many important characters from creodonts, and indeed from all primitive placentals, that any true kinship seems impossible. "It would appear, therefore," he adds, "that the distinctive characters of the marsupials, whether primitive or secondary, were at all events fixed and constant as far back as the early Tertiary, at a time when the various placental orders show a marked degree of approximation to one another."

Dr. Matthew divides creodonts into three main groups. First, an "adaptive" group, as represented by the families

Arctocyonidæ and *Miacidæ* in which the carnassials, when developed as such, are formed by $\underline{pm.\ 4}$ and $\overline{m.\ 1}$, as in modern

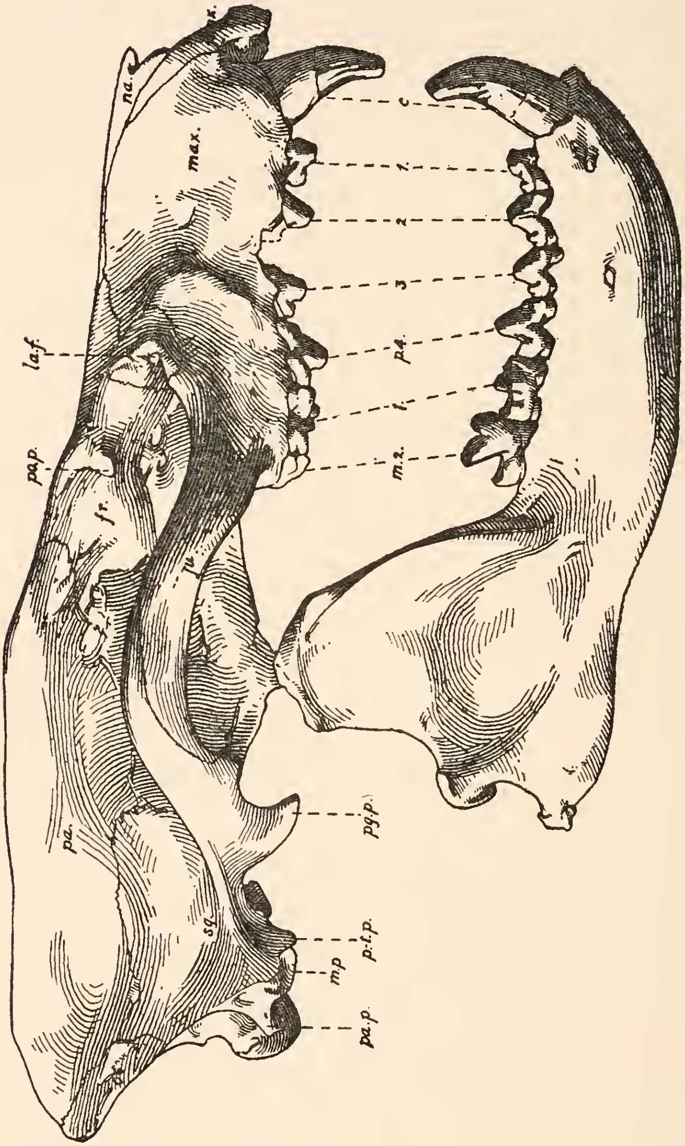


FIG. 2.—Skull of *Limnocyon verus*, a typical "inadaptive" Creodont. Natural size.
From Matthew, *Mem. Amer. Mus. Nat. Hist.*

Carnivora (fig. 1). Second, an inadaptive modification, as represented typically by the *Hyænodontidæ* and *Oxyænidæ*, in which the carnassials, if differentiated at all, are contributed by $m.\frac{1}{2}$ or $m.\frac{2}{3}$. Third, a primitive group, the *Oxyclænidæ*, in which the molars are of a generalised type, tritubercular above and tuberculo-sectorial below, with sharp, angulated cusps.

In the systematic portion of this memoir a number of new Eocene forms are named and described. In connection with this it may be mentioned that in a note published in *Science*, vol. xxix. p. 620, Mr. O. A. Peterson refers the creodont originally described as *Amphicyon superbus* to a new genus under the name of *Daphænodon*.

New creodonts from the Egyptian Oligocene form the subject of a short paper communicated by Prof. H. F. Osborn to the *Bulletin* of the American Museum of Natural History, vol. xxvi. p. 415, in which several new species are described, one of them being assigned to a new genus, under the name of *Metasinopa*.

Reverting to Dr. Matthew's memoir, a few words may be devoted to the Insectivora, which are regarded by the author as a very primitive and ancient group, near akin to the ancestors of placentals as a whole. Their modern representatives—if all are rightly included in a single order—have, however, become so widely sundered by the extinction of intermediate types that it is exceedingly difficult to give any definition of the order other than one based almost entirely on primitive characters. The antiquity of the group is confirmed by the fact that Insectivora occupied a much more important position in comparison with other orders during the Middle Eocene than is the case in later Tertiary times or at the present day. The three extinct families *Leptictidæ*, *Pantolestidæ* and *Hyopsodontidæ* are referred by the author to the insectivorous order; the second and third of these having been hitherto associated with other groups. The Bridger Eocene also contains other Insectivora of more or less uncertain affinity, some of these approximating in dental characters to moles and shrew-mice, while others are more like the Malagasy tenrecs. The *Pantolestidæ* display indications of affinity with creodonts, whereas the *Hyopsodontidæ* seem to be related to the condylarthrous ungulates. As regards existing Insectivora, the author is of opinion that these approximate to the less specialised marsupials to a greater degree than is the case with any other placental order; this being notable in the structure of the auditory bulla, the relations of the mastoid, the incompletely ossified palate bordered behind by a ridge, the marginal or extra-orbital lachrymal foramen, the elongated nasal bones, and the backward position of the orbits.

Rodents seem to have occupied the attention of palæontologists only to a small degree during the year. A paper was, however, contributed in 1908 by Dr. L. v. Méhely to vol. vi. of the *Annales Musei Nationalis Hungari* on the claim of the Pliocene Hungarian *Prospalax priscus* to be regarded as the ancestor of the modern mole-rats of the genus *Spalax*. In the author's opinion the original ancestral form of the family was a rodent near akin to the Indo-Malay bamboo-rats (*Rhizomys*). From this have sprung, on the one hand, *Prospalax* and *Spalax*, while another branch is formed by the African *Tachyoryctes*. In describing the remains of a hare from the ossiferous fissures of Ightham, Kent, in vol. xii. p. 295, of the *Scientific Proceedings* of the Royal Dublin Society, Mr. M. A. C. Hinton takes occasion to state that he considers it advisable to employ the name *Lepus variabilis* (instead of *timidus*) for the blue hare, on account of the double sense in which the latter title has been employed. This Kent hare is regarded as a distinct race, under the name of *L. v. anglicus*; and it is pointed out that, as all the Pliocene European hares appear to pertain to the *variabilis* group, the brown hare (*L. europæus*) may be looked upon as a recent intruder into Europe. Remains of hamsters have been recorded by Mr. E. T. Newton, in the *Geological Magazine* for 1909, from the Norfolk Forest-bed.

Turning to ungulates, the most remarkable event of the year, so far as that group is concerned, is the discovery by Miss D. Bate of the remains of a peculiar type of goat in a cave in the island of Majorca, in the Balearic group. The specimens obtained include the skull (unfortunately imperfect in front) and certain other parts of the skeleton and indicate a new generic type for which their describer proposes the appropriate name of *Myotragus balearicus*. Except for a noticeable shortening of the whole, and especially a marked convexity of the profile of the lower jaw, the skull is, in general character, that of a small goat. The lower jaw, in addition to a highly convex inferior border, is, however, peculiar in being furnished with only a single pair of lower incisors, curiously similar to those of a rodent, the resemblance being not confined to the fact that these teeth are highly curved, chisel-like in form and grow from persistent pulps, but that their enamel is restricted to the lower and outer surface and that the point is worn off obliquely. Unfortunately, the tip of the upper jaw is broken off and, at first sight, it seems natural to infer that the premaxillæ carried a pair of rodent-like upper incisors. How to account otherwise for the wearing away of the summits of the permanently-growing lower incisors is indeed a difficult matter, but the reacquisition of a lost tooth appears to be a phenomenon unknown. As regards the rest of its skeleton, *Myotragus* is characterised by the extreme shortness of the cannon-bones of both fore and hind limbs—an abbreviation considerably in excess of that which occurs in either the musk-ox or the takin. As to the date at which this "rodent goat" became exterminated, there is no clue. Miss Bate's paper is published in the *Geological Magazine* for 1909.

Another discovery of ruminant remains in the Mediterranean Islands—this time Crete—likewise demands brief mention. As recorded in the *Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich*, vol. liv. p. 424, Dr. C. Keller has recently discovered in the ruins of the palace of King Minos at Knossus remains of the aurochs, or extinct wild-ox (*Bos taurus primigenius*), which he regards as explaining the myth of the minotaur and the labyrinth. The labyrinth has been shown to be the royal palace and the minotaur, it is now suggested, was neither more nor less than the aurochs which was captured half-tamed, and exhibited in the arena.

Remains of the aurochs and of the bison preserved at Stuttgart form the basis of a paper on those animals by Dr. Max Hilzheimer, published in No. 66 of *Mitteilungen aus dem Kgl. Naturalienkabinett zu Stuttgart*. In the author's opinion, the modern bison (*Bos bonasus*) and the Pleistocene *B. priscus* lived side by side in Europe and it is suggested that the latter, together with the existing bison of the Caucasus, is more nearly related to the American than to the typical Lithuanian bison. In confirmation of the contemporary existence of *bonasus* and *priscus* prehistoric sketches apparently indicative of two distinct types of bison are cited. A fossil bison from Siberia (*B. primitivus*) is regarded as the most generalised member of the group, its skull showing a larger intrusion of the parietals on to the frontal aspect than in any of the other forms. A paper on remains of aurochs and bison in the Dantzic Museum has been contributed by Mr. La Baume to *Schrift. Nat. Ges. Danzig*, vol. xii. pp. 45 *et seq.*

Reference to Messrs. Matthew and Cook's identification of remains apparently indicating the occurrence of a hippotragine antelope in the Pliocene of Nebraska has been already made. This discovery has been supplemented by the still more remarkable one of remains of tragelaphine antelopes, apparently allied to the African bushbucks and kudus, in the Pliocene of Nevada, which have been described by Dr. J. C. Merriam in the *Bulletin of the Department of Geology* of the Publications of California University, vol. v. pp. 319–30, under the generic names of *Ilingoceros* and *Sphenophalos.*

With the exception of the aberrant Indian nilgai, the tragelaphine antelopes are now restricted to Africa, and the typical hippotragines are wholly confined to that continent, although their relatives the oryxes range into Syria and Arabia. Both groups occurred, however, in Southern Europe and Asia during the Lower Pliocene period and the new and unexpected discovery seems to indicate that at the same time they ranged over Central and North-Eastern Asia, whence they effected an entrance by way of Bering Strait into America. All this lends further support to the view that the modern antelopes of Africa are comparatively recent immigrants into that continent.

As regards fossil *Cervidæ*, the only paper of any importance appears to be one by Mr. Rudolf Hermann on the extinct relatives of the roebuck and their antlers, published in *Schrift. Nat. Ges. Danzig*, vol. xii. pp. 81 *et seq.*

To vol. xxvi. of the *Bulletin* of the American Museum (pp. 1–7) Dr. W. D. Matthew contributes a short paper on the widely distributed Lower Tertiary selenodont genus *Ancodon*, or *Ancodus*, as it is commonly termed. *Tapinodon*, *Arretotherium*, and *Merycopotamus* are regarded as allied types, and it is pointed out that as regards foot-structure *Ancodus*, or *Hyopotamus*, as it was formerly called, appears to come much closer to the *Suidæ* than to the Oreodonts. This confirms the opinion that the latter form an exclusively American type, probably related to the camels.

In a more ambitious article, issued as No. 3 of the fourth volume of the *Memoirs* of the Carnegie Museum of Pittsburg, Dr. O. A. Peterson discusses the osteology and affinities of the allied but bunodont family *Entelodontidæ*—a group likewise common to the northern half of both hemispheres. These

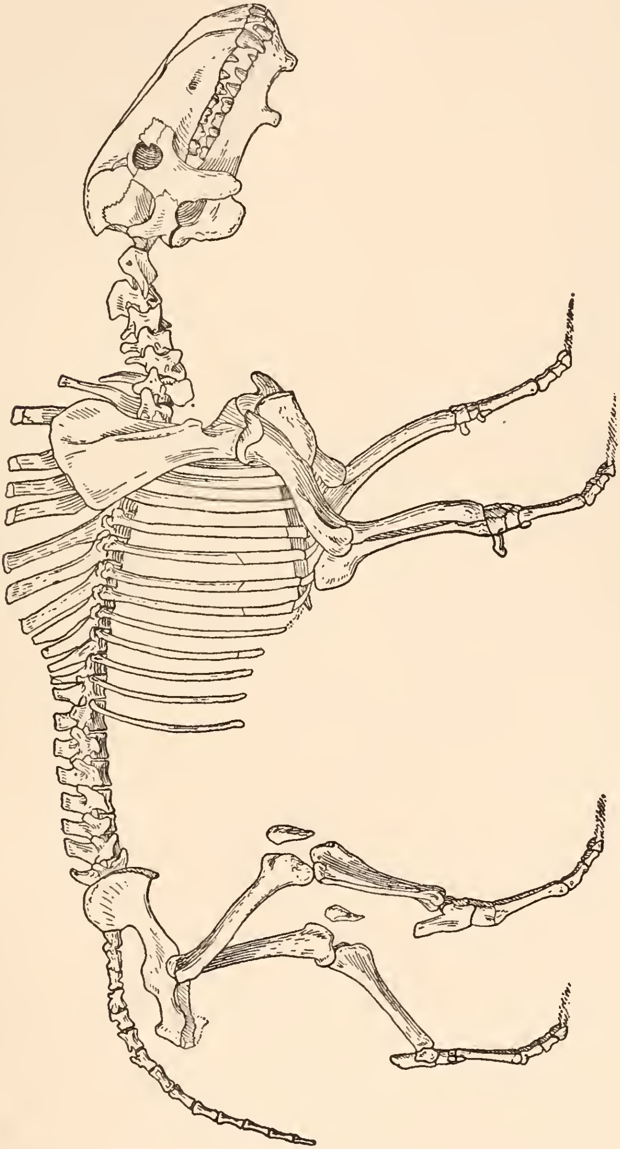


Fig. 3.—Skeleton of *Archæotherium ingens*. About $\frac{1}{18}$ natural size.
From Peterson's *Revision of the Entelodontidæ*.

huge, hog-like animals, of which the best-known are *Entelodon* and *Archæotherium*, appear to have branched off from the pig-like stock in the early Eocene, but their ancestral type still remains to be discovered. All the members of the group are characterised by the presence of a large descending flange from the inferior border of the zygomatic arch, while most of them have a second projection descending from the chin. The European *Entelodon*, as shown by its enlarged premolars, is a more specialised form than the American *Archæotherium*; the most specialised of all in this respect being the Indian *Tetraconodon*, in which the premolars were enormous. So far as can be judged from their osteology, coupled with the undoubtedly omnivorous character of their dentition, their long limbs, and their wide geographical distribution, it seems highly probable that these giant swine-like animals were capable of adapting themselves, when necessary, to changed conditions of environment.

Certain remains from very superficial beds in South Africa have been considered by Dr. R. Broom to afford sufficient evidence for naming a new species of hartebeest (*Bubalis priscus*) and another of *Equus*, namely *E. capensis*; the latter being distinguished from other members of the genus by its superior size. Dr. Broom's papers appeared in the third part of vol. vii. of the *Annals* of the South African Museum, issued on April 28, 1909. The exact date of publication in this instance is a matter of some importance, for in the October issue of the *Proceedings* of the Zoological Society Prof. W. Ridgeway has described, under the name of *E. hollisi*, the fore part of an equine jaw from a presumed late Tertiary deposit in British East Africa.

A brief reference will suffice to Mr. W. J. Sinclair's monograph of the Typotheria of the Santa Cruz beds of Patagonia, published in the *Memoirs* of the Princeton Expedition to Patagonia, since it is in the main only an expansion of the preliminary paper published in 1908 on the same subject and mentioned in my article in Science Progress for that year.

Passing to the Proboscidea, attention may be directed to the description by Dr. C. W. Andrews in the *Geological Magazine* for 1909 (pp. 349, 350) of a new mastodon (*Tetrabelodon dinotheriodes*) probably from the Loup Fork beds of Kansas. The species is founded on a lower jaw in which the symphysis is bent down, so that the incisors were apparently almost vertical.

Here it may be conveniently mentioned that Prof. H. F. Osborn has contributed a note to *Nature* (vol. lxxxi. p. 139) on the probable feeding habits of the Egyptian Tertiary genera

Palæomastodon and *Mœritherium*. In the case of the latter it is suggested that there was no proboscis, and doubts are raised as to its claim to be regarded as the ancestral form of the proboscidean line.

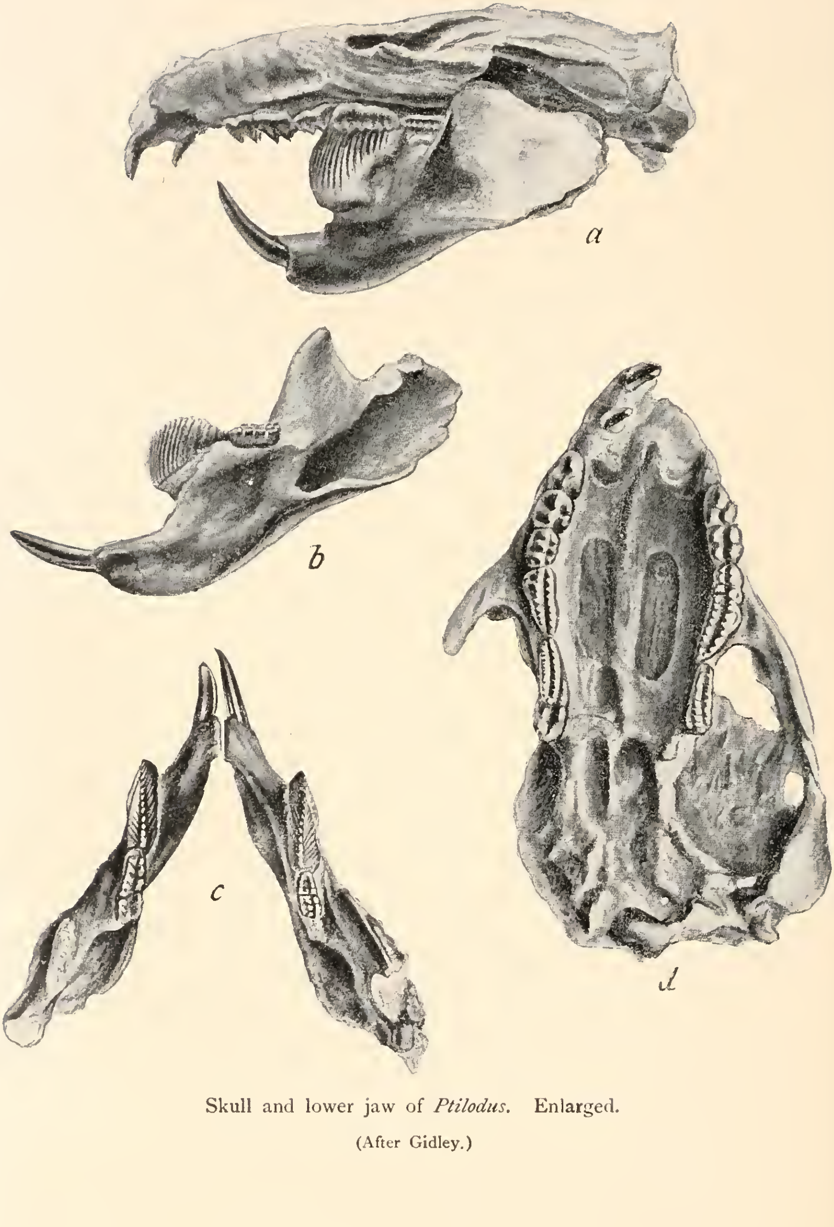
In my article for 1908 brief reference was made to a paper by Dr. R. S. Lull on the evolution of the Proboscidea. Since the same paper has also appeared, in the *Smithsonian Report*, during the year now under review, a fuller notice may be given. The affinities of the group to the Sirenia on the one hand and to the Hyracoidea on the other are affirmed, while *Mœritherium* is admitted to be the ancestral stock. The broad discs on the molars of the African elephant and the relatively small size of the tusks in its Indian cousin are regarded as decadent characters. From the circumstance that the tusks of the North American *Elephas columbi* and *E. imperator* are spiral while those of the apparently related *E. antiquus* are relatively straight, the author is of opinion that the two first-named forms have been independently evolved from a species more or less closely related to the Siwalik *E. planifrons* and are not, as often considered, western races of the mammoth, which they greatly exceed in size. On the contrary, it is suggested that the mammoth itself, on account of the form of its tusks, may have originally been an American derivative from the *columbi-imperator* type. If, however, the tusks of the Indian elephant be degenerate, any argument derived from their form as to a non-relationship between that species and the mammoth seems to be altogether invalidated.

During the year Prof. O. Abel has published a further instalment of his important studies on the Tertiary cetaceans of Europe giving in vol. cxviii. part i. of the *Sitzungsberichte der k. Academie der Wissenschaften, Wien*, a restoration of the skeleton of *Eurhinodelphis cocheteuxi*, of the Belgian Upper Miocene, in which the prolongation of the toothless rostrum far in advance of the lower jaw is well shown. The length of the figured skeleton, which is probably that of a male, is 16 feet, but the majority of specimens of this species are smaller. From the strong development of the caudal vertebræ, indicative of powerful tail-muscles, it is inferred that these cetaceans were swift swimmers, while the free cervical vertebræ permitted, as in the modern fresh-water *Iniidæ* and *Platanistidæ*, of considerable movements of the head. These circumstances, taken in connection with the long, toothless rostrum, projecting far in advance of the lower jaw and the weak state of the dentition generally, suggest that these long-snouted dolphins swam on the surface of the sea, where they captured their food—probably fishes—in much the same manner as does the skimmer among birds. In the same issue Dr. Abel also describes the skull of *Saurodelphis argentinus* from the Argentine Pliocene and shows that the genus was nearly allied to the existing Amazonian genus *Inia*, of which it may have been the ancestral form.

Another dolphin related to *Inia* has been described from the Patagonian Tertiary by Prof. F. W. True in vol. lii. p. 441 of the *Smithsonian Miscellaneous Collections* under the name of *Proinia patagonica*. The similarity in the form of the skull, in spite of its imperfect condition, is stated to be sufficient to indicate with certainty the near affinity of the extinct to the modern genus. On the other hand, *Proinia* displays no resemblance to the squalodonts, one of which occurs in the same formation, thus throwing doubt on Dr. Abel's theory of the derivation of the *Iniidæ* from the *Squalodontidæ*. In the same article Mr. True describes a new specimen of the skull of the Patagonian *Prosqualodon australis*, a genus and species first brought to notice by the present writer in 1894.

That the South American ground-sloths effected an entrance into North America during the later Pliocene or early Pleistocene period has long been well known, but it is only by slow degrees that we are learning the extent of their distribution in that continent. The latest addition to the list of North American ground-sloths is made by Prof. T. D. A. Cockerell, who in vol. vi. p. 309 of the *University of Colorado Studies* describes the skull of a mylodont from Walsenburg, Colorado. In the absence of any evidence to the contrary this is referred to the typical genus *Mylodon*, but without specific determination.

The long-disputed question as to the systematic position of the genus *Plagiaulax* of the Dorsetshire Purbeck and the nearly allied North American *Ptilodus* appears to have been finally settled by the discovery of a nearly complete skull and parts of the skeleton of a species of the latter in the Fort Union beds of Montana. Both Owen and Falconer regarded *Plagiaulax* as a diprotodont marsupial, and this view was adopted by the present writer in the *Catalogue of Fossil Mammalia* in the



Skull and lower jaw of *Ptilodus*. Enlarged.

(After Gidley.)

[p. 665

British Museum. Cope, who proposed the name Multituberculata for the group, eventually suggested affinities with the monotremes, and of late years *Plagiaulax* and its relatives have very generally been included in the Monotremata. The new specimen indicates not only that the original reference to the diprotodont marsupials was correct, but also shows that the teeth from the English Purbeck described as *Bolodon* belong to the anterior part of the cheek-series of *Plagiaulax*, while the American specimens on which the genus *Chirox* is based occupy a similar position in the jaws of *Ptilodus*.

In concluding his description of the new specimens of *Ptilodus* in vol. xxxvi. (pp. 611–27) of the *Proceedings* of the U.S. National Museum Mr. J. W. Gidley remarks that:

"Falconer and Owen referred *Plagiaulax* to the Diprotodontia, but differed in their opinions regarding its probable habits and taxonomic relations. Falconer compared *Plagiaulax* with *Hypsiprymnus* (*Potorous*) and sought to prove that the former was a saltatory herbivorous marsupial, allied to the rat-kangaroos. Owen just as strongly contended that it was carnivorous in habits and more probably related to the extinct carnivorous *Thylacoleo*. Owen's conclusions regarding the carnivorous habits of *Plagiaulax* lose much of their force since it is now apparent that his principal arguments were based on an error in the interpretation of a most important factor, namely, the normal position of the jaw. Viewing the lower jaw of *Ptilodus* properly articulated with the upper one (*a* and *b*), it is observed that passing forward it pitches downward at a considerable angle, bringing the plane of the tooth-row below the condyle and the incisors into a semi-procumbent position as in the diprodonts. It will be noted also that the greater part of the thin cutting blade of p_4 does not come in contact with the upper teeth but stands free in the mouth. If the lower jaw of *Plagiaulax* is thus placed, the condyle is above the tooth-row and not below it, as stated by both Owen and Falconer. The premolar teeth likewise drop away from the level of the molar series, forward, so that the anterior ones could scarcely have come in contact with any teeth of the upper jaw. It is further observed that, as in *Ptilodus*, the ridges on the sides of the cutting blades viewed laterally run nearly at right angles to the plane of the molars; thus these ridges which have always been described as being 'oblique' in the fossil forms, are after all placed in the same relative position in the mouth as those of the ridged premolars of living species. Assuming this position for the lower jaw and recognising the fact that the blade-like premolars did not oppose teeth of like structure in the upper jaw, the carnivorous characters pointed out by Owen seem to disappear, while the general resemblances to the less specialised diprotodonts become more than ever apparent. . . .

"The evidence that *Ptilodus* and *Plagiaulax* were not carnivorous in habits seems conclusive; but as to whether they were insectivorous, herbivorous, or frugivorous there may still be some differences of opinion. I am inclined to consider them as frugivorous, since the incisors were well fitted for picking small fruits or berries, while the large cutting-blades of the lower premolars were admirably adapted to cutting or slicing the rinds of tough-skinned berries or to chopping up fleshy fruits held against the blunt-pointed premolars of the upper jaw. For masticating the seeds of such small fruits and berries the multituberculate molars were amply sufficient."

In connection with the foregoing, reference may be made to a paper by Miss P. H. Dederer in vol. xliii. (pp. 616–18) of the *American Naturalist* on the affinities of the South American marsupials of the genus *Cœnolestes.* From the structure of the skull and teeth these animals appear to be more nearly related to the polyprotodonts than to the diprotodonts, among which they have hitherto been placed. In fact, the large pair of lower incisors, which may well be an adaptive feature, forms practically the only diprotodont character, the dentition in other respects being essentially polyprotodont. Although the paper relates only to the existing forms, it has an important palæontological application, for if *Cœnolestes* be a polyprotodont, or the representative of an independent subordinal group, it follows that the same holds good for the extinct representatives of the same family, the *Epanorthidæ*, so abundant in the Santa Cruz formation of Patagonia.

To the *Geological Magazine* for 1909, pp. 210 and 211, Mr. B. H. Woodward has contributed a paper on the fossil marsupials of Western Australia, in which it is shown that remains of *Diprotodon* occur in association with those of existing species of kangaroos, thereby suggesting that the former animal became extinct at no very distant date.

Much interest attaches to the description of remains of two birds from the Pleistocene asphalt-beds of Bancho la Brea, California, by Mr. S. H. Miller in the *Publications of California University*, *Bull. Dep. Geol.*, vol. v. pp. 285 and 305. The first species is known by a tarsometatarsus, which appears to indicate a peacock of the typical Indo-Malay genus *Pavo,* Assuming the determination to be correct, we appear to have in this case an instance of a migration from Asia analogous to that of the tragelaphine antelopes referred to above. In the second paper the author describes, under the name of *Teratornis*, remains of an accipitrine bird of larger size than a condor, and characterised by the powerful beak, and enormous nasal apertures. Of minor interest is a note on the skeleton of a perching bird from the Pliocene of Leghorn, contributed by Mr. W. P. Pycraft to the Zoological Society's *Proceedings* for 1909, p. 368, where the specimen is assigned to the genus *Anthus*.

Commencing my notice of work on reptiles with the dinosaurian group, reference may be made to the description by Dr. E. Steckow, in the *Centralblatt für Mineralogie*, 1909, pp. 700–705 of some unusually well-preserved tracks of *Iguanodon* from the Lower Cretaceous of Bael Rehburg, Munich.

As mentioned in my article of last year, Dr. Hay some time ago criticised the pose of the skeleton of *Diplodocus carnegiei*, as mounted at Pittsburg and suggested that the limb-bones should be somewhat angulated to one another. Whether this criticism be justified or not, there seems to be no doubt that the foot should be turned on one side in sloth-fashion instead of being wholly plantigrade.

Recently Mr. J. Ternier, in a paper published in the *Sitzber. Ges. Naturfor., Berlin*, 1909, pp. 193 *et seq.*, has gone considerably further than Dr. Hay, and published a figure of the *Diplodocus* skeleton in which the scapula and coracoid are placed very low down and the humerus and femur articulated nearly horizontally, so that the body of the reptile is brought within a short distance of the ground, while the head and neck are reared aloft in swan-like fashion; the feet being applied flat to the ground. Soon after the publication of Mr. Ternier's paper a communication from Prof. O. Abel appeared in the *Verh. k.k. Zool. Bot. Ges. Wien*, 1909, pp. 117–21, in which it is urged that the sauropod dinosaurs were elephant-footed; that is to say they had semi-digitigrade feet supported posteriorly by foot-pads. To this Mr. Ternier (*Sitzber. Ges. Naturfor., Berlin*, 1909, pp. 527–57) makes a long reply, in which it is maintained that all these reptiles were strictly plantigrade. In a third paper, *l.c.* pp. 507–536, Mr. Ternier answers those who have criticised his original article on the pose of *Diplodocus*.

Very brief mention will suffice for a note by Mr. O. M. Watson, in the *Proceedings* of the Geological Society for 1909 on the Aëtosaurus-like *Ornithosuchus woodwardi* from the Elgin Sandstone of Lossiemouth.

Remains of a large crocodile from the Judith River beds of Montana have been referred by Dr. W. J. Holland in vol. vi. p. 281, of the *Annals* of the Carnegie Museum, Pittsburg, to a new genus and species under the name of *Deinosuchus hatcheri.* Although the skull is unknown, the length of this reptile is estimated at between thirty and forty feet and the species is regarded as one of the largest known crocodiles by its describer, who makes, however, no reference to *Rhamphosuchus crassidens* of the Indian Pliocene, of which the dimensions have been estimated as considerably greater.

Steneosaurs from the Oxford Clay of Peterborough have engaged the attention of both Dr. C. W. Andrews (*Ann. Mag. Nat. Hist.* ser. 8, vol. iii. pp. 299–308) and Dr. E. Auer (*Palæontographica*, vol. lv. pts. 5 and 6), each of whom was working on the subject without the knowledge of the other. As the English paper appeared first, the new names given by Dr. Andrews are entitled to stand. *Steneosaurus leedsi*, of that author, is characterised by the great length and slenderness of the flattened snout, as well as by the unusually large size of the temporal fossæ. A second species, *S. nasutus*, also long-snouted, is distinguished by the relative shortness of the temporal fossæ, the forward direction of the orbits, the sculpturing of the frontals, and the large number of teeth. A shorter-snouted species, *S. durobrivensis*, is characterised by the possession of only thirty-three pairs of teeth; while *S. obtusidens* differs from other members of the genus by its thicker and more massive snout and heavy, blunt-crowned teeth, of which there are twenty-eight pairs in the lower jaw.

Although known only by portions of the lower jaw, the discovery of a land rhynchocephalian in the Jurassic of Wyoming is a matter of considerable interest, as it is the first evidence of the existence of such reptiles in North America. These jaws are described in vol. xxxvii. pp. 35–42 of the *Proceedings* of the U.S. National Museum by Mr. C. W. Gilmore, who regards them as indicating a new genus and species, for which the name *Opisthias rarus* is proposed. So far as can be determined this reptile appears to be nearly related to the existing *Sphenodon* on the one hand and to the extinct *Homœosaurus* on the other.

In addition to his work on the steneosaurs Dr. Andrews has devoted his attention during the year to the plesiosaurians of the Peterborough Oxford Clay, with the result that he has found himself in a position to describe, in the *Annals and Magazine of Natural History*, ser. 8, vol. iv. pp. 418, 429, three new generic types. The first of these, *Tricleidus seeleyi*, is a small elasmosaurian plesiosaur allied to *Cryptocleidus* but distinguished by the shortness and width of the skull, the presence of twenty pairs of teeth, the existence of basisphenoid processes on the pterygoids, the broad and abruptly truncated parasphenoid and the possibility of the existence of two distinct elements in the quadrate region. There are twenty-six vertebræ in the neck. The second genus, *Picrocleidus*, established for the *Murænosaurus veloclis* of Seeley, is a longer-necked small plesiosaur, having no less than thirty-nine cervical vertebræ, all characterised by the shortness of their centra, of which the terminal faces are nearly flat and wider than high. The interclavicle, unlike that of *Tricleidus*, is reduced to a small triangle of bone, while the clavicles, if present at all, form mere filmy splints.

Of still greater interest is a blunt-nosed pliosaur described as *Simolestes vorax* and distinguished by the shortness and width of the skull, in which the snout lacks the elongation characteristic of both *Pliosaurus* and *Peloneustes*. The two halves of the lower jaw meet in a short symphysis, of which the inferior surface makes a marked angle with the lower border of the ramus itself. *Simosaurus* may be related to the imperfectly known *Thaumatosaurus* of the Lower Oolite of Würtemberg; the author suggesting that the Liassic forms referred by the present writer to the latter are generically distinct.

Plesiosaurians from Russia form the subject of a paper by Prof. A. Riabinia, published in *Mem. Comm. Géol., St. Petersburg*, livr. 43, in which the Engish *Peloneustes philarchus* is identified from Jurassic deposits and *Cimolioscarus bernhardi* from other beds of Cretaceous age. From presumably Kimeridjian strata at Simbirsk, on the Volga, Mr. N. Bogoluboro, in *Ann. Géol. et Min. de la Russie*, vol. xii., describes a new plesiosaur under the name of *Cryptocleidus simbirskensis*.

The same author, in the paper cited, likewise names a new ichthyosaur (*Ichthyosaurus steleodon*), from the Neocomian strata of the Simbirsk district; while a new ichthyosaur from the North German Chalk is described by Dr. A. Broili in vol. lv. pp. 295–302 of the *Palæontographica* with special reference to the "hypophysenloch" in the group generally.

From ichthyosaurs to mosasaurs is an easy transition and reference may therefore be made in this place to a paper on remains of the latter group from the Supra-Cretaceous beds of Orenburg contributed by Mr. N. Bogolutow to vol. xii. of *Ann. Géol. et Min. de la Russie*, in which a new *Liodon* is described.

Turtles and tortoises have formed the subject of several papers published during the year, among which special interest attaches to one by Mr. G. R. Wieland, published in the *American Journal of Science*, vol. xxvii., on the gigantic American Cretaceous turtles *Archelon* and *Protostega*. Apparently in conformity with the needs of a pelagic existence, the carapace in these turtles has been lightened by a great reduction in the size of the costal plates; this reduction being carried to the greatest extent in *Archelon*, where the absorption has also extended to the neural bones, many of which seem to have been diminished to mere films. Upon these aborted neurals is, however, imposed a series of digitated epineural dermal ossifications, undoubtedly corresponding to the neural keel of the living leathery turtle (*Dermochelys*) and discharging the function of the aborted neurals. *Archelon* seems, indeed, to have been covered with a continuous leathery skin and to have carried a series of dorsal keels comparable with those of *Dermochelys*. It is thus evident that *Archelon* shows a marked tendency to do away with the bony plates of the upper shell and to replace them with a superficial series of smaller bones comparable to the mosaic-like carapace of the leathery turtle.

In this respect the structure of *Archelon* and the allied contemporary genus has a most important bearing on the phylogeny of the turtles and, indeed, of the chelonian order generally. For the leathery turtle is referred by many naturalists to a group (the Atheca or Athecata) totally distinct from the one including all other chelonians. But if, as the American Cretaceous forms appear to indicate, the carapace of the leathery turtle is a secondary structure originally superimposed on the vanishing carapace of a turtle of a more or less ordinary type, the essential distinction between Athecata and other turtles is brushed away at one stroke, and the leathery species is nothing more than a highly specialised offshoot from the typical stock. The point is not, indeed, at present fully proved, although the American specimens go a long way in indicating that such proof will ultimately be forthcoming and Dr. Wieland is of opinion that those who regard *Dermochelys* as a specialised modification from the type of the *Chelonidæ* have somewhat the best of the argument. A new species of leathery turtle from the Miocene of Maryland, referable to the extinct genus *Psephophorus*, is described by Mr. W. T. Palmer in vol. xxxvi. pp. 369–73, of the *Proceedings* of the U.S. National Museum.

In the same volume, pp. 191–199, Dr. O. P. Hay describes remains of a marine turtle from the Niobrara beds of Wyoming referable to *Toxochelys stenopora* and likewise remains of a terrapin from the Fort Union beds of Montana provisionally assigned to the genus *Chisternon*, with the specific designation *C. interpositum*. This genus, which is typically from the Bridger Eocene, together with the Canadian *Boremys*, is remarkable for the presence in the carapace of a preneural bone interpolated between the first neural and the nuchal.

Considerable interest attaches to the description by Dr. A. Smith Woodward, in *Proc. Dorset Nat. Hist. Club*, vol. xxx. pp. 143–145, of a fine skull of *Pleurosternum* from the Purbeck of Swanage.

In the seventh volume, pp. 285–289, of the *Annals* of the South African Museum, Dr. R. Broom makes an attempt to refer to their proper horizons the extinct vertebrates of the Karu formation of South Africa. Another paper in the same issue, pp. 270–278, likewise by Dr. Broom, is devoted to the description of various extinct South African reptiles and amphibians. Among the former, attention may be specially directed to a new generic type of theriodont described as *Bauria cynops*, of which the skull is remarkably mammal-like and specially noteworthy in the absence of a bar between the orbit and the temporal fossa—a feature differentiating it from all other theriodonts. In a third paper, *op. cit.* pp. 283, 284, the same author describes the shoulder-girdle of *Cynognathus*.

Footprints from British Permian deposits form the subject of a paper by Mr. G. Hickling in the *Manchester Memoirs*, vol. iii. No. 22, written from a stratigraphical rather than a palæontological point of view. These prints serve, in the author's opinion, to associate the Red Sandstones of Dumfries-shire and the footprint-beds near Elgin to the Permian; while tracks recently discovered near Exeter will, it is expected, throw light on the age of the red rocks of Devonshire.

In a paper (*Proc. U.S. Nat. Mus.* vol. xxxvii. pp. 11–28) on the Carboniferous air-breathing vertebrates in the collection of the U.S. National Museum, Mr. R. S. Moodie directs special attention to the skeleton forming the only known example of *Isodectes punctatus*, and unfortunately lacking the skull. The close relationship of this reptile to the Microsauria seems to be well established, although to what group of reptiles it comes nearest cannot at present be ascertained. Some distant resemblances to the Brazilian *Mesosaurus* are pointed out.

In the paper just cited, as well as in one published in the *Journal of Geology*, vol. xvii. pp. 38–82, Mr. Moodie describes a number of American Carboniferous stegocephalian amphibians, among which several genera and species are new. The author considers that stegocephalians should be divided into the five suborders Branchiosauria, Microsauria, Aistopoda, Temnospondyli, and Stereospondyli. Of these, the Branchiosauria were salamander-like in form and for the most part devoid of heavy dermal armour, being naked except for small oval scales on the back and the usual chevron-shaped ventral armature characteristic of stegocephalians generally. The long and compressed tail indicates an aquatic mode of life; and this is confirmed by the presence in many forms, at least during the early stages of existence, of gills. "The group of the Branchiosauria are without doubt the direct ancestors of the modern salamanders and perhaps of the other groups as well." The new genus and species *Micrerpeton caudatum* is the only known North American member of the group, and also the only one from the Carboniferous. Special attention is directed to the similarity existing between the lateral line-system of this genus and that of the larva of the existing American salamander *Necturus*.

In the second of the above-mentioned papers by Dr. R. Broom in vol. vii. of the *Annals* of the South African Museum two Karu stegocephalians are referred respectively to the European genera *Trematosaurus* and *Capitosaurus*, under the names of *T. kannemeyeri* and *C. africanus*.

A labyrinthodont skeleton from oil-shales at Airby, New

South Wales, has been described by Dr. A. Smith Woodward in vol. viii. pp. 317–319 of the *Records* of the Geological Survey of New South Wales, under the name of *Bothriceps major*; its main claim to distinction from the typical *B. australis* of the Hawkesbury beds being the apparently larger size of the orbits.

In addition to his work on the air-breathers, Dr. Broom (*op. cit.* pp. 251–369) has devoted considerable attention to the fossil fishes of the upper division of the Karu series, which include specimens from the Rouxville district of the Orange River Colony and from the Ficksburg and Ladybrand districts. Those from the former locality may be of Upper Triassic and those from the latter of Lower Jurassic age. Apart from a new hybodont shark, a cœlacanth, and three species of *Ceratodus*, the majority of the remainder belong to fresh-water palæoniscids. Compared with the fauna of the Australian Hawkesbury beds, a marked similarity in generic types is noticeable. *Dictyopyge*, *Clithrolepis*, and *Pholidophorus* are, for instance, common to both areas, while the Australian palæoniscid *Myriolepis* represents the South African *Oxygnathus*.

Another faunistic memoir is the fourth part of Dr. A. S. Woodward's fishes of the English Chalk, issued in the Palæontographical Society's volume for 1908. This part treats of the genera *Pachyrhizodus*, *Tomognathus*, *Belonostomus*, *Protosphyræna*, and their allies, but none of the species described are new to science. Special attention is directed to the enormous length of the pectoral fins of *Protosphyræna*, a genus first discovered in the English Chalk but named from American specimens, on account of the teeth of the English forms having been erroneously referred to *Saurocephalus* and the fins to *Ptychodus*. In the fifth part of the same memoir (1909) the author treats of the pycnodont ganoids, the sturgeons, and the fringe-finned ganoids, and commences the chimæroids, as represented by *Edaphodon*; a new species of *Macropoma* being described in the fringe-finned group.

The 1909 volume of the Palæontographical Society's monographs likewise contains a further instalment of Dr. R. H. Traquair's memoir on the British Carboniferous ganoids; the group dealt with in this instance being the palæoniscids. No new forms are named; but an interesting restoration of the bones of the face and shoulder-girdle of *Cycloptychius carbonarius* is given.

In an important memoir on fossil fishes from several formations, published as vol. ix. pt. 5 of the *Memoirs* of the American Museum of Natural History, Dr. Bashford Dean directs special attention to the nature of the pelvic fins of the arthrodirans, which he finds to be of dermal and not of cartilaginous origin, and therefore unlike the pelvics of any known dipnoan. Accordingly, the supposed affinity of arthrodires to dipnoans is questioned.

Brief reference will suffice to a note by Mr. F. Chapman in vol. xxi. p. 452 of the *Proceedings* of the Royal Society of Victoria on the occurrence of teeth of the European selachian genus *Corax* in the Cretaceous of Queensland.

Mr. C. R. Eastman is the author of an important memoir on the Devonian fishes of Iowa, which, although published by the Iowa Geological Survey, vol. xxviii., in 1908, did not reach this country till the following year. After a preliminary sketch of the evolutionary history of fishes and a discussion of their taxonomy, the author divides the group into two classes; namely, the Agnatha, with the subclasses Cyclostomi and Ostracophori, and Pisces, with the subclasses Elasmobranchii, Holocephali, Dipneusti and Teleostomi. The forms found in the Devonian of Iowa are then discussed in detail, with descriptions of a certain number of new species.

In connection with the above may be noticed a paper on the fish-fauna of the Alberta shales of New Brunswick by Mr. M. L. Lambe, published in vol. xxxviii. of the *American Journal of Science*. The Alberta shales, which have been hitherto regarded as Carboniferous, appear from their fish-fauna to be of Devonian age; the general facies of the fauna being remarkably similar to that of the Scottish Devonian, as is especially indicated by the *Palæoniscidæ*, of which the genera are the same as those found in Scotland, while the species are closely allied. Certain new forms are named.

Mr. Maurice Leriche has, as usual, been busy with fossil fishes from various parts of the world. The first paper (*Ann. Soc. Géol. du Nord*, vol. xxxvii. p. 302) relates to the Tertiary sharks of California which, although previously regarded as distinct, are in many cases considered by the author to be inseparable from European species. In a second communication to the same journal, *l.c.* p. 227, Mr. Leriche describes the Eocene and Paleocene fishes of the Reims district. The formations in question comprise the sands of Châlons-sur-Vesle and the Cernay conglomerate, the lignitic clays and the Faluns of Pourcy. All the remains definitely identified belong to previously known species. The subject of a third paper by the same author, *l.c* p. 366, is the Carboniferous fish-fauna of the north of France. Most of the species are identical with those from other localities, but a new *Deltodus* is named and described.

Finally Mr. Leriche, *op. cit.* vol. viii. pp. 5 and 6, redescribes a very large cochliodont dental plate from the Belgian Carboniferous, which had been previously assigned to *Sandalodus robustus.* It is, however, a true *Deltodus*, and appears to be specifically identical with the so-called *Sandalodus morrisi*, of the English Carboniferous, which must now be known as *Deltodus morrisi.*

Students of the English fossil fish-fauna will be interested in a note by Dr. Smith Woodward in the *Proceedings* of the Geological Association, vol. xxi. pp. 322 and 323, on a second specimen of the ganoid *Dipteronotus cyphus* from the Lower Keuper of Bromsgrove, Worcestershire. This genus is a dapedoid near akin to *Clithrolepis.* Although the paper was read in 1909, it was not published till the first week of 1910.

At the close of a description of certain new types of the remarkable serrated Palæozoic fossils upon the evidence of which the genera *Edestus*, *Helicoprion*, etc., have been established, Dr. O. P. Hay, in a paper published in the *Proceedings* of the U.S. National Museum, vol. xxxvii. pp. 43–61, discusses the nature of those structures and their probable position in the animal body. That they pertain to elasmobranch fishes is admitted by all, but the idea that they are teeth, comparable to those of *Cestracion* although situated outside the mouth, is, in the author's opinion, untenable if only for the reason that none of the specimens exhibit any signs of wear. It is inferred that these structures were partially embedded in the flesh of the fishes to which they belonged, with a considerable portion exposed externally and also that they were situated in the middle line of the body.

In Dr. Hay's opinion the most probable explanation of the position and function of these structures is "that some ancient elasmobranchs developed in front of a median dorsal fin, or in place of it, not a single spine but a succession of spines. The new compressed spine, serrated in front and behind, arose in front of the older ones. Nevertheless, the root of the new spine became directed backward beneath and on each side of the preceding one, so as to embrace it. At first probably the older spines were shed, but in time they began to cohere and thus form a compound spine. In *Edestus* this was straight or slightly bent. All of it, or nearly all, except the serrated teeth, were buried in the flesh. As more and more elements were added the organ became more curved and finally in some species (*Helicoprion* and *Lissoprion*) formed a spiral, which was directed backward and the last turn of the shaft of which was elevated enough to keep the teeth from cutting into the skin. Such a weapon could be brought into action only if its possessor had dived under its victim and brought the spine across its abdomen, thus disembowelling it. It is in this way that the stickleback (*Gastrosteus*) attacks its enemies."

These organs are now referred to the four genera *Edestus*, *Toxoprion*, *Lissoprion*, and *Helicoprion*. In the first the saw is straight or nearly so, in the second the shaft is bent to a certain extent, while in the other two it forms a complete spiral coil.

THE UNIVERSITY OF LONDON AND AN IMPERIAL INSTITUTE OF SCIENCE[1]

By AUGUSTUS D. WALLER, M.D., F.R.S.

Late Dean of the Faculty of Science of the University of London

The custom that I find in force at this University is one that no man, however retiring of disposition he may be, can fail to obey. For it is a duty which I understand is never shirked, that any member of a Faculty, on returning from abroad, should report himself and his enlarged experience to a meeting of the whole University, and it is therefore no less a duty that cannot be shirked that a visitor from abroad should respond to the invitation to address the University on some topic that may be expected to be of common interest

I have been informed that, coming as I do from the University of London—which to many people abroad, and indeed to some people in London itself, is a somewhat mysterious entity—it would be welcome that I should tell the University of California something about the University of London.

I shall not offer you an epitome of its calendar, nor of its past history, nor describe to you its colleges and schools, nor dilate upon its constitution and its government. Rather than attempt to describe the University of London as it was yesterday and is to-day, I prefer to direct my attention and yours upon its immediate future, and upon some of the first principles that determine the healthy University in the healthy community—the *mens sana in corpore sano.*

I am not speaking now as a practical man, but as an idealist. Therefore I feel all the more free to confess as a first article of my University faith, that in last resort the motive of pure science must always be the practical motive, and that every student is required to render service to the community whose manifold services he enjoys. "Here's to the latest discovery,

[1] Being the substance of an address delivered to the University of California, Berkeley, Cal. U.S.A., on September 23, 1909.

and may it never be of use to anybody," is not the thought to which I should ever say "hear, hear," even as an after-dinner sentiment.

"I want to know" is indeed the most natural of all expressions in the mouth of a student of science. To the practical man of affairs, of whom immediate and decisive action is required, what can be of more indispensable necessity than the clear and comprehensive knowledge that can rightly guide his immediate and decisive action? I believe that no man has ever been sufficiently sensible of this need until he has been placed in circumstances that have forced him to take action in the absence of such knowledge. And the medical profession, where the watchword is, or should be, "I want to help," is above all other professions that in which there is most call for mutual help between the man whose first duty is to know and the man whose first duty is to help. The immediate aim of each is different, the ultimate aim of both should be the same—to contribute his best endeavour to the commonwealth of knowledge and of power. Yet the man who only "wants to know" is too prone to despise the practical requests of the man who "wants to help"; and the man who "wants to help" is too prone to ignore the service of the man who "wants to know." Each can help and teach the other, but the scientist must also want to help, as the physician must also want to know.

There is apt to be a kind of antagonism between the mind of the practical man and the mind of the scientific man. And if they never meet, that antagonism remains unopposed and futile or mischievous—the worst form of mental paralysis is *paralysis ignorans*. Let them meet, therefore—best of all let them meet in the common-room of their University—and from their opposed and complementary forces new mental strength will arise in the service of the commonwealth. Their antagonism will become co-operative and effective.

Co-operative Antagonism.—We are apt to be fretted by opposition. Our opponent is so entirely in the wrong and so wilfully obstructive of our plans and efforts. But for his blind or malicious hindrance it would have been so easy for us to "triumph over difficulties." Not so. Opposition and difficulty are of the essence of all achievement. The obstacles that are enough to repulse the weak and worthless character confirm, corroborate and, it may be, exasperate the energy that is necessary to work and to success. Just clench your fist, and feel the muscles of your forearm while you do so; you will find that in the act not only the *flexor* muscles that bend your fingers are at work, but also the *extensor* muscles which are their antagonists.

To grasp the handle of a weapon the antagonism of your extensor muscles is as necessary as is the action of your flexor muscles. Firmness is a result of the co-operative antagonism of opposing forces. The strong measure requires an effective opposition as well as a powerful ministry; and so, if we must be fretted by opposition and criticism, let us be fretted and irritated and strengthened to justify the faith that moves us, rather than daunted and discouraged by the peculiar difficulties that seem to have gathered in *our* special path, and to be *our* special misfortune. And when we stumble, let us stiffen ourselves in the knowledge that a stumbling-block surmounted is a stepping-stone in an upward path.

The Conservative Principle and the Progressive Principle.—There are two great principles involved in the welfare of every living thing—of every organised mass—whether man or nation, trade or profession or art or science, church or college or university—namely, the conservative principle and the progressive principle—the principle of imitation and of obedience and of heredity—the opposite principle of initiation.

Imitation and Initiation.—Any organised living mass—be it a single animal or an organised body of men—by virtue of the conservative principle of heredity, of repetition of like by like, of imitation of action that has achieved success, of obedience to custom that has survived—works at smaller cost than if each individual organism had perforce to work out afresh its own salvation, evolve by itself its own fittingness in the service and mastery of its surroundings.

But the child that can only imitate and repeat the actions of its parents and teachers contributes nothing to the excellence of the family and of the nation and of the race. The upward progress of each and every community requires the costly flame of initiative and discovery and invention, the burntofferings of talent and of genius at the altar of the common wealth and health.

The apprentice must first learn at the feet of his masters, believe what he is told, imitate what he sees done, copy good models, be the echo and the assistant of the experienced craftsman along well-beaten paths. But life is short and the arts are complicated, and even the apprentice who is never to become more than an efficient journeyman, still more so the apprentice of rarer clay who shall contribute to the commonwealth of knowledge and of power, is required to be something more than the faithful imitator of his teacher. He will be required to initiate; he should learn early from his teacher, by example rather than by precept, that the end and aim of his apprenticeship is not merely the actual knowledge and skill that experience can confer, but the ability of his own brain to deal with new or unexpected conditions.

In all provinces of human activity success is a resultant of the happy blend between these two complementary principles—imitation, the conservative principle—initiation, the progressive principle. But while in all provinces the conservative element —being, so to say, the means and the consequence of wholesale economy in nature—must bulk the larger, the progressive element, as the activating ferment that animates the mass, weighs but little in the scales of practical life. Yet fortunately, perhaps, while the pure ferment is of such rarity and tenuity that it fails sometimes to turn the scale even in the laboratory, it is the all-pervading and quickening leaven; and the rough goods of the market-place contain it and carry it abroad in the unceasing stream of useful applications to human wants.

The Method of Experiment and the Method of Least Change.—In every province of human activity—and in particular in that whereby knowledge and the power to use knowledge are continuously transmitted from generation to generation, namely, in the province of education—the faculty of imitation is easier to exert and easier to develop than is the more costly and more capricious faculty of initiation. Yet this rare and costly and, we must add, dangerous ingredient is of primary necessity in education. "Dangerous" we have said. Yes. Since we cannot surely tell, among the countless novelties issued from the genius or the vagary of the exceptional brain, which may be the hits and which the misses among all the innumerable projectiles by which our attention is solicited.

But while this extreme can be dangerous, there would seem to be very little danger in England of our running the risks inseparable from experiment and innovation. For the genius of our nation is a practical genius that looks upon the conservative way as the better way and makes its changes as slowly as may be by gentle gradient from precedent to precedent. That is the safe and easy way, the way of nature; and to this preference of fact copied over fancy tried may fairly be ascribed our own constitutional prosperity. We fight shy of the logical conclusions of the doctrinaire, and of the leap in the dark that seems to him so sure and so safe. We prefer to imitate the method of nature, the conservative method of least change.

Stagnation.—Yet there is danger in every extreme, even that of caution and "safety," and in our case the greater danger would seem to be on the conservative side of the beam, towards stagnation, rather than on the progressive side of innovation and experiment. We have been a most favoured nation in the great development of transport that has characterised the last century of the world's history. We still enjoy the fruits and the satisfactions of our good fortune and of our energy. But one may have too much of a good thing, if satisfaction should be permitted to blunt intellectual initiative, and to relax the practical endeavour to continue to excel.

We assuredly err on the conservative side in our educational methods. We are educated and governed by the time-honoured methods, of which the keynote and dominant chord are imitation and repetition and dialectics and old customs, to the almost universal exclusion of the most costly, dangerous, yet most valuable ingredient of human life—originality of thought and of enterprise.

The New Conditions.—These are the most characteristic qualities required by the new conditions of life, where men move in large masses and control large measures of the energy surrounding them. We need in the international struggle for welfare and for existence knowledge and power commensurate with the forces placed in our hands by the modern applications of physical science.

And the roots of applied science are pure science. And the propulsive force in the roots and trunk and branches of each living tree, as in every organism—be it science or art or craft, man or college of men or nation—is in last resort the quality and the character of the units constitutive of that organism; their specific power of initiative, added to the excellence of the heritage to which the initiative of their ancestors gave birth.

Education the most Radical Interest.—Education is the most radical of all interests. Granted that it must be true to nature as to its conservative principle. But let us also clearly recognise that education, now more than ever, requires to be urged still farther in obedience to the progressive principle—namely, in the direction of teaching the pupil to use his own mind in his profession, rather than to copy the mind of his professors. Yet since to copy and to imitate is in a measure the lot of all men, and the brain of the most original thinker is but a field in which other men's thoughts have germinated and multiplied; and since perforce to copy and imitate is the first and most natural act of life, let us insist that our professors and teachers shall themselves use their own minds and not suffer themselves to drop into the easy jog-trot of routine or pedantry. No doubt we want our professors to be *learned* men, but we also want them to be *learning* men, since they are the living models set before the minds of the rising generation.

The Combination between Teaching and Research.—It is upon the combination between teaching and research, and not upon their separation, that the intellectual welfare of a community and of an individual depends. For while it is a fact that one man may be the discoverer for himself alone, rather than the discoverer to others, and that a different man may possess special excellence as the interpreter and mouthpiece of other men's discoveries, it is no less true that the best guide to any district of knowledge is the man who has been there himself as an explorer or as a pioneer. It is in the blend between research and teaching that both research and teaching find their most effective expression. And in the resultant effect it is difficult to say which of the two elements is the more essential. The combination between them may be compared to that of common salt, in which both elements are necessary to the qualities of the compound. All good teaching involves research, all good research involves teaching. Faraday was at once a great inquirer and a great teacher, and in lesser degree every inquirer is a great teacher, every teacher is an inquirer. The professor, reader, lecturer, or tutor who fulfils his task as a mechanical repeater of dicta and dogmata is of hardly greater value than a text-book read aloud. The teacher who is also an active searcher and learner reacts upon his pupils with the convincing power of reality and of example; the combining power of his thought is that of active thought—thought in its nascent state.

University Research Fellowships.—It is to the credit of the new University of London that in the official recognition of its teachers the first qualification required on behalf of any man or woman who asks to be recognised as a University teacher is evidence of ability to increase knowledge by his own investigations—and further, that in the case of young teachers, where proof of such ability has not yet been given but may reasonably be expected, a system has been adopted of "recognition on probation" for a limited time, provided that the conditions of work are such as to permit the "probationer" to fulfil his promise and give proof of his ability to acquire knowledge at first hand by his own investigation. This excellent system might well be further developed; the active young teacher in a polytechnic, on a minimum salary of £150 and a maximum teaching time of 500 hours, would feel that the University was indeed helping and not hindering his efforts if his "probationary recognition" not merely required conditions protecting him in his own interest from over-teaching at an under-wage, but actually carried with it conditions forwarding his self-development and justifying his sense of fellowship in and loyalty to the University in which he is recognised. A "minor research fellowship" of £50 from the University chest, added to a teacher's salary of £150, would be in every way an appropriate and a remunerative expenditure of University funds; its direct return would be secured in the form of a higher average teaching power, apart from the accidental and incalculable return in the form of exceptional genius helped to earlier distinction, under conditions more favourable than at present to the mental development of the teacher during the best years of his life. Not to speak of the Colleges and Schools of the University, there are at present in the Polytechnics alone upwards of sixty recognised University teachers. Can it be doubted that the allocation of £1,000 in the form of twenty "minor research fellowships" would be hardly less valuable in the interests of the University and of the community than that of the same sum to a single University Professorship? Excellent as has been the allocation of the County Council Grant of £10,000 per annum to University Professorships, it is extremely desirable that it should be expanded and extended over a wider area. No one acquainted with the relations between the University of London and its several Colleges, Schools, and Institutions can doubt that the return to the University in the form of real influence and power, through the good-will and loyalty of its recognised teaching staff, would become incalculably extended by the carefully administered distribution of research fellowships among a teaching *personnel* that includes the picked men of what is actually a *corps d'élite* of capable and ambitious young men throughout the Metropolis. These picked men have proved their value and capacity under often adverse conditions, and against obstacles that have served to test their mettle; they are marked as eligible for further promotion by the fact that they have received recognition as teachers of the University. And looking forward to the future in the light of the past, is it not a wise policy that the University should broaden its base in the community and cast its net wide? Is not the Faraday of the future as likely to be found among the ranks of the ambitious young teachers of Polytechnic schools as among those of the equally ambitious if more favoured and less strenuously tried teachers in the Schools and Colleges of the University? But I do not desire to imply that any distinction of class or kind is to be admitted between recognised teachers of school, college and institution, when the status of recognised teacher is in itself a distinction and a token that the person so distinguished has excelled his fellows in ability and in working power. I would have accessible to all such teachers alike, not only minor research fellowships of the University on a lower scale of emolument, but full fellowships on a higher scale. I would have ordinary as well as minor fellowships, fellowships of £200 as well as fellowships of £50, from which, in correspondence with the higher scale of remuneration, a correspondingly higher standard of value should be the return, both as regards the work done by the recipients and as regards the consolidation of University influence upon the whole body of recognised teachers in the Schools and Colleges. Beyond this stage of full fellowship, at the Professorship grade, the University teacher appointed and paid by the University can 'be left to work out his own intellectual salvation and, as a student among students, to contribute to the common welfare.

The First Duty of the new University of London.—The first duty of the University is not to favour this or that College or School by the allocation of the scanty resources over which it has control to ordinary College professorships or lectureships, but to strengthen itself, and at the same time the whole field of its influence, by devoting its resources to the direct encouragement of research in association with teaching, and to the centralisation by that agency of the intellectual forces now scattered in these Colleges, Schools, and Institutions.

I am not pleading for the separate endowment of research, but for the further official recognition of research as an integral constituent of normal teaching at all grades of the University programme. Indeed, so far from urging that the "University research fellow" should devote the whole of his time to research, I should support the precisely opposite principle, and insist that some portion of his time should be devoted to teaching. It is upon regular teaching in some form that the average researcher must ultimately rely for his regular livelihood. This is especially so in the Faculty of Science and in that of Medicine, since the Hospital physician and surgeon is, above all, a teacher of the principles and practice of his profession. It is less so in other faculties which serve in greater measure as the channels and ante-chambers of the practical and commercial and legal and political professions.

But in any profession there is no mental gymnastic more valuable to the mind of the researcher than the instruction of other minds in the field of knowledge to which his own special interests belong.

The Royal Commission.—The organisation of the University of London is now under the scrutiny of a Royal Commission. The field covered by its reference is vast and complicated, and the task of reviewing and co-ordinating the local interests of the various colleges, schools, and other institutions more or less closely connected with the University is likely to be heavy and lengthy. I do not propose to enter upon any discussion of these various interests, nor to suggest any scheme for a reconstitution of the University of London. But in connection with "the provisions for teaching and research that should exist in the Metropolis, and their connection with similar provisions existing in other parts of the United Kingdom and of His Majesty's Dominions beyond the seas," I shall sketch a scheme that commends itself to my mind as a concrete and feasible outcome of the foregoing considerations.

The teaching *personnel* of Colleges, Schools, and Institutions of the University of London form as many separate groups of men (and women) very slightly attached to "the University." These groups may be pictured as a collection of variously coloured strands more or less loosely attached to an imaginary central point called "The University"; of these several groups, two principal groups—University College and King's College—form a distinct and united body at this point—they have incorporated themselves there, and without any surrender of College identity are by reason of that "incorporation" entitled to be regarded as the commencing embodiment of a true University. A third group, the Imperial College of Science and Technology, is nominally a School of the University, but in reality is entirely independent of its control, and, as far as we know, desires to remain so. Other groups—the Medical Schools, the London School of Economics, the East London College, the Birkbeck College, the three Women's Colleges, the six Polytechnics, are loosely tacked on to the imaginary point called the University of London. The collective value of the stuff in these disconnected strands is very great, but it is in great measure wasted for lack of a transverse bond of union threading together the strands themselves. That bond of union by which the loose fabric should be knitted together should be found in the Boards of Studies and Faculties of the University, which are the groups of teachers with like interests, attached to the several colleges, schools, and institutions.

The Faculties.—The organisation of these groups on Faculty lines, which are to be regarded as lines transverse to the lines of colleges and schools, would form as it were woof to web in the fabric of a real University.

The stuff of which any University is formed consists of men and women; they are at present organised on College lines; what is required to knit them together into a University fabric is a further organisation on Faculty lines. This further organisation is by no means difficult of accomplishment; its lines are already laid down in Boards of Studies, and the financial support, without which no policy or enterprise can be sustained, is not of any forbidding magnitude. University research fellowships, tenable by recognised teachers of the University, would afford a means of forming from the Boards and Faculties lists or panels in various subjects, the members of which as "research fellows of the University" should be liable, when called upon, to deliver at the head-quarters of the University courses of lectures on subjects of which their own investigations had rendered them authoritative exponents. It is during the first ten or fifteen years of his teaching career that the teacher's mental activity is keenest and that his quality is made apparent. I would, if funds permitted, definitely recognise and encourage the development of power of such a teacher, by the allocation to him of a research fellowship that should be expected to occupy half his working time, and to supply him with half his living wage, and that should cause him to bring to the central lecture theatre of the University real additions to knowledge, and to the lecture theatre of his school augmented mastery of the subject he has to teach. I can imagine no condition of life more enviable than that of a keen-brained man or woman during the best ten years of intellectual life, from, say, the age of twenty-five to that of thirty-five, in receipt of a salary of £200 for teaching during half the week and of a fellowship of £200 for "researching" during the other half. I am convinced that under such conditions of life the return in teaching power would repay the outlay in money, and that from among the workers thus supported the exceptional man or woman would be far more likely to emerge than is the case under our present conditions.

Quite independently of the interests of the exceptional mind —which being exceptional cannot be expected to be of frequent occurrence—it is by the general levelling-up influence to be secured by the encouragement of individuality of thought throughout the teaching *personnel* that the return of value for value expended would be most certainly assured.

An Object-lesson.—The University of London is not altogether without experience in the direction of intercollegiate centralisation on Faculty lines. The Physiological Laboratory, to which some years ago it devoted a portion of the somewhat limited space placed at its command in the Imperial Institute building, can be appealed to as an object-lesson on a small scale of the principles that should be applied to the more comprehensive organisation. Its teaching *personnel* consists of a panel of University lecturers, liable to be called upon, when convenient to themselves and to the University, to deliver a course of eight lectures upon a special department of science with which they are acquainted at first hand, and in which they are of recognised authority. The panel, at present composed of thirty-seven persons, consists of (1) the recognised teachers of the University, and (2) other distinguished experts in science from the United Kingdom and from the Dominions. Of these thirty-seven members, eighteen are recognised teachers of the University of London; six are distinguished specialists living in London; eight are teachers in the Universities of Oxford, Cambridge, Liverpool, and Bristol; and five are professors and experts belonging to Toronto, Winnipeg, Johannesburg, and Alexandria.

The Committee of Management is a mixed Committee, composed of (i) members of the Senate, (ii) other persons interested in this particular aspect of University life. The category of "other persons" includes members chosen by reason of their knowledge of and interest in its subject-matter, as well as members interested to the extent of fulfilling the functions of the pious founder; and in this category of "other persons" are to be found the most valuable servants of the University of London. I think it may be permissible to name them in connection with this particular object-lesson. Sir Lauder Brunton, having no other official connection with the University, is the Chairman of the Committee; Sir Walter Palmer, as a graduate in Science, interested in the welfare of Science and in the general welfare of the University, defrayed the equipment expenses of the Laboratory. Thus as regards administration, this department of the University, while subject to the ultimate authority of the Senate, is not under the exclusive control of any one party or college or school, but is guided in the most absolutely smooth and harmonious manner by a mixed Committee of members of the Senate and other distinguished persons, with whom the sole object is the efficiency of the department in the interests of the University; and from what I have said it is clear that the interests of the University are not viewed solely in an exclusively local sense, but with distinct bearing upon "the facilities for education and research which the Metropolis should afford for specialist and advanced students in connection with the provision existing in other parts of the United Kingdom and in His Majesty's Dominions beyond the Seas." I have quoted the words of the reference to the Royal Commission appointed this year on the organisation of the University of London. The rough sketch that I have just given of the constitution and working of a University organisation is the description of what has been actually going on for the last eight years in the Imperial Institute and as a department of the University of London.

In other subjects—notably in Botany, Geology and Zoology—an organisation of advanced lectures on similar lines has taken place, and only requires for its proper development facilities similar to those that have been enjoyed by Physiology.

An Imperial Institute of Science.—It is essential to the success of such an organisation that it should be from the outset concentrated and centralised by the University itself, in lecture-rooms and laboratories and libraries under its direct control. If the organisation of panels of Research Fellows of the University is to be common to all the teachers of all its Colleges, Schools, and Institutions, its local habitation must be at the University itself, not at any one or more of its colleges. In this connection, as well as in connection with the provision required for teaching and research "in the Metropolis, in the United Kingdom, and in the Dominions beyond the Seas," the Imperial Institute at South Kensington is clearly indicated as the proper habitation of a college of men drawn from among the active teachers in the Metropolis, in the United Kingdom and in the Dominions.

All the materials are ready to our hand for the foundation of an Imperial College of Learning and Science that should one day become in relation to British Learning and Science what the Collège de France has been and is in the intellectual life of France, and fulfil the purpose for which the purchase of the South Kensington Estate was recommended fifty-eight years ago by the late Prince Consort.

The building stands ready amid a group of active Colleges, occupied partly by the offices of the University, partly by the offices of the Imperial Institute itself. But in both parts, the natural and fitting service that it ought to fulfil is shown by the existence of active laboratories which have arisen in it as the natural and fitting organs of an Imperial Institute of Science. What nobler service could be assigned to the Imperial Institute than that of an Imperial Institute of Science and Learning, a central meeting-place of an intellectual *corps d'élite*, composed of the most active learners and teachers of the Metropolis and of the United Kingdom and of His Majesty's Dominions beyond the Seas?

The building was intended as an Imperial Institute of Commerce; that intention has not been fulfilled, and cannot be fulfilled at South Kensington. Yet, if we believe that Commerce rests upon applied Science, as applied Science rests upon pure Science, is it not an even wider fulfilment of the original purpose, and a fuller satisfaction of the generous support of that purpose from all quarters of the British Empire, to form an Imperial Institute of Science as a provision in the Metropolis for University teaching and research, and as "a facility afforded by the Metropolis for specialist and advanced students in connection with the provision existing in other parts of the United Kingdom and of His Majesty's Dominions beyond the Seas"—an Imperial clearing-house of knowledge?

To recapitulate the whole argument:

The quickening factor of initiative—*i.e.* of research—must permeate the substance of the receptive and imitative element in teaching. Research and teaching form an indissoluble compound, each a necessary complement of each. Research apart from teaching, teaching apart from research, are equally unnatural and comparatively ineffective factors. It is essential to the welfare and efficiency of a University, and especially so in the case of the University of London, that in the organisation of its teaching staff at all grades effect should be given to this dual principle—no research without some teaching, no teaching without some research.

The practical measures by which it is possible to give effect to this dual principle in London are such as would at the same time constitute an intercollegiate bond of union formed by the University between its Colleges, Schools, and Institutions through its Faculties and Boards of Studies.

The formation of this bond of union should consist in the foundation of an Imperial Institute of Science and Learning, of which the present Imperial Institute building should be the home and head-quarters, and its *personnel* select panels of University Research Fellows. Such panels should consist of professors, recognised and probationary teachers, and other distinguished persons in London, in the United Kingdom and in His Majesty's Dominions beyond the Seas, selected and nominated by Boards of the Faculties, appointed by the University.

REVIEWS

The Principles of Pathology: Vol. II. "Systemic Pathology." By J. GEORGE ADAMI and ALBERT G. NICHOLLS. [Pp. xv + 1082.] (London: Henry Frowde and Hodder & Stoughton, 1910. 30*s*. net.)

THE present volume, in which Dr. A. G. Nicholls co-operates with the author, completes the treatise on pathology undertaken by Prof. J. G. Adami, the first part of which has been already noticed in SCIENCE PROGRESS (vol. iv. p. 163). It comprises the consideration of the special pathology of the different organs of the body, or, as the authors prefer to call it, "Systemic Pathology."

Prof. Adami put us in a happy frame of mind by his admirable discourse on the various difficult matters with which he dealt in the first volume, but his readers can hardly be so well drunk that they will fail to detect the deterioration in the execution of this second part. The intention is admirable. Adami has himself laid down most clearly that pathology is a co-ordinated science and not a catalogue of phenomena, and in the preface to the present volume our expectation of something good is kept up by some pregnant sentences pointing out that the consideration of morbid function must go hand in hand with the study of morbid structure. But, in the body of the book, these very proper principles are followed out in a half-hearted kind of way, and good intentions have paved the way to a mediocre performance.

The proportionate amount of space devoted to the various divisions of the subject is curious. The pathology of the formed elements of the blood occupies sixteen pages, this trivial treatment being excused on the ground that it is a specialised subject dealt with in special text-books. But the section on the skin runs to seventy pages, that on the eye to forty-eight—both are specialised subjects with their own special text-books, and neither affords very suitable material to illustrate the general principles of pathology, material which may be freely drawn from the morbid changes in the tissue blood. The liver has but thirty-four, and the whole of the urinary system only fifty-nine pages out of a total of 1016. The relative detail in which the various subjects in a book such as this should be considered would seem to be determined by considerations, on the one hand, of their theoretical weight in a philosophical system of pathology, and on the other by their practical importance. Text-books, after all, are intended for the instruction of students, and it seems to us of the utmost importance that, in trying to impart a proper knowledge of the fundamental conceptions of pathology, one should utilise to the fullest possible extent material with which the student is likely to come into intimate contact. In other words, his data should be drawn as far as may be from diseases which are common in his experience and from material, histological and the like, with which he can readily obtain direct personal acquaintance. It seems further clear that it is well to impart information in proportion as it is available, and above all to discourage the leach after the curious and exceptional, except in cases where uncommon conditions are of material theoretical importance. Judged by standards such as these, it is difficult to recognise that the book as a whole has been well planned.

In more detail, we may refer to the treatment of one of the common objects of the post-mortem room, the "nutmeg liver," which occupies just one page (pp. 456–7). In the first place, in discussing the ætiology, the assumption is tacitly made that any condition within the thorax which obstructs the return of blood to the right heart must necessarily raise the blood-pressure in the branches of the hepatic vein within the liver, and consecutively the ultimate destruction of liver-cells is attributed to pressure. No mention is made here of the possibility, at least as much in accordance with the observed facts, that the liver-cells are killed by an imperfect blood-supply due to slowing of the blood-stream, though there is a passing reference to this effect on page 29. The cut surface of the liver is said to "resemble that of a nutmeg," and the cells in the peripheral zone of the lobules are described without qualification as being fatty. Both these statements are frequently untrue: fortunately the ordinary student will see "nutmeg livers" so often that he will probably be able to find this out for himself. The picture given to illustrate the condition is extraordinarily bad and we know of at least one intelligent student who was for some time in doubt as to whether it represented a macroscopical or a microscopical view. It is, in fact, macroscopical, and there is no illustration of the minute anatomy. Contrasting with this, we find an equal space of one page (p. 502) devoted to two illustrations of a growth of the pancreas of which—so says the text—only two cases have been described, and which is of the smallest theoretical interest. Nearly two pages with four illustrations are given up to the rare condition blastomycetic dermatitis, which is of importance only as showing that this kind of organism may be pathogenic in man. The whole pathology of the teeth is finished off in a page and a quarter.

From the student's point of view, perhaps the most serious drawback is the imperfection of the illustrations. They are rather few in number, often ill-chosen and in many cases the execution is not above criticism. In the section on the eye, for example, there are six illustrations: two are bacteriological, two histological (one of these is of a curiosity) and two are photographs of patients. To illustrate the pathology of the skin, there are three plates and twenty-nine figures in the text: nine figures illustrate parasites or histological sections, while the plates and twenty text illustrations are given up to pictures of patients. With the fullest appreciation of the range of knowledge which our science embraces, we cannot allow that such illustrations are appropriate to a text-book on pathology, especially when they are present to the exclusion of such pictures as would assist the student to interpret the naked-eye appearances in terms of the essential cellular changes which underlie them. There is, for example, a picturesque plate of a negro with white spots, but nothing to show the histological changes in any of the common forms of dermatitis. The pictures themselves would be admirable in a clinical treatise on dermatology but they are altogether out of place here. On a rough enumeration, just one-fifth of the 310 illustrations are essentially clinical. This tendency culminates in a table (p. 905) of differential diagnosis of a variety of diseases of the breast by their clinical signs and symptoms, and the only illustration having reference to the important subject of mammary cancer is a horrible plate showing a patient with "neglected carcinoma of heart." The contrast is very striking in this respect between the present volume and two text-books of pathology published last year—Beattie and Dickson's *Special Pathology* and the *Lehrbuch* edited by Aschoff: in both of these the illustrations are conspicuously good.

Disappointment is apt to exasperate, and it is no doubt disappointment of the hopes raised by Adami's first volume which brings into such definite relief the imperfections of the present book. There is, in the text, much that is good. The chapters on the organs of circulation and respiration are distinctly better than most of the book, and the section on the kidney is meritorious if cursory. It is better not to enter upon a speculative correlation of relative virtue with individual authorship, though there is a considerable temptation—and in places not much difficulty—to identify the spirit and manner of the *General Pathology*. As a whole the text is somewhat above, and the illustrations much below, the average found in other books which cover approximately the same ground. The book is too big and too expensive to be generally adopted by students.

A. E. BOYCOTT.

Children in Health and Disease: A Study of Child-Life. By DAVID FORSYTH, M.D., D.Sc., Physician to the Evelina Hospital for Sick Children, etc. [Pp. xix + 362.] (London: John Murray, 1909. Price 10*s*. net.).

THIS book, while written by a medical man as a result of his professional experience and for members of his own profession, is one which should be of equal value to the educationalist and may be read with interest and profit by the intelligent mother or nurse. It deals in the first place with the physiology of child-life, which the author very rightly regards as commencing at the time of conception, and subdivides into antenatal, natal and postnatal physiology. With reference to growth in weight he lays stress on the fact that the percentage increase is a more instructive indication of a child's progress than is the absolute increase, and considers that the amount of food required by an infant should be estimated by the weight of the child and not by its age, as is the usual method. Put more exactly, as 80 per cent. of the total energy derived from food is converted into heat, and the loss of heat depends upon the extent of the body surface, the calorific value of the food given must bear a definite relationship to the body surface. From the facts that a definite ratio exists between body weight and surface area and that the calorific value of milk is readily ascertained from its composition, it can easily be calculated what quantity of milk is needed by an infant of given weight. A short table is given constructed on the above lines, which should prove very simple and satisfactory in practice.

As regards growth in individual organs we may notice that although an infant's brain nearly doubles in weight during the first six months after birth, the frontal lobes, the site of the intellectual centres, do not begin to attain the prominence that is a feature of the adult brain, until the sixth year. When that fact is appreciated we shall surely have an end of the cramming of little children so common in our elementary schools, where a class of six-year-olds was recently asked by an inspector "How many twopenny-halfpenny toys could I get for forty pence?"

After a couple of very interesting chapters on the psychology of childhood, the author proceeds to discuss school life from both medical and educational points of view. Especially suggestive are his remarks on the hygienic conditions of boarding-schools and the need of medical inspection among the children of the upper classes: if, as he says, it is the usual practice to send the younger boys to bed not earlier than ten o'clock it is no wonder that nervous troubles are common.

A useful chapter is devoted to Idiotic and Feebleminded Children, and the remainder of the book is occupied by the consideration, on very broad lines, of the important subject of disease in childhood. The section dealing with the Examination of Infants and Young Children is written with great detail and is evidently the outcome of many years' experience of the difficulties of this branch of medical work. It should be of great help to the young practitioner who may leave his hospital equipped with all the theoretical knowledge necessary for his success, and yet find himself at a loss when his patient, aged six or eight months, resents the application of it.

We are glad to note that the author is free from modern "fads"—for instance, unlike some would-be reformers, he insists on the importance of a warm soft covering for both legs and arms, taking into consideration the relatively great heat-loss suffered by children : and again in the question of moral training he considers that the first steps can be taken "only by exacting implicit obedience, and this control should not be relaxed until repeated practice has ingrained the primitive canons of conduct as habits of thought and of action."

We have less sympathy with the suggestion of a table railed round and covered by a waterproof mattress as an alternative playground to the floor for a lively baby, and we think enough attention is not given to the means of locomotion—crawling, sidling and the like—which precede the upright walking. But these are minor points in a book which is otherwise excellent and very pleasant reading. There is a good index ; but the frontispiece, which is the only illustration, does not strike us as of sufficient scientific value to compensate for the fact that its inclusion in this prominent position may deter many non-medical readers from the perusal of a book which would be of the greatest value to them.

JESSIE D. GRANGER EVANS.

Stonehenge and other British Stone Monuments Astronomically Considered. By SIR NORMAN LOCKYER. Second Edition. [Pp. xvi + 499, 106 illustrations.] (London : Macmillan & Co., Ltd., 1909. Price 14*s.* net.)

SIR NORMAN LOCKYER'S first book upon this subject was published in 1894, under the title of *The Dawn of Astronomy* ; it dealt mainly with Egyptian temples, and devoted only two or three pages to Stonehenge ; but in 1906 he brought out, as a continuation, the first edition of the present work, devoted almost entirely to rude stone monuments. The present edition contains nearly two hundred pages of new matter, relating mainly to monuments examined after the publication of the first edition, and forty more illustrations—many from photographs by Lady Lockyer—which of themselves make it a possession to be desired by any one interested in the subject.

Sir Norman Lockyer found that in Egypt many of the temples were so arranged that the rays of the rising or of the setting sun at the solstices or equinoxes shone through the entrance and the whole vast length of the building into the sanctuary, but that the axes of others were directed too far northward to have any relation to the sun, and must have referred to some star ; he was confirmed in this view by the changes anciently made in those temples which were not oriented to the sun (because, owing to precessional changes, "once a solar temple, a solar temple for thousands of years, once a star temple, only *that* star temple for something like three hundred years"), and he then formed the ideas that the rising of a star long enough before the sun to enable sacrifices to that luminary to be made ready for the moment of its appearance would be watched for, and that the year of the erection of a star temple might be inferred from the date at which some brilliant star would have risen or set in the right part of the horizon. We believe that his applications of this theory to the dates of foundations of Egyptian temples have not commanded the unanimous assent of Egyptologists but, as no chronology for Egypt has yet been universally accepted, Sir Norman Lockyer may perhaps be entitled to consider his own as good as any other.

The observance of the rising sun, whether daily or at special seasons, was, in Sir Norman Lockyer's opinion, propounded to the populace as a religious duty, but was carried on by the priesthoods all over the world for the very practical purpose of ascertaining the exact length of the year, and the recurrence of the proper seasons for sowing, etc., so that all agricultural operations might seem to require their sanction, and receive it—for a consideration.

When Sir Norman Lockyer returned from Egypt he determined to see how far these ideas found support, at Stonehenge in the first place, and at other rude stone monuments afterwards. Stonehenge has long been thought by many archæologists to have had some astronomical purpose or connection, and it is well known that the whole structure is set towards the point at which the sun rises at midsummer; the exact place of the rising of the sun varies in the course of years, though very slightly as compared with that of the rising of the stars, and, having adopted the axis of the avenue of earthen banks which leads north-easterly from the circles as the most satisfactory line of sight, Sir Norman Lockyer decided that Stonehenge was founded between 1900 and 1500 B.C. This date, being deduced from the direction of the earthworks, does not necessarily settle that of the erection of the stone circles, and it is not unlikely that those now remaining may belong to a much later period. At Stonehenge our author saw reason to think that there were references to the sunrisings and settings in May, August, November, and February, all old pagan festivals, and still the Scottish quarter-days; he finds the same things elsewhere in Britain and in Brittany, and considers the "May-November year" to be older than the "solstitial" year. Our oldest existing remains may indeed belong to a May-November year period, but it would be so difficult to fix the half-quarters before the times of the solstices and equinoxes were settled that the solstitial year must surely have come first somewhere.

From Stonehenge Sir Norman Lockyer conducts his readers—in many cases "personally"—to Brittany, Cornwall, Dartmoor, Somersetshire, Wales, Cumberland, Westmorland, Yorkshire, and Scotland, as well as to other Wiltshire monuments, and finds everywhere references in the stone circles, lines of stones, and dolmens or cromlechs, sometimes to the sunrisings and settings in the "May year," or in the "solstitial year," the observance of which supplanted, he thinks, that of the former, and sometimes to "clock-stars," by which the time could be determined during the night, or to "warning-stars," the rising or setting of which gave notice of the approach of sunrise.

We have stated Sir Norman Lockyer's propositions at considerable length, because they have been virulently and perhaps not always quite fairly attacked, sometimes by archæologists who are not astronomers, and sometimes by astronomers who are not archæologists; on the other hand, they have been supported by naval surveying officers, who, having studied the monuments carefully, are entitled to rank both as astronomers and archæologists, and we ourselves see nothing unreasonable in them.

While, however, we take no exception to the author's general principles, we cannot unhesitatingly accept all his conclusions. In Egypt it was apparently the custom to erect a massive temple to mark the direction of sun or star rising, but in north-western Europe the easier and more economical plan of setting up one or more stones in the desired line was adopted, and it is assumed, if a line of stones points to where a certain star rose in a certain year, that it was in that year that they were set up; but it is obvious that the fact that stones are now found in such a position does not prove that they were there at all when the star so rose—say between 1000 and 2000 B.C. Of course if a great number of lines point in the same direction it may reasonably be inferred that something was meant by it; but Sir Norman Lockyer's dates vary from 2330 B.C. at Tregaseal to 250 B.C. in Scotland for Arcturus, and he dates the stones at Shap and Boroughbridge as far back as 3000 and 4000 B.C. by α Centauri, without any other evidence. It has also been pointed out that some of the stones on which he relies cannot be seen from the circle with which he connects them. He indeed seems to suggest intermediate signal-stations; but why should there have been so much elaborate and unnecessary machinery? The dolmens or cromlechs, which he thinks were used partly as dwellings for the priests and partly as observatories, he has found to be mostly oriented for the sunrise; that they were so oriented is no doubt true, but it is a question whether that were not rather a matter of ritual than of astronomical use, for the number of dolmens in some places and of circles in others has been so great that they could not all have been required for observatories, or even for shrines or sanctuaries. In short, it seems not unlikely that Sir Norman Lockyer's facts may in many cases be explained in a different way from that which he suggests; still, even if his work should not prove to be the universal key which he thinks it to be, it is certainly one which should be possessed by every student of this difficult but fascinating subject, and which cannot be neglected by any.

A. L. Lewis.

Introduction to the Preparation of Organic Compounds. By Emil Fischer. Translated by R. V. Stanford. [Pp. xix + 175.] (London: Williams & Norgate, 1909. Price 4*s*. net.)

A translation into English of a foreign book of laboratory procedure must be judged both on its merits and by its applicability to the system of practical instruction prevalent in this country. In this instance the mere fact that it is a translation of the eighth German edition is sufficient testimony of the reception which has been accorded to the book in Germany. Without, however, wishing to detract in any way from the merits of the translation, in the writer's opinion it is far more desirable for the English student to make use of the work in German and so obtain that practice in the use of the language which is indispensable when the original literature has to be consulted.

The University Chemical Laboratory at Berlin, in which this preparation book is used by all students, is in the happy position of being always crowded with students eager to undertake the work of original investigation. Consequently, by imposing stringent qualifying tests, the Professors are able to discriminate between the various candidates on the ground of their practical ability, and so select those best qualified to undertake researches under their guidance. It is an open secret that many of the more difficult preparations described were introduced with this object in view.

Since the investigation of the proteins has acquired such importance in Berlin, Fischer's laboratory has proved to be a great centre of attraction for medical men and physiologists anxious to work in this field but generally untrained in chemical manipulation. The preparations described in the second part of the book have been written to give such students an insight into the methods of dealing with carbohydrates and proteins; certain of the earlier preparations are indicated as fitted to give them the necessary preliminary practice. For the training of physiological chemists this part of the book can hardly be considered to be as well suited to English conditions as the courses suggested by Dr. Aders Plimmer and in use at University College, London.

An interesting glimpse of that devotion to the organisation of the minutest detail, which is so characteristic of the Germans, is afforded by the Introduction, which contains a long list of precautions necessary to prevent accidents.

The list of preparations is extensive and sufficiently diversified to give experience in the customary methods. The methods are described in that crisp and clear style so characteristic of Prof. Fischer's own papers. The original literature bearing on the subject is often referred to but no attempt is made in the text to deal with the theory of the interactions described.

The translation seems well done, though it is inaccurate in places. Thus the rendering of "Steinnuss" by "Brazil nut" on page 139 is calculated to deceive the reader. Very objectionable is the abbreviation of litre to "li"! It would be better if the nomenclature adopted by the Chemical Society were followed throughout.

E. F. A.

Crystalline Structure and Chemical Constitution. By A. E. H. TUTTON, D.Sc., F.R.S., A.R.C.Sc. (Lond.) [Pp. viii + 204.] (London: Macmillan & Co., Ltd., 1910. Price 5*s*. net.)

THIS is the first of a series of manuals in which a number of scientific workers will describe their own researches in special branches of science. The present volume gives an interesting description of Dr. Tutton's classical investigations on the relations of certain salts of the three alkali metals, potassium, rubidium, and cæsium, which resemble each other so closely in their chemical properties, and of ammonium and thallium, which are less nearly allied. The compounds studied were the orthorhombic simple sulphates and selenates, and the monoclinic hydrated double sulphates and selenates with zinc, magnesium, or other dyad metals. Those who have already read a brief account of the work which appeared in an article in the first number of SCIENCE PROGRESS (July 1906) will welcome the opportunity of studying the subject in more detail. The results obtained after twenty years of patient application, in which all the most advanced methods of research were utilised, are full of scientific interest. Though confirming in many respects the theories of Barlow and Pope, they show that the "packing" which the latter assume in the building up of crystal structure cannot be of a very close description, as it admits of the substitution of the five atoms of ammonium for one of rubidium with very little modification of the form or even of the dimensions of the space-lattice of the crystal structure.

The variations in the crystallographic form of these compounds when rubidium, cæsium, ammonium or thallium are substituted in turn for potassium, or selenium for sulphur, are illustrated by detailed tables of the changes in the angles between the principal faces, and in the parameters and (in the monoclinic form) the angle β. The most interesting feature disclosed by these relations is the possibility they hold out of ascertaining the spatial relations of the atoms of the different elements in the space-lattice. It may be suggested that such changes in shape would be more easy to realise from tables showing the variations in azimuth and polar distance of the normals to the faces, referred to the horizontal plane in the orthorhombic and the plane of symmetry in the monoclinic system, and in these days of two and three circle goniometers such tables would be prepared with the minimum of trouble and calculation.

One of the most valuable features of the book is the description of the delicate apparatus devised by the author for carrying out the exact measurements which his researches necessitated. The cutting and grinding goniometer by which thin sections in any required direction may be obtained with the greatest accuracy; the monochromatic illuminator by which light of any required wave-length is available for optical work; the interferometer which enables the most trifling movement of a plane surface in the direction of its normal to be observed; and the dilatometer and elasmometer by which such observations are applied to the determination of the linear expansion with increase of temperature and of the elasticity of crystal plates.

Dr. Tutton has resisted the temptation to indulge in the speculations on crystal structure to which the subject and his results would so easily lend themselves, preferring to dwell on the important and suggestive facts which his careful work has revealed. He has not always, however, we learn from the introductory pages, restrained his scientific imagination so severely. He gives us an interesting quotation from an essay published as early as 1884 in the *Science Schools Journal*, entitled "Protylian Vapourings":

> "Let us say that an atom of hydrogen contains x primary particles or atoms, held together by their inertia of revolution, then our theory says that sodium contains $23x$ primary atoms; consequently the ring must be more closely packed, and less energy can be externally manifested. Similarly potassium has $39x$ primary particles to the atom, and mercury $200x$ primary particles."

If, as Dr. Tutton suggests, we read "electronic corpuscles" for "primary particles," the passage represents very fairly the views now held on the constitution of matter.

JOHN W. EVANS.

The Interpretation of Radium. By FREDERICK SODDY, M.A. [Pp. viii + 256.] (London: John Murray, 1909. Price 6*s*. net.)

THIS book contains an accurate presentation in non-technical language of the modern interpretation of radium, namely, that it is an element undergoing spontaneous disintegration. The text is based upon the subject-matter of a course of lectures delivered recently by the author in the University of Glasgow. It is pointed out at the beginning that the science of Radioactivity is not an expansion or extension of existing principles but a new step regarding the ultimate nature of the atom. The fact that the author, in conjunction with Prof. Rutherford, carried out much of the work which laid the foundations of the hypothesis of atomic disintegration, increases considerably the interest of the book.

The opening chapters deal with the experimental effects of radioactivity, the continuous emission of energy by radio-elements, and the properties of the α-, β-, and γ-rays. The two alternatives, as to whether the energy of radioactive elements comes from outside sources or from within the atom itself are discussed, and it is shown that the weight of evidence is in favour of the latter assumption. The book then deals with the properties of the various bodies which are formed by radioactive changes. Starting with Uranium (atomic weight, 238) the changes are traced down to Radium G (atomic weight, 206. Lead?). Many admirable experiments are described in the text: the spinthariscope for counting α-particles, the radium clock, the geological interest attaching to radioactivity and many other interesting subjects are dealt with.

The book is clearly and forcibly written and can be recommended to all interested in the subject as well as to those who wish to begin its study.

R. W. Forsyth.

An Atlas of Absorption Spectra. By C. E. Kenneth Mees, D.Sc. [Pp. 74.] (London: Longmans, Green & Co. Croydon: Wratten & Wainwright, Ltd. 1909. Price 6*s*. net.)

During the special researches necessary for the production of satisfactory photographic dyes and light-filters, the workers in Messrs. Wratten & Wainwright's laboratories at Croydon were forced to prepare an atlas of absorption spectra, because no such atlas, dealing with the less refrangible end of the spectrum, was available. Dr. Mees now publishes the result of their compilation with suitable explanations and indices.

The volume contains the photographic absorption curves of 170 dyes in common use and 76 filters; each curve is fitted with a wave-length scale and ordinates showing "intensities." The photographs from which the curves are reproduced were taken with a specially designed spectrograph, the apparatus and method being fully described in the text.

The first requirement was to eliminate, so far as possible, the selective sensitiveness of the plate employed; to this end, "Spectrum Panchromatic" plates were used in conjunction with a suitable standard filter, the combination giving a fairly flat colour curve between λλ 7200 and 3900. Solutions of the dyes in pure water were placed in front of the slit in a rectangular vessel divided into two wedge-shaped compartments: one compartment contained the solution of the dye under examination, the other pure water. In this way the thickness of the solution traversed by the beam of light from a Nernst lamp was varied, along the slit, and the photographs indicate the absorption effects of different thicknesses of the solution, or, what is nearly the same thing, the variation of the absorption with the concentration. This device being impracticable with the filters, the variation in intensity was attained by the interposition of a wedge of neutral-tinted glass.

Of the photographic maps themselves but little can be written, except that they are excellent and should prove exceedingly useful in the many departments of scientific research where it is now so desirable to employ light of a restricted wave-length range. Where the amount of light available is unlimited, there should now be no difficulty in referring immediately to a dye, or a filter, which will give almost any desiderata in this direction. The indices given by Dr. Mees explain succinctly the nature and source of the dye employed in each case, the strength of the solution used in the test and the stability of the dye under the action of light. Each filter, also, is indexed under its special designation.

In the investigation of "colour" in photographic processes Dr. Mees occupies a pre-eminent position, and we welcome this atlas with the assurance that the results are based on truly scientific bases and will greatly facilitate research in many branches of science.

W. E. Rolston.

Dynamo Laboratory Manual: Vol. I. Direct current Studies and Tests. By William Suddards Franklin and William Esty. [Pp. viii + 152.] (London: Macmillan & Co., Ltd., 1909. Price 7*s*. 6*d*. net.)

A laboratory manual of any kind is always sure of two attacks; one set of critics say it tells the student too much, the other that it tells too little.

Each set may be justified in its criticisms, because everything depends on what the student knows and how he has acquired his knowledge. This manual would, generally speaking, be very suitable for students taking their second-year course in electrical engineering, and the tests would give, if carried out systematically, not only a clear idea of the fundamental principles of direct current machinery, but at the same time a grasp of ordinary commercial tests. The cautions given to the students would, if observed, diminish the amount of money paid by him in fines.

The reviewer would quarrel with some of the statements which are made in various parts of the book. Dogmatic statements of fact in a manual of this sort should only be made where it is impossible to verify them by the outlined experiments, but even when this is the case the statement, if only true in certain cases, had better not be made.

On page 2 there is the assertion "Any alternating current ammeter may be used for measuring direct current," and on page 6 we are told "A Wattmeter may have its range increased by shunting the current coil," and no mention is made of the very rigorous conditions that the shunt has to fulfil. The student who had learnt to regard text-books as infallible would get at some time a rude awakening if he followed these rulings blindly.

It is, however, a worse offence to make a statement about an experiment which the tests carried out would show to be wrong, as on page 44, where we are told under the heading of Theory that "The series motor is a so-called variable-speed motor; that is to say, that speed varies greatly with load when the supply voltage is constant and the motor operates better at starting than the shunt motor which is called a constant-speed motor."

This statement is open to two objections: first there is the use of the word "operates," the meaning of which is vague and secondly if the statement "the motor operates better at starting" be taken to mean that the motor has a higher starting torque than a shunt motor, it is only true when the starting current is *greater* than the full load current as if say only half the normal current of the machine is allowed to flow from the mains, the series machine will have only half the starting torque of that given by a shunt motor.

J. T. IRWIN.

An Introduction to the Study of Biology. By J. W. KIRKALDY and I. M. DRUMMOND. [Pp. vi + 259.] (Oxford: The Clarendon Press, 1909. Price 6*s*. 6*d*.)

THIS book would be found useful as a companion to a course of practical work in elementary biology.

It is divided into three parts: *Part I.* (pp. 1–39) treating of the unicellular organisms—Amœba, Saccharomyces, Sphærella, Vorticella and Paramœcium, and concluding with a chapter containing a comparative study of these and pointing out their relation to various allied forms, *e.g.* Monocystis, desmids, diatoms, bacteria, etc. *Part II.* (pp. 40–90) treating of simple multicellular organisms—Spirogyra, Mucor, Hydra, Obelia, together with various allied forms, *e.g.* Vaucheria, Eurotium, Fucus, Chara, Agaricus, corals, etc. *Part III.* (pp. 91–249) treating of the higher plants and animals—earthworm, crayfish, dogfish, frog, rabbit, mosses, liverworts, ferns, pine and sunflower.

The book is illustrated with about a hundred diagrams. Of these about one-fourth are apparently new, while the rest are from the works of Gœbel, Darwin, Scott, Strasburger, Howes and other well-known sources.

The book gives an account of the structure and physiology of each of the organisms which are generally taken as types for practical study and enables the student to obtain an insight into the relation of the chosen types to one another. The introduction of various allied forms tends also to broaden the student's view. Thus after an account has been given of the structure, etc. of the crayfish, it is compared with the earthworm and the hydra, its position among the Arthropoda is referred to, and an outline is given of the structure and life-history of the gnat as a representative of the Insecta.

G. A. FREEMAN.

Plane Trigonometry. By H. S. CARSLAW. [Pp. xviii, 293 + xi.] (London: Macmillan & Co., Ltd., 1909. Price 4*s*. 6*d*.)

IT is extremely difficult to write with freshness on such a well-known subject as elementary trigonometry, but Dr. Carslaw in his attempt has achieved a fair measure of success. The first part of the work deals with trigonometry up to the solution of triangles, and upon this portion of the book there is little to remark. The examples on the very elementary parts of the subject are numerous and graphical methods are freely introduced. A knowledge of logarithms is assumed, and four-figure tables of the necessary functions are given at the end. The proofs of the addition theorems for all values of the angles are particularly clear and good; and the "ambiguous case" in the solution of triangles is clearly set forth.

The last chapter of Part I. deals with circular measure, and is the first distinctive chapter in the book. It contains an interesting discussion on the length of a curve, introducing the representation of rational and irrational numbers by points along a straight line. The definition of the length of a circular arc follows on that of the "limit of a sequence," and the whole subject is treated in a fresh and interesting manner.

The first chapter of Part II. deals with the triangle and its associated points, lines and circles, and should be included in Part I.; Part II. would then begin with De Moivre's theorem and its applications, a usual and suitable arrangement. After the factorisation of algebraic and trigonometrical functions there is a good chapter on inverse notation, a function and its inverse function being well illustrated graphically. In this part of the book, graphs, which can be very easily overdone on the more elementary parts of the subject, serve an admirable purpose as illustrations of the convergence of series. The power series, and the series

$$u_0 + u_1 \cos\theta + u_2 \cos 2\theta + \ldots,$$
$$u_0 + u_1 \sin\theta + u_2 \sin 2\theta + \ldots,$$

are thus illustrated by figures for which the author acknowledges his indebtedness to Mr. Whipple. This treatment should prove stimulating to the student who is anxious to learn sound methods rather than to work out complex examples in a slipshod way. The introduction of this modern treatment into an elementary book is a step forward and is a justification for a new trigonometry, intended for the use of higher classes in secondary schools and junior students at the universities. The author is careful to indicate, in the series with which he deals, points needing further examination; while all series which depend upon the use of a complex variable are excluded from the scope of the work. If Dr. Carslaw will give us a Part III., dealing with these series in as fresh and intelligible a manner as that in which he has treated the series of a real variable, the learner will owe him no small debt of gratitude.

We notice misprints on pages 141 and 278.

The graph of y = cosec x on page 62 would be improved if the origin were in the same relative position as that of its companion graph, y = sec x: and it should be noted that the sides and angles of the pedal triangle of ABC are of different forms according as ABC is an acute- or obtuse-angled triangle.

F. G. CHANNON.

Problem Papers in Mathematics. By R. C. FAWDRY. [Pp. viii + 240.] (London: Macmillan & Co., Ltd., 1909. Price 4*s*. 6*d*.)

ANY teacher who has undertaken the preparation of candidates for army examinations must have felt the difficulty of obtaining suitable examples to set to his pupils. Whatever may be thought of the mathematics required for Sandhurst and Woolwich, the fact remains that a distinct type of example is set, and that that type is not to be found in the ordinary school text-books. Mr. Fawdry has done a useful piece of work in compiling this book, which contains problems set to the Military Side at Clifton during the past six years, as well as questions drawn from other sources. The problems are arranged in graduated sets, and rise by easy stages from arithmetic, algebra and geometry, through trigonometry, to mechanics, analytical geometry and differential and integral calculus. Students preparing for Mathematics I. or II. will find the book most useful: and we imagine that it will be a great relief to teachers to have such a good store of examples ready to hand.

Revision papers, and a few papers from the examination for school certificates, complete a book which should meet with a cordial reception.

F. G. CHANNON.

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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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