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PROCEEDINGS

OF

THE ROYAL SOCIETY.

January 7, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Experimental Researches on the Propagation of Heat by Conduction in Muscle, Liver, Kidney, Bone, and Brain." By J. S. LOMBARD, M.D., formerly Assistant Professor of Physiology in Harvard University. Communicated by CHARLES E. BROWN-SÉQUARD, M.D., LL.D., F.R.S. Received December 7, 1885.

(Abstract.)

The apparatus employed in the present investigations was the same thermo-electric one that was used in the experiments on conduction of heat in bone, brain-tissue, and skin, described in a former paper,* but the mode of application of the thermo-pile to the tissue was somewhat different. The tissue, whether hard or soft, was placed on a thin copper plate, which formed the floor of a square hole cut in the bottom of a small light wooden box. The pile, having been applied to the upper surface of the tissue, was held in place by means of a pasteboard collar, which was made fast with pins to the edges of the box. In the case of the soft tissues, light weights were affixed to the pile to regulate the pressure. With bone, in order to insure intimate contact between the pile and the tissue, and between the latter and the copper plate, a little marrow was used. The unoccupied space in the box was filled with finely chopped cotton-wool. The box had pasteboard uprights attached to its sides, by which it was suspended from the

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* "Proc. Roy. Soc.," vol. 34, pp. 173, 198.

sliding arm of a stand. The inferior surface of the copper plate was brought in contact with water of a temperature lower than that of the air by fractions of a degree of centigrade, as in the former experiments referred to.

Experiments on Muscle.

The muscles examined were those of the head, thigh, and leg of the sheep.

It soon was noticed that the rate of transmission differed somewhat, according as the muscle was examined in the direction of its fibres or perpendicularly to them; and this fact led to the division of the experiments into two classes, according as the line of conduction was parallel or at right angles to that of the fibres.

Tables I and II give results obtained under these two conditions respectively. The results represent 104 observations on conduction parallel to the direction of the fibres, and 100 observations on conduction at right angles to this direction.

Table I.—Conduction of Heat through 10 mm. of Sheep's Muscle, parallel to the direction of the Fibres.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	33·985391	58·359600	19·959500
„ 6 „	51·215703	77·219200	34·712200
„ 9 „	66·775211	93·108500	49·133700
Permanent thermal condition ...	82·730123	99·500000	63·557000

Table II.—Conduction of Heat through 10 mm. of Sheep's Muscle, perpendicular to the direction of the Fibres.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	27·038177	40·837000	11·373300
„ 6 „	40·701253	60·789900	26·283600
„ 9 „	58·174220	84·384200	39·203600
Permanent thermal condition ...	76·614920	99·422300	50·911200

It will be seen that parallel conduction shows the higher percentages—average, maximum, and minimum—at every period. The average percentages of superiority of parallel conduction over conduction at right angles are as follows:—

At the end of 4 minutes.		At the end of 6 minutes.		At the end of 9 minutes.		Permanent thermal condition.
6·947214	....	10·51445	....	8·600991	....	6·115203

The conductivity of muscle, unlike that of the other tissues examined, does not appear to depend, at least in any marked manner, upon the degree of freshness of the tissue. So long as the muscle is kept in a moist state, it seems to conduct equally well whether recently removed from the animal or after decay has commenced; and when the conductivity has been decidedly lowered by exposure to the air, it generally can be partially, and sometimes completely, restored, by moistening the tissue with water or fresh animal juices.

Experiments on Liver.

The liver examined was that of the sheep. Table III gives the results of sixty experiments on this organ.

Table III.—Conduction of Heat through 10 mm. of Sheep's Liver.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	45·628640	61·618700	27·367800
" 6 "	64·338080	79·448900	48·352500
" 9 "	81·164906	93·171900	64·383800
Permanent thermal condition ...	93·043060	99·500000	78·004000

The conductivity of liver diminishes steadily and rapidly after death, and is not restored by moisture or fresh animal matter, although these latter seem to reduce the rate of loss.

Experiments on Kidney.

The observations were made on sheep's kidney. Tables IV and V give respectively the results of thirty experiments on the cortical substance, and of an equal number on the medullary tissue.

The tables show that at every period of the observations, excepting the maximum for the ninth minute, which gives a slight balance in favour of the medullary tissue—the cortical tissue is the better conductor.

Table IV.—Conduction of Heat through 10 mm. of the Cortical Substance of Sheep's Kidney.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	44·512983	53·013900	27·725100
,, 6 ,, ...	64·946250	72·318700	59·696400
,, 9 ,, ...	82·431483	87·027000	78·138000
Permanent thermal condition ...	97·715600	99·500000	93·466600

Table V.—Conduction of Heat through 10 mm. of the Medullary Substance of Sheep's Kidney.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	36·541850	46·861700	19·867800
,, 6 ,, ...	56·686350	69·645700	39·955000
,, 9 ,, ...	71·536316	87·121000	53·310900
Permanent thermal condition ...	91·947716	98·676300	78·150400

Both cortical and medullary substances behave like liver as regards the diminution of conductivity after death, and the effect of water and fresh animal matter on this loss.

Experiments on Bone.

The observations were made on the tibia and the ilium of the sheep. The experiments were divided into three classes, according as the tissue was compact, spongy or combined compact-spongy. Some 200 experiments were made, which were divided about equally between the three varieties of tissue.

Tables VI, VII, and VIII give the results of these experiments. According to the tables, spongy tissue stands first in average, maximum, and minimum conductivity, at every period, and the combined compact-spongy tissue comes next, also as regards all three valuations and every period of time.

Table VI.—Conduction of Heat through 10 mm. of the Compact Tissue of the Head of Sheep's Tibia.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	24·067400	30·377800	16·106900
,, 6 ,, ...	36·305560	40·411400	30·831300
,, 9 ,, ...	47·959167	54·529600	35·357000
Permanent thermal condition ...	70·770483	75·717700	56·179500

Table VII.—Conduction of Heat through 10 mm. of the Spongy Tissue of the Head of Sheep's Tibia.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	35·939917	54·671700	21·894200
,, 6 ,, ...	52·274800	74·495300	31·398500
,, 9 ,, ...	70·911200	89·498600	41·547200
Permanent thermal condition ...	89·779800	97·225600	62·436700

Table VIII.—Conduction of Heat through 10 mm. of Sheep's Ilium. Compact and Spongy Tissues combined.

Time.	Percentages of heat transmitted.		
	Averages.	Maxima.	Minima.
At the end of 4 minutes........	32·736216	45·292000	18·351600
,, 6 ,, ...	48·374200	64·755300	31·195800
,, 9 ,, ...	61·197667	81·495700	40·602900
Permanent thermal condition ..	74·741783	95·458500	59·718700

Both compact and spongy tissues lose their conducting power more or less rapidly after removal from their natural surroundings; spongy tissue much more quickly than compact. Spongy tissue may regain the greater part of the loss of its conductivity, after the application of water or fresh animal matter, but this is not the case with compact tissue; however, moisture seems to slightly reduce the rate of loss in the latter. With regard to the compound tissue—compact-spongy, the changes which its conductivity undergoes present simply a varying mean of those of its two components. After long exposure to the air, the bone being well dried, the conductivities of compact and of spongy tissue are found to closely approximate each other.

Experiments on Brain.

The experiments on this tissue had reference only to the changes of its conductivity, due to exposure to the air, and to the effect of moisture and fresh animal liquids on these changes.

Like liver and kidney, the tissue of brain quickly loses its power of conduction after death, and neither moisture or fresh animal matter can restore this loss, although they may diminish its rate.

II. "Further Researches into the Function of the Thyroid Gland and into the Pathological State produced by Removal of the same." By Professor Victor Horsley, B.S., F.R.C.S. Communicated by Professor Michael Foster, M.D., Sec. R.S. Received December 10, 1885.

In December, 1884, I showed that the thyroid gland was intimately connected with the process of mucin metabolism, that if the thyroid gland in monkeys was removed with antiseptic precautions (the same ensuring healing of the wound in three days) the consequences to the animal were—(1) symptoms of general nervous disturbance evidenced by tremors, paroxysmal convulsions, functional paralysis, mental hebetude, and finally complete imbecility; (2) profound anæmia coupled with leucocytosis; (3) all the symptoms of the disease discovered within the last decade and termed myxœdema; (4) that just as in the acute form of the disease just named there was found to be a great accumulation of mucin in the connective tissues throughout the body (mucinoid degeneration), and in the blood, and as a consequence the same post-mortem appearances; (5) that at the same time there was a great activity in the mucin-secreting glands, and, further, that the parotid gland under these abnormal circumstances secreted mucin in large quantity, the gland cells at the same time disintegrating.

During the past year I have confirmed my previous observations, and greatly extended them, and have firm basis for my original opinion that the function of the thyroid gland is indispensable to the higher animals, and that it is duplex, since, in the first place, it regulates the formation of mucin in the body; and, in the second place, it aids in the manufacture of blood-corpuscles. My researches during the past year (1885) have been directed towards the investigation of (1) the circumstances which influence the course of the extensive disturbance of general nutrition which follows the loss of the gland; (2) the direct effect of the said fall in nutrition upon the nerve-centres; and (3) the hæmapoietic function of the gland.

(1.) I find that the determining factor *par excellence* of the value of the gland as regards its influence on the general metabolic processes of the animal is *Age.* The effect of removing the gland in the young animal is the rapid appearance of violent nerve symptoms and death in a few days; in a rather older animal, *i.e.*, a one-year old dog, the symptoms are less violent, later in their appearance, and the animal survives perhaps for a fortnight or three weeks; in a very old animal the removal of the gland simply hastens the torpor of old age; these observations refer to dogs and cats. In the higher animals, monkeys, the operation on a young individual produces the same result as in a young dog, but, as I showed last year, an older animal, if kept under ordinary circumstances, will survive for six or seven weeks, dying at the end of that time of myxœdema. On the whole, therefore, it appears that the thyroid gland is of extreme importance when tissue metabolism is most active, and that it diminishes as the senile state advances. Huschke has shown that the relative weight of the thyroid body to the body weight is greatest at birth, that it rapidly diminishes during the next few weeks, and that it steadily decreases as age advances. Finally, the structural degeneration of the gland in old age is well known. It is clear, therefore, that the gland plays an important and constant part in the metabolism of the body; I desire here to draw special attention to the fact that the symptoms of old age, namely, wasting of the actively functional parenchymatous tissues, atrophy, and falling out of the hair, decay of the teeth, dryness and harshness of the skin, tremors, &c., are exactly the most prominent features of the myxœdematous state, whether it occurs naturally in the human being, prematurely, as in cretinism, or artificially, as in my experiments on monkeys. It is, perhaps, well to remark here that, as might have been foreseen, the previous state of nutrition of the body determines to a large extent the rapidity of onset and the course of the symptoms.

The next circumstance of extreme importance which influences the course of the symptoms is the *Temperature* at which the animals are kept after the gland has been removed. I showed last year that one of the most obvious features of the fall of nutrition which follows the loss of the gland was a steady diminution of the body heat, this suggested to me a line of research which has yielded a striking result. I have kept another series of animals (on whom I have performed thyroidectomy under the conditions above stated) at a constant temperature of 90° F.,* and when they exhibited any nerve symptoms, *i.e.*, tremors, &c., were placed in a hot-air bath at a temperature of 105° F. The effect of this has been to lengthen the duration of life (in all but very young animals) four or five times the extent of that observed in the first series. Instead of living four to seven weeks they now live as many months. At the same time several additional facts of importance are noted, and the symptoms before referred to are so modified as to require the addition of a third stage to the two I described in 1884. (These observations refer solely to monkeys.) The animals kept under the extra high temperature above noted thus pass through three stages—(1) neurotic, (2) mucinoid, (3) atrophic. I have said that the neurotic stage under these circumstances may be scarcely marked, or if the nerve symptoms occur, and the animal be put in the hot-air bath, they soon disappear. Next the animal lives through the mucinoid stage, *i.e.*, myxœdematous condition, and arrives in the third stage—the atrophic. Now, the symptoms of the second stage are just as much subdued as those of the first, there is no excessive secretion of mucus, the parotid glands do not swell, and the post-mortem examination does not reveal the extensive mucinoid degeneration observed in the first series. Finally, the third, atrophic, stage into which the animal passes is evidenced by great emaciation, functional paresis and paralysis, imbecility, falling blood pressure and temperature, with death by coma.

I am disposed to regard this fact of the animals passing through these neurotic, mucinoid stages, and dying at the end of the atrophic, as the key to the observation that cretins in whom the thyroid gland is very slowly destroyed, and very chronic cases of myxœdema, do not exhibit much mucinoid degeneration.

(2.) I will now briefly enumerate the direct effect of the fall of nutrition produced by the loss of the thyroid gland on the nerve-centres: (*a*) Effect on cortex.† The tetanus obtained by stimulating the cortex is remarkably changed (even as soon as one day after the thyroidectomy in a dog, who exhibited violent symptoms in twenty-four hours) by the fact of the fall (when the current was shut off) being as sudden as that observed on stimulating the corona radiata. Next, that the tetanus in a more advanced case is soon exhausted, the curve approaching the abscissa soon after the initial rise; at the same time the curve is followed by clonic epileptoid spasms, which, however, are soon exhausted. Stimulation of the corona radiata and spinal cord also gave the customary tetanus, which, like that of the cortex, was rapidly exhausted. These stimulations of the nerve-centres sup-

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* In my first experiments (1884) the animals were kept at a temperature varying from 60° to 70° F.

† Graphically recorded according to method described by Prof. Schäfer and myself ("Proc. Roy. Soc.," vol. 39, p. 404).

pressed the thyroid tremors just as voluntary movements do. Another evidence of the changes in the cortex is the frequency with which continuous stimulation will evoke the appearance of clonic spasms on the original tetanic curve, the latter not being followed by epilepsy when the current is shut off.

(*b*.) Effect on the spinal cord. The tetanus obtained by stimulating the spinal cord like that of the cortex rises slowly to the highest point, and then steadily falls towards the abscissa although the stimulation is maintained, and when the current is shut off the muscle completely relaxes, having absolutely lost its tone, and this tonic paralysis is not recovered from for ten to fifteen seconds. Stimulation of the spinal cord to fatigue, after some time has elapsed so as to produce exhaustion of the preliminary tetanus, evokes a tremor of eight to ten per second.

Tracings from an old animal (cat) which had survived the operation some months, and also from a dog, in which case the symptoms had been very severe for some days, exhibited only a very feeble tetanus in the former instance, and no reaction at all in the latter; this being the ultimate state of depression of function which the nerve-centres had arrived to.

(3.) I have thought it as well to add to the anatomical and physiological proofs I gave last year of the thyroid gland being a hæmapoietic structure by counting the number of corpuscles in the blood of the thyroid artery and vein respectively. After discounting any possible alteration in the relative number of the corpuscles in the two vessels by changes in the fluid constituent of the blood which may have happened in the gland, the much greater number of corpuscles in the vein (+7 per cent.) confirms the deductions drawn from my previous observations.

To sum up, the functions of the thyroid gland appears to me to be two-fold as already suggested, viz.: (1) Control of mucin metabolism, (2) Hæmapoiesis. The metabolic processes in the body may be regarded broadly as resulting in Construction and Destruction. The products of destruction are the waste products of tissue change, and being, as such, harmful to the organism, are cast out by the excretory organ. It appears to me that the thyroid gland aids in excretion of mucinoid substances or their precursors, not of course by excretion properly speaking, that is, casting them out from the body, but by metamorphosing them into some other form which is useful to the system. That this process, whatever it is, is of vital importance to the young mammal (seeing that interference with it causes death in a few days) is obvious, and such as it is the loss of it is distinctly connected with the appearances of the diseases known as myxœdema, cretinism, and senile degeneration. Finally, this defect in the circle of metabolism determines the appearance of so-called functional disorders of the nervous system.

III. "Contributions to the Anatomy of the Central Nervous System of Plagiostomata." By ALFRED SANDERS, M.R.C.S., F.L.S. Communicated by Dr. GÜNTHER, F.R.S. Received December 11, 1885.

(Abstract.)

After referring to the literature of the subject, the author gives a short account of the macroscopic appearance of the brains of the following species of Plagiostomata, viz., *Raja batis*, *Rhina squatina*, *Scyllium catulus*, and *Acanthias vulgaris*. He then refers to the distribution of the cranial nerves, especially of the trifacial and vagus, pointing out the resemblance of the distribution of the last-mentioned nerve in Rhina to that described by Gegenbaur* in Hexanthus; the difference lying in the fact that in the former the rami branchiales of this nerve, the number of which correspond to the number of the branchial arches, divide into two terminal branches only, the rami anteriores and posteriores, the third, the rami pharyngei, being absent.

On the other hand, in Scyllium the rami branchiales do not divide, the terminal twigs, representing the rami pharyngei, only being present.

The lobi olfactorii consist of two parts, the lobe proper and the peduncle. The lobe itself is more or less pear-shaped, broader at the anterior end where it abuts on to the olfactory organ, and narrower behind where it passes into the peduncle. It consists of three layers, counting from before backward, or from outside inward. The posterior, which is also the internal layer, occupies more than half of the lobe, and consists entirely of a mass of small cells embedded in a network of fibrillæ and granular neuroglia. This network is of extreme tenuity, and the cells contained therein are oval, pear-shaped, or spherical in shape, and contain a nucleus and nucleolus; they give off processes which join the network. In front of these, and outside to a certain extent, is found a layer consisting of glomeruli olfactorii; these are elongated or pear-shaped masses arranged with their long axes in the direction of the nerve fibres. They consist of a central core of closely intertwined fibrillæ; externally the fibrils are of rather larger size; they run longitudinally in reference to the glomerulus; in their course elongated cells are developed.

The anterior or external layer consists of interlacing bundles of fibres which pass from the anterior ends of the glomeruli into the olfactory organ. The bundles themselves are flat, but the fibrillæ of which they are composed are round.

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* "Jenaische Zeitschrift," Bd. 6, 1871.

The structure of the peduncle resembles that of the olfactory lobe, and gradually passes into that of the cerebrum at the posterior end. In Scyllium, Rhina, and Acanthias it contains a passage which puts the ventricle of the olfactory lobe into communication with that of the cerebrum. In Raja, however, both the lobe and the peduncle are solid.

The cerebrum contains two ventricles which posteriorly communicate with a single chamber, the foramen of Monro; this is the case in Scyllium, Rhina, and Acanthias, but in Raja only a very small ventricle is present which represents the foramen of Monro, the remainder of the cerebrum being solid. Round the external surface of the cerebrum there is a layer of granular neuroglia with comparatively few cells. The remainder of the parenchyma consists of a mass of cells, larger ones, 13μ to 10μ in diameter, occupying the centre, and smaller ones predominating towards the internal surface. In Scyllium the cells are arranged in groups of four or five, and in Raja also in groups of from nine to twenty-one, which make a meandering pattern through the parenchyma in some parts. At the base of the cerebrum there are four special groups of cells, two being placed in the outer walls and two in the inner walls; the outer groups are associated with the fibres of the anterior commissure, and the inner groups are associated with the fibres of the crura cerebri.

The third ventricle is a gutter-shaped channel, long in Scyllium, but shorter in Raja, which leads from the cerebrum into the optic lobe; above, it is closed in by processes of the pia mater which enter the ventricle and the foramen of Monro, forming a choroid plexus; below, the third ventricle communicates by a passage, the infundibulum, with the ventricles of the hypoarium; the parenchyma in this lobe contains numerous cells measuring from about 13μ by 7μ to $6u$ in diameter, which give off numerous processes to join a fine network which pervades the whole. The ventricle is lined by an endothelium which is continuous with a space in the hypophysis cerebri. There is a small tubercle in front of the optic lobe which corresponds to the tuberculum intermedium of Gottsche,* and from it a bundle of fibres can be traced passing towards the ventral surface of the medulla oblongata, which corresponds to the fibres of Meynert.

The optic lobes which homologise with the anterior corpora quadrigemina form a cover arching over the aqueduct of Sylvius, in the same position as in the Teleostei; they are much thicker, but more simple in structure. Neither the tori longitudinales nor the tori semicirculares, those tuberosities which form prominences on the floor of the aqueduct in the Teleostei, are present in the Plagiostomata. Three layers may be distinguished in the optic lobe; the external

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* "Müller's Archiv," 1835.

occupies about two-thirds of the thickness, and consists of longitudinal fibres which are derived from the optic tract, and numerous cells which attain their maximum number in this layer; they are mostly spherical, but fusiform cells with their long axes placed radially are occasionally found.

The second layer consists of bundles of transverse fibres partly derived from the lateral columns of the medulla oblongata, and partly from the commissura ansulata; they correspond with the transverse fibres in the tectum lobi optici of the Teleostei.

The third layer is characterised by large cells, which are rounded or sometimes pyriform; they usually give off only one process which is directed outwards, and joins the above-mentioned transverse fibres. These cells differ in their arrangement in the different species, they are spread out in a flat layer in the optic lobe of the Scyllium and Raja. In Rhina and Acanthias they form a group in the central tuberosity that projects into the aqueduct of Sylvius, resembling the arrangement in the Turtle. The small cells which were described in the first layer extend in diminishing numbers into this third layer.

The cerebellum in Scyllium, Rhina, and Acanthias presents a very large ventricle which in Raja is nearly obliterated; the intimate structure resembles that of the Teleostei. There are the four layers, the molecular with Purkinje cells, the granular and the fibrous layers. The latter is connected by the crura cerebelli ad medullam through an inferior lobe with the restiform bodies of the medulla oblongata; there is also an anterior cord passing longitudinally into the optic lobe which represents the crura cerebelli ad cerebrum (Quain). In the granular layer, in addition to the numerous cells forming that layer, there are little masses of fibrillæ inextricably wound together resembling glomeruli on a small scale; in other respects there is nothing peculiar in the structure of the cerebellum.

The molecular and the granular layers are continued on the surface of the restiform bodies in all the species examined, and in Raja nearly as far as the posterior end of the fourth ventricle, but the absence of the Purkinje cells marks a difference of structure.

In the spinal cord the grey substance of the ventral cornu contains numerous large cells arranged in an imbricated manner with their long axes directed obliquely from the ventral to the dorsal surface. Their shape is generally elongated, and they give off several processes. In the cord the ventral cornua are directed horizontally, but towards the posterior end of the medulla oblongata they are gradually depressed toward the ventral surface, and are finally lost in the grey substance on the floor of the fourth ventricle. The dorsal cornua contain numerous nuclei. There are four longitudinal columns in the spinal cord, the ventral longitudinal columns beneath the central canal, the lateral columns at the sides, and the dorsal columns above. Mauthner's fibres are not present in the Plagiostomata, the fibres of the ventral longitudinal columns varying very slightly in size; but perhaps, it may be mentioned here that large fibres, two in number, occupying positions corresponding to those of the Mauthner's fibres in Teleoštei, occur in Ceratodus; they have the peculiarity of possessing several axis cylinders inclosed in a single medullary sheath.

The ventral columns form projecting longitudinal cords in the floor of the fourth ventricle. They can be traced into the ventral side of the posterior commissure which occupies the usual place at the posterior boundary of the third ventricle.

The lateral columns on passing forward diminish greatly in number, the internal fibres are lost in the neighbourhood of the posterior commissure; those that are external seem to join the transverse fibres of the optic lobe, those between the two disappear in the region above the hypoarium, some crossing the crura cerebri which disappear in the same region.

The optic nerves form a chiasma, the lower part of which is formed by the nerves of the two sides intersecting each other in bundles, but in the upper part the remainder cross each other *en masse*. The principal origin of this nerve is the optic lobe, where the outer two-thirds are occupied by its tract; a few fibres, however, are derived from the hypoarium.

The oculo motorii are derived from two ganglia situated on the floor of the aqueduct of Sylvius; they pass nearly straight down to the ventral surface of the medulla oblongata. At this region there is a system of transverse commissures connected with the second layer of the optic lobe, which corresponds to the commissura ansulata of Teleostei (Gottsche).*

The ganglion of origin of the trochlearis was not found, but the fibres decussate at a part between the optic lobe and the cerebellum corresponding to the valve of Vieussens.

The trifacial is derived from three roots, one of which comes forward from the posterior part of the medulla oblongata, where it can be traced into the lateral columns. The other comes backward through the tuberosity of the trifacial by the side of the medulla; these two cross each other at their entrance into the nerve, the third comes from a group of cells in the grey substance of the floor of the fourth ventricle.

The abducens can be traced from the ventral surface of the medulla oblongata at about its centre into the lower edge of the ventral longitudinal columns.

The facial can be traced into a small bundle of fibres which passes backward into the spinal cord in the substantia gelatinosa centralis just above the central canal.

* *Loc. cit.*, p. 439.

About this region there is a system of arched commissural fibres, the fibræ arcuatæ. They seem to be connected with the crura cerebelli and medullam. They occur not only through the external part of the ventral surface, but also through the central portions.

The acusticus and glossopharyngeal arise from the grey substance on the floor of the fourth ventricle.

The vagus arises from a series of rounded tuberosities situated on the side of the floor of the fourth ventricle; each root arises from a separate tuberosity.

The spinal nerves arise by dorsal and ventral roots; the latter from the ventral horn of grey substance. The former pass obliquely into the interior of the cord and there divide into two bundles; one bundle from the anterior part of the root is directed backward, the other bundle from the posterior part of the root is directed forward. They pass over the next nerve both in front and behind, and join the lateral columns of the cord. This arrangement was first described by Stieda.*

January 14, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Action of Sunlight on Micro-organisms, &c., with a Demonstration of the Influence of Diffused Light." By ARTHUR DOWNES, M.D. Communicated by Professor MARSHALL, F.R.S. Received December 9, 1885.

Eight years ago, conjointly with my friend Mr. Blunt, I communicated to the Royal Society an account of an experimental inquiry into the action of sunlight on the micro-organisms of putrefaction and decay.†

We adduced evidence, conclusive in our opinion, that the solar rays were very hostile to these lowly forms of life; so much so that under favourable conditions bright sunlight, sufficiently prolonged, would altogether prevent their appearance in fluids which, under

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* "Zeitsch. f. Wiss. Zoologie," Bd. 23, 1873.

† "Proc. Roy. Soc.," vol. 26, p. 488, and vol. 28, p. 199.

similar conditions of temperature and the like, but screened from light, swarmed in a very few days with countless saprophytes.

By means of suitable absorptive media, we learned that the most active rays were those of the more refrangible end of the spectrum.

Seeking an explanation of the facts thus observed, we proceeded in the first instance by analogy.

We found that light had an oxidising action on many organic substances of comparatively simple composition, and we demonstrated that, in the presence of free oxygen, the molecule of oxalic acid might be speedily and entirely resolved into water and carbonic acid by the action of light, more especially by those rays to which I have already referred.

Proceeding to more complex substances, we applied the same method to one of those singular bodies, the so-called soluble or indirect ferments.

In less than a month the properties, and, inferentially, the substance, of the invertive diastase of yeast were destroyed by light.

Once more we found that we had to deal with an oxidation. Finally, our inference that the action of sunlight on the organisms of our cultures would likewise prove to be an oxidation was confirmed by direct experiments, in which the effect varied in proportion to the amount of free oxygen present.

As yet no one has repeated these investigations in their entirety, but sufficient confirmatory evidence has accumulated to justify me, I think, in briefly placing the case before the Society as it now stands, with one or two additional observations of my own, and to afford me an opportunity of replying to one or two points of criticism.

The earliest corroboration of our work came out on the reading of our first memoir on this subject.

Mr. Warington had that same evening, in a paper to the Chemical Society, notified, but was unable to explain, the inhibitory action of light on the process of nitrification. Our experiments at once suggested to Dr. Gilbert the interpretation, since confirmed by several observers,* that light was inimical to the nitrifying ferment.

Gladstone and Tribe ("Journ. Chem. Soc.," August, 1883) found that light was detrimental to the development of fungoid growths in solutions of cane-sugar exposed to atmospheric air.† Tyndall re-

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* Soyka, in "Zeit. f. Biol.," 1878; Schloesing and Muntz, "Journ. of Chem. Soc." (Abstr.), April, 1880.

† It is right to state that van Tieghem in his investigations on the organisms appearing in olive oil ("Bull. Soc. Bot.," xxviii, p. 186), found that *Penicillium glaucum* developed in oil at the most illuminated spots. I have not been able to see the original paper, and am therefore not acquainted with the conditions of the experiment, especially as regards the nature of the illumination, and the access of free oxygen.

ported to the British Association in 1881 the results which he had obtained in exposing flasks of animal and vegetable infusions to the influence of an Alpine sun. Corresponding flasks shaded from the light became turbid in twenty-four hours, "while thrice this time left the exposed ones without sensible damage to their transparency." He satisfied himself that this was not due to differences of temperature. The amount of insolation was insufficient, however, to permanently sterilise the cultures after removal to a warm kitchen.

Confirmatory evidence also, invested with a special value by the author's great experience of saprophytic life, has more recently been adduced by Professor E. Duclaux.* With the usual precautions he introduced into a flat-bottomed Pasteur flask a drop of pure culture of the organism to be studied, and dried it under a desiccator. After the desired period of exposure to the sun, the flask was charged with sterilised nutrient liquid, *of a kind specially suitable to the development of the particular organism*, and placed at a favourable temperature in an incubator. Corresponding flasks were kept in the dark.

He finds that the general rule, that the spores of these organisms resist adverse influences better than their vegetative forms, holds good in regard to the effects of light. This accords with our own observations on the insolation of germs in water.† M. Duclaux, however, reserves for a future memoir his conclusions on the insolation of the *micrococci*—among which the formation of spores is not certainly known.

The very hardy spores of the *Bacillus*, to which he has given the name of *Tyrothrix filiformis*,‡ were destroyed by thirty-five days' exposure to an autumnal sun. *T. geniculatus* was more resistant, but showed signs of commencing enfeeblement. *T. scaber* was only retarded in development by insolation during the month of August, 1884, but a further exposure to the end of September—a not very fine month—sterilised two flasks out of four.

Similar spores had survived for three years in the dark. Like Professor Tyndall, he was satisfied that these results were not effects of temperature; his insolated flasks scarcely reached any point higher than their fellows kept in darkness in an incubator.

He concluded, also, from further experiments, that the injurious influence of light here manifested was probably an affair of oxidation.

M. Duclaux very rightly insists on the importance of careful adaptation of the nutrient liquids to the organisms operated upon, observing that, otherwise, spores might be regarded as dead which had only, perhaps, been enfeebled. "C'est là l'objection," he continues,

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* "Ann. de Chim. et de Phys." 6e ser., t. v, Mai, 1885.

† "Proc. Roy. Soc.," vol. 28, pp. 203–4.

‡ "Études sur la lait," "Ann. de l'Instit. Agronomique," 1879–80.

"qu'on peut adresser aux expériences très bien conduites, du reste, et très intéressantes de M. A. Downes." ("Proc. Roy. Soc.," vol. 26, p. 488, 1877.)

As regards the earlier of our memoirs, to which M. Duclaux refers, this criticism is just. At that date knowledge of revivification of germs in different media was neither so generally diffused nor so precise as it now is. This advance we owe not least to Professor Duclaux himself. Accordingly, in our first experiments we regarded non-appearance of life in our insolated tubes of Pasteur solution, or of urine, as proof of destruction of the organisms which they had originally contained. In our second and more complete memoir, however, we reserved our opinion on this point.

But it was an essential principle of the method on which we worked, and the key to our success, that our nutrient fluids should be sufficiently resistant to bacterial growth to hinder the development of organisms, through the night, or during cloudy days, from outrunning the inhibitory effects of insolation. It would probably, for example, not often be possible to secure in England the results which Tyndall obtained on the Alps with apparently considerable bulks of very putrescible materials. Pasteur solution and the like are at their best but limited media of nutrition;[*] yet under special circumstances, as, for example, in the demonstration of the action of diffused daylight given below, it is necessary to largely increase their resistance to decomposition.

Moreover, the question whether the germs in our solutions were or were not actually dead, does not affect the truth of our induction. I cannot put this more pithily than has Professor Duclaux himself, in a very courteous communication with which he has favoured me. "You have clearly shown," he says, "that an insolated germ is a sick, sometimes very sick, germ; death is but a step further."[†]

It was an *à priori* probability that micro-organisms should vary considerably in their powers of resistance to the oxidising influences of light. In our previous papers, indeed, we gave examples of this in the frequent survival over *Bacteria* of some less sensitive form of *Saccharomyces* or *Mucedo*.

I have lately met with an instance which may be worth recording, as it enabled me to isolate a *Bacterium* of which I can find no previous description. In each of a number of thickish glass tubes I had sealed up 3 c.c. of distilled water, together with a small bulb containing an

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[*] For prolonged insolation I have for another reason quite abandoned their use. They are liable to take on a brown coloration in sunshine, and I have had, in consequence, to abandon a laborious series of experiments.

[†] M. Duclaux tells me that he has also confirmed our observation that the diastases are destroyed by sunlight. He operated on the *diastase présure*, the coagulating principle of rennet.

equal quantity of carefully sterilised peptone solution (double strength). Some of these tubes were insolated on an outside shelf facing south; others were incased in laminated lead alongside.

After the desired period of insolation, the bulbs were broken by a jerk, and the tubes, now containing 6 c.c. of peptone solution of ordinary strength (2 per cent.), were removed to a warm cupboard kept at about 20° C. By a week's exposure, May 29—June 5, bacterial development was already retarded (sixty hours as compared with twenty-four). After insolation for nearly four weeks, May 29—June 24, ordinary bacterial development appeared in two incased tubes in thirty-six hours. In two insolated tubes, at the same date, nothing was seen till the fourth day, when small flakes began to form, and by August 3 had settled into a dirty-white collection, leaving the supernatant liquid clear, presenting a notable contrast to the uniform turbidity of the incased.

These flakes were found to consist of compact spherical or cylindrical nodulated masses of zooglœa. They closely resembled in general appearance the *Ascococcus Billrothii* or *Ascobacteria* of v. Tieghem, but I was utterly unable to demonstrate the gelatinous envelopment from which those organisms take their name.

On teasing out a portion the colony was found to consist of closely felted small rods, motile when freed from the mass, about $0{\cdot}6\mu$ diameter, and $2{\cdot}0$—$3{\cdot}0\mu$ long.

On September 28, after four months' exposure, the remaining tubes, three insolated and one incased, were taken in. Nine days elapsed before the latter became hazy with *Bacteria*. In eleven days one of the insolated contained flakes, such as I have above described. In a day or two later similar flakes formed in another of these tubes. The third insolated tube subsequently broke down with a scanty development of *Bacteria*, not distinguishable from the kinds found in the incased.*

It is evident that the zooglœa-lump-forming Bacterium was especially resistant to sunlight, and so became isolated in almost pure cultures in four-fifths of the tubes insolated for a month and upwards.

I wish now to direct attention to the fact that the tubes of the experiment which I have just described, were exposed repeatedly to considerable elevations of temperature. The meteorology of Greenwich may be taken as sufficiently identical with that of Chelmsford,

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* This experiment—an insolation of germs in water only—might be regarded, and possibly rightly so, as confirmatory of what we have previously written on the resistance of germinal matter in a fluid devoid of nutrient material. But it should be remembered that the supply of free oxygen was necessarily limited in these sealed tubes, being rather less than 5 c.c. in each, and I am unable at present to say whether this amount would be sufficient to oxidise the germs ordinarily present in 3 c.c. of distilled water.

where the investigation was made.* At the Royal Observatory the means of the maxima in the sun's rays were :—

June........	126·1° F.	(52·3° C.).
July........	143·0 F.	(61·6 C.).
August......	129·2 F.	(54·0 C.).

It cannot be doubted that these tubes were often exposed to a temperature of 140° F. (60° C.), and on at least one occasion (July 27) to 160° F. (71° C.).

The incased tubes had for radiant heat a somewhat greater absorptive power than the bare glass of the insolated. For temperatures below 100° F. (38° C.) this difference was comparatively slight; at 100° F. it was 4·5° F. (2·5° C.).

It is certain, therefore, that any deleterious influence of heat should tell more on the incased than on the insolated. Yet at the end of four months *Bacteria* appeared, retarded in development, it is true, but still morphologically identical with the forms originally found in similar solutions.

I lay stress on these facts, because an Australian observer has declared† that we, and Professor Tyndall with us, have mistaken effects of heat for supposed effects of actinism.‡

But Dr. Jamieson's paper in abstract has gone the usual round of German year-books and periscopic notes of English journals, until an impression has arisen that there is no satisfactory evidence of injurious influence of light on micro-organisms. I trust, therefore, to be permitted a few words in reply.

Dr. Jamieson insolated Cohn's solution in phials, and found that in a short space of time bacterial development might thereby be entirely prevented. But in some of his experiments he thought that he succeeded better in hot weather than in cool, and he failed to produce any effect in diffused light. He asked himself, therefore, whether the results he had noted might not have been, after all, due to heat.

This was a very legitimate question, but, instead of solving it by direct observation, he unfortunately recalled to mind experiments§ in which *B. termo* had apparently been killed by seven days' exposure to 45° C. (113° F.), by fourteen hours at 47° C. (116·6° F.), by three or

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* Greenwich.......... *Lat.* 51·28 N. *Long.* 0·00.
Chelmsford......... „ 51·44 N. „ 0·28 E.

† Royal Society of Victoria. June, 1882.

‡ We cannot dispense with some word such as this to connote energy not necessarily coincident with effects either of solar heat or luminosity. In using it above, I go a little further than Professor Tyndall, who has not, I believe, yet given any opinion as to the form of radiant energy, except that it is not *heat*, which he found to hinder bacterial development.

§ "Eidam. Beit. zur Biol. der Pfl.," Heft iii, p. 208.

four hours at 50—52° C. (122—125·6° F.), and by one hour at 60° C. (140° F.). He argued that some of these temperatures were commonly attained in the sun's rays in Australia, and even in England; he thought that 125° F. (51·6° C.) would occasionally be experienced for a few hours, and he concluded that his, our own, and Tyndall's results were all effects of temperature.

The argument is fallacious. It is true that some organisms in certain stages of their development may be destroyed by lower degrees of heat than is commonly supposed. Duclaux has given an instance in which forty-eight hours at 38° C. (100·4° F.) was fatal to some very old yeast globules.

But even of these it is true only in their vegetative forms; their spores (and the spore-form is doubtless that in which the micro-organisms originally existed in our nutrient liquids) resist elevations of heat far surpassing anything noted above. Were it otherwise bacterial life would probably soon cease to be. It is obviously incorrect to argue that, because some organisms in some phase of their existence are destroyed by moderate heat, all organisms, in all phases and under all conditions, are so too, and any inference drawn from such reasoning must be rejected.

Moreover, I have already shown that the laminated lead used in our experiments absorbed radiant heat in greater degree than the bare glass, and consequently that our incased tubes would be more affected by solar heat than our insolated. And I need only refer to our previous demonstration, that the greatest effect on micro-organisms is produced by those rays which occupy the cooler portion of the spectrum.

Dr. Jamieson failed with diffused light. His failure was due to his method of experimenting.*

I have already said that an essential element of success is to apportion the natural resistance of the cultivation liquids to the amount of light available. Bacterial development once started usually outruns even direct sunlight, both by increasing the opacity of the fluid, and by quickly reducing the amount of oxygen. Naturally diffused light would be far slower in action than the direct solar ray, and we must select either very cool weather for the experiment, or must choose solutions of considerable resistance.

Keeping this principle in view, I have found it easy to show that diffused light possesses properties differing only in degree from those which we have demonstrated in regard to direct sunlight. I have made a number of experiments in which ordinary thin test-tubes plugged with cotton-wool were placed in a box (20·5 cm. cube) lined

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* Moreover, he seems to have placed his bottles inside a window. The absorptive power of glass has always prevented me from succeeding in such circumstances.

with white paper, having one side open, and tilted at such an angle as to receive diffused light straight from the white clouds of the northern sky. By no possibility could direct sunlight find an entrance. In the box were placed maximum and minimum thermometers, each pair with bulbs respectively incased or left bare.

In September, 1883, using Cohn's solution five times the ordinary strength, in five days, four out of six incased tubes were noted as "turbid," and the other two as "hazy," with bacteria. The exposed tubes were recorded "beautifully clear." At the end of two days more the latter were still clear, but in each were specks of mycelium. The survival of mycelial growth over bacterial has already been alluded to in the present paper, and is seen in Dr. Jamieson's own experiments.

But mycelial growth itself may be hindered by diffused light.

In March, 1884, a *slightly acid* Cohn's solution, two and a half times ordinary strength, being specially selected, I found that at the end of ten days $\frac{5}{6}$ of the incased tubes contained mycelial specks, the six exposed tubes being perfectly free. At the end of fifteen days my notes were:—"Disks of mycelium plug $\frac{5}{6}$ of incased, $\frac{2}{6}$ of exposed; small tufts of mycelium in the remaining incased tube, and in one of the exposed, three remaining exposed quite clear. The difference in appearance of the two sets is remarkable."

The means of the thermometrical readings during two periods were:—

		Encased.	Exposed.
Sept. 1883 ..	Max.	63·5° F. (17·5° C.)	64·0° F. (17·8° C.).
	Mim.	44·7 F. (7·0 C.)	45·0 F. (7·2 C.).
Oct. " ..	Max.	56·0 F. (13·3 C.)	57·8 F. (14·3 C.).
	Min.	35·6 F. (2·0 C.)	36·8 F. (2·6 C.).

As the incased tubes were the better absorbers, so are they now seen to be the better radiators, and conditions of temperature were accordingly slightly more adverse to development of organisms in them as compared with the exposed.

I now conclude this paper with a reference to the researches of Herr Pringsheim on chlorophyll.* I refer to them with especial gratification, as evidence of the truth of a generalisation which I had ventured to draw from our experiments.

The micro-organisms of our solutions may be regarded as examples of protoplasm in its simplest forms, but there are no grounds for supposing that this "life-stuff" should be subject to hyperoxidation by light only when it exists in a *Bacterium*, or a mycelial thread. On the contrary we have probably to deal with a general law, and, without protective developments of cell wall, or of colouring matter which

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* "M. B. Akad. Wiss.," Berlin, 1879.

should filter out injurious rays, &c., living organisms could hardly endure the solar light.

Pringsheim operated on chlorophyll tissues. By means of a lens and a heliostat he concentrated upon them sunlight, from which by suitable media he had sifted out the heat rays. In a few minutes the green colouring matter was destroyed, the protoplasmic circulation arrested, the protoplasm disorganised, and the cell flaccid and inert. He found, as we had found, that the more refrangible rays were the most powerful, and he, too, concluded that he was dealing with an oxidation, for in an atmosphere of hydrogen or of carbonic acid these destructive results no longer ensued.

The experiments of Siemens* and Déhérain† also demonstrate both the destructive influence of the electric light on vegetation and the protective effect of a glass screen.

[NOTE.—According to an abstract in "Journal of Science," 3rd ser., vol. vii, p. 594, M. Duclaux has since published the results of his observations on six species of *micrococci*, apparently of pathogenic kind.‡

Forty days of insolation (May 4—June 13) proved sufficient to kill and less to attenuate these germs in the moist state. In a desiccated condition eight days (May 26—June 3) proved fatal; in July none resisted three days' exposure at a south window which received the sun only from nine to one o'clock each day, and where the temperature did not exceed 102° F. (39° C.). Fifteen days of July sun destroyed the micrococci in the moist state. He had not in these experiments eliminated any partial influence of temperature. January 4, 1886.—A. H. D.]

II. "Notes upon the Straining of Ships caused by Rolling." By FRANCIS ELGAR, LL.D., F.R.S.E., Professor of Naval Architecture and Marine Engineering in the University of Glasgow. Communicated by Sir E. J. REED, F.R.S. Received December 28, 1885.

(Abstract.)

It does not appear that any serious attempt has yet been made to investigate the amounts, or even the nature, of the principal straining

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* "Rep. Brit. Assoc.," 1881.

† "Journ. Chem. Soc." (Abst.), Jan., 1883. Considerations of time and space prevent me from noticing many other observations of interest in connexion with this subject; *e.g.*, of Engelmann on *Pelomyxa* ("Arch. f. Phys.," xix, 1879) and on *B. photometricum* ("J. Roy. Micr. Soc.," Abst., 1882–2), or of Stahl, "On the Arrangement of Chlorophyll Bodies in Plant-Cells" ("Bot. Zeit.," 1880).

‡ "Comptes rendus," 1885.

actions which the rolling of a ship brings into play, or of the effect of those straining actions upon the material of which the hull is composed. Various writers, from Bouguer in 1746, down to Professor Macquorne Rankine in 1866, and Sir E. J. Reed in 1871, have discussed the straining actions that are caused by longitudinal racking and bending when a vessel is floating in statical equilibrium. Sir E. J. Reed elaborately investigated the subject in a paper contained in the "Philosophical Transactions of the Royal Society" for 1871, and gave examples of the amounts and distribution of the stresses caused by such straining actions in several typical ships of Her Majesty's Navy. Mr. W. John supplemented this by a paper on the strength of iron ships, read before the Institution of Naval Architects in 1874, in which similar results were given for various classes of vessels in the mercantile marine.

The later investigations of these longitudinal straining actions apply not only to the case of a ship floating in equilibrium in still water, but also to cases in which she is (1) in instantaneous statical equilibrium across the crest of a wave; and (2) in instantaneous statical equilibrium across the hollow of a wave—the wave-length being equal to the length of the ship.

Cases frequently occur which show that the maximum stresses of the material of a ship's hull are not in proportion to the results obtained by the ordinary calculations; and that certain deductions that have been drawn from those results are by no means sound. For instance, it is said to follow from the analogy between the longitudinal bending action upon a ship afloat and that upon a loaded girder, that there is little or no stress exerted upon that portion of a ship's plating which is in the vicinity of the neutral axis for the upright position; and the inference has been drawn that, subject to the consideration of the sides being occasionally brought, in some degree, into the positions of flanges of a girder by large inclinations, the thickness of the material may be decreased with advantage near the neutral axis. Now it cannot be shown that the plating which is in the vicinity of the neutral axis when the ship is upright, is ever brought into such a position by the rolling of a vessel as to be much affected by mere longitudinal bending.

The reason commonly given for not decreasing considerably the thickness of side plating in the vicinity of the upright neutral axis, viz., that when a ship is in an inclined position, this plating may be so placed as to offer the greatest resistance to longitudinal bending is seen, if the matter be properly considered, to be obviously unsatisfactory.

Other propositions respecting the relative distribution of stress in various parts of the structure have been deduced from considerations and assumptions upon which the ordinary calculations of longitudinal strength are based; and rules have, in consequence, been proposed for regulating the strength of the principal component parts of ships' hulls. It is only necessary here to say, that many of these deductions, like the one already noticed, are unsound, and are not consistent with the effects that may be observed of straining action at sea.

A considerable experience at sea, where the writer has closely observed the effects of straining action caused by twisting moments, and a further experience in investigating the stresses to which the various portions of ships' hulls are subjected according to the theories referred to, and in comparing the results so obtained with the visible evidences of straining action, have convinced him that the stresses caused by twisting moments are much greater than is generally supposed, and that no rules for regulating the strength of ships can be satisfactory if based upon hypotheses that exclude all practical consideration of twisting moments.

The straining action which will be considered in this paper is that caused by the twisting moments which operate when a ship rolls from side to side; and which are caused by differences in the longitudinal distribution of the moments of the forces that cause rotation, and those which resist rotation.

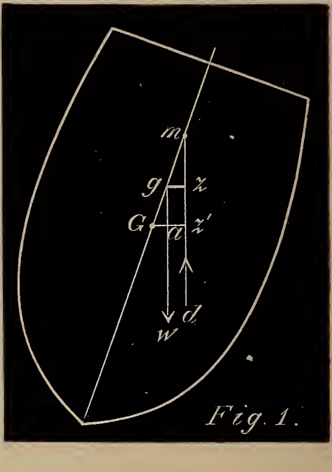

Fig. 1.

Let a unit of length included between two transverse vertical sections be taken at any point in a ship's length, and let fig. 1 be the section of the ship at that point. The section may be taken as uniform over this short length. The energy of rotation of this unit of length will be $\frac{\omega^2}{2g} wk^2$; where ω is the angular velocity in the upright position, w is the weight of the unit of length and its contents, and $\frac{w}{g}k^2$ is the moment of inertia of the unit of length about the axis of rotation.

In order to form an equation of energy and work, we require to assume an axis of rotation for the ship; and the assumption here made is, that the axis of rotation is a principal longitudinal axis through the centre of gravity G of the whole ship and her contents. A ship's axis of rotation is not, in reality, fixed; but that may for the present be disregarded. The important point in connexion with it is that, whatever position the instantaneous axis may occupy at any given moment, it is the axis about which each unit of length of the ship is then rotating, with the same angular velocity. This condition follows from the rigidity of the ship, or rather from the structure being so nearly rigid that any motion of one part relatively to another, about the axis of rotation, is so small that it may be neglected.

When the unit of length shown in section in fig. 1 is inclined to an angle θ from the upright, the principal forces which act upon it are—first, the weight w of every part of the ship and her contents that is contained in this length, acting vertically downwards through its centre of gravity g; and, secondly, the weight of the volume of displacement d for the unit of length under consideration, acting vertically upwards in a line, dm, through its centre. These forces are equivalent to the couple $d \times gz$, and a vertical force at g equal to $w-d$.

Let G be the point in which the axis of rotation through the centre of gravity of the ship intersects the section in fig. 1. Then the moment which resists the inclination of the section at any angle θ will be the resultant of the two couples $d \times gz$ and $-(w-d)\,\mathrm{G}a$. Let $w-d=\delta$. The work done in inclining the unit of length in fig. 1 to the angle of inclination Θ will be $d\int_0^\Theta gz\,d\theta-\delta\int_0^\Theta \mathrm{G}a\,d\theta$. If a curve be constructed with the length of the ship for an abscissa line, and the values of $d\int_0^\Theta gz\,d\theta-\delta\int_0^\Theta \mathrm{G}a\,d\theta$ for ordinates—these values being set off at points in the length to which the sections for which they are calculated correspond—it will represent the longitudinal distribution of the work done in opposition to the action of the righting moments. The base line in fig. 2 represents the length of the ship. Suppose the first ordinate to be at the plane of division for which the section of the ship is as shown at fig. 1, we then require to determine the value of $d\int_0^\Theta gz\,d\theta-\delta\int_0^\Theta \mathrm{G}a\,d\theta$, at this section, which may be readily done.

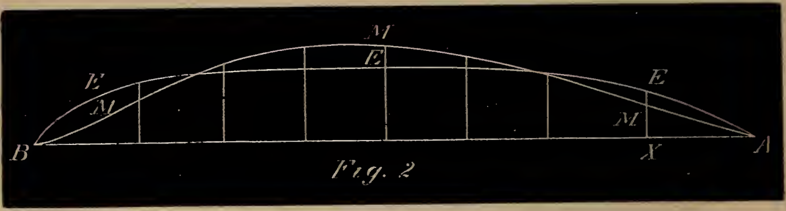

Fig. 2

XM in fig. 2 represents $d\int_0^{\Theta} gx\,d\theta - \delta\int_0^{\Theta} \mathrm{G}a\,d\theta$, or the energy expended in inclining an unit of length at X against the resistance of the righting moment to the angle Θ. If similar values be determined for units of length at each of the other ordinates, and a curve MMM be drawn through the points so obtained, MMM will give the longitudinal distribution of the work done against the resistance of the righting moments, or of what was called by Canon Moseley the dynamical stability.

Let EEE in fig. 2 be a curve which shows in a similar manner the longitudinal distribution of the energy of rotation. XE will be the value of $\frac{\omega^2}{2g}wk^2$ for the unit of length at that ordinate; and the other ordinates of the curve will be the values of $\frac{\omega^2}{2g}wk^2$ at the corresponding points in the ship's length.

The difference between the energy of rotation of any portion of the vessel's length AX and the work expended in inclining that portion, in opposition to the moments of the forces exerted by its weight and the fluid pressures upon it, is equal to the area MEA in fig. 2. This area measures the excess of energy of rotation in the volume between A and X, which is communicated through the hull of the ship to some other part of her length at which the energy of rotation per unit of length is less than the work required per unit of length to incline it to the angle Θ. This excess of energy is transmitted, by means of a twisting moment upon the hull, to the part of the ship's length where it is utilised in overcoming resistance.

A graphical representation of the longitudinal distribution of the mean twisting moments which act upon the hull when the vessel is rolling to an angle Θ on each side of the upright position, may be made by means of a curve so that any ordinate of the curve will give the mean value of the twisting moment upon a section at the point for which the ordinate is drawn.

The values thus obtained for the twisting moments will be mean values only. The variation of twisting moment at any section of the hull during a roll, and consequently the maximum twisting moment, may be readily determined; and a method of doing this is described at length in the paper.

The results given by the investigations described apply only to ships rolling from side to side in still water, assuming that the water offers no resistance to rolling motion. It is obvious, however, that the twisting moments thus obtained must often be greatly exceeded when a vessel is rolling and pitching while lying or moving across a series of long ocean waves. In these circumstances the bow or stern frequently has so little immersion that the righting moment acting upon a portion of one end is momentarily very small, and almost the whole of the energy of rotation is applied to the production of twisting moments. The resistance of the water would also often increase the twisting moments.

It now remains to be seen what can be done in the way of determining the stresses upon the hull which are caused by the twisting moments. We can learn something of the nature and distribution of those stresses; but, at present, their amounts cannot be calculated with any reliable approach to accuracy. Experiments are required upon the torsion of thin shells of various prismatic forms in order to furnish the requisite data for dealing with so complicated a case as that of a ship's hull. The difficulty of obtaining such data is very great; but pending the time when it is to be hoped this want will be supplied, it may be useful to draw attention to some of the general considerations which affect the twisting moments and the distribution of the twisting strains and stresses over a ship's hull; and to the bearing which these have upon the important practical problems that relate to the structural strength of ships.

The best data available for guidance in judging of the distribution of strain and stress due to twisting over the structure of a ship are to be found in M. de St. Venant's investigations of the torsion of prisms.* These investigations assist us to form a general idea of the manner in which a ship's structure may be strained by twisting; and they also indicate the nature of the experiments that are necessary to furnish data for more exact investigations. The mean amount of the twisting moment upon a ship's hull at any transverse plane of division, and also the maximum twisting moment, may be obtained by the method described in the present paper. The torsional strength of the hull at that section will depend (1) upon the thin iron or steel shell of which the structure consists, being stiffened internally so as to effectually resist change of form; and (2) upon the ratio which the strength of a section of such form, when so stiffened, bears to that of a

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* "Mémoires présentés par divers Savants à l'Académie des Sciences de l'Institut Impérial de France," tome quatorzième, 1856. "Mémoire sur la Torsion de Prismes, &c." Par M. de Saint-Venant," pp. 233–560. Also Thomson and Tait's "Natural Philosophy," vol. i, Part II, secs. 699–710.

hollow circular cylinder of the same thickness and the same sectional area. Experiments upon the torsional strength of hollow prisms of various forms, having the same sectional area and thickness of shell, can alone determine the latter point; while, at the same time, such experiments would serve the further purpose of showing how the condition above referred to—that the shell shall be stiffened internally so as to effectually resist change of form—can best be complied with.

The distribution of the torsional stresses over the transverse section of a ship's hull is obviously different from the distribution of the stresses due to longitudinal bending. The parts subjected to greatest stress by twisting are those which are near to the centre of gravity of the transverse section; and they are the side plating near the neutral axis of longitudinal bending in the upright position and the middle portions of the plating of the decks. Those parts of the hull which are usually made the strongest, viz., the strakes of side and bottom plating that are farthest from the neutral axis, and the upper deck stringer plate, are those which are least affected by twisting. It is probably owing, in great measure, to the straining action caused by twisting, that experience has proved it to be necessary to make the outside plating of a ship of nearly uniform thickness over the whole section; and it cannot be because of the reason sometimes given, that the plating in the vicinity of the neutral axis when a ship is upright is often brought by rolling into positions in which it is greatly strained by longitudinal bending.

The importance of many of the structural arrangements of ships that are described in the present paper, which practical experience has shown to be necessary, may be understood from these considerations; and it will also be seen that no rules for regulating the strength of ships are likely to be satisfactory if based, as is often done, upon the hypothesis that the straining actions caused by longitudinal bending are so much more important than all others that it is sufficient to regard them alone.

III. "Proteid Substances in Latex." By J. R. Green, B.Sc., B.A., Demonstrator of Physiology in the University of Cambridge. Communicated by W. T. Thiselton Dyer, C.M.G., F.R.S., Director of Royal Gardens, Kew. Received January 4, 1886.

In the study of the metabolism of plants, the nature of the products resulting therefrom, and the different forms assumed by these bodies during the changes involved, attention has been chiefly directed to the seed. No doubt special facilities for investigation are afforded thereby, for the different bodies can be detected there by the aid of the microscope, and their behaviour under the action of different reagents watched. Hence valuable results have been arrived at, and our knowledge of vegetable metabolism has made considerable advance. By the investigations of Hoppe-Seyler,* Weyl,† and Zoller,‡ the similarity of vegetable proteids to those occurring in animals was pointed out, members of the globulin family at least being shown to exist. Later Vines, by an exhaustive examination, both macroscopic and microscopic, of a very large number of seeds, has added greatly to our knowledge of these bodies, proving that besides globulins, a form of albumose, albuminates, and coagulated proteids are to be isolated, and showing the actual conditions and proportions in which these are present in the seeds.§

It is evident, however, that our knowledge of the seed, even if made complete, will not give us all the information we require concerning the nitrogenous metabolism of the plant. The condition of the proteid matter at a time antecedent to its appearance as reserve material must be considered as equal in importance and in interest. The round of changes going on normally in the leaves and the soft tissues of the stem has hitherto remained unknown, nor had we any knowledge of the condition and characters of the proteid bodies circulating in the plant, and met with in the latex and in the soft green parts until recently, when Martin|| published an account of his investigations into the nature and action of the ferment present in the Papaw plant (*Carica papaya*) and has therein described certain proteids which he has found to be present in the dried milk of the fruit of the plant. These he says are four in number, two belonging to the group of the albumoses, a globulin and an albumin. To the albumoses, which are the most plentiful in amount, he gives the names of α and β phytalbumose.

During the summer of 1884 I was enabled, through the kindness of Mr. Thiselton Dyer, Assistant Director of the Royal Gardens, Kew, to make some investigations into the composition of the latex of several caoutchouc-yielding plants belonging to the natural orders *Apocyneæ* and *Sapotaceæ*.¶ In most cases the latex was a very complex fluid, containing, besides proteids and carbohydrates, considerable

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* "Med.-Chem. Unters.," 1867.

† "Zeitschr. f. Physiol.-Chem.," i, 1877; "Ber. d. deut. Chem. Ges.," xiii, 4, 1880.

‡ "Ber. d. deut. Chem. Ges.," xiii, 10, 1880.

§ "Journal of Physiology," vol. iii, No. 2.

|| *Ibid.*, vol. v, No. 4, and vol. vi, No. 6, p. 336.

¶ [These samples, thirty-four in number, were collected for Dr. Vines with very great pains and trouble by Mr. D. F. A. Hervey, Resident Councillor, Malacca.—W. T. T. D.]

quantities of caoutchouc, resinous matters, &c., the latter being very variable in amount, and absent from some samples. The material was collected in the Malay Peninsula from the plants, and a little alcohol having been added as a preservative, was sent to England in sealed bottles. On its arrival at the laboratory, some of the bottles had their contents hardly at all changed except that the large amount of caoutchouc contained in the fluid had undergone the process technically known as coagulation, and was floating in a milky liquid. Others had become quite spoiled in transit, the latex having deposited a quantity of amorphous matter, which gave a xanthoproteic reaction, and seemed to be coagulated proteid. In the *débris* besides this, there appeared under the microscope, a number of small droplets of caoutchouc, a few sphæro-crystals, some spicular crystals, and some flat plates of rhomboidal form.

Examination of these last as to proteids not being practicable, attention was given to the uninjured samples, which differed in no way from each other. The particular experiments, whose results are detailed below, were made upon the latex of a plant, the Malay name of which was given as "Gegrip puteh."*

The mass of caoutchouc floating in the fluid was allowed to drain dry, and was then with difficulty cut up into small pieces and macerated some in water and some in salt solutions. Soaking for several days failed to extract anything of a proteid nature from it. Attention, therefore, was directed to the liquid remaining after its separation. This, as said above, was milky in appearance, of a faintly yellow colour, aromatic smell, and neutral reaction. Under the microscope it was at once apparent that the milky appearance was due to minute droplets of caoutchouc which had not separated out with the bulk. There was nothing granular or amorphous visible, showing that the proteids had not been precipitated by the alcohol used. To free the latex from the caoutchouc, filtration under vacuum pressure through a porous pot was necessary, when the droplets formed a film round the earthenware, and as the liquid was gradually removed they fused together, giving rise to a thin sheet of india-rubber. The fluid passed through the pot clear and in a condition fit for examination.

In this liquid so prepared a very curious proteid body was found to exist, differing in important particulars from any hitherto described as occurring in plants.†

Its presence was readily shown by the

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* [Yielded by an Apocynaceous plant, *Parameria glandulifera*. The selection of this particular sample, which happened to stand first in a series of thirty-four, was a little unfortunate, as it is not a very characteristic caoutchouc-yielding species.—W. T. T. D.]

† In a communication made to the Cambridge Philosophical Society I have already given a brief account of its properties and reactions ("Proc. Camb. Phil. Soc.," vol. v, Part III, p. 183, October term, 1884).

xanthoproteic reaction, the orange colour on the addition of the ammonia being very marked. On warming the liquid gradually to boiling point there was no coagulation or opalescence, and on adding nitric acid there was no precipitate. Hence the body does not belong to the groups of albumins or globulins. On dropping the boiled liquid into large excess of alcohol, a precipitate was slowly formed, which after standing some hours settled to the bottom of the vessel. These reactions suggested a member of the class of peptones, and as these proteids, though thrown down from their watery solutions by alcohol, are not changed by contact with the spirit, the precipitate was allowed to remain as it settled for about three weeks. At the expiration of that time the alcohol was decanted off, and the precipitate dried. It was then found to be freely soluble in distilled water, and to give, as the original latex did, a well-marked xanthoproteic reaction.

A further resemblance between this body and the group of peptones was revealed by its behaviour when submitted to dialysis. A quantity of the solution of the precipitate that had been standing under alcohol was put into a dialyser and suspended in twice its volume of distilled water. After two days the fluid outside the dialyser was examined. It gave readily the xanthoproteic reaction, and on addition of a large volume of alcohol a marked opalescence appeared, which on standing became a precipitate. Hence this proteid body appeared to have considerable resemblance to the group of peptones, if not to be a member of it.

Further examination, however, brought to light some points that indicated a relation to the albumoses also. Saturation of the solution of the alcohol precipitate by solid $MgSO_4$ gave a copious precipitate, which was redissolved on adding water. The liquid outside the dialyser in the last experiment behaved similarly. The precipitation took place with equal readiness whether the reaction of the solution were neutral or slightly either alkaline or acid. Till recently the precipitation of a proteid by saturation of its solution with a neutral salt was held to be a mark of a globulin, but this reaction cannot now be held to be sufficient of itself to prove this. Halliburton has shown* that it is possible to precipitate serum albumin by such a process, the salt necessary being the double sulphate of magnesium and sodium. Heynsius has stated† that peptone itself may be thrown down from its solution by ammonium sulphate in similar quantity; a statement that is endorsed by Martin.‡ Pollitzer§ denies this, as far as true

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* "Journal of Physiology," vol. v, No. 3, p. 182.

† "Pflüger's Archiv," Bd. 34, s. 330.

‡ *Loc. cit.*, p. 343.

§ "Ueber den Nahrwerth einiger Verdanungsproducte des Eiweisses," "Pflüger's Archiv," Bd. xxxvii, H. 5 & 6, 1885.

peptone is concerned, and shows that by the process peptones and albumoses may be separated. A recent paper by Kühne* also discusses this question, and shows that true peptone remains in solution while the ammonium sulphate throws down all the albumoses. He further explains the results that Heynsius arrived at, by showing that the commercial specimens of peptones that the latter used and thought to be pure were largely mixed with albumoses. Though peptone has not yet been precipitated by saturation of its solution with neutral salts, it seems to be almost the only form of proteid that has refused to behave so, and it seems to be rather a question of what salt will throw down a particular proteid, than that such precipitation is a mark of any particular group.

The solution of the alcohol precipitate differed also from that of an animal peptone, in not giving a pink colour on the addition of sodic hydrate and a drop of cupric sulphate (biuret reaction). It agreed with it, however, in not giving a precipitate with potassic ferrocyanide and acetic acid.

Careful investigation of this body disproved the idea that it might really consist of a mixture of an albumose and a peptone for the solution of the precipitate, whether prepared by saturation with neutral salt, or by treatment with excess of alcohol, uniformly answered all the tests applied as described above. The dialysate also behaved on all these points just as the solution before dialysis. There is no doubt, therefore, that the body was a single one and not a mixture.

In examining the proteids found in other plants this body was again met with, and its reactions investigated at some length. It will be convenient therefore to postpone summarising them until later.

A little later in the year Mr. Dyer kindly sent me a bottle of the latex of *Mimusops globosa*, Gærtn (*Sapotaceæ*).† This differed very much from that of the East Indian latex-yielding trees, being a thick, almost pasty, liquid of white appearance and sour smell. It would not filter clear through paper and was therefore submitted to the action of the filter-pump used before. The diluted filtrate, and a watery extract of the dried residue, were taken for examination.

The solution thus obtained proved on investigation to contain two proteid bodies, which could be separated from each other with tolerable ease. On heating the solution gradually, having first neutralised, a little opalescence appeared, but it did not become particulate even at the boiling point. When the liquid was made either

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* "Albumen und Peptone," "Verhand. d. Naturhist.-Med. Ver.," Heidelberg, N. F., Bd. iii, H. 4, 1885, s. 286.

† [The well-known source of the Gum Balata of British Guiana, from which the specimen was obtained. The specimens were kindly procured by Mr. G. S. Jenman, Superintendent of the Botanic Garden, British Guiana.—W. T. T. D.]

acid or alkaline, however, it behaved differently. In a nitric acid solution an opalescence was noticeable when the temperature had risen to 85—90° C. This was not removed by the addition of more nitric acid. On keeping the vessel for some time at this temperature, the opalescence became a precipitate, which was soluble at ordinary temperatures in alkalis, slightly so in water, but not in nitric acid. The solutions gave the xanthoproteic reaction. A curious point about this body was the slowness with which the precipitate formed, it appearing not at all like the usual conversion into coagulated proteid on a rise of temperature, but more like a slow precipitation by the reagent at that particular point. This was confirmed by several experiments, one of which, often repeated, was the following. A quantity of the extract was made acid with nitric acid and warmed to 75° C., a point considerably below that at which the precipitate was first observed to form. It was then allowed to cool, and as the temperature was gradually falling, the precipitate slowly separated out. The body seemed then to be slowly precipitated by nitric acid, but not at the ordinary temperature.

In an alkaline solution its behaviour was somewhat different. The opalescence set in at 79° C., and a bulky precipitate settled out slowly at 85° C. This was soluble to a large extent in nitric acid, and was reprecipitated when the liquid was made alkaline. A solution in caustic soda of the precipitate caused by nitric acid at 85° C. behaved similarly. The precipitation here also seemed to be caused by the reagent and not by the temperature, for the alkaline liquid deposited the proteid body on cooling just as the acid one did, and in about the same time as when the temperature was kept constant at 85° C. Both precipitates were unaltered in the separation; each went into solution readily in its appropriate medium, the solutions all giving the xanthoproteic reaction.

This proteid gave no precipitate with acetic acid and potassic ferrocyanide.

After removal of this body by repeated boiling and filtration, the clear fluid gave a good xanthoproteic reaction. On applying some of the tests used in the case of the East Indian latex, the same peptone-like body was found to be present. It dialysed readily, and the solution in water gave a precipitate on saturation with solid $MgSO_4$.

Hence it appears that the latex of *Mimusops globosa* contains two proteids, one a member of the albumose group, precipitated under certain conditions by nitric acid or by potash, but not by boiling, and the other more nearly related to the peptones.

In 1823, Boussingault and Mariano de Rivero* published some observations on the latex of the cowtree of South America (*Brosimum*

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* "Mémoire sur le Lait de l'Arbre de la Vache (Palo de Vaca)," "Annales de Chimie et de Physique," t. xxiii, 1823, p. 219.

galactodendion, Don), one of the *Artocarpeæ.* They describe it as containing, among other constituents, a fibrous matter of animal nature, which was obtained by evaporating the latex down to dryness, washing the residue with essential oils to free it from waxy and resinous matters, and then getting rid of the essential oils by pressing dry and boiling with water. This treatment left them a brown mass which contained nitrogen. On heating this on hot iron they say it burned, giving off an odour similar to that coming from meat heated in the same way. This matter was not soluble in alcohol, and when obtained by repeated extraction with hot spirit, was left as a residue composed of white flexible threads. Thinking it possessed all the characteristics of animal fibrin they gave it the same name.

Since the date of their paper no information has been forthcoming as to the real nature of this vegetable fibrin. A quantity of the latex was obtained by Dr. Vines from Dr. Ernst of Caracas, and a bottle of it was, by his kindness, made available for the purposes of this investigation. The fluid had been mixed with a small amount of alcohol with a view to its preservation during its transit to England, and the treatment had been not quite so successful as that of the East Indian latex, some, but not much, of the proteid having been coagulated by the spirit. Still the fluid was of thick creamy consistency, and on digestion with water, and subsequent filtration, yielded a strongly proteid solution.* Extracts were made with water and with solutions of neutral salts, but the resulting liquid behaved in the same manner by whichever method it was prepared.

This extract contained two proteids, one of which was of the nature of an albumin. When the solution made with distilled water was examined, it was found to contain no salts capable of holding a globulin in solution, the only ones present being a mere trace of phosphates. The solution, on being dialysed till free from salts altogether, did not deposit any precipitate. On being boiled there was a well-marked coagulum, and after filtration the now coagulated matter gave a strongly marked xanthoproteic reaction. When the solution was gradually heated in the usual apparatus† the coagulation of the proteid took place at 68° C. The other tests for a proteid were fairly satisfactory, but were applied with more difficulty than with an animal albumin. With Millon's reagent there was a white precipitate, which went brick-red on boiling; with copper sulphate and sodic hydrate the violet colour was obtained, but not unless the soda solution was very strong. There was, however, no precipitate with acetic acid and potassic ferrocyanide.

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* The results of my examination of this latex, and a summary of the properties of the bodies found in this and other vegetable fluids described later, were communicated to the Physiological Society at its Cambridge meeting, May 9, 1885.

† Gamgee's "Physiological Chemistry," p. 15.

This body is of great interest, as till lately no true albumin has been described as occurring in plants. Ritthausen's albumins, described by him in 1872,* as found in seeds, have been shown by later observers, notably by Vines, to be rather globulins held in solution by the neutral salts present in the seeds. Even Ritthausen himself admits that the existence of a true albumin in seeds had not been established satisfactorily as lately as 1877.‡ In Martin's paper already referred to, he describes a body which he has found to be present in Papaw juice, which has the properties of an albumin. It is coagulated on boiling, is not precipitated on dialysing an extract of the juice, nor on saturating the solution by solid neutral salt. The body just described as occurring in the latex of *Brosimum* seems to be identical with this. It is noteworthy that both in the case of Martin's albumin and that which has just been described, the albumin appears to be a form of the circulating and not of the reserve proteid. Boussingault's vegetable fibrin was probably this albumin coagulated by the action of the hot alcohol used in its extraction. There was no other body in the latex that would become coagulated proteid.

The other proteid found in this latex remained in solution after boiling and filtering off the coagulated albumin. It was hence not changed by heating; it dialysed easily through a membrane, was precipitated but not coagulated by alcohol, and was precipitated by saturation of its solution by solid $MgSO_4$. It was therefore the same body as described above as a constituent of the East Indian latex. In the *Brosimum* latex there was a larger amount of it present, and its reactions were therefore carefully confirmed. Besides those already mentioned, two more peculiarities were noticed. In dilute solution, a stream of CO_2 passed through it for several hours caused a precipitate. On submitting it in concentrated or dilute solution to the action of artificial gastric juice, it underwent conversion into a true peptone, which gave a biuret reaction as readily as peptone prepared by the same method from fibrin or other animal proteid. There was not, however, during the digestion, any formation of acid albumin.

To protect the result from a danger of error arising from peptone being present in the artificial gastric juice employed, the experiment was performed as follows:—

A certain amount of the proteid was taken from under alcohol, dissolved in water, and the solution decolorised by filtration through animal charcoal. A solution of pepsin in 0·4 per cent. HCl was made and filtered. To a quantity of the proteid solution an equal

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* "Die Eiweiss-Körper der Getreidearten, &c.," 1872.

† *Loc. cit.*

‡ "Pflüger's Archiv," xv, 1877, p. 284.

bulk of this extract was added, and a similar quantity of the same was added, in another vessel, to as much water as the quantity of the proteid solution taken. The two were submitted to a temperature of 40° C. for twenty-four hours. The biuret test was then applied to both, care being taken to have equal quantities taken, and the same amount of caustic soda and copper sulphate added to each. Peptone was shown to be present in both, but the colour was the deeper in the case of the proteid solution. Hence, though a trace of peptone was present in the juice employed, the experiment showed formation from the proteid in the latex.

All the material investigated so far had been taken from the plant a considerable time before being examined; also a certain but varying amount of alcohol had been mixed with it. There was consequently a double possibility of decomposition of some sort having taken place. In one case at least there was no doubt that a certain portion of the proteid had been coagulated. It seemed desirable therefore to investigate certain plants that could be obtained in fair quantity in the fresh condition, and as laticiferous tissues were those in which most proteid matter would be found, choice was made of *Manihot glaziovii* Muell. Arg. (*Euphorbiaceæ*)* and the common lettuce, *Lactuca sativa*, L. (*Compositæ*). A considerable number of the young plants of the former of these was kindly raised by Mr. Irwin Lynch, at the Botanic Garden, Cambridge, and on their attaining a height of about 10 feet they were cut down and examined. On wounding them a milky latex exuded, but it was impossible to get this to flow in sufficient quantity to work with, hence another method of extracting it proved necessary. The young plants were cut down, their stems taken and freed from leaves and branches, and the cortex scraped off by a blunt knife. The mass of tissue was then finely minced, pounded in a mortar, and put into a quantity of water just sufficient to cover the pulp. After standing for twenty-four hours the whole was strained in a press through a coarse cloth, yielding a filtrate, turbid, and full of small particles of *débris*, chlorophyll granules, &c. In quantity it was about twice the bulk of the water used; this solution therefore was diluted latex, containing also any soluble matter originally present in the parenchymatous tissue of the cortex. Filtration, repeated many times, freed it ultimately from all colouring matters and *débris* arising from the preliminary treatment. Any soluble proteid existing temporarily or permanently in the tissue was hence in this extract.

The proteids normally present in the sieve tubes of *Manihot* have not been determined, but it is fair to assume that they do not materially differ from those of *Cucurbita*. From these Zacharias† has found it possible to extract a proteid body which behaves like a globulin.

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* [The commercial source of Ceara rubber.—W. T. T. D.]

† "Bot. Zeitg.," February, 1884, p. 67.

It is insoluble in distilled water or in sulphate of soda solution, but is soluble in weak acids or alkalis. Its precipitate in distilled water is changed by contact with alcohol into a white stringy mass. It gives the xanthoproteic reaction, and that with copper sulphate and potassic hydrate. Fischer* has observed also that the fluid contents of the sieve tubes in *Cucurbita* become coagulated on heating.

The first investigation of the extract prepared as above, was not easy on account of the difficulty of getting rid of the soluble phosphates, which were found to be present in considerable quantity. They were removed by warming with ammonia, but the last traces were very hard to throw down. The liquid finally, however, ceased to give a precipitate with ammonium molybdate. Besides the phosphates the salts present were sulphates and chlorides, but both were much smaller in amount than the phosphates.

Having freed the extract from phosphates, it was found to coagulate on boiling, and the coagulum gave the xanthoproteic reaction. On heating it more slowly an opalescence was found to appear at 74—76° C., which was replaced by a precipitate at about 80° C. After filtering this precipitate off, no further opalescence took place up to boiling point. Dialysis for some time caused a precipitate, though not a very bulky one. Saturation of the neutralised liquid with $MgSO_4$ gave a precipitate, and a stream of CO_2 through a weak solution did the same. These reactions, taken together, indicated the presence of a globulin, of pretty much the same character as that found in *Cucurbita* by Zacharias and Fischer. They were not, however, quite conclusive, as several of the methods used would have thrown down, if it were present, the body described as occurring in latices examined before. This body was therefore looked for and found. After getting rid of the globulin by heating and filtering, the liquid gave the same reactions as those described before as belonging to that body. The dialysis especially was well marked, alcohol giving a proteid precipitate readily with the liquid outside the dialyser. The globulin was not so readily isolated, but it proved possible to get it by dialysis. It was not present in such large quantity as the other, and was more readily precipitated completely from its solution by saturation with solid $MgSO_4$, for the fluid, when both were present, gave a xanthoproteic reaction after it had ceased to give a precipitate on boiling. It was also precipitated on very large dilution.

Hence in the extract of *Manihot* are two proteids, one being globulin in nature and agreeing in its reactions with that of Zacharius and of Fischer; being satisfactorily separated from the other without injury only by dialysis; both giving precipitates on saturation with solid $MgSO_4$. A similar body to this globulin has been described by

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* "Berichte d. deutsch. bot. Gesell.," vol. ii, No. 7, 1885.

Martin* as being present in Papaw juice. He speaks of it as being precipitated on boiling, the coagulating point being 70—74° C.; precipitated on dialysis; by CO_2 from dilute solution; and by saturation of its neutral solution with $MgSO_4$. The two appear to be identical.

An extract of *Lactuca sativa* was prepared in a similar way to that described in the case of *Manihot*. In this there was no globulin, but instead a proteid resembling Vines's† hemialbumose. It was precipitated on the addition of nitric acid, and the precipitate was largely soluble in excess. Addition of potassic ferrocyanide to this solution gave a precipitate. On filtering off the nitric acid precipitate it was found to be soluble in water and dilute alkalis, and the solution was not coagulated on boiling. The precipitate gave the xanthoproteic reaction. It differed from Vines's body in its solutions not giving the biuret reaction, but agreed with it in not dialysing. After removal of this albumose the extract contained in solution a quantity of the dialysable proteid described as occurring in previous cases.

Before leaving the investigation it seemed well to examine a plant which should belong to an order not specially laticiferous. The common cabbage (*Brassica oleracea*, L.), being succulent, was selected. Its examination was not particularly fruitful, bringing to light only the fact that the dialysable proteid was present there as well as in the other plants. It was not in this case examined very closely. No other proteid was found.

My researches, so far, agree with those of Martin in not showing the presence of true peptone in plants.

List of Proteids Found.

1. Dialysable proteid, resembling peptone.

This occurred in all plants examined. Its reactions may be summarised here:—

a. Soluble in water.
b. Not coagulated on boiling.
c. Precipitated slowly by alcohol, but not coagulated by the reagent.
d. Diffuses readily through membrane.
e. Is not precipitated by nitric acid, nor by acetic acid and by potassic ferrocyanide.
f. Is precipitated on saturation of its neutral or acid solution with solid $MgSO_4$.
g. Is precipitated slowly by a stream of CO_2 through its dilute solution.
h. Is converted into true peptone by the action of pepsin.
i. Does not give the biuret reaction.

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* *Loc. cit.*

† *Loc. cit.*

The body most nearly resembling this which has hitherto been described is that which is stated by Martin* to be produced by the action of papaïn on the proteids present in papaïn juice. It differs from the one now described in that it gives the biuret reaction, and is precipitated by acetic acid and potassic ferrocyanide. He says nothing as to its power of dialysis.

2. Hemialbumose (*Lactuca*)—

   a. Soluble in distilled water.
   b. Not coagulated on boiling.
   c. Precipitated by nitric acid and by acetic acid and potassic ferrocyanide.

This resembles very closely Vines's hemialbumose, and the body which Martin* has called α-phytalbumose. It differs in not giving the biuret reaction.

3. Albumose (*Mimusops*)—

   a. Soluble in distilled water.
   b. Not coagulated by boiling in neutral solution.
   c. Precipitated slowly by nitric acid at a temperature approaching 70° C.
   d. Not precipitated by acetic acid and potassic ferrocyanide.

4. Albumin (*Brosimum*)—

   a. Soluble in distilled water.
   b. Coagulated at 68° C.
   c. Not precipitated by acetic acid and potassic ferrocyanide.

5. Globulin (*Manihot*)—

   a. Precipitated by dialysis of its solution.
   b. Coagulated on heating to 74—76° C.
   c. Precipitated on saturation of neutral or acid solution with solid $MgSO_4$.
   d. Precipitated on large dilution.
   e. Precipitated by a stream of CO_2 through dilute solution.

Both the albumin and the globulin seem to be the same bodies as described by Martin as occurring in Papaw juice. The probable identity of the former with Boussingault's vegetable fibrin has already been alluded to.

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* *Loc. cit.*

IV. "The Coefficient of Viscosity of Air." By Herbert Tomlinson, B.A. Communicated by Professor G. G. Stokes, P.R.S. Received January 6, 1886.

(Abstract.)

The author has had occasion, whilst investigating the internal friction of metals, to determine the coefficient of viscosity of air. The viscosity of air has already engaged the attention of several distinguished experimenters, amongst others, of G. G. Stokes, Meyer, and Clerk Maxwell. The results obtained, however, differ so widely that it was considered necessary to institute fresh researches into the same subject.

The author employed the torsional vibrations of cylinders and spheres, suspended vertically from a horizontal cylindrical bar, and oscillating in a sufficiently unconfined space. The bar was suspended by a rather fine wire of copper or silver attached to its centre, which, after having been previously subjected to a certain preliminary treatment with a view of reducing the internal molecular friction, was set in vibration. The vibrations were performed in a large box, which was rendered sufficiently air-tight to prevent currents of air from vitiating the results. The wire, which was about 97 cm. in length, was suspended in an air-chamber, the double walls of which enclosed between them a layer of water. This air-chamber was in turn surrounded by a second, also provided with double walls which contained sawdust in the space between them. The object of the two air-chambers was to protect the wire as much as possible from small fluctuations of temperature, which last had been found to render the internal friction of the metal very uncertain.

The coefficient of viscosity of air was obtained from observations of the diminution of the amplitude of vibration, produced by the resistance of the air to the oscillating spheres or cylinders attached to the horizontal bar, arrangements having been made so that the vibration-period of the wire should remain the same, whether the cylinders or spheres were hanging to the bar or not. In deducing the value of the coefficient of viscosity from the logarithmic decrement, the author has availed himself of the mathematical investigations of Professor G. G. Stokes.*

Five sets of experiments were made with hollow cylinders and wooden spheres, in the construction and measurement of which considerable care was taken. When the cylinders were used arrange-

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* See Professor Stokes's paper "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums," "Trans. Camb. Phil. Soc.," vol. ix, Part II, 1850.

ments were made to eliminate the effect of the friction of the air on their ends. The following are the results:—

Length in centimetres.	Diameter in centimetres.	Vibration-period in seconds.	Temperature of the air in degrees centigrade.	Coefficient of viscosity of the air in C.G.S. units.
		Cylinders.		
60·875	2·5636	6·8373	12·02	0·00018171
60·885	0·9636	7·0590	14·63	0·00018122
60·875	2·5636	3·0198	11·69	0·00018024
53·175	2·5636	2·9994	10·64	0·00017845
		Spheres.		
	6·364	2·8801	9·35	0·00017820

Maxwell has proved* that the coefficient of viscosity of air is independent of the pressure and directly proportional to the absolute temperature. We can, therefore, calculate from the above data what would be the value of the coefficient of viscosity at 0° C.; and when this is done, in the case of each of the five sets of experiments, we obtain the following values:—

Set of experiments.	Coefficient of viscosity of air at 0° C.
1st	0·00017404
2nd.................	0·00017201
3rd	0·00017284
4th	0·00017359
5th	0·00017230

The mean of these numbers is 0·00017296 with a probable error of only 0·14 per cent. The formula for finding μ_t, the coefficient of viscosity of air at the temperature t° C., is therefore—

$$\mu_t = 0\cdot00017296\left(1+\frac{t}{273}\right).$$

The value of the coefficient of viscosity of air at 0° C. given above, though much nearer to that obtained by Maxwell than any which has been got by other observers, nevertheless differs from it by more than 8 per cent. Maxwell experimented with dry air freed from carbonic acid, but it does not seem possible that the small amount of aqueous vapour and carbonic acid present in ordinary air can be credited with a diminution of 8 per cent. in the viscosity; nor can

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* "Phil. Trans.," 1866, vol. 156, Part I.

the author explain in any way the difference between his own result and that of Maxwell.

[The method followed by Maxwell is liable to be vitiated to a very sensible degree by small errors of level of the movable disks, especially when they are closest to the fixed disks. The final adjustment is stated to have been that of the fixed disks, and no special precautions seem to have been taken to secure the exact horizontality of the movable disks. By a calculation founded on the equations of motion of a viscous fluid, I find that at the closest distance (about the one-sixth of an inch) at which the fixed and movable disks were set, an error of level of only 1° 8′ would suffice to make the internal friction appear 8 per cent. too high.

In Mr. Tomlinson's reductions no allowance has at present been made for the effect of the rotation of the spheres or cylinders about their own axes, which is not quite insensible, as it would be in the case of a ball pendulum. The introduction of a correction on this account would slightly diminish the values resulting from the experiments, especially in the case of the sphere, where it would come to about 4 per cent.—G. G. S.]

January 21, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Family Likeness in Stature." By FRANCIS GALTON, F.R.S. With an Appendix by J. D. HAMILTON DICKSON, Fellow and Tutor of St. Peter's College, Cambridge.

Received January 1, 1886.

I propose to express by formulæ the relation that subsists between the statures of specified men and those of their kinsmen in any given degree, and to explain the processes through which family peculiarities of stature gradually diminish, until in every remote degree of kinship the group of kinsmen becomes undistinguishable from a group selected out of the general population at random. I shall determine the constants in my formulæ referring to kinship with a useful degree of precision. These constants may provisionally and with some reservation be held applicable to other human peculiarities than stature, while the formulæ themselves are, I presume, applicable to every one-dimensioned faculty that all men possess in some degree, but that different men possess in different degrees.

I selected stature for the subject of this inquiry, for reasons fully set forth in two recent publications,* which dealt with one small portion of the ground covered by the present memoir, and from which it will be convenient that I should make as I proceed occasional short extracts, in order to complete the present argument and to save cross-reference. The reasons that combine to render stature an excellent subject for hereditary inquiry are, briefly, the ease and frequency of its measurement, its constancy during adult life, its inconsiderable influence on the death-rate, its dependence on a multiplicity of separate elements, and other points that I shall dwell on as I proceed, namely, the ease with which female statures are transmuted to their male equivalents, and so enabled to be treated on equal terms with male statures, the tendency of the parental statures to blend in inheritance, and the disregard of stature in marriage selection.

Stature-schemes.—It is an axiom of statistics that large samples taken out of the same population at random are statistically similar, and in such inquiries as these which do not aim at minute accuracy, they may be considered identical. Thus the statures in every group, say of 1000 male adults, when distributed in order of their magnitudes at equal distances apart and in a row, will form almost identical figures; it being only towards either end of the long row that irregularities will begin to show themselves. These are unimportant in the present inquiry and I disregard them. The Diagram S, fig. 1, shows the outline of such a group of statures. It is drawn to scale, each of the statures being supposed to have been represented by a vertical line of proportionate length, standing on a horizontal base, the lines being at equal distances apart, and the whole system being compressed into the space between two termini, which may be set at any convenient distance asunder. The vertical lines in the figure do not indicate these statures, but they are divisions, ten in number, between each of which 100 stature lines are compressed. The first and last stature will not touch the termini, but will be removed from them by a half-interval. As it will be convenient to assign a name to this figure, I will call it a

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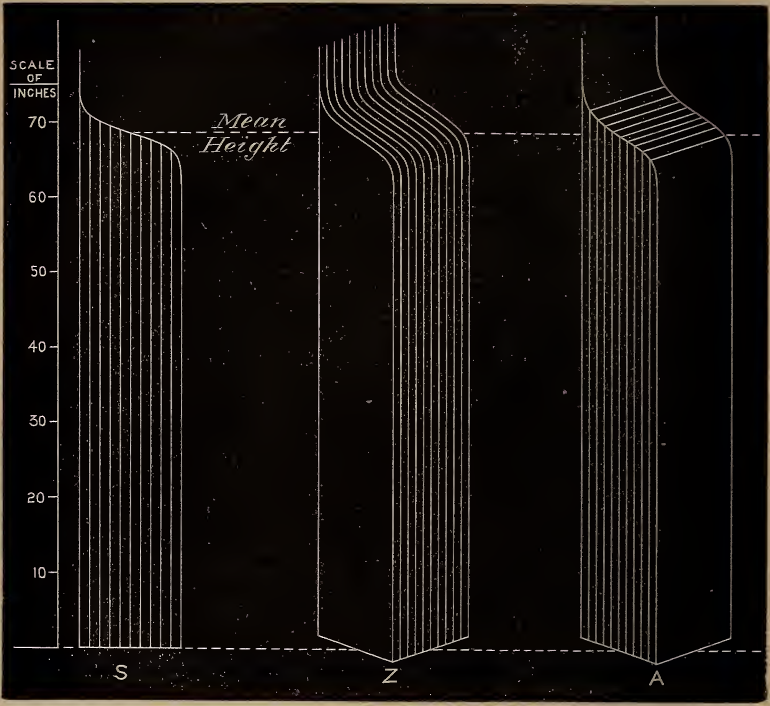
* (1.) "Presidential Address to the Anthropological Section of the British Association in 1885." (2.) "Regression towards Mediocrity in Hereditary Stature." "Journ. Anthrop. Institute," 1885, p. 246. The latter is a reprint of that portion of the former with which I am now concerned, together with some additional matter; it contains tables and diagrams, and should be referred to in preference.

Fig. 1.

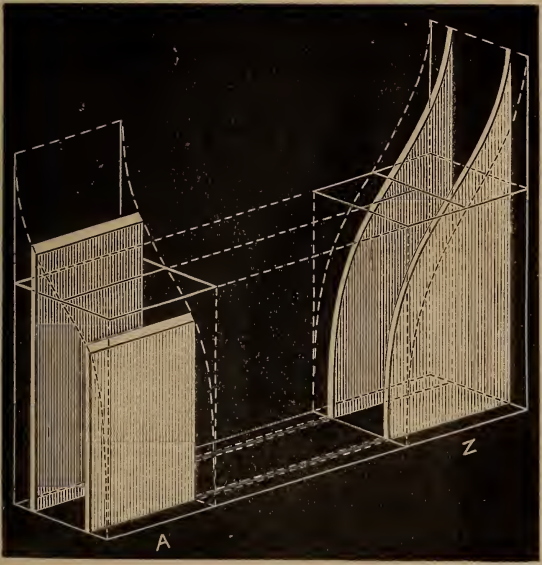


"stature-scheme." The numerous cases near mediocrity that differ little from one another, cause the middle portion of the upper boundary of the stature-scheme to assume a gentle slope, which increases rapidly towards either end, where the increasing rareness of more and more exceptional cases causes that boundary line to slope upwards, as an asymptote to one of the termini, and downwards as an asymptote to the other.

Now suppose that instead of compressing 1000 statures between the termini, I compressed 1000 × 1000, or a million of them, the stature-scheme would be unaltered, except that such small irregularities as might have been previously seen would become smoothed. The height of the middlemost or median stature-line would remain the same as before, and so would the heights of the lines standing at each quarter, each tenth, and at every other proportionate distance between the termini. Or again, instead of arranging the lines in a single scheme, we might arrange them in a thousand schemes, which as we have seen, would be practically identical in shape, and we may place these schemes side by side, as is done in Z, fig. 1, forming a "squadron" numbering 1000 statures each way, the whole standing upon a square base. Our squadron may be considered as made up of *ranks* (parallel to the plane of zx) as in Z, or of *files* (parallel to the plane of zy) as in A. The ranks, as we have seen, are all similar stature-schemes, the files are all rectangles which have the same breadths but are of dissimilar heights.

It is now easy to give a general idea, to be developed as we proceed, of the way in which any large sample of a population gives rise to a group of distant kinsmen in any given degree, who are statistically (in all respects except numbers) undistinguishable as regards their statures from themselves. I must suppose for convenience of explanation, that tall, short, and mediocre men are equally fertile (which is not, however, strictly the case, the tall being somewhat less fertile than the short*), and then on referring to fig. 2, the fortunes of the distant descendants of two of the rectangular files of squadron A will be seen traced.

FIG. 2.



As the number of kinsmen, in any remote degree we please to specify, of the men in each of the two files is about the same; I take 1000 of them in each case. Again, as the stature-schemes of those kinsmen are identical with those of equal numbers of men taken at random, as samples of the general population, it follows that they

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* Oddly enough, the shortest couple on my list have the largest family, namely sixteen children, of whom fourteen were measured.

will be identical with one another. Every other rectangular file being similarly represented, a complete squadron Z of the kinsmen is produced. It is obvious, then, that the squadrons A and Z are identical, and as the ranks of Z have proceeded from the files of A, the result is that the two squadrons will stand at right angles to one another. The upper surface of A was curved in rank, but was horizontal in file; that of Z is curved in file, but is horizontal in rank.

Kinsmen in near degrees are represented by squadrons of intermediate form. These will not have lost the whole of the curvature in rank of A, nor will they have acquired the whole of the curvature in file of Z. Consequently they will be curved moderately in both ways.* Also it will be found that the intersection of their surfaces by the horizontal plane of median height forms in each case an approximately straight line that assumes different and increasing inclinations, in the successive squadrons of intermediate shape between A and Z. These lines are indicated by straight lines on the squares below the squadrons in fig. 4, which represent the square bases upon which the squadrons stand.

I shall now show how these curves in rank and file should be treated. But before doing so, it is necessary to remark that female adult stature (I speak throughout of adults) may be safely transmuted to its male equivalent by multiplying it by a constant constant, which as regards my data is 1·08. After this has been done, the transmuted female statures may be treated on equal terms with the male statures, and the word "men" or other masculine term will include both sexes, unless otherwise stated distinctly. This procedure is adopted in the present memoir.

It is now generally recognised that the statures in every ordinary population are distributed in approximate conformity with what might have been inferred, if it were known that their variations were governed by such conditions as those upon which the exponential law of frequency of error is based. Therefore the upper boundary of the stature-scheme is approximately a curve (I call it an "ogive") that admits of mathematical expression. The abscissæ of the normal ogive (fig. 3) are values of the probability integral $\frac{1}{\sqrt{\pi}}\int_0^t e^{-t^2}dt$, and the ordinates are the corresponding values of t. These are given in column A of Table I. Column B contains the same values divided by 0·477, by which means they are expressed in units of the probable error. I find it convenient to call the ordinates to an ogive (drawn

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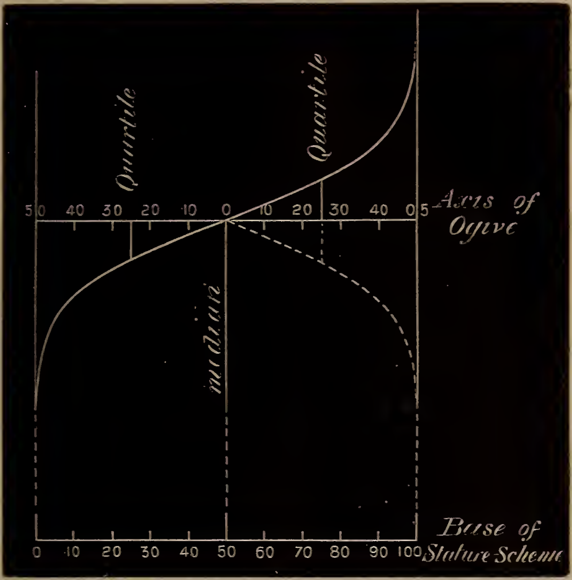
* A plaster model of one of these intermediate forms was exhibited at the meeting by Mr. J. D. H. Dickson, who stated that his recent mathematical investigation of the properties of their surfaces, had shown that no strictly straight line could be drawn upon them.—F. G.

FIG. 3.



from its axis) by the name of "deviates," and to describe either of those two symmetrical deviates of the normal ogive that stand at ± 25° by the name of "quartile deviate," or, more briefly, "quartile." I also give this name to the mean length of the upper and lower quartile, in those ogives which are drawn from observed data, and which are not strictly symmetrical. The numerical value of the quartile is identical with that of the well-known but here inappropriate term of "probable error."

Construction of Stature-Schemes and of Ogives from Observations.—The method of drawing an ogive from observations of stature is as follows. The observations (see Tables III, IV, and V, and compare with VI and VII) are sorted into grades, such as ". . . cases of 60 inches and under 61," ". . . cases of 61 inches and under 62," &c. If we are constructing a stature-scheme, or desire to obtain the median value of the series, we have to consider these values of inches, but in constructing no more than an ogive, which is only the upper boundary of a stature-scheme, it suffices to consider them as successive grades of 1 inch each, and I reckon the first grade not as 0, but as 1. This has been done in column A, Table VI, for the sake of treating different groups on a uniform plan. The number of cases in these grades are then summed from the beginning, and the sum, up to each grade inclusive, is written down, as shown in column B in Table VI. The percentage values of these, taking the total number of observations as 100, are written in column C. A series is there obtained which shows how many per cent. of the statures fall short of the parting value that separates each pair of adjacent grades. Thus if n per cent. of the statures fall within the first r grades, that is to say, are less than the value of the rth parting line, then $100-n$ per cent. of them will exceed that value. Consequently, if the observations are read off and recorded to the utmost nicety, r will be the value of the ordinate representing the stature which has to be erected on a base line at n per cent. of its length from one of its ends. In short, a base line of any convenient length has to be divided into 100 parts, and an ordinate of a length proportionate to r erected at the division n. As observations are never read off and recorded with perfect accuracy, a correction has here to be applied according to the circumstances of the particular case, whenever we are drawing a stature-scheme, and not merely an ogive. If the records are kept to the nearest mth part of an inch, the phrase "exceeding r inches" would really mean exceeding $r-1/m$ inches." This then is the true parting value corresponding to the nominal r. In drawing ogives, and not stature-schemes, this correction may of course be disregarded. Having erected ordinates corresponding to each value of r, their tops are connected by straight lines forming a polygonal boundary that approximates to the curvature of an ogive, and would become one if it were corrected with a free hand, or otherwise smoothed. The centre of the ogive lies at the intersection of the curve with the ordinate drawn from the base at the fiftieth division, and the horizontal axis of the ogive runs through that point of intersection (see fig. 3).

A half-ogive, whose ordinates are the mean lengths of the symmetrically disposed ordinates of the complete ogive, is constructed on the same general principles, but more simply, because the base from which it is plotted coincides with the axis of the ogive, and the graduations run alike, viz., from 0° to 50°.

In Table VII, the entries in the first lines of each of the three groups it contains, are the lengths of the ordinates that have been measured from the bases of ogives constructed from the data in Table VI. The abscissæ corresponding to the measured ordinates, are in every case the same fractional lengths of the bases. The entries in the second lines are the differences between these several ordinates and the median ordinate; they are, therefore, the deviates. The entries in the third lines are the negative deviates written under the corresponding positive ones. The entries in the fourth lines are the means of the values of the positive and negative deviates, disregarding their signs.

Comparison of Ogives.—The ogive being drawn according to the observations, its axis is divided into 100 parts, the fiftieth division being reckoned as 0°, then the deviates standing at the ± graduations of 10°, 20°, 25°, 30°, 40°, and 45° are measured. The mean of each pair of lengths, not regarding signs, has then to be divided by the mean lengths of the deviates at ± 25°, that is by the quartile deviate, and so is made to yield a series that is directly comparable with column B in Table I. The closeness with which it conforms to that standard series is the test of the closeness with which the observations conform to the law of frequency of error.

Table II effects this comparison for all the series that I have to deal with in the present paper. The values are entirely unsmoothed, except in two named instances, being taken from measurements made to the above-mentioned polygonal boundary. I thought it best to give these interpolated values in this, their rudest form, leaving it to be understood that with perfectly legitimate correction the accordance would become still closer. I do not carry the comparison beyond 45°, partly because my cases are not numerous enough to admit of a fair comparison being made, and chiefly because I am well aware that conformity is not to be expected towards the end of any series. I am content to deal with nine-tenths of the observations, namely, those between 0° and 45°, and to pay little heed to the remaining tenth, between 45° and 50°. It will be seen that the conformity of more than one half of each series is closer than to the first decimal place, and that in absolute measurement it is closer than to one-tenth of an inch.

Arithmetic and Geometric Means.—I use throughout this inquiry the ordinary law of frequency of error, which being based on the assumption of entire ignorance of the conditions of variability, necessarily proceeds on the hypothesis that *plus* and *minus* deviations of equal amounts are equally probable. In the present subject of discussion our ignorance is not so complete; there is good reason to suppose that *plus* and *minus* deviations, of which the probability is equal, are so connected together that the ratio between the lower observed measurement and the truth is equal to that between the truth and the upper observed measurement. My reasons for this were explained some years ago, and were accompanied by a memoir by Mr. Donald Macalister, showing how the law of frequency of error would be modified if based on the geometric, instead of on the arithmetic mean.* Though in the present instance the former process is undoubtedly the more correct of the two, the smallness of the error here introduced by using the well known law is so insignificant that it is not worth regarding. Thus the mean stature of the population is about 68·3 inches, and the quartile of the stature-scheme (the probable error) is 1·7 inch, or only about one-fortieth of its amount, and the

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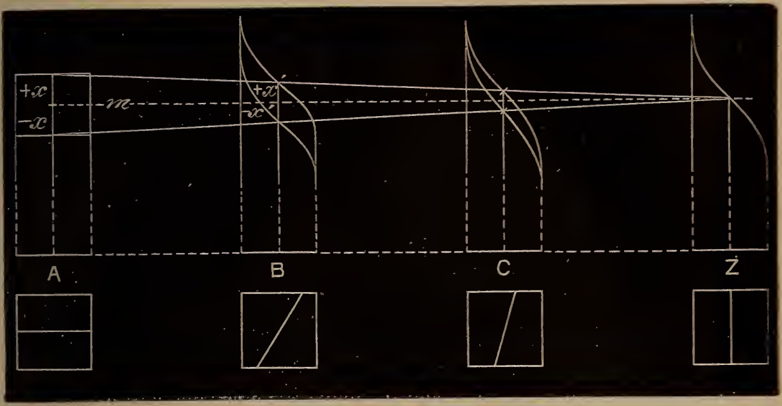
* "Proc. Roy. Soc.," vol. 29 (1879), pp. 365, 367.

difference between $40^2/39$ and 41 is that between about 41·025 and 41·000, or only about 6 per thousand.

Regression.—It is a universal rule that the unknown kinsman in any degree of any specified man, is probably more mediocre than he. Let the relationship be what it may, it is safe to wager that the unknown kinsman of a person whose stature is $68\frac{1}{4} \pm x$ inches, is of some height $68\frac{1}{4} \pm x'$ inches, where x' is less than x. The reason of this can be shown to be due to the combined effect of two causes: (1) the statistical constancy during successive generations of the statures of the same population who live under, generally speaking, uniform conditions; (2) to the reasonable presumption that a sample of the original population and a sample of their kinsmen in any specified degree are statistically similar in the distribution of their statures. To fix the ideas, let us take an example, namely, that of the relation between men and their nephews:—(*a.*) A sample of men, and a sample of the nephews of those men, are presumed to be statistically alike in stature, that is to say, their mean heights and their quartile deviates of height will be of the same value. I will call the value of this quartile p. (*b.*) Each family of nephews affords a series of statures that are distributed above and below the common mean of them. They are deviations from a central family value, or, as we may phrase it, from a nepotal centre, and it will be found as we proceed (it results from what appears in Tables III, IV, and V) that these deviations are in conformity with the law of error, and that the quartile values (probable errors) of these systems of deviations, which we will call f, are practically uniform, whatever the value of the central nepotal family stature may be. (*c.*) It will be found, as it is reasonable enough to anticipate, that the system of nepotal centres is distributed above and below the median stature of the population, in conformity with the law of frequency of error, and with a quartile value that we will call d. It follows from (*a*) that we possess data for an equation between p, f, and d, which, from a well-known property of the law of error, assumes the form $d^2+f^2=p^2$. Now the unknown nephew is more likely to be of the stature of his nepotal centre than any other stature that can be named. But the system of statures of nepotal centres is more concentrated than that of the general population (d^2 is less than p^2). That is to say, the unknown nephew is likely to be more mediocre than the known man of whom he is the nephew. What I shall have to show is expressed in fig. 4, where A and Z are side views of squadrons such as A and Z in fig. 2. [They are drawn shorter than the stature-schemes in fig. 1, and therefore out of scale, to save space, which is an unimportant change, as it is only the variation in the ogives we are now concerned about.] Let m represent the level of mediocrity above the ground, $m+x$ and $m-x$ the heights of any two rectangular files in

FIG. 4.



the squadron of known men. We have seen that x becomes 0 in remote degrees of kinship, and I shall show that in intermediate degrees the value of x'/x is constant for all statures in the same degree of kinship. This fraction is what I call the ratio of regression, and I designate it by w. Consequently the above formula becomes $w^2p^2+f^2=p^2$, which is universally applicable to all degrees of kinship between man and man, so long as the statistics of height of the population remain unchanged.

Hence in the squadrons, the curvature in rank is an ogive with the quartile value of wp, and in file with one having the quartile value of f, these two values being connected by the above formula. If the squadron is resolved into its elements, and those elements are redistributed into an ordinary stature-scheme, the quartile of the latter will be p.

Another way of explaining the universal tendency to regression may be followed by showing that this tendency necessarily exists in each of the three primary relationships, fraternal, filial, and parental, and therefore in all derivative kinships. Fraternal regression may be ascribed to the compromise of two conflicting tendencies on the part of the unknown brother, the one to resemble the given man, the other to resemble the mean of the race, in other words to be mediocre. It will be seen that this compromise results in a probable fraternal stature that is expressed by the formulæ $(p^2-b^2)/p^2$, in which b is a constant as well as p, therefore the ratio of fraternal regression is also a constant. Filial regression is due (as I explained more fully than I need do here, in the publications alluded to in the second paragraph) to the concurrence of atavism with the tendency to resemble the parent. The remote ancestry in any mixed population resembles, as has been already said, any sample taken at random out of that population, therefore their mean stature is mediocre; consequently the parental peculiarities are transmitted in a diluted amount. Parental regression is shown to be the necessary converse of filial regression by mathematical considerations, kindly investigated for me by Mr. Dickson, in the Appendix to this memoir in Problem 1. It is easy in a general way to see that this would be the case, but I find it not easy otherwise to prove it. Still less would it be easy to prove the connexion between filial and mid-parental regression, which depend on considerations that are thoroughly investigated in the Appendix.

Data.—I will now describe the data from which I obtain my conclusions. They consist of two sets of practically independent observations, though they do in some small degree overlap.

(1.) Special observations. These concern variation in height among brothers. I circulated cards of inquiry among trusted correspondents, stating that I wanted records of the heights of brothers who are more than 24 and less than 60 years of age; not necessarily of all the brothers of the same family, but of as many of them as could be easily and accurately measured, the height of even two brothers being acceptable. If more than one set of brothers were entered on the same card, the entries were of course to be kept separate. The back of the card was ruled vertically in three parallel columns: (*a*) family name of each set of brothers; (*b*) order of birth in each set; (*c*) height, without shoes, in feet and inches. A place was reserved at the bottom for the name and address of the sender. The circle of inquiry widened, and I closed it when I had obtained returns of 295 families, containing in the aggregate 783 brothers.

I look upon these returns as quite as trustworthy as any such returns are likely to be. They bear every internal test that I can apply to them very satisfactorily. They are commonly recorded to quarter and half inches.

(2.) R.F.F. data. By this abbreviation I refer to the Records of Family Faculties that I obtained in the summer of 1884, in reply to an offer of prizes. I have been able to extract from these the heights of 205 couples of parents, with those of an aggregate of 930 adult children of both sexes. I have transmuted all the female heights to their male equivalents, and have treated them thus transmuted on equal terms with the measurement of males, except where otherwise expressed. These data have by no means the precision of the special observations. There is in many cases considerable doubt whether the measurements refer to the height with the shoes on or off; many entries are, I fear, only estimates, and the heights are commonly given only to the nearest inch. Still, speaking from a knowledge of many of the contributors, I am satisfied that a fair share of these returns are undoubtedly careful and thoroughly trustworthy, so that I have reason to place confidence in mean results. They bear those internal tests that I apply to them better than I should have expected, and when taken in connexion with and checked by the special data, and used with statistical caution, they have proved very valuable to me.

I have discussed these materials in a great variety of ways to guard myself against rash conclusions, but I shall not present more than three primary tables, which contain sufficient materials for determining the constants of the formulæ to be used.

The first of them (Table III) refers to the children of what I call "mid-parents" of various statures. A mid-parent is the imaginary mean of the two parents, after the female measurements have been transmuted to their male equivalents, so that a mid-parent of 70 inches in height refers to a couple whose mean stature under the above reservations is 70 inches. I have given data in the "Journ. Anthrop. Inst." (*loc. cit.*) to show that we need not regard differences in stature between the parents, inasmuch as the distribution of heights among the children proves to be statistically the same, so long as the mid-parentages are alike, whether the two parents are the same or of different statures. This blending of paternal and maternal qualities in the stature of the offspring is one great advantage in selecting stature as a subject for the present inquiry.

General Population.—(1.) Its variability. The value of the quartile deviate in the population ogive (that is to say, the probable error) may be deduced from the bottom lines of any one of the three Tables III, IV, and V. Those in III and IV refer to data that are in part but by no means wholly the same, that of V refers to almost totally distinct data. The work is shown in Tables VI and VII; in the former the ordinates are calculated whence the ogive is drawn, in the latter I have given the values of the measured ordinates at the same points along its axis as those to which the ordinates given in Table I refer. The values of the quartile that I obtain in this way from the three cases are 1·65, 1·7, and 1·7. I should say that the more careful treatment that I originally adopted happened to make the first of these values also 1·7, so I have no hesitation in accepting 1·7 as the proper value of p for all my data.

(2.) Variability of system of mid-parents. I have published data in the memoir already alluded to, to show that marriage selection takes small account of stature, which is another great merit in stature as a subject for this inquiry. Some further proof of this may be got by comparing the variability of the system of mid-parents with that of the general population. If the married couples had paired together regardless of stature, their mean heights would be elements of a statistical system identical with one in which the pairs had been selected at random. In this latter case the quartile value of the system of mid-parents would be $1/\sqrt{2}\,.\,p = 1{\cdot}21$ inch. Now, I find the quartile of the series of the mid-parental system obtained from the two columns in Table III, that are headed respectively "Heights of the mid-parents" and "Total number of mid-parents," to be 1·19 inches,* which is an unexpectedly exact accordance.

(3.) Median Stature. I obtain the values 68·2, *68·5*, 68·4, from the three series mentioned above, but the middle value, printed in *italics*, is a smoothed value. This is one of the only two smoothed values in the whole work, and has been justifiably corrected because the one ordinate that happens to accord closely with the median is out of harmony with all the rest of the curve. This fortuitous discrepancy amounts to more than 0·15 inch. It does not affect the quartile value, because neither the upper nor the lower quartile is touched, and, therefore, the half-interquartile remains unchanged. It must be recollected that the series in question refers to R.F.F. brothers, which are a somewhat conditioned selection from the general R.F.F. population, and could not be expected to afford as regular an ogive as that made from observations of men selected from the population at hazard. It is undoubtedly in this group that the least accuracy was to have been expected.

Mean Ratios of Regression in the Primary Degrees of Kinship.—(1.) From the stature of mid-parents of the same height, to the mean of the statures of all their children. I have already (*loc. cit.*) published the conclusions to which I arrived about this, but it is necessary to enter here into detail. The data are contained in Table III, where each line exhibits the distribution of stature among the children of all the mid-parents in my list, who were of the stature that forms the argument to that line. The median stature in each successive line is the mean stature of all the children, and is given at the side in the column headed "Medians." Their values are graphically represented in fig. 5. It will be there seen that these value are disposed about a straight line. If the median statures of the children had been the same as those of their mid-parents this line would have accorded with the line AB, which, from the construction of the table, is inclined at an angle of 45° to the line "Mean Stature of Population," which represents the level of mediocrity. However, it does not do this, but its position is inclined at a smaller angle, θ, such that

$$\tan\theta : \tan 45 \; :: \; 2 : 3.$$

This gives us the ratio of regression ($=w$) in the present case; and, therefore, in the notation I adopt $w = \frac{2}{3}$.

(2.) From the stature of men of the same height, to the mean of the statures of all their children. We have just seen that when both

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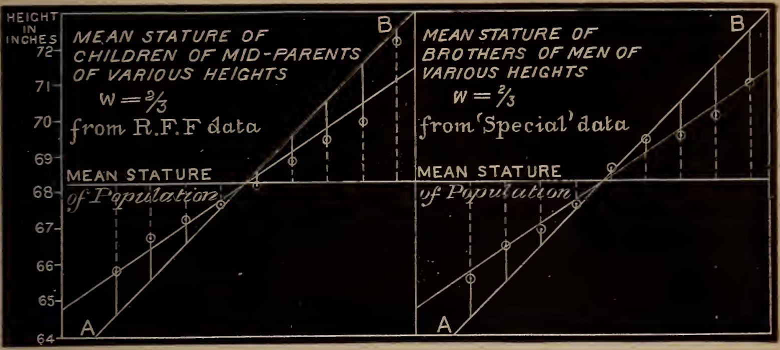
* In all my measurements the second decimal is only approximately correct.

Figs. 5 and 6.



parents have a deviate of $\pm x$, the mean of the deviates of all of their family centres will be $\pm\frac{2}{3}x$. It follows that if one parent only has that deviate $\pm x$, and if the stature of the other parent is unknown, and, therefore, on the average, mediocre, the mean of the statures of their children will be half the above amount, or $\frac{1}{3}$. I cannot test this conclusion very satisfactorily by direct observation, for my data are barely numerous enough to enable me to deal even with the mid-parentages. They are consequently insufficient to deal with a question involving the additional large uncertainty of the stature of one of the parents. I have, however, tabulated the data, but do not think it worth while to give them. They yield a ratio of regression of 0·40 instead of 0·33 as above. I disregard it, and adopt the latter, namely, $w=\frac{1}{3}$.

(3). From the stature of men of the same height to the mean of the statures of their mid-parents. By treating the vertical columns of Table III in the same way as we have just dealt with the horizontal lines, we obtain results of the same general form as in the last paragraph but one, though of different values.

Taking the height of a group of men of the same stature (viz., the "Adult Children") as given in the line that forms the heading to the table, we find the median stature of all their mid-parents, whence I deduce in this case $w=\frac{1}{3}$. The apparent paradox that the same table should give results by no means converse in their values for converse degrees of kinship, will be more conveniently examined later on.

(4.) From the stature of men of the same height to the mean of the statures of all their brothers. In seeking for this I shall at first confine myself to the more accurate special data, reserving to the end a comparison between their results and those derived from the R.F.F. The entries in the column headed "medians" in Table V are graphically represented in fig. 6, whence I deduce the value of $w=\frac{2}{3}$.

Variability of Statures of "Co-kinsmen" about their common mean Value.—By "co-kinsmen" I desire to express the group distributed in any one line of Tables III, IV, V, or of other tables constructed on a like principle. They are the kinsmen in a specified degree, not of a single person, but of a group of like persons, who probably differ both in ancestry and nurture. For example, the persons to whom the entries opposite 68·5 in Table III refer are not brothers, but they are what I call "co-fraternals," or from another point of view, "co-filials," namely, the children of numerous mid-parentages, differing variously in their antecedents, and alike only in their personal statures.

Co-filial Variability.—It appears from Table III that the mean of the quartiles derived from the successive lines, and which I designate by f, is 1·5 inch; also that the quartiles are of nearly the same value in all of the lines, allowance being made for statistical irregularities. A protraction on a large sheet of the individual observations in their several exact places, gave the result that the quartile was a trifle larger for the children of tall mid-parentages than for those of short ones. This justifies what was said some time back about the use of the geometric mean; it also justifies the neglect here of the method founded upon it, on the ground that it would lead to only an insignificant improvement in the results.

We have now obtained the values of the three constants in the general equation $w^2p^2+f^2=p^2$, when it is used to express the relation between mid-parentages and cofilials. Thus the quartile of the population being $p=1·7$, it was shown both by observation and by calculation, that the quartile of the mid-parental system was $1/\sqrt{2}\,.\,p$, or 1·21. It was also shown that the ratio of regression in that case was $\frac{2}{3}$, consequently the general equation becomes $(\frac{2}{3}\times1·21)^2+(1·5)^2=(1·7)^2$, or $0·64+2·25=2·89$, which is an exact accordance, satisfactorily cross-testing the various independent estimates.

Converse Ratios of Regression.—We are now sufficiently advanced to be able to examine more closely the apparent paradox that the ratio of regression from the stature of mid-parents of the same height to the mean of the statures of their sons should be $\frac{2}{3}$, while that of men of the same stature to the mean of the statures of their several mid-parents should be, not the numerical converse of this, but $\frac{1}{3}$. We may look upon the entries in Table III as the values of (vertical) ordinates in z to be erected upon it at the points where those entries lie, and which are specified by the arguments of "heights of mid-parents" written along the side, as values of ordinates in y, and of "heights of adult children" written along the top, as values of ordinates in x. The smoothed result would form a curved surface of frequency. I accordingly smoothed the table by writing at each intersection of the lines that separated the vertical columns with those that separated the horizontal lines, the sums of the four adjacent entries. Then I drew lines with a free hand through all entries, or interpolations between entries, that were of the same value. These lines formed a concentric series of elliptical figures, passing through values of z that diminished, going outwards. Their common centre at which z was the greatest, and which therefore was the portion of maximum frequency, lay at the point where both x and y were of the same value of $68\frac{1}{4}$ inches, that is, of the value of the mean stature of the population. The line in which the major axes of the ellipses lay was inclined nearer to the axis of x than that of y. It was evident from the construction that the median value of the entries, whether in each line or in each column of the table, must lie at the point where that line or column was touched by the projection of one of these ellipses. It was easy also to believe that the equation to the surface of frequency and the lines of loci of the above-mentioned points of contact, admitted of mathematical expression. Also that the problem to be solved might be expressed in a form that had no reference to heredity. In such a form I submitted it to Mr. J. Hamilton Dickson, who very kindly undertook its solution, which appears as an Appendix to this paper, and which helps in various ways to test and confirm the approximate and uncertain conclusions suggested by the statistical treatment of the observations themselves. I shall make frequent use of his mathematical results, both in respect to this problem and to another one (also given in the Appendix), in the course of my further remarks.

As regards the present subject of the connexion between the regression in direct and in converse kinships, it appears that it wholly depends on the relation between the quartiles of the two series of "arguments," and is expressed by the formula $c^2w = p^2w'$. In this case $c^2 = (1{\cdot}21)^2 = 1{\cdot}46$, and $p^2 = 2{\cdot}89$; also $w = \frac{2}{3}$; therefore $w' = \frac{1}{3}$ nearly.

It will be observed that in all cases of converse kinship, from man to man—as from man to brother, and conversely; from man to nephew, and conversely; from father to son, and conversely; $c = p$, therefore in these the ratio of regression is the same in the converse as in the direct kinship.

Brotherly Variability.—The size of human families is much too small to admit of the quartile of brotherly variability being determined in the same way as that of the population, namely, by finding the quartiles in single families, but there are four indirect ways of finding its value, which I will call b.

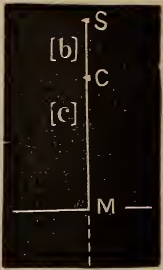
(1.) A collection of differences (see Table VIII) between the statures of individual brothers, in families of n brothers, and the mean of all the n statures in the same family, gives a quartile value, which I will call d, whence b may be deduced as follows:—Suppose an exceedingly large family (theoretically infinitely large) of brothers; their quartile would be b. Then if we select from it, at random, numerous groups of n brothers in each, the means of the mid-deviates of the several groups would form a series whose quartile is $1/\sqrt{n} \times b$. Hence b is compounded of this value and of d; that is to say,

$$b^2 = d^2 + 1/n \times b^2 \quad \text{or} \quad b^2 = \frac{n}{n-1} d^2.$$

I treated in this way four groups of families, in which the values of n were 4, 5, 6, and 7 respectively, as shown in Table VIII, whence I obtained for b the four values of 1·01, 1·01, 1·20, and 1·08, whose mean is 1·07.

(2.) Let c be the quartile of a series of brotherly centres whose quartile is unknown and has to be determined, and that the statures of the individual brothers diverge from their several family centres C_1C_2 . . ., with a quartile b, the whole group of brothers thus forming a sample of the ordinary population; consequently $c^2 = p^2 - b^2$. Now in fig. 7, MS represents the deviate in stature of a group of like persons who are not brothers, and MC represents the mean of the mid-deviates of their respective families of brothers. It can be shown (see Appendix, Problem 2) that if the position of c varies with respect to M with a quartile $= \sqrt{p^2 - b^2}$, and if S varies with respect to c with a quartile $= b$, then, when S only is observed, the most probable value of CM is such that $\frac{\text{CM}}{\text{SM}} (= w) = \frac{p^2 - b^2}{p^2}$,

Fig. 7.



$$\text{or } b^2 = p^2(1 - w).$$

Substituting 1·7 for p, and $\frac{2}{3}$ for w,

$$b = 0{\cdot}98 \text{ inch.}$$

(3.) It can also be shown (see Appendix, Problem 2) that the variability of particular mid-brotherly deviates, C_1C_2 . . ., about C, the mean of all them, is such that its quartile $= \frac{cb}{\sqrt{(c^2+b^2)}}$. Now the distribution of values in each line of Table V, whose quartile $= f$, is due to the combination of two variables. The one is the variability of $C_1C_2 \ldots$, about C; the other is the variability of the individual brothers in each family, about C_1, C_2, &c., respectively. Therefore $f^2 = \frac{c^2b^2}{c^2+b^2} + b^2$. Substituting for c^2 its value $p^2 - b^2$, we obtain

$$b^2 = p(p - \sqrt{(p^2 - f^2)}).$$

The observed value of f in Table V is 1·24, whence we obtain $b = 1·10$.

(4.) Pairs of brothers may be taken at random, and the differences noted between their statures; then under the following reservation, as regards the differences to be taken, we should expect the observed quartile of the differences to be $= \sqrt{2} \times b$. The reservation is, that only as many differences should be taken out of each family as are independent. A family of n brothers admits of $n \,.\, n-1/2$ possible pairs, but no more than $n-1$ of these are independent and only these should be taken. I did not appreciate this necessity at first, and selected pairs of brothers on an arbitrary system, which had at all events the merit of not taking more than four pairs of differences from any family, however numerous. It was faulty in taking three differences instead of only two from a family of three, and four differences instead of only three from a family of four, and therefore giving an increased weight to those families, but in other respects the system was hardly objectionable. On the whole the introduced error would be so slight as scarcely to make it worth while now to go over the work again. By the system adopted I found a quartile value of 1·55, which divided by $\sqrt{2}$ gives $b = 1·10$ inch.

Thus far we have dealt with the special data only. The less trustworthy R.F.F. give larger values of b. An epitome of all the results appears in the following table.

	Values of b obtained by different methods and from different data.	
	Specials.	R.F.F.
(1.) From families	1·07	1·38
(2.) From w (Tables V and IV)	0·98	1·31
(3.) From f (Tables V and IV)	1·10	1·14
(4.) From pairs of brothers	1·10	1·35
Mean	1·06	

The R.F.F. results refer to brothers only and not to transmuted sisters, except in method (2), where the paucity of the data compelled me to include them. I should point out that the data used in these four methods differ. In (1) I did not use families under four. In (2) and (3) I did not use large families. In (4) the method of selection was as we have seen, again different. This makes the accordance of the results still more gratifying. I gather from the above that we may securely consider the value of b to be less than 1·10, and allowing for some want of precision in the special data, the very convenient value of 1·0 inch may reasonably be adopted.

We are now able to deal completely with the distribution of statures in every degree of kinship of the kinsmen of those whose statures we know, but whose ancestral statures we are ignorant of or do not take into account. We are, in short, able to construct tables on the form of III, IV, and V, for every degree of kinship, and to reconstruct those tables in a way that shall be free from irregularities. The fraternal relation as distinguished from the co-fraternal has also been clearly explained.

In constructing a table of the form of III, IV, and V, we first find the value of w for the degree of kinship in question, thence we deduce f by means of the general equation $w^2p^2+f^2=p^2$ (p is supposed to be known, or for the general purpose of comparing the relative nearness of different degrees of kinship as tested by family likeness in stature, it may be taken as unity). The entries to be made in the several lines are then to be calculated from the ordinary tables of the "probability integral."

As an example of the first part of the process, suppose we are constructing a table of men and their nephews. A nephew is the son of a brother, therefore in his case we have $w=\frac{1}{3}\times\frac{2}{3}=\frac{2}{9}$; and $f=p\sqrt{(1-w^2)},=1{\cdot}66$.

Form of Data for calculating Tables of Distribution of Stature among Kinsmen.

From any group of persons of the same height, to their kinsmen as below.	Mean regression w.	Quartile of individual variability, $f(=p\times\sqrt{(1-w^2)})$.
Mid-parents	2/3	1·27
Brothers	2/3	1·27
Fathers or sons	1/3	1·60
Uncles or nephews	2/9	1·66
Grandfathers or grandsons	1/9	1·69

Trustworthiness of the Constants.—There is difficulty in correcting the results obtained solely from the R.F.F. data, by help of the knowledge of their general inaccuracy as compared with the special data. The reason is that this inaccuracy cannot be ascribed to an uncertainty of equal ± amount in every entry, such as might be due to a doubt of "shoes off" or "shoes on." If it were so, the quartile deviate of the R.F.F. would be greater than that of the specials, whereas it proves to be the same. It is likely that the inaccuracy is a result of the uncertainty above mentioned, which would increase the value of the quartile deviate, combined with a tendency on the part of my correspondents to record medium statures when they were in doubt, and which would reduce the quartile deviate. What the effect of all this might be on the value of w in Table IV, which is a datum of primary importance, I am not prepared to say, except that it cannot be great. While sincerely desirous of obtaining a revised value of w from new and more accurate data, the provisional value I have adopted may be accepted as quite accurate enough for the present.

Separate Contribution of each Ancestor to the Heritage of the Child.—I here insert a short extract from my paper in the "Journ. Anthrop. Inst.," with slight revision, as this memoir would be incomplete without it.

When we say that the mid-parent contributes two-thirds of his peculiarity of height to the offspring, it is supposed that nothing is known about the previous ancestor. But though nothing is known, something is implied, and this must be eliminated before we can learn what the parental bequest, pure and simple, may amount to. Let the deviate of the mid-parent be x (including the sign), then the implied deviate of the mid-grandparent will be $\frac{1}{3}x$, of the mid-ancestor in the next generation $\frac{1}{9}x$, and so on. Hence the sum of the deviates of all the mid-generations that contribute to the heritage of the offspring is $x(1+\frac{1}{3}+\frac{1}{9}+\&c.)=x\frac{3}{2}$.

Do they contribute on equal terms, or otherwise? I have not sufficient data to yield a direct reply, and must, therefore, try the effects of limiting suppositions. First, suppose the generations to contribute in proportion to the values of their respective mid-deviates; then as an accumulation of ancestral deviates whose sum amounts to $x\frac{3}{2}$, yields an effective heritage of only $x\frac{2}{3}$, it follows that each piece of heritable property must be reduced, as it were, by a succession tax, to $\frac{4}{9}$ of its original amount, because $\frac{3}{2}\times\frac{4}{9}=\frac{2}{3}$.

Another supposition is that of successive proportionate diminutions, the property being taxed afresh in each transmission to $1/r$ of its amount, so that the effective heritage would be—

$$x\left(\frac{1}{r}+\frac{1}{3r^2}+\frac{1}{3^2r^3}+-\right)=x\left(\frac{3}{3r-1}\right)$$

and this must, as before, be equal to $x\frac{2}{3}$, whence $\frac{1}{r}=\frac{6}{11}$.

A third possible supposition of the mid-ancestral deviate in any one remote generation contributing more than would be done by an equal mid-parental deviate, is notoriously incorrect. Thus the descendants of "pedigree wheat" in the (say) twentieth generation show no sign of the remarkable size of their mid-ancestors in that degree, but the offspring in the first generation do so unmistakably.

The results of our only two valid limiting suppositions are therefore (1) that the mid-parental deviate, pure and simple, influences the offspring to $\frac{4}{9}$ of its amount; (2) that it influences it to the $\frac{6}{11}$ of its amount. These values differ but slightly from $\frac{1}{2}$, and their mean is closely $\frac{1}{2}$, so we may fairly accept that result. Hence the influence, pure and simple, of the mid-parent may be taken as $\frac{1}{2}$, of the mid-grandparent $\frac{1}{4}$, of the mid-great-grandparent $\frac{1}{8}$, and so on. That of the individual parent would therefore be $\frac{1}{4}$, of the individual grandparent $\frac{1}{16}$, of an individual in the next generation $\frac{1}{64}$, and so on.

[I do not propose here to discuss the reason why the effective heritage of the child should be less than the accumulated deviates of his ancestors. It is obviously connected with considerations that bear on stability of type.]

Pure breed.—In a perfectly pure breed, maintained during an indefinitely long period by careful selection, w would become $=0$, and the value of b would be changed, but apparently only a little. Call its new value β. It may be roughly estimated as follows. In mixed breeds the value of b includes the probable uncertainty of the implied value of the contributions inherited from the mid-grandparents, and from the mid-ancestry of each preceding generation. This can be but a trifle. Suppose the quartile of the uncertainty in the implied stature of each grandparent to be even as much as 1·7 inch (we need not wait to discuss its precise value), then the quartile of the uncertainty as regards the implied mid-grandparental stature would be $1/\sqrt{4}\times$ that amount, or say 0·8. The proportion of this, which would on the average be transmitted to the child, would be only $\frac{1}{4}$ as much, or 0·2. From all the higher ancestry put together, the contribution would be much less than this, and we may disregard it. The result then is $b^2=\beta^2+0·04$. Taking $b=1·07$, this gives $\beta=1·05$ inch.

Probable Stature of the Child when the Statures of several of his Kinsmen are known.—First we have to add their several contributions as assessed in the last paragraph but one, and to these we have to add whatever else may be implied. A just estimate of the latter requires the solution of a very complex problem. Thus:—a tall son has a short father; this piece of knowledge makes us suspect that the mother was tall, and we should do wrong to set down her unknown stature as mediocre. Our revised estimate would be further modified if we knew the stature of one of her brothers, and so on. Moreover, the general equation $w^2p^2+f^2=p^2$ may cease to hold good. The possible problems are evidently very various and complicated, I do not propose to speak further about them now. It is some consolation to know that in the commoner questions of hereditary interest, the genealogy is fully known for two generations, and that the average influence of the preceding ones is small.

In conclusion, it must be borne in mind that I have spoken throughout of heredity in respect to a quality that blends freely in inheritance. I reserve for a future inquiry (as yet incomplete) the inheritance of a quality that refuses to blend freely, namely, the colour of the eyes. These may be looked upon as extreme cases, between which all ordinary phenomena of heredity lie.

Appendix. By J. D. HAMILTON DICKSON.

Problem 1.

A point P is capable of moving along a straight line P′OP, making an angle $\tan^{-1}\frac{2}{3}$ with the axis of y, which is drawn through O the mean position of P; the probable error of the projection of P on Oy is 1·22 inch: another point p, whose mean position at any time is P, is capable of moving from P parallel to the axis of x (rectangular co-ordinates) with a probable error of 1·50 inch. To discuss the "surface of frequency" of p.

1. Expressing the "surface of frequency" by an equation in x, y, z, the exponent, with its sign changed, of the exponential which appears in the value of z in the equation of the surface is, save as to a factor,

$$\frac{y^2}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

hence all sections of the "surface of frequency" by planes parallel to the plane of xy are ellipses, whose equations may be written in the form,

$$\frac{y^2}{(1{\cdot}22)^2}+\frac{(3x-2y)^2}{9(1{\cdot}50)^2}=\text{C, a constant} \quad . \; . \; . \; . \quad (2)$$

2. Tangents to these ellipses parallel to the axis of y are found, by differentiating (2) and putting the coefficient of dy equal to zero, to meet the ellipses on the line,

$$\left.\begin{aligned} &\frac{y}{(1{\cdot}22)^2}-2\frac{3x-2y}{9(1{\cdot}50)^2}=0, \\ \text{that is} \quad &\frac{y}{x}=\frac{\dfrac{6}{9(1{\cdot}50)^2}}{\dfrac{1}{(1{\cdot}22)^2}+\dfrac{4}{9(1{\cdot}50)^2}}=\frac{6}{17{\cdot}6} \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \quad (3)$$

or, approximately, on the line $y=\frac{1}{3}x$. Let this be the line OM.

From the nature of conjugate diameters, and because P is the mean position of p, it is evident that tangents to these ellipses parallel to the axis of x meet them on the line $x=\frac{2}{3}y$, viz., on OP.

3. Sections of the "surface of frequency" parallel to the plane of xz, are, from the nature of the question, evidently curves of frequency with a probable error 1·50, and the locus of their vertices lies in the plane zOP.

Sections of the same surface parallel to the plane of yz are got from the exponential factor (1) by making x constant. The result is simplified by taking the origin on the line OM. Thus putting $x=x_1$, and $y=y_1+y'$, where by (3)

$$\frac{y_1}{(1{\cdot}22)^2}-2\frac{3x_1-2y_1}{9(1{\cdot}50)^2}=0$$

the exponential takes the form

$$\left\{\frac{1}{(1{\cdot}22)^2}+\frac{4}{9(1{\cdot}50)^2}\right\}y'^2+\left\{\frac{y_1^2}{(1{\cdot}22)^2}+\frac{(3x_1-2y_1)^2}{9(1{\cdot}50)^2}\right\}\quad . \quad . \quad (4)$$

whence, if e be the probable error of this section,

$$\left.\begin{aligned}\frac{1}{e^2}&=\frac{1}{(1{\cdot}22)^2}+\frac{4}{9(1{\cdot}50)^2}\\ \text{or [on referring to (3)]}\quad e&=1{\cdot}50\sqrt{\frac{9}{17{\cdot}6}}\end{aligned}\right\}\quad . \; . \; . \; . \; . \; . \; . \quad (5)$$

that is, the probable error of sections parallel to the plane of yz is nearly $\frac{1}{\sqrt{2}}$ times that of those parallel to the plane of xz, and the locus of their vertices lies in the plane zOM.

It is important to notice that all sections parallel to the same coordinate plane have the same probable error.

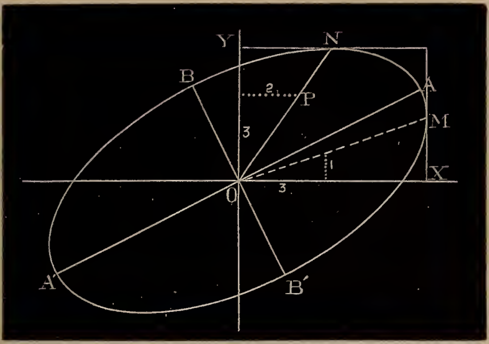
4. The ellipses (2) when referred to their principal axes become, after some arithmetical simplification,

$$\frac{x'^2}{20{\cdot}68}+\frac{y'^2}{5{\cdot}92}=\text{constant},\quad . \; . \; . \; . \; . \; . \; . \quad (6)$$

the major axis being inclined to the axis of x at an angle whose tangent is 0·5014. [In the approximate case the ellipses are $\frac{x'^2}{7}+\frac{y'^2}{2}=$ const., and the major axis is inclined to the axis of x at an angle $\tan^{-1}\frac{1}{2}$.]

5. The question may be solved in general terms by putting YON$=\theta$, XOM$=\phi$, and replacing the probable errors 1·22 and 1·50 by a and b respectively: then the ellipses (2) are

FIG. 8.



$$\frac{y^2}{a^2}+\frac{(x-y\tan\theta)^2}{b^2}=C, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

equation (3) becomes

$$\left.\begin{aligned}&\frac{y}{a^2}-\tan\theta\frac{x-y\tan\theta}{b^2}=0\\ \text{or}\quad&\frac{y}{x}=\tan\phi=\frac{a^2\tan\theta}{b^2+a^2\tan^2\theta}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and (5) becomes

$$\frac{1}{e^2}=\frac{1}{a^2}+\frac{\tan^2\theta}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

whence

$$\frac{\tan\phi}{\tan\theta}=\frac{e^2}{b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

If c be the probable error of the projection of p's whole motion on the plane of xz, then

$$c^2=a^2\tan^2\theta+b^2,$$

which is independent of the distance of p's line of motion from the axis of x. Hence also

$$\frac{\tan\phi}{\tan\theta}=\frac{a^2}{c^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Problem 2.

An index q moves under some restraint up and down a bar AQB, its mean position for any given position of the bar being Q; the bar, always carrying the index with it, moves under some restraint up and down a fixed frame YMY′, the mean position of Q being M: the movements of the index relatively to the bar and of the bar relatively to the frame being quite independent. For any given observed position of q, required the most probable position of Q (which cannot be observed); it being known that the probable error of q relatively to

Q in all positions is b, and that of Q relatively to M is c. The ordinary law of error is to be assumed.

If in any one observation, $MQ=x$, $Qq=y$, then the law of error requires

$$\frac{x^2}{c^2}+\frac{y^2}{b^2} \quad \ldots\ldots\ldots\ldots \quad (12)$$

to be a minimum, subject to the condition

$$x+y=a, \text{ a constant.}$$

Hence we have at once, to determine the most probable values of x', y',

$$\frac{x'}{c^2}=\frac{y'}{b^2}=\frac{a}{b^2+c^2}, \quad \ldots\ldots\ldots \quad (13)$$

and the most probable position of Q, measured from M, when q's observed distance from M is a, is

$$\frac{c^2}{b^2+c^2}a.$$

It also follows at once that the probable error v of Q (which may be obtained by substituting $a-x$ for y in (12)) is given by

$$\frac{1}{v^2}=\frac{1}{c^2}+\frac{1}{b^2}, \text{ or } v=\frac{bc}{\sqrt{b^2+c^2}} \quad \ldots\ldots \quad (14)$$

which, it is important to notice, is the same for all values of a.

Throughout this discussion the technical term "probable error" has been used; it may in every instance be replaced by Mr. Galton's very apt name "quartile," in which case the results of these problems may be read in conjunction with Mr. Galton's papers.

Table I.

Ogive, or Normal Curve of Distribution of Error.

Abscissæ reckoned from 0° to ± 50° (value of the probability integral).		Corresponding ordinates (or deviates).	
		Value of the deviate when modulus = 1, A.	Value of deviate reduced proportionately to quartile = 1, B.
	10	0·179	0·38
	20	0·371	0·78
Quartile	25	0·477	1·00
	30	0·595	1·25
	40	0·906	1·90
	45	1·163	2·44

Table II.

Comparison of observed Ogives with the Normal.

	Abscissæ of the half-ogive.						Value of the unit in inches.
	10.	20.	25.	30.	40.	45.	
Normal ogive, from Table I....	0·38	0·78	1·00	1·25	1·90	2·44	1·00
General population, R.F.F.....	0·33	0·74	1·00	1·23	2·06	2·62	1·7
Population of brothers, R.F.F..	0·36	0·78	1·00	1·41	1·95	2·12	1·7
,, ,, specials.	0·38	0·79	1·00	1·25	1·92	2·46	1·7
Mid-parentages..............	0·35	0·79	1·00	1·28	2·12	2·78	1·2
Brothers in random pairs, R.F.F.	0·47	0·84	1·00	1·29	2·11	2·64	1·4
,, ,, specials.	0·42	0·78	1·00	1·25	1·88	2·44	1·4

Note.—The second decimal is only approximate.

Table III (R.F.F. Data).

Number of Adult Children of various Statures born of 205 Mid-parents of various Statures.
(All Female Heights have been multiplied by 1·08).

Height of the mid-parents in inches.	Heights of the adult children.														Total number of		Medians.
	Below	62·2	63·2	64·2	65·2	66·2	67·2	68·2	69·2	70·2	71·2	72·2	73·2	Above.	Adult children.	Mid-parents.	
Above	..	..	..	..	..	..	..	..	..	..	..	1	3	..	4	5	
72·5....	..	..	..	..	..	..	..	1	2	1	2	7	2	4	19	6	72·2
71·5....	..	..	..	..	1	3	4	3	5	10	4	9	2	2	43	11	69·9
70·5....	1	..	1	..	1	1	3	12	18	14	7	4	3	3	68	22	69·5
69·5....	..	..	1	16	4	17	27	20	33	25	20	11	4	5	183	41	68·9
68·5....	1	..	7	11	16	25	31	34	48	21	18	4	3	..	219	49	68·2
67·5...	..	3	5	14	15	36	38	28	38	19	11	4	..	..	211	33	67·6
66·5....	..	3	3	5	2	17	17	14	13	4	..	..	..	..	78	20	67·2
65·5....	1	..	9	5	7	11	11	7	7	5	2	1	..	..	66	12	66·7
64·5....	1	1	4	4	1	5	5	..	2	..	..	..	..	..	23	5	65·8
Below	1	..	2	4	1	2	2	1	1	..	..	..	..	..	14	1	
Totals	5	7	32	59	48	117	138	120	167	99	64	41	17	14	928	205	
Medians ..	..	..	66·3	67·8	67·9	67·7	67·9	68·3	68·5	69·0	69·0	70·0					

Note.—In calculating the medians, the entries have been taken as referring to the middle of the squares in which they stand. The reason why the headings run 62·2, 63·2, &c., instead of 62·5, 63·5, &c., is that the observations are unequally distributed between 62 and 63, 63 and 64, &c., there being a strong bias in favour of integral inches. After careful consideration, I concluded that the headings, as adopted, best satisfied the conditions. This inequality was not apparent in the case of the mid-parents.

Table IV (R.F.F. Data).

Relative number of Brothers of various Heights to Men of various Heights, Families of Six Brothers and upwards being excluded.

Heights of the men in inches.	Heights of their brothers in inches.														Total cases.	Medians.
	Below 61·7	62·2	63·2	64·2	65·2	66·2	67·2	68·2	69·2	70·2	71·2	72·2	73·2	Above 73·7		
Above 73·7	..	..	..	..	1	..	1	..	1	4	3	3	3	2	18	
73·2	1	..	..	..	..	..	1	1	2	1	3	4	..	3	16	
72·2	1	..	1	2	1	1	..	8	6	8	11	5	4	3	51	70·3
71·2	..	..	..	4	4	4	9	11	15	12	8	11	3	3	84	69·3
70·2	1	..	2	4	3	7	6	12	25	18	11	8	1	3	101	69·3
69·2	..	..	4	6	13	12	18	29	29	24	15	6	2	1	159	68·6
68·2	1	..	..	3	6	7	15	16	29	12	11	8	1	..	109	69·9
67·2	1	..	4	3	8	14	21	15	19	6	9	..	1	1	102	67·7
66·2	..	..	1	7	10	12	14	7	12	7	4	1	..	..	75	67·2
65·2	..	1	1	4	13	9	8	6	13	3	4	1	..	1	64	67·2
64·2	..	1	..	6	4	7	3	3	6	4	4	2	..	..	40	67·3
63·2	..	..	..	..	1	1	4	..	4	2	..	1	..	..	13	
62·2	..	..	..	..	1	..	..	..	..	..	..	..	..	..	1	
Below 61·7	..	..	..	..	..	..	1	1	..	1	..	1	1	..	5	
..	5	2	13	39	65	74	101	109	161	102	83	51	16	17	838	

Table V (Special Data).

Relative number of Brothers of various Heights to Men of various Heights, Families of Five Brothers and upwards being excluded.

Heights of the men in inches.	Heights of their brothers in inches.													Total cases.	Medians.
	Below 63	63·5	64·5	65·5	66·5	67·5	68·5	69·5	70·5	71·5	72·5	73·5	Above 74		
74 and above	1	1	..	..	..	..	..	1	1	..	5	3	12	24	
73·5........	..	..	..	..	..	1	3	4	8	3	3	2	3	27	
72·5........	..	..	..	..	1	1	6	5	9	9	8	3	5	47	71·1
71·5........	..	1	..	1	2	8	11	18	14	20	9	4	..	88	70·2
70·5........	..	..	1	1	7	19	30	45	36	14	9	8	1	171	69·6
69·5........	..	1	2	1	11	20	36	55	44	17	5	4	2	198	69·5
68·5........	..	1	5	9	18	38	46	36	30	11	6	3	..	203	68·7
67·5........	2	4	8	26	35	38	38	20	18	8	1	1	..	199	67·7
66·5........	4	3	10	33	28	35	20	12	7	2	1	..	..	155	67·0
65·5........	3	3	15	18	33	36	8	2	1	1	..	..	..	110	66·5
64·5........	3	8	12	15	10	8	5	2	1	..	..	..	..	64	65·6
63·5........	5	2	8	3	3	4	1	1	..	1	..	..	1	20	
Below 63	5	5	3	3	4	2	..	..	..	..	..	..	1	23	
Totals	23	29	64	110	152	200	204	201	169	86	47	28	25	1329	

Table VI.

Construction of Ogives from Observations.

(The Statures are here distributed in grades of one inch each.)

The number of the grade.	General population. (R.F.F.)			Population of brothers. (R.F.F.)			Population of brothers. (Specials.)		
	Number of cases in each grade.	Sums from the beginning.	Per cents.	Number of cases in each grade.	Sums from the beginning.	Per cents.	Number of cases in each grade.	Sums from the beginning.	Per cents.
	A.	B.	C.	A.	B.	C.	A.	B.	C.
1........	5	5	0·5	5	5	0·6	23	23	1·7
2........	7	12	1·3	2	7	0·7	29	52	3·9
3........	32	44	4·8	13	20	2·4	64	116	8·7
4........	59	103	11·1	39	59	7·1	110	226	16·9
5........	48	151	16·3	65	124	14·8	152	378	28·2
6........	117	268	28·9	74	198	23·7	200	578	43·1
7........	138	406	43·8	101	299	35·7	204	782	58·4
8........	120	526	63·1	109	408	48·7	201	983	73·4
9........	167	693	74·7	161	569	68·0	169	1152	86·0
10........	99	792	85·4	102	671	80·2	86	1238	92·4
11........	64	856	92·2	83	754	90·1	47	1285	95·9
12........	41	897	96·7	51	805	96·2	28	1313	98·8
13........	17	914	98·5	16	821	98·1	25	1338	100·0
14........	14	928	100·0	17	838	100·0	..	..	..

Table VII.

Measurement of Ogives.

The Entries are Ordinates to the Curves constructed from Table VI, at points which are situated in every case, at the same fractional divisions, either of their bases as in the first lines of each of the three groups, or of their axes as in the other lines.

	Abscissæ, reckoned from the middle of the ogive, in percentages of the length of its axis.												
	−45	−40	−30	−25	−20	−10	0	+10	+20	+25	+30	+40	+45
General population (R.F.F.)	2·85	3·75	5·27	5·68	6·10	6·75	7·32	7·85	8·60	9·05	9·50	10·67	11·65
	4·47	3·57	1·95	1·64	1·22	0·57	0·00	0·53	1·28	1·73	2·18	3·35	4·33
							0·00	0·57	1·22	1·64	1·95	3·57	4·47
				Means.............			0·00	0·55	1·25	1·68	2·07	3·46	4·40
Population of brothers (R.F.F.) ..	3·55	4·38	5·60	6·12	6·54	7·35	7·95*	8·59	9·17	9·63	10·00	11·00	11·80
	4·40	3·57	2·35	1·73	1·41	0·60	0·00	0·64	1·22	1·68	2·05	3·05	3·85
							0·00	0·60	1·41	1·73	2·35	3·57	4·40
				Means.............			0·00	0·62	1·32	1·70	2·40	3·31	3·61
Population of brothers (specials) ...	2·45	3·15	4·28	4·72	5·12	5·76	6·45	7·10	7·77	8·10	8·52	9·65	10·75
	4·00	3·30	2·17	1·73	1·33	0·69	0·00	0·55	1·32	1·65	2·07	3·20	4·30
							0·00	0·69	1·33	1·73	2·17	3·30	4·00
				Means.............			0·00	0·64	1·33	1·69	2·12	3·25	4·15

* The values are unsmoothed with this one exception. Its correction in no way affects the line headed "means."

Table VIII. (Special Data.)

Number of cases in which the Stature of individual Brothers was found to deviate to various amounts from the Mean Stature of their respective families.

Number of brothers in each family	4	5	6	7
Number of families	39	23	8	6
Amount of deviation.	Number of cases.	Number of cases.	Number of cases.	Number of cases.
Under 1 inch	88	62	20	21
1 and under 2	49	30	18	14
2 and under 3	15	17	5	6
3 and under 4	4	3	3	1
4 and above	..	3	2	..

II. "The Early Development of *Julus terrestris*."* By F. G. HEATHCOTE, M.A., Trin. Coll. Cam. Communicated by Professor M. FOSTER, Sec. R.S. Received January 6, 1886.

The following are the principal results of my investigations on the early development of *Julus terrestris* since June, 1882.

When laid the eggs are oval in shape, white, and covered with a thick chitinous chorion. The nucleus is situated in a mass of protoplasm in the centre of the ovum. This mass of protoplasm is of irregular shape, but its long axis corresponds with that of the ovum. From it, anastomosing processes radiate in all directions, forming a network throughout the egg. The yolk-spherules are contained within the meshes of this network. The nucleus is not a distinct vesicle but its position is marked by chromatin granules. There is no nucleolus.

Early on the second day the nucleus and the central mass of protoplasm apparently divide into two parts. But this division is not complete, the two resulting masses with their nuclei remaining connected by a network of protoplasm. Each of these divides in the same incomplete manner, so that we now have four segments all connected together. This process is continued until there are a considerable number of segmentation masses present, and early on the

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* Mr. J. D. Gibson Carmichael, F.L.S., has kindly identified the species for me as *Julus terrestris*, Leach, 1814.

third day the first formation of the blastoderm begins. Early on the third day some of the segmentation masses make their appearance on the outside of the ovum at different parts, and there undergo rapid division, the resulting cells spreading out to form the blastoderm. At the close of the blastoderm formation, the ovum consists of an external layer of flat cells—the epiblaast—with deeply stained oval nuclei, these cells being continuous on the one hand with one another, and on the other with the cells in the yolk by means of fine processes of protoplasm. The cells in the interior of the yolk are the direct descendants of the first segmentation masses. They constitute the hypoblast.

The fate of these hypoblast cells is various; some of them are employed in the formation of the mesoblastic keel which I am about to describe, that is, in the formation of the splanchnic and somatic mesoblast. Another part gives rise to the hypoblastic lining of the mesenteron, while a third part remains in the yolk after the mesenteron is formed, and gives rise to mesoblast cells which are employed in the formation of various muscles and of the circulatory system.

With regard to the retention of the primitive connexion of the cells of the ovum until this stage, nothing of the sort has, I believe, been described before except by Sedgwick in Peripatus. The most important part it seems to me is not the connexion of cell to cell but of layer to layer by means of processes of the cells.

About the middle of the fourth day several of the hypoblast cells approach the epiblast in the middle line of what will eventually be the ventral surface of the embryo. This is the first beginning of a mesoblastic keel such as Balfour has described for *Agelena labyrinthica*. When a fair number of these cells are assembled in the middle line of the ventral surface a change takes place in the cells of the epiblast just outside them. They become more rounded, their nuclei become round; in fact they come to resemble the cells which I have described as assembling immediately below them.

The epiblast cells in the middle ventral line after altering their shape increase by division and take a considerable share in the formation of the keel. The hypoblast cells below them also increase, and on the fifth day the mesoblastic keel is complete. Both epiblast and hypoblast have taken part in the formation of this keel.

At the end of the sixth day the keel is still present, but the cells of which it is composed are becoming elongated in the direction parallel to the surface. At the same time they continue to multiply and spread themselves out so as to form two definite layers within the epiblast. These are the splanchnic and somatic layers of the mesoblast. The cells of the splanchnic and somatic mesoblast are connected.

On the seventh and eighth days the keel gradually disappears, and the layers of mesoblast spread round a great part of the embryo, rather more than half way round. On the ventral surface the epiblast cells assume a columnar form, thus giving rise to the ventral plate.

The mesoblast now becomes thicker on each side of the middle ventral line. Both layers are concerned in this thickening, and at these points the two layers become indistinguishable. Outside the thickenings, that is further away from the middle ventral line, the two layers are closely applied to each other, and to the epiblast as before. The effect of these changes is that the greater part of the mesoblast is now arranged in two parallel longitudinal bands along the ventral surface of the embryo; these bands being connected across the middle line by a thin portion consisting of a single layer.

The two longitudinal bands now begin to be constricted off into the mesoblastic somites. The latter are formed from before backwards, and their position corresponds with that of the future segments of the body. The number of the somites is eight, corresponding with that of the eight segments with which the embryo is finally hatched. The somites are at first solid, afterwards a cavity appears in them.

Early on the ninth day the stomodæum is formed as an invagination of the epiblast near one end of the ventral surface. Shortly after the first formation of the stomodæum, the proctodæum appears as a shallow somewhat wide invagination of the other end of the ventral surface.

The body-segments already established by the segmentation of the mesoblast now become more apparent, each being marked by a deep transverse furrow in the epiblast. The hypoblast cells are still present within the yolk, but are gradually becoming collected in the median line, just below the mesoblastic bands. The stomodæum and proctodæum become more deeply invaginated, extending a considerable distance into the yolk, and at the same time the hypoblast cells begin to form the mesenteron, arranging themselves around a central lumen.

On the tenth day the ventral flexure is formed by a deepening of the transverse furrow between the seventh and eighth segments. It is, therefore, first formed nearer the anal end of the embryo. As the furrow deepens and the embryo increases in size, the last segment grows in length. At the same time the embryo curves round towards the ventral surface. The effect of this is that the end segment is bent round against the head. The eighth segment is considerably longer than the others except the head, and the tissues there show a considerable difference. Even as late as the twelfth day, when the nervous system is far developed in all other parts of the body, in the eighth segment the tissues are imperfectly differentiated, the nerve cords not showing any ganglia, but lying on the epiblast and not quite separated from it. At a later period of development the anal segment is constricted off from this segment, while from its anterior part the future segments formed in the course of development are developed.

Just before the appearance of the ventral flexure the embryo develops a cuticular envelope over the whole surface of the body. This is the so-called amnion of Newport. Just before the formation of the ventral flexure the nervous system is formed. The first traces of this consist in a thickening of the epiblast on each side of the middle line. This is soon followed by the formation of a shallow furrow between the thickened parts; this longitudinal furrow corresponds with that described by Metschnikoff in Strongylosoma. The bilobed cerebral ganglia are formed first, and the nerve cords are formed from before backwards, a pair of ganglia being present for each segment except the last. The posterior portion of the nerve cords is completed at a considerably later stage of development. The nerve cords are widely separated, but are connected by a thin median portion. In later embryonic life they are closely approached to one another, and almost form one cord.

On the eleventh day the embryo has increased considerably in size. The ventral flexure is complete, and the animal lies with the long end segment folded closely against the rest of the body, the end of the tail being against the stomodæum. The nervous system is now completely separated from the epiblast, and the epiblast has assumed the adult form. It now separates a second membrane like that which is formed on the tenth day.

The splanchnic layer of mesoblast covers the mesenteron, the stomodæum, and proctodæum.

Within the yolk, which is still present in great quantity in the body cavity, there are present a number of hypoblast cells. These, as have already been mentioned, give rise to the circulatory system and to various muscles. They may, therefore, be now considered as mesoblastic cells which have been directly derived from the hypoblast.

On the twelfth day the Malpighian tubes are formed as blind outgrowths of the proctodæum, the nervous system is further developed, and the first rudiments of the appendages begin to appear. Late on this day the animal is hatched with only the rudiments of its appendages. I propose to reserve a full description of this stage for a future paper.

III. "On Radiant Matter Spectroscopy: Note on the Spectra of Erbia." By WILLIAM CROOKES, F.R.S. Received Jan. 7, 1886.

I have recently succeeded in getting the earth erbia in a sufficiently pure state to allow me to examine its phosphorescent spectrum without the interference which might be produced by the presence of yttria, samaria, holmia, thulia, $Y\alpha$ or ytterbia. As in the case of yttria* the spectrum is best seen when erbic sulphate is heated to redness and submitted to the electric discharge in a high vacuum. The addition of calcic sulphate interferes with the purity of the spectrum. In this respect erbia differs from samaria, as the latter earth seems to require the presence of some other metal to develop its phosphorescent properties.

The phosphorescent spectrum of erbia consists of four green bands, of which the following measurements have been taken :—

Scale of spectroscope.	λ	$\frac{1}{\lambda^2}$	Remarks.
9·750°	5564	3230	Approximate centre of a wide band, shading off at each side.
9·650°	5450	3367	Approximate centre of a band, narrower and somewhat fainter than the first band.
9·525°	5318	3536	Approximate centre of a narrow band, bright and moderately sharp on each side.
9·400°	5197	3702	Approximate centre of a band, similar in appearance to the first band, but brighter.

Fig. 1 shows the erbia phosphorescent spectrum drawn to the $\frac{1}{\lambda^2}$ scale.

These bands do not correspond in position to any in either the yttrium or samarium spectrum. The nearest approach to a coincidence is between the first erbia green and the samarium green, but when the two spectra are examined one over the other it is seen that the samarium band is less refrangible than the erbium band.

The first green of $Y\alpha$ occurs midway between the first and second greens of erbia, and the second $Y\alpha$ green comes between the second and third erbia greens.

Pure erbia is of a beautiful rose-pink colour.† When illuminated

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* "Phil. Trans.," vol. 174, p. 913, par. 71.

† Rose-coloured erbia has already been obtained by Professor Cléve, who a year ago presented me with a specimen of the earth as pure as the one which is the subject of this paper.

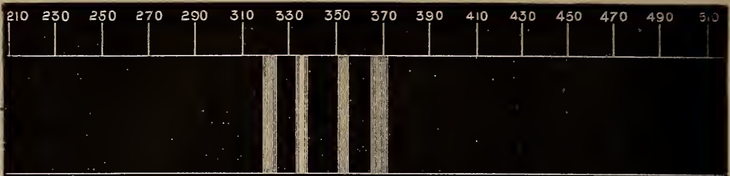

Fig. 1.

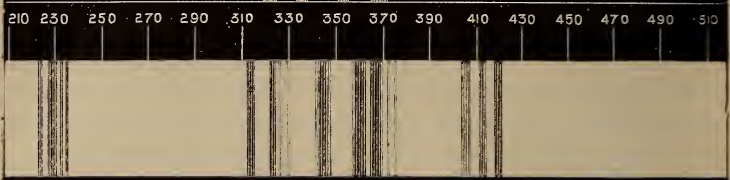

Fig. 2.

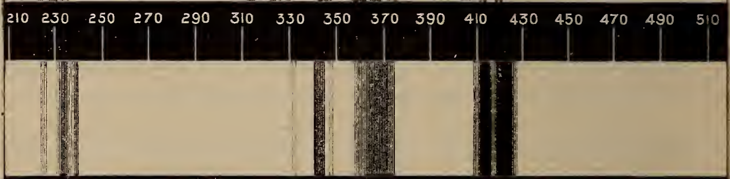

Fig. 3.

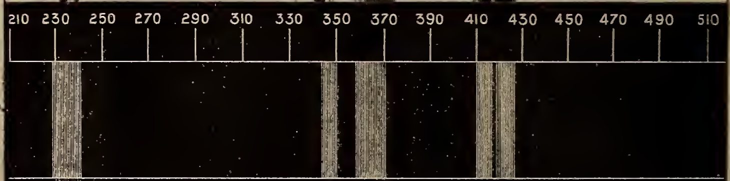

Fig. 4.

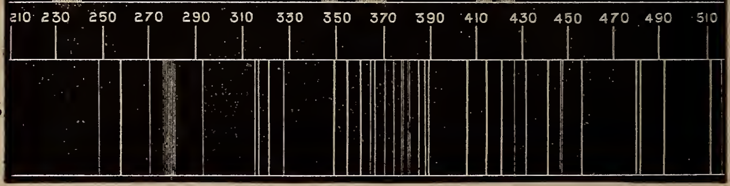

Fig. 5.

by sun or electric light and examined in the spectroscope it gives a spectrum of black lines and bands as sharp and distinct as the Fraunhofer lines. Fig. 2 shows the erbia spectrum by reflection. It is strange that this most characteristic property has been recorded by so few observers. Indeed, the only notice of it I have come across is a passing remark of Professor Cléve's that "the light reflected by dry erbia shows absorption-bands."

Fig. 3 shows the absorption spectrum given by a solution of pure erbic chloride. It differs in some respects from the drawings mapped from older observations, as the absorption lines of holmia and thulia are absent. The fine group of lines in the green of the reflection spectrum is also absent in the absorption spectrum.

The spectrum of bright lines emitted when erbia is rendered incandescent in the blowpipe flame has been often observed, but the lines in this case are luminous on a fainter continuous background and are not particularly sharp, whilst the reflection spectrum consists of black lines sharply defined on a continuous spectrum.

The spectrum emitted by incandescent erbia is shown in fig. 4.

Fig. 5 shows the characteristic lines in the spark spectrum of erbium, taken from a concentrated acid solution of erbic chloride, with a Leyden jar in a shunt circuit.

I have thought it advisable to give these five spectra of erbium, as they show how entirely different the phosphorescent spectrum is to any other spectrum given by this element.

IV. "On the Clark Cell as a Standard of Electromotive Force." By the Lord Rayleigh, M.A., D.C.L., Sec. R.S. Received January 7, 1886.

(Abstract.)

This paper, supplementary to that "On the Electrochemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells,"[*] gives the further history of the cells there spoken of, and discusses the relative advantages of various modes of preparation. The greatest errors arise from the liquid failing to be saturated with zinc sulphate, in which case the electromotive force is too high. The opposite error of *super*-saturation is met with in certain cases, especially when the cells have been heated during or after charging. Experiments are detailed describing how cells originally supersaturated have been corrected, and how in others the electromotive force has been reduced by the occurrence of supersaturation consequent on heating. If these errors be avoided, as may easily be done; if the mercury be pure (preferably distilled *in vacuo*), and if either the paste be originally neutralised (with zinc carbonate), or a few weeks be allowed to elapse (during which the solution is supposed to neutralise itself), the electromotive force appears to be trustworthy to $\frac{1}{1000}$ part. This conclusion is founded upon the comparison of a large number of cells prepared by the author and by other physicists, including Dr. Alder Wright, Mr. M. Evans, Dr. Fleming, Professor Forbes, and Mr. Threlfall.

As regards temperature coefficient, no important variation has been discovered in saturated cells, whether prepared by the author or by others. In all cases we may take with abundant accuracy for ordinary applications—

$$E=1·435\{1-0·00077(t-15°)\},$$

the temperature being reckoned in centigrade degrees. For purposes of great delicacy it is advisable to protect the standards from large

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[*] "Phil. Trans.," vol. 175, 1884.

fluctuations of temperature. Under favourable circumstances two cells will retain their relative values to $\frac{1}{10000}$ for weeks or months together.

Unless carefully sealed up, the cells lose liquid by exudation and evaporation, and then the electromotive force gradually falls. Marine glue appears to afford a better protection than paraffine-wax, and there seems to be no reason why cells thus secured should not remain in good order for several years.

In cells of the H-construction (§ 29 of former paper) the leg containing the amalgam (but not the one containing pure mercury) is liable to burst, apparently in consequence of a tendency to alloy with the platinum. Protection with cement of the part of the platinum next the glass has been tried, but no decisive judgment as to the adequacy of this plan can as yet be given.

Recent cells, intended for solid zincs, have been made of a simplified pattern—nothing more, in fact, than a small tube with a platinum wire sealed through its closed end. The zincs are not recast, and the paste is prepared from (unwashed) mercurous sulphate rubbed up in a mortar with *saturated* solution of zinc sulphate and a little zinc carbonate. A stock of paste may be prepared and retained for use in a bottle.

Experiments are described tending to prove that the irregularities observed during the first few weeks of the life of a cell prepared with acid materials, have their origin principally at the mercury electrode.

Cells prepared with dilute solutions have a lower temperature coefficient (about 0·00038), but would be more difficult to use as standards whose value is to be inferred from the mode of preparation.

Details are given of H-cells charged with amalgams of zinc and mercury in both legs, without mercurous sulphate. A very small proportion of zinc is sufficient to produce the maximum effect. Pure mercury, neither alloyed with zinc nor in contact with mercurous sulphate, has an uncertain electromotive value.

Since the comparison of cells does not absolutely exclude a small general alteration of electromotive force with age, further determinations of the standard cell (No. 1) have been effected by means of the silver voltameter. The results—

Table XVIII.

Date.	E.M.F. of No. 1 at 15° C. in B.A. volts.
October, 1883, to April, 1884	1·4542
November, 1884	1·4540
August, 1885	1·4537

are very satisfactory, and indicate a constancy sufficient for almost all practical purposes.

Finally, some comparisons are given between Clark cells and Daniells, with equi-dense solutions, both of Raoult's pattern and of that described recently by Dr. Fleming.

V. "Account of a new Volcanic Island in the Pacific Ocean." By WILFRED ROWELL, H.B.M. Consul in Samoa. In a letter to the Hydrographer of the Admiralty. Received January 17, 1886.

Hydrographic Department, Admiralty, S.W.,
16th January, 1886.

SIR,

I HAVE the honour to forward to you a photograph and a copy of a letter received from H.M. Consul at Samoa, relating to a volcanic island recently formed by a submarine volcano, in the vicinity of the Friendly Group in the Pacific Ocean, which I think may be of interest to the Royal Society.

I also forward a chart of the locality showing the position of the new island.

It is unfortunate that, as the hydrographical knowledge of the vicinity is very imperfect, no information exists as to the depths from which this island has pushed its way.

(Signed) W. J. L. WHARTON,
Hydrographer.

The Secretary, Royal Society.

(Enclosure.)

H.B.M. Consulate, Samoa,
November 21st, 1885.

SIR,

I HAVE the honour to report that whilst on a passage from the "Friendly" Islands to the "Navigators" Islands, on board the steam ship "Janet Nicoll," we observed a newly-risen volcanic island. I was informed in "Tongatabu" that the eruption was first remarked from that island (a distance of over 40 miles) on Tuesday, 13th of October. We passed it on Sunday, the 8th of November, at a distance of about 1½ miles.

The following will be the approximate position by compass bearings:—

Peak of "Kao" Island over centre of "Tefoa" Island, north by east.

West end of "Honga Tonga" Island, south by east.

Centre of crater from ship west 1½ miles.

The island appeared to be about two miles in length north by west and south by east, of about 200 feet in height, having a reef on the northern extremity of half a mile in extent, with one also to the southward of 1½ miles (approx.).

I have the honour to enclose a photograph which I took at the time the island bore west 1½ miles, and which although very inferior and taken under circumstances of considerable difficulty, will give some idea of the appearance of the eruption.

(Signed) WILFRED ROWELL,
H.B.M. Consul, Samoa.

To the Hydrographer of the
Admiralty, Whitehall.

January 28, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Local Magnetic Disturbance in Islands situated far from a Continent." By Staff-Commander E. W. Creak, R.N., F.R.S., of the Admiralty Compass Department. Received January 11, 1886.

[Plate 1.]

It has been known for many years past that in the islands of St. Helena and Ascension observations of the three magnetic elements made at different stations gave remarkably divergent results, caused by some undefined local magnetic attraction.

Thus in the observations at St. Helena,* carried out under the auspices of the British Government in 1840–46, it was found that the observed inclination and intensity differed considerably at the two stations of Longwood and Sister's Walk, 2½ miles apart, and these again differed from those made on board ships in the anchorage.

This question of local magnetic disturbance is one which has engaged the attention of observers on continents and islands adjacent to them, in making magnetic surveys, when sometimes large areas have been found affected, and in others but very small ones. In such cases the normal values of the three magnetic elements have been obtained either by calculation from observations made in supposed undisturbed districts, or by graphic methods.

Thus in discussing the results of the magnetic survey of Scotland, made by the late Mr. Welsh in 1857–58,† Professor Balfour Stewart obtained the values of the local disturbances in the islands of Skye and Mull by calculating the normal lines of equal values of the magnetic elements for the mainland of Scotland, and extending beyond the adjacent islands on the west coast.

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* See pp. 60, 61, vol. i of Observations made at the Magnetical and Meteorological Observatory at St. Helena. Published under the superintendence of Lt.-Colonel Sabine, 1847.

† Magnetic Survey of Scotland. See Report of British Association, 1859.

As an instance of unusual local disturbance in that part of the British Islands, the Compass Hill in the island of Canna,* near Skye, may be mentioned. Here the disturbance was sufficient to render a compass useless, and every small movement of the observing instruments gave different results.

For Ascension and St. Helena, and some other islands situated far from a continent, normal values of the magnetic elements have been obtained from observations at sea, and the object of this paper is to show how this may be done, and the resulting amount and nature of the disturbances in the islands visited.

The following values of the three magnetic elements observed at eleven islands represent the data collected for this purpose.

An inspection of them shows that they consist of a series of observations made on land, and which when made at different stations on the same island give divergent results caused by some local magnetic disturbance.

In order to obtain undisturbed or normal values, observations made on board ships, in which the amount of magnetic disturbance is known, have been adopted.

These normal values obtained on board ship are from observations made with the ship's head placed on eight or sixteen azimuths equally distributed round the horizon in the process called "swinging," all effects of the iron of the ship being eliminated by the methods set forth in the "Magnetical Instructions for H.M.S. 'Challenger.' "

It is important to note that these swingings took place in the neighbourhood of the island to be magnetically examined, and some small corrections had to be applied for differences of geographical position from the land stations. An example is given below of the method of deriving the normal values of the magnetic elements for a position on the green outside the Dockyard, Bermuda.†

The observations have been divided into two sections—first, those made on islands situated north of the magnetic equator, and in which the local disturbances have been generally found to be due to an excess of blue‡ magnetism above the normal; secondly, those made on islands situated south of the magnetic equator, in which the disturbances are generally caused by an excess of red magnetism.

Section I.—Islands situated North of the Magnetic Equator.

Bermuda Islands.

The Bermuda group is that on which a more complete series of

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* Topographically and magnetically examined by the late Captain Evans, R.N.

† This position has been selected as the declination is there almost undisturbed. It appears to be a suitable place for future observations.

‡ In a freely suspended magnet the north-seeking end has red magnetism, the other end blue magnetism.

observations has been made than in the other islands, and as the results are remarkable they are the first to be discussed.

Table I shows some values of the declination observed previously to the "Challenger's" visit in 1873, but reduced to that epoch by allowing an annual change of 2′ increasing. This annual change has been deduced from the best available observations at four stations.

Table I.—Declination at Bermuda, 1873.

Station.		Declination.	Difference from normal.	Observer.
Bastion C, Ireland Island		7° 24′ W.	+0° 6′	Hill.
" "		8 00 "	+0 42	Barnett.
" "		6 26 "	−0 52	Bodie.
Fort Cunningham Flagstaff		5 00 "	−2 18	Barnett.
Hen Island Magazine		7 54 "	+0 36	"
(Near) lighthouse on Gibbs' Hill		3 56 "	−3 22	Bodie.
In the dockyard		7 30 "	+0 12	Hill.
Mount Langton		9 00 "	+1 42	Lefroy.
Anchorage in Grassy Bay.	H.M.S. "Boscawen"	7 12 "	−0 06	Bodie.
	" "Cornwallis"	7 13 "	−0 05	"
	" "Pembroke"	7 19 "	+0 01	"
Green outside dockyard		*7 18* "	*Normal*	"*Challenger.*"

In Table II are recorded the magnetical results of the "Challenger" observed at Bermuda in 1873.

In Tables I and III will be found the differences between the observed magnetical elements and the normal values at a station on the green outside the dockyard, deduced from the results of swinging in a position 15 miles south of it.

Results of swinging at sea, corrected for effects of ship's iron.	Correction for diff. of geographical position.	Resulting normal values at green outside dockyard.
Declination, 7° 13′ W.	+ 5′	*7° 18′* W.
Inclination, 65 18 N.	+15′	*65° 33′* N.
Total force, 12·22	+0·03	*21·25*

In the accompanying diagram No. 1 the differences of declination are shown at each station, and in diagram No. 2 the differences of inclination and vertical force.

These results appear to point to the existence of a strong focus of blue magnetism situated above the position indicated by the dotted

H.M.S. "Challenger." Results of Magnetical Observations at the Bermuda Islands, 1873.

Table II.—Elements of Terrestrial Magnetism observed on Land.						Table III.—Differences of Magnetic Elements from the Normal Values.			
Place of observation.	Date.	Declination.	Inclination.	Total force.	Vertical force.	Declination.	Inclination.	Vertical force.	Remarks.
	1873.								
Tatem Island............	June 5	5° 56′ W.	67° 02·8′ N.	..	..	−1° 22′	+1° 30′	..	Differences of the declination at positions of swinging:— "Boscawen" = −0° 6′ "Cornwallis" = −0 5 "Pembroke" = +0 1
Cricket ground, Somerset Island................	April 7	5 09	65 44·2	12·345	11·259	−2 09	+0 11	+0·112	
Ducking stool, Mt. Langton	June 6	6 59	66 44·7	12·129	11·146	−0 19	+1 12	−0·001	
Spanish Point	April 12	7 42	67 19·8	12·417	11·461	+0 24	+1 47	+0·314	
Governor's garden, Mt. Langton	" 11	8 54	67 20·0	12·362	11·410	+1 36	+1 47	+0·263	
Cemetery, Ireland Island,.	" 9	5 58	66 27·6	..	..	−1 20	+0 55		
Sparyard, Ireland Island ..	June 2	6 04	66 33·8	..	..	−1 12	+1 01		
Clarence Cove............	" 3	9 57	66 44·0	12·443	11·435	+2 39	+1 11	+0·288	
Green outside dockyard ...	May 13	7 10	66 25·8	12·309	11·275	−0 08	+0 53	+0·128	
Ditto, assumed normal values reduced from results of swinging.......	" 31	7 18	65 33·0	12·250	11·147	0 0			
Cobbler's Island..........	June 11	8 08	..	..	..	+0 50			
St. George's, Button Island	April 12	7 15	..	..	..	−0 03			
Octopus Island	" 14	5 4	..	..	..	−2 14			
Barge Island.............	" 14	4 13	..	..	..	−3 05			
Wreck Hill..............	June 11	5 20	..	..	..	−1 58			
Boaz Island Bridge.......	" 4	5 11	..	..	..	−2 07			

line of the diagram, the red ends of the declination and dipping needles being attracted towards this focus with a force varying according to the place of observation.

Thus in the case of the declination at all stations situated to the north-east of the focus, the westerly declinations are seen to be in excess, and those to the south-west in defect of the normal value.

In the inclination and vertical force a great increase of value may be seen in passing from the seaward side of the islands towards the area enclosed by the dotted line of the diagram.

A portion of the disturbance just noticed may possibly be due to the ferruginous nature of the soil at Bermuda;* but this does not detract from the evidence just adduced of the existence of a strong focus of blue magnetism about the position assigned to it in the diagram.

In the eastern extremity of the group there are also evidences of local magnetic disturbance at the observing stations of Hen Island, Button Island, and Fort Cunningham.

Madeira.

	Declination.	Inclination.	Total force.	Observer.
Casa Branca..........	16° 49′ W.			H.M.S. “Challenger,” 1873.
”	17 8	56° 14′ N.	8·784	
Cliff west of Loo Rock .	18 25			
” ”	19 35			
” ”	20 33	55 12	9·184	
Normal values	*20 33*	*56 36*	*9·49*	

The above observations of declination, made near Funchal, on the south side of the island, differ considerably (with one exception) from the normal, 3° 44′ being the greatest difference.

There is also much disturbance in the inclination. In one position visited by the officers of the “Challenger,” and at 1 foot above the ground, a value of 48° 46′ was observed, and at the usual height of the observing stand—$3\frac{1}{4}$ feet—over the same spot, 56° 18′. At two other positions 20 yards apart, the inclination differed 40′.

The greatest difference in the values of the total force is a decrease of 0·71 below the normal.

These results point to the importance of adopting an uniform height for the observing stand if comparable results are to be obtained.

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* See “Remarks on the Chemical Analyses of Samples of Soil from Bermuda.” By General Sir J. H. Lefroy. Bermuda, 1873.

Teneriffe. Canary Islands.

	Declination.	Inclination.	Total force.	Observer.
Sta. Cruz	21° 48′ W.	55° 18′ N.	9·546	H.M.S. "Challenger," 1873.
Normal values	*20 00*	*52 42*	*9·230*	

At this island observations were only made at one land station, but there is evidence of a strong development of blue magnetism at the observing station.

Santa Cruz is on the east side of the island, and the westerly declination was found to be in excess of the normal value by 1¾°. The inclination was 2½°, and the force 0·3 in excess of their respective normals.

St. Vincent. Cape de Verde.

	Declination.	Inclination.	Total force.	Observer.
Porto Grande	17° 3′ W.	43° 6′ N.	8·577	H.M.S. "Challenger," 1876.
" "	18 52	..	..	
Normal values	*19 54*	*42 52*	*8·540*	

The observations were made on the west side of the island. Here the principal disturbance occurs in the westerly declination, which is nearly 3° in defect of the normal value. This points to the presence of increased blue magnetism on the inland side of the observing station.

The inclination and force are but little affected.

St. Paul Rocks. (Atlantic Ocean.)

	Declination.	Inclination.	Total force.	Observer.
Land station........	16° 22′ W.	22° 32′ N.	7·00	H.M.S. "Challenger," 1873.
Normal values	*15 51*	*22 30*	*6·94*	

At these rocks the declination and total force are slightly disturbed, the inclination agreeing well with the normal.

Sandwich Islands.

	Declination.	Inclination.	Total force.	Observer.
Honolulu .... *Normal values*	9° 34′ E. *8 47*	39° 57′ N. *39 32*	8·512 *8·450*	H.M.S. "Challenger," 1875.
Hilo, Cocoanut Island *Normal values*	7° 29′ E. *8 15*	38° 39′ N. *37 32*	8·698 *8·330*	Ditto

At Honolulu blue magnetism is predominant at the observing station, which is on the west side of the island, the easterly declination being $\frac{3}{4}$° in excess of the normal. The inclination and force are slightly increased.

A marked difference is seen between the observed declinations at Honolulu and Hilo. At the first-named place the easterly declination observed on the *west* coast is increased $\frac{3}{4}$°, whilst at the latter the easterly declination observed on the east coast is *decreased* $\frac{3}{4}$°.

The large increase in the observed inclination and force at Hilo above the normal, coupled with the diminished declination, point to a strong development of blue magnetism in that portion of the island visited.

This concludes our list of islands in which, with the exception of Madeira, blue magnetism appears to predominate. Madeira, however, requires a much more extended series of observations to be made on its shores before any conclusions can be drawn as to the prevalence of either blue or red magnetism.

Section II.—Islands situated South of the Magnetic Equator.

Ascension.

	Declination.	Inclination.	Total force.	Observer.
Georgetown .... Green Mountain .... *Normal values*	23° 6′ W. 22 32 *22 41*	7° 56′ S. 9 57 *7 37*	6·133 6·217 *6·100*	H.M.S. "Challenger," 1876.

The observations at this island are complete at two stations. At Georgetown, on the coast, there is but little local disturbance, but at the Green Mountain Station the observed inclination exceeds the normal in value by $2\frac{1}{3}$°, the force by 0·12 (nearly), thus pointing to an excess of red magnetism.

St. Helena.

	Declination.	Inclination.	Total force.	Observer.
Longwood Magnetic Observatory	22° 48′ W.	21° 21·7′ S.	6·030	Observatory results.
Sister's Walk*	..	18 16 20 3	6·287 ..	Ross Crozier } 1840.
Castle gardens	19 38	...	..	Ross, 1840.
At the anchorage	22 17	17 55	..	Dupetit Thouars, 1839.
Normal values	*22 53*	*18 37*	*6·075*	Ross, 1840.

At Longwood Magnetic Observatory the declination is apparently undisturbed, whilst in the Castle Gardens it differs 3¼° from the normal value. The inclination at Longwood differs 2¾° from the normal, pointing to the presence of an excess of red magnetism in that locality. The total force is not much disturbed.

At Sister's Walk the inclination at two stations, 50 yards apart, differs 1¾°, one being in excess of the normal by 1½° nearly, the other twenty minutes in defect; the larger disturbance being due to red magnetism. The total force is increased as much as 0·2.

The result of the inclination by Dupetit Thouars, which is 42′ below the value derived from Ross's observations, seems to indicate that a lower value might be accepted as the normal.

Tristan d'Acunha.

	Declination.	Inclination.	Total force.	Observer.
Near Julia Point	23° 5′ W.	40° 40′ S.	Not observed	H.M.S. "Challenger," 1873.
Normal values	*21 15*	*41 42*	*6·36*	

The observing station at this island was situated on the N.W. coast near some cliffs extending to the eastward. The westerly declination is increased about 1¾° above the normal, but as the inclination is slightly affected by blue magnetism, it is uncertain whether it is the red magnetism in the adjacent cliffs to the eastward repelling the red end of the compass needle, or the blue magnetism near it which causes the increase of the declination.

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* At Sister's Walk, Crozier's observing station was 50 yards S.S.E. of Ross' station. The dip circles were interchanged at the time to prove the difference observed was not due to instrumental error.

Kerguelen Island.

	Declination.	Inclination.	Total force.	Observer.
Christmas Harbour ..	33° 38′ W.	70° 50′ S.	11·032	H.M.S. "Challenger," 1874.
Howe's Foreland	..	72 0	..	
Normal values	*34 35*	*70 53*	*11·003*	
Accessible Bay	33° 34′ W.	..	..	H.M.S. "Challenger," 1874.
Betsy Cove	34 0	71° 47′ S.	11·422	
Normal values	*34 57*	*71 7*	*11·087*	
Observatory Bay	35° 48′ W.	71° 56′ S.	11·143	Perry, 1 Jan., 1875.
Swain's Haulover....	..	71 0	10·288	
Thumb Peak........	..	71 7	..	
Hog Island	35 54	..	..	H.M.S. "Challenger," 1874. 1 Jan., 1875.
Normal values	*35 20*	*71 21*	*11·171*	

The "Challenger" was not swung in the neighbourhood of Kerguelen Island, and the normal values have therefore been derived from the results of single observations made at sea between the swinging, at the Cape of Good Hope, and that in lat. 63° 30′ S., long. 90° 47′ E. They are therefore less exact, but sufficiently so for the purpose of showing that the disturbances on the island proceed from red magnetism.

Thus, at Christmas Harbour, Accessible Bay, and Betsy Cove, where there was high land to the westward of each observing station, the westerly declination is about 1° less than the normal. At Howe's Foreland and Betsy Cove the inclination is increased, and at the latter place the total force is considerably above the normal. At Observatory Bay and the adjacent stations the disturbance is comparatively of a moderate amount, except in the total force at Swain's Haulover, which shows an unusually large diminution.

Juan Fernandez.

	Declination.	Inclination.	Total force.	Observer.
Near Fort St. Juan Bautista	..	39° 40′ S.	8·138	H.M.S. "Challenger," 1875.
Normal values.......	*16° 40′* E.	*37 54*	*7·860*	

The sea observations from which the normal values have been deduced were made at two positions—one before, the other after the ship's arrival at Juan Fernandez.

The above observations, like those at St. Helena and Ascension, show the existence of an excess of red magnetism above the normal from the increased values of the inclination and total force.

The general results of the observations just discussed is to show that in islands far from a continent and north of the magnetic equator, the local disturbances of the three magnetic elements are caused by an excess of blue magnetism above the normal values due to the position of the islands on the earth considered as a magnet. South of the magnetic equator red magnetism is in like manner predominant.

Considering, however, that the observations were made with the instruments between 3 and 4 feet above the ground, the disturbances are not large.

As an instance of large disturbance the results obtained at the bluff, Bluff Harbour, in the South Island, New Zealand, may be mentioned. In 1857, during the land survey by the local government officials, the following values of the declination were observed.*

On the summit of the bluff......	6° 54′ E.
30 feet north of the same position......	9 36 W.
„ west „	5 04 E.
„ east „	46 44 E.
Normal from sea observations..........	16 20 E.

On the summit of the bluff there was thus shown to be a strong focus of red magnetism.

During the survey of the South Island by the officers of H.M.S. "Acheron," it was found necessary to give up the use of compass-bearings at this place, and adopt the plan of observing nothing but true bearings.

The evidences of local magnetic disturbance form a great difficulty in estimating the values of the secular change in these islands for past years. For example, Madeira may be mentioned, where it has been seen that a change in position of a few feet gave very different results of the inclination, and even at the same position the height at the observing instrument above the ground must be taken into consideration if comparable results are to be obtained.

Before concluding this paper, I desire to draw attention to the following remarks on this subject of local magnetic disturbance.

Firstly, reasons have been given for believing "that terrestrial magnetism is not produced in any important degree by magnetic forces external to the earth."†

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* "Transactions New Zealand Institute," 1873, vol. vi, p. 7.

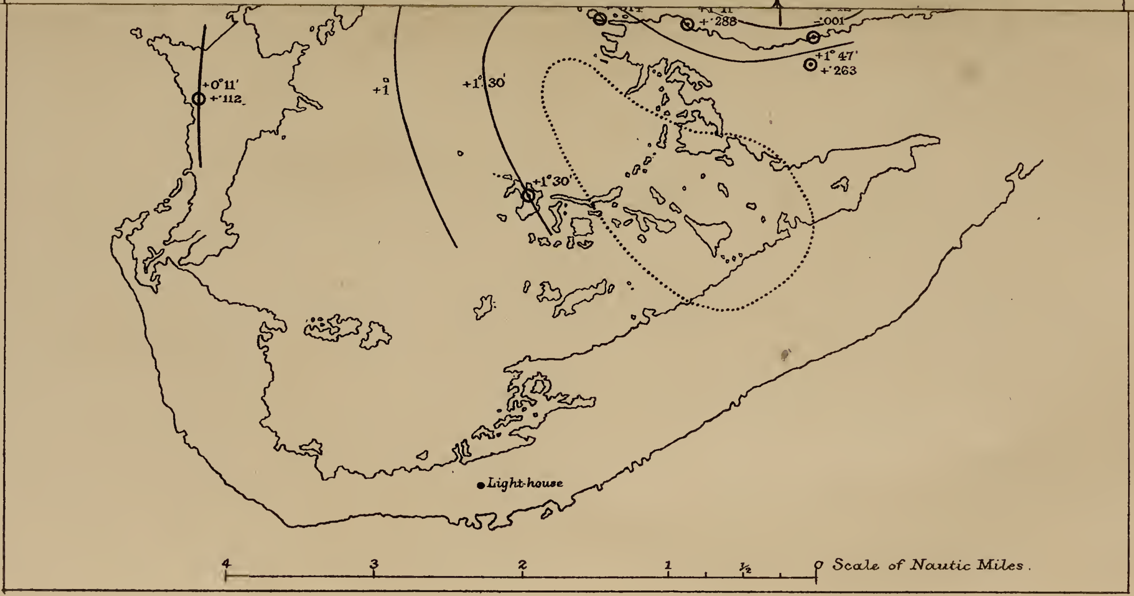
† See "Treatise on Magnetism," p. 100, par. 43, by Sir G. B. Airy, K.C.B., Ast. Royal, 1870.

No. 1.

Bermuda Islands. Western Portion.

Declination { The Figures in this plan are taken from Tables I and III.
The dotted line shows assumed position of blue focus.

The black curves denote equal values of disturbance in the Declination.



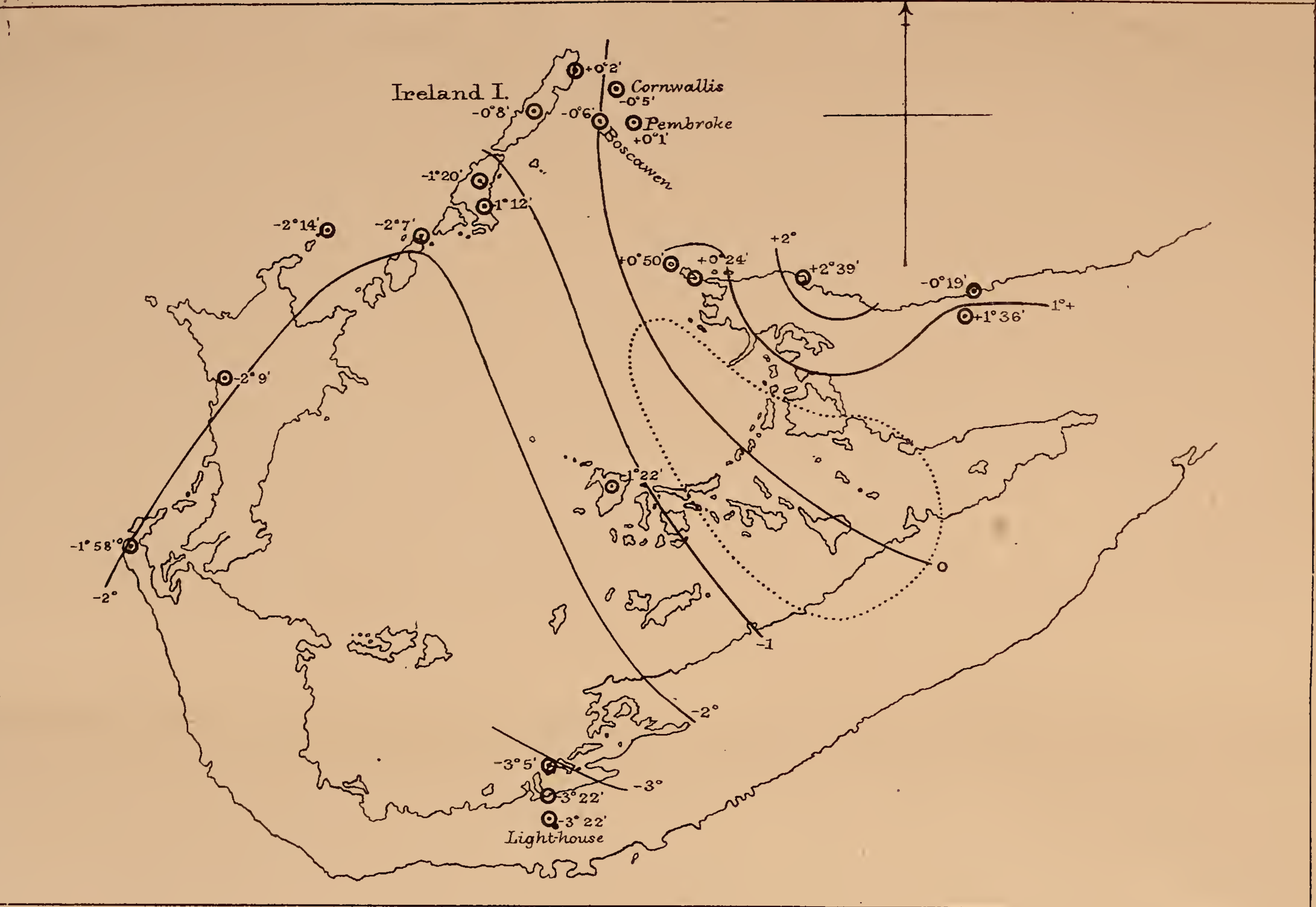
West, Newman & Co. lith.

N° 1.

Bermuda Islands. Western Portion.

Declination { The Figures in this plan are taken from Tables I and III.
The dotted line shows assumed position of blue focus.

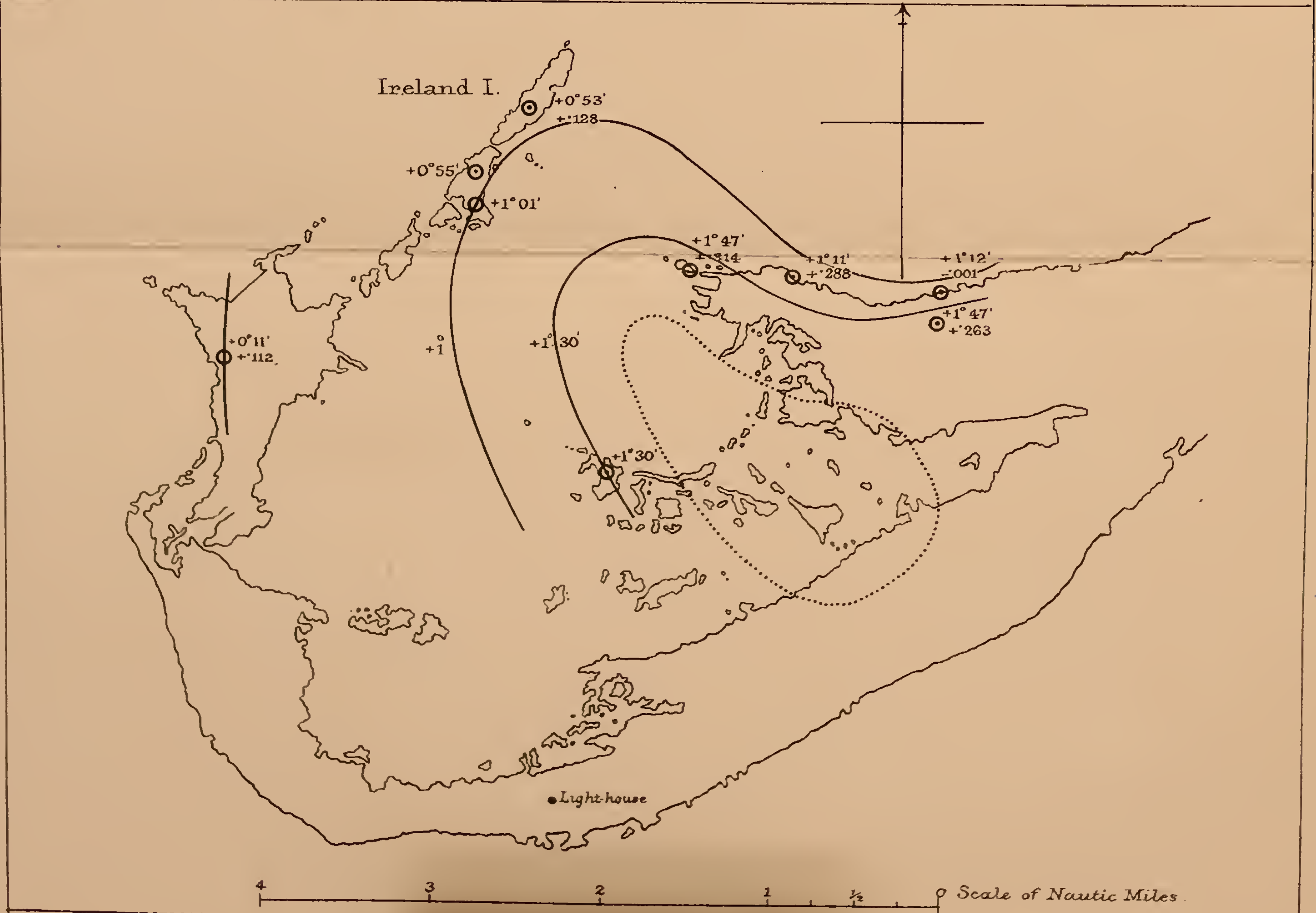
The black curves denote equal values of disturbance in the Declination.



N° 2.

Inclination
Vertical Force { The Figures in the plan are taken from Table III.
The dotted line shows assumed position of blue focus.

The black curves denote equal values of disturbance in the Inclination.



West, Newman & C° lith.

Secondly, "that terrestrial magnetism does not reside in any important degree on the earth's surface" . . . . . "and therefore the source of magnetism must lie deep."*

In view of these reasons and the results obtained from the observations recorded in this paper, I draw the possible conclusion: That the increase of magnetic force observed in the islands under discussion proceeds from portions of those islands which have been raised to the earth's surface from the magnetised part of the earth, forming the source of its magnetism.

II. "Description of some Remains of the Gigantic Land-Lizard (*Megalania prisca*, Owen) from Queensland, Australia, including Sacrum and Foot-Bones. Part IV." By Sir Richard Owen, K.C.B., F.R.S., &c. Received January 13, 1886.

(Abstract.)

The author, continuing to receive through the kindness of Dr. Bennett, F.L.S., of Sydney, New South Wales, and of Mr. George F. Bennett, Corr. M. Zoological Society, Toowoomba, Queensland, Australia, fossil remains of *Megalania* from a drift-bed of King's Creek, selected therefrom specimens contributing to the restoration of *Meg. prisca*, and which he had not obtained at the dates of communication of the papers to the Royal Society which have appeared in the "Phil. Trans." for the years 1864, 1880, and 1881.

These specimens add to the characters of the sacrum, and give those of the terminal segment of one of the limbs of the extinct horned Saurian.

The metapodial series are remarkable for the great breadth in proportion to the length of the bone; that of one of the digits being as broad as long, and testifying to its character as a metatarsal by the distal trochlea for the articulation of a proximal phalanx. The digits were unguiculate, indicative of terrestrial life and locomotion.

The subjects selected for description in the present paper are illustrated by drawings of the natural size, which accompany the text.

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* See *idem*, p. 101, par. 44.

III. "On the Development of the Cranial Nerves of the Newt." By Alice Johnson, Demonstrator of Biology, Newnham College, Cambridge, and Lilian Sheldon, Bathurst Student, Newnham College, Cambridge. Communicated by Professor M. Foster, Sec. R.S. Received January 14, 1886.

The peripheral nervous system of the Newt does not begin to develop until the medullary canal has become completely separate from the external epiblast. A neural ridge appears on the dorsal surface of the medullary canal, and, at regular intervals, paired lateral prolongations of it grow out and form the spinal and cranial nerves. The former grow down between the medullary canal and muscle-plates, the original outgrowth, in each case, becoming the dorsal root, while the ventral root is formed later.

In the head, where there are no muscle-plates, the nerves are situated nearer to the surface. The 3rd, the 5th, the common rudiment of the 7th and 8th, the 9th, the 10th, and, probably, the olfactory nerves, all develop as outgrowths of the neural ridge. We have not traced the development of the 4th and 6th nerves.

At first the cranial nerves are of necessity attached to the dorsal surface of the brain, since they arise from the neural ridge. The attachment next widens out, extending further down the side of the brain. Later, the dorsal part of the attachment is lost, the ventral part alone remaining as the permanent root. There is no secondary attachment entirely distinct from the first, and no part of the nerve is at any time situated dorsal to the root. This description applies specially to the 5th and 7th nerve-roots, but it is probably true for all the cranial nerves in which the shifting of the roots takes place.

As soon as the cranial nerves become apparent, a series of thickenings of the inner layer of the epiblast appears on each side of the head. These thickenings, which are paired, are situated slightly above the level of the notochord, and correspond respectively to the 5th, 7th, 9th, and probably also to the 10th nerves. They are destined to give rise to the mucous canals of the head. The olfactory organ and ear have similar relations to the 1st and 8th nerves, and may possibly belong to the same morphological category as these sense-organs; but this point seems very doubtful. The mucous canals are at first confined to the head, but afterwards are present in the trunk also, and increase greatly in number.

The nerves are directed outwards and downwards from the dorsal surface of the brain, each towards the sensory epiblastic thickening corresponding to it. After a short time, the two structures fuse together indistinguishably, so that a mass of cells is formed at this point, continuous, on the one hand, with the external epiblast, and, on the other, with the nerve-root. The nerve-elements of this mass are probably derived from the nerves, and the sense-elements from the epiblast. This description holds for the 5th, 7th, and 9th nerves. We have not ascertained whether it is also true of the 10th. The common rudiment of the 7th and 8th nerves fuses with the sensory epiblast at two points—one behind the other. The posterior of these gives rise to the ear, the anterior to the sense-organ of the 7th nerve. Soon after the fusion has taken place, the common rudiment of the two nerves divides into two complete trunks, one innervating the ear, and the other the sense-organ of the 7th nerve.

At the point where the 5th nerve is fused with the skin, the Gasserian ganglion is formed. Later, it begins to separate from the epiblast, retiring from the surface, but remaining still attached to it by a cord of cells, which constitutes the ophthalmic branch of the 5th nerve. At the same time, a main trunk grows out from the ganglion and soon divides into two branches—the inferior and superior maxillary.

The 7th and 9th nerves develop similarly to each other, but in a somewhat different way from the 5th. In each the main trunk passes onwards from its point of fusion with the epiblast towards the corresponding visceral cleft and fuses again with the epiblast of its dorsal wall, the 7th nerve, as usual, innervating the 1st cleft, and the 9th nerve the 2nd cleft. The clefts at this time are not perforated, but represented merely by lateral outgrowths of the fore-gut, which have fused with the epiblast at the points of contact. The ganglion which has been formed on the upper part of the trunk in connexion with the sensory epithelium next sinks inwards, remaining attached to the epiblast by a nerve-branch, as in the case of the 5th nerve. The lower part of the trunk becomes completely detached from its connexion with the epiblast, and gives off two branches, one behind, and one in front of the visceral cleft. Thus the first two clefts are each provided with a post-branchial and a præ-branchial branch, derived from the 7th and 9th nerves.

We believe that all these branches of the cranial nerves are derived from the original outgrowths from the brain, none of them being split off from the external epiblast, as Mr. Beard has described in Elasmobranchs, and Mr. Spencer in Amphibia. The fusion of some of the nerves with the skin seems to be comparable with what is known to occur in vertebrate embryos generally, in the nose and ear, where the nerve grows out from the brain to fuse with the sensory epithelium, and we take all such cases to be merely instances of the innervation of sense-organs. The innervation is generally completed very early in the nose and ear, and, in the Newt, it appears to take place rather earlier than usual in the mucous canals. Our observations seem to point distinctly to this conclusion rather than to the view that the nerves are, even partially, derived from the external epiblast.

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"On the Changes produced by Magnetisation in the Length of Rods of Iron, Steel, and Nickel." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Professor FREDERICK GUTHRIE, F.R.S. Received April 1. Read April 23, 1885.

The earliest systematic experiments on the effects produced by magnetisation upon the length of iron and steel rods were those of Joule, an account of which is published in the "Phil. Mag." of 1847. The experiments were made with bars 36 inches long, which were placed inside a solenoid 38 inches long; and the variations in the length of the bars when currents of electricity were passed through the solenoid were measured by means of a delicate micrometer, each division of which indicated a change of $\frac{1}{138528}$ inch.

Using bars of iron and soft steel, he found that their length was increased by magnetisation, the elongation varying up to a certain point as the square of the intensity of the magnetisation, temporary or permanent, of the bar, and he remarked that the elongation was, for the same magnetisation, greater in proportion to the softness of the metal.

When the metal was hard steel it appeared that "the bar was slightly *increased* in length every time that contact with the battery was broken." On passing the first current through the magnetising coil the length was unaffected, but when the circuit was broken after the passage of this current there occurred a small elongation equivalent to a fifth of a micrometer division, and each succeeding make and break of the current was accompanied by a further small elongation.

These experiments were made with currents of gradually increasing strength; Joule appears never to have tried what would be the effect of applying the same current twice in succession. Had he done so there is reason to believe, as will appear hereafter, that effects of a somewhat different character would have been observed. He attributed the increase in length when the current was interrupted "to the state of tension in the hardened steel," adding that he "found that soft iron wire presented a similar phenomenon when tightly stretched."

The phenomena were, however, not exactly identical in the two cases. From the account which he proceeds to give of his experiments with stretched wires, it appears that when the tension of the wire exceeded a certain limit, the effects produced by the current were exactly the opposite of those which occurred when the wire was unstretched; magnetisation, instead of causing the wire to lengthen temporarily, caused it to shorten, while it resumed its original length when the magnetising force ceased to act. Using a soft iron wire a quarter of an inch in diameter, Joule found that when it was loaded with a weight of 408 lbs. the effects were the same in direction (though smaller in degree) as when the wire was unstretched; its length increased when it was magnetised, and diminished to the same extent when it was demagnetised. When, however, the load was increased to 740 lbs. the effects were reversed, and magnetisation produced temporary retraction. After describing this experiment Joule expresses his belief that with a tension of about 600 lbs. (roughly the mean of 408 and 740), "the effect on the dimensions of the wire would cease altogether in the limits of the electric currents employed," *i.e.*, that currents which produced on his tangent galvanometer deflections ranging from 6 to 58 degrees would neither increase nor diminish the length of his quarter inch wire when stretched with a weight of 600 lbs. If he had actually made the experiment he would perhaps have found that the length of the wire was increased by a weak current, that a current of medium strength would have had no effect whatever, and that one of his stronger currents would have caused the wire to retract.

Joule's experiments have many times been repeated, and his results generally confirmed. In particular Professor A. M. Mayer of the United States, carried out a series of very careful experiments with apparatus of elaborate construction and great delicacy.* The conclusions at which he arrived were in accord with those of Joule so far as regards iron; but in the case of steel there is apparently some discrepancy. Mayer found that (after the first magnetisation) the steel rods with which he worked, whether soft or hard, were invariably shortened when the circuit was made and lengthened when it was broken, the same current being used for the first and for the subsequent magnetisations. This result is, however, not necessarily inconsistent with Joule's, because the conditions of the experiment were not the same, the second current which Joule applied being stronger than the first, and the third stronger than the second. Again, while in the case of Joule's "soft steel" the movements were in the same direction as those observed with iron (though smaller in degree), Mayer's "soft steel" behaved in exactly the opposite manner, the movements (after the first magnetisation) being in the same direction as those which occurred when harder steel was employed. This difference may be accounted for, as Mayer himself suggests, by supposing that his so-called "soft steel" was harder than Joule's. Possibly too there was a sufficient difference in the magnetising forces employed in the two cases to affect the results of the experiments. More will be said on this point further on. The effects resulting from the *first* action of the magnetising current are

* "Phil. Mag.," vol. xlvi, p. 177.

altogether distinct. The permanent magnetisation so produced was found by Mayer to impart a small permanent elongation to rods of soft and blue-tempered steel, and a small permanent retraction when the steel was tempered yellow. Mayer's paper also contains some new facts relating to details of minor importance.

In 1882 Professor Barrett published an account in "Nature," vol. xxvi, p. 585, of some experiments which he had made not only on iron but also on bars of nickel and cobalt, with a view of ascertaining the effect of magnetisation on their length; cobalt, he discovered, behaved like iron, but the elongations were smaller; nickel, however, *retracted* under the influence of magnetisation, the amount of its retraction being twice as great as the elongation of iron under similar circumstances.

The knowledge on the subject up to the present time may be summarised as follows:—

1. Magnetisation causes in iron bars an elongation, the amount of which varies up to a certain limit as the square of the magnetising force. When the "saturation point" is approached the elongation is less than this law would require. The effect is greater in proportion to the softness of the metal.

2. When a rod or wire of iron is stretched by a weight, the elongating effect of magnetisation is diminished; and if the ratio of the weight to the section of the wire exceeds a certain limit, magnetisation causes retraction instead of elongation.

3. Soft steel behaves like iron, but the elongation for a given magnetising force is smaller (Joule). Hard steel is slightly elongated both when the magnetising current is made and when it is interrupted, provided that the strength of the successive currents is gradually increased (Joule). The first application of the magnetising force causes elongation of a steel bar if it is tempered blue and retraction if it is tempered yellow; subsequent applications of the *same* external magnetising force cause temporary retraction whether the temper of the steel be blue or yellow (Mayer).

4. The length of a nickel bar is diminished by magnetisation, the maximum retraction being twice as great as the maximum elongation of iron (Barrett).

In order that the results obtained by Joule and Mayer might be comparable with those of my own experiments, I have made an attempt to estimate the magnetising forces with which they worked.

In the first series of Joule's experiments—those in which he observed the elongation of iron and steel rods not under traction, he used a coil of the following dimensions:—

Length of coil........	38 in.	=	96·5 cm.
Internal diameter	1·5 „	=	3·8 .,
Length of wire.......	110 yds.	=	10,058 „

Each turn would contain $3\cdot8\pi$ cm. of wire (or rather more) = 12 cm. Therefore the number of turns would be 10,058/12=838. If there were more than one layer of wire, the number of turns would be fewer. The magnetising force would be nearly—

$$4\pi\frac{838}{96\cdot5}C=109C,$$

C being the current in C.G.S. units.

In Joule's experiments with stretched wires another coil was used.

Length of coil........	11·5 in. =	28·5 cm.
Internal diameter	1 „	
Length of wire.......	33 yds.	=1188 in.
Thickness of wire	0·1 in.	

The number of turns of wire would therefore be about $1188/1\cdot1\pi=344$, and the magnetising force about

$$4\pi\frac{344}{28\cdot5}C=152C,$$

C being, as before, the current in C.G.S. units.

Joule also describes his tangent galvanometer, and gives the deflection which the magnetising current produced in every case. The galvanometer "consisted of a circle of thick copper wire 1 foot in diameter, and a needle half an inch long furnished with an index." The radius, therefore, was 6 inches = 15·2 cm., and the constant, G, approximately $2\pi/15\cdot2=0\cdot41$.* The horizontal component of the earth's magnetic force was, at the date of Joule's paper, about 0·17; thus the factor by which the tangents of the angles of deflection should be multiplied to give the deflecting currents in C.G.S. units is $0\cdot17/0\cdot41=0\cdot41$.

The greatest deflection recorded in Joule's experiments with iron was 62° 48′, the tangent of which is 1·95; the magnetising force was therefore

$$1\cdot95\times0\cdot41\times109=87.$$

The greatest deflection in his experiments with steel was $70°30'=\tan^{-1}2\cdot824$, the corresponding magnetising force being 126.

The greatest galvanometer deflection in the experiments with stretched wires was 61° 25′, the tangent of which is 1·835, the corresponding current 0·75 C.G.S. units, and the magnetising force 114.

Mayer used a coil 60·25 inches=153 cm. in length, the number of turns being 1919. The magnetising force at the centre of his coil was therefore about $4\pi\frac{1919}{153}C=157\cdot5C$.

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* See Clerk Maxwell's "Electricity," vol. ii, p. 325.

It is less easy to estimate the strength of his current, since he gives no galvanometer readings, nor any details as to the electromotive force and resistance of his battery. The resistance of his coil he states to have been 0·75 ohm, and that of the rest of the circuit (exclusive of the battery) about 0·25 ohm, making 1 ohm in all. He used a battery of twenty-five Bunsen cells, and, in his own words, "the above interpolar resistance showed that the maximum effect of magnetisation would be given by connecting the twenty-five cells five in couple and five in series." This implies that the resistance of the battery as thus arranged would be not far from 1 ohm, which, unless the cells were very small, is surprisingly high. Taking the electromotive force of a Bunsen cell to be 1·9 volts, the electromotive force of Mayer's battery would be 9·5 volts, and the current $9{\cdot}5/2=4{\cdot}75$ ampères$=0{\cdot}475$ C.G.S. unit. The magnetising force would therefore be about $157{\cdot}5\times0{\cdot}475=75$ nearly. But 1 ohm is probably too high an estimate for the resistance of the battery. Assuming the resistance of the battery, leading wires, and connexions to be 0·5 ohm (which is the lowest reasonably probable estimate), the current would be 7·6 ampères$=0{\cdot}76$ C.G.S. unit, and the magnetising force $157{\cdot}5\times0{\cdot}76=118$ units. In point of fact the force was probably something more than 75 and less than 118.

In my own experiments both the magnetising coil and the rods of metal were much shorter than those used by Joule and Mayer. The length of the coil is 11·5 cm.; it contains 876 turns of wire 1·22 mm. in diameter, wound in twelve layers on a brass tube with boxwood ends. The internal diameter of the tube is 1·5 cm., that of the coil is 1·9 cm., and its external diameter is 5·2 cm. The mean length of the diagonals of the cylindrical layers of wire is 12 cm., and the field at the centre due to a current through the coil is approximately $4\pi\frac{876}{12}C=918\,C$ when C is expressed in C.G.S. units, or 91·8C when C denotes the current in ampères. The strongest current which I have hitherto used was 3·27 ampères, and the greatest magnetising force was $3{\cdot}27\times91{\cdot}8=300$ units, Joule's maximum having been 126, and Mayer's, on the most favourable estimate, not greater than 118.

The apparatus for performing the experiment, which is of a very simple nature, is shown in fig. 1.* A mahogany table, TT, supported by three stout legs, the lower ends of which are let into a base board, G, carries a lever arrangement for causing the elongating or retracting rod under examination to deflect a mirror, M, which turns about its horizontal diameter upon knife edges. The lower end of the rod rests in a conical recess formed in a brass plate which is attached to a hinged board: the height of the plate can be adjusted by turning a screw, S. The rod passes through the coil, and also through a hole

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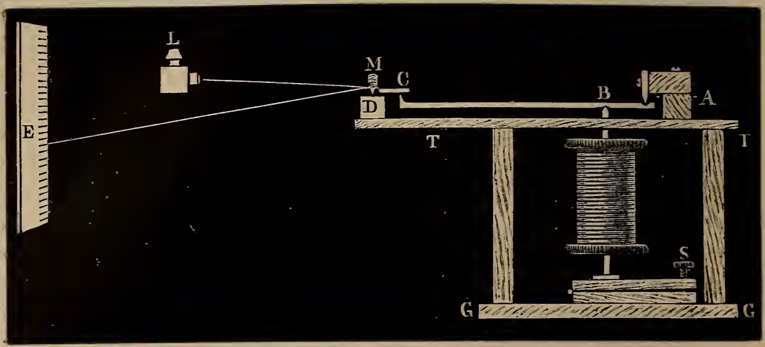
* The diagram is not drawn to scale.

FIG. 1.



in the table, and its upper end, which is chisel-shaped, acts at B upon a brass lever, one end of which abuts upon the knife edge, A, and the other upon a short arm, C, fixed to the back of the mirror. Shallow notches in the form of obtuse angles are cut in the lever (which is of square section) at the points where it bears upon the knife-edge fulcrum and the end of the rod. The end of the lever remote from the fulcrum has the form of a chisel, the edge of which is turned upwards and fits into a shallow groove cut transversely in the arm of the mirror. By means of a magic lantern, L, illuminated by a lime-light, the image of a horizontal wire is, after reflection from the mirror, projected upon a distant vertical scale, E. A slight deflection of the mirror causes a considerable movement of the image of the wire upon the scale. The actual dimensions are as follows:—The distance AB=10 mm., BC=170 mm., DC=7 mm., DE=3200 mm. (10 feet 6 inches). The multiplying power of the arrangement, the beam of light being horizontal, is 3200 × 17 × 2/7=15,543 times.* The scale is one of Elliott's ordinary galvanometer scales, each division of which is equal to a fortieth of an inch or 0·64 mm. Therefore a movement of the focussed wire through one scale division indicates a difference in the length of the rod of 0·64/15,543=0·000041 mm. The length of magnetic metal in the rods used is 100 mm., so that a movement of one division shows a difference of 0·00000041 in the length, equal to about a two and a half millionth part.

For projecting the image of the wire upon the scale, a half-plate photographic portrait lens of high quality was used. When the best definition was secured, it was possible after a little practice to read the deflections to a quarter of a scale division with tolerable certainty.

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* The multiplier 2 is used because the angle through which the reflected beam of light is deflected is twice the angle of deflection of the mirror.

In working with this apparatus, three possible sources of error were soon revealed. The first was due to the expansion of the rod in consequence of the heating of the coil by the current. This effect could be distinguished from the elongation resulting from magnetisation, by the fact that the latter took place quite instantaneously, while the expansion due to heat was gradual; but it was likely to lead to uncertainty in estimating the amount of permanent elongation accompanying the permanent or residual magnetism of the rod. This uncertainty could be reduced to a minimum by taking care to close the circuit only for a second or two when making an observation. The second possibility of error resulted from the gradual yielding of the magnetic rod, or, more probably, of some part of the base or frame of the apparatus, under the pressure, small though it was, of the brass lever. This may to a great extent be obviated by adjusting the apparatus and leaving it for half an hour before making an observation; but I am not quite sure that it ever entirely disappeared, for even though the image of the wire remained perfectly steady upon the scale so long as the apparatus was quite undisturbed, it is possible that the shocks produced by magnetising and demagnetising the rod might cause a sudden slight upward movement of the image, thus making the permanent elongation of the rod appear to be somewhat less than it in fact was. I think, however, that the error, after the apparatus has been at rest for a sufficient time, must be very small. In observing the purely temporary elongation resulting from so much of the magnetisation as is purely temporary, no uncertainty whatever need arise from this cause, for the experiment can easily be repeated as often as may be necessary to obtain uniform results; but an observation of the permanent extension cannot be repeated without dismantling the apparatus and demagnetising the rod, after which its condition will probably not be exactly the same as before.

Lastly, errors may arise from the electromagnetic attraction existing between the coil and the rod, which tends to draw a uniform rod into such a position that the middle point of its axis shall coincide with the centre of the coil. As at first constructed, the coil in my apparatus was attached by means of screws to the under side of the table, and the rod under examination passed freely through it, touching nothing whatever except the brass plate at the bottom and the lever at the top. The length of the *magnetic* portion of the rod—that which was the subject of the experiment—was in every case, as already stated, exactly 10 cm., or 1·5 cm. less than the length of the coil. But the distance from the brass base plate to the lever was 21 cm., and in order to increase the experimental rods to this length, pieces of thick brass wire were screwed or soldered to their two ends; thus the rods when prepared for the experiment were of a compound form, consisting of iron, steel, or nickel in the middle, and brass at each end

The relative lengths of the pieces of brass were so adjusted that when the compound rod was fixed in position, the centre of the magnetic portion of it coincided as nearly as possible with the centre of the coil. This arrangement was, however, found not to be entirely satisfactory. It was difficult to secure the required coincidence with perfect accuracy, and it was necessarily somewhat disturbed during the adjustment of the image of the wire upon the scale; while, even supposing that the geometrical coincidence was perfect, it might well happen that owing to inequalities in the magnetic properties or physical condition of the rod, the source of error might still exist. A simple experiment showed conclusively the immense importance of guarding against any trace of this interaction between the rod and the coil. A compound rod of iron and brass was prepared, such that when it was placed with one end uppermost the centre of the iron was somewhat below that of the coil, and when the other end was uppermost the centre of the iron was about 5 mm. above that of the coil. A current was passed through the coil when the iron was in the first position and a certain elongation was indicated. The position of the rod was then reversed and the same current passed. It was expected that the apparent elongation would be diminished, but in point of fact an actual *retraction* equivalent to two or three scale divisions was indicated. The sucking action of the coil caused the lower end of the rod to press upon the base with increased force; the base yielded a few hundred thousandths of a millimetre, and this was sufficient, in spite of the real elongation of the rod, to cause the image of the wire upon the scale to move in the direction of retraction.

It appeared that the only method of avoiding with certainty the misleading effects of this attraction between the coil and the rod would be to attach the two together. The pressure upon the base would then depend simply upon the joint weights of the coil and the rod, and would not be varied by any interaction between them. The coil was therefore detached from the table, and its ends were fitted with corks through which the experimental rod was passed, care being taken that it fitted tightly at both ends. The arrangement was then exactly as shown in fig. 1, the coil being supported solely by the rod; and it was so used in all the experiments described in this paper.

Before giving an account of the new effects which I have obtained, it may be well to state how far the maximum elongations and retractions of iron and nickel bars, as indicated by my apparatus, accord with those published by previous experimenters. This is done in the subjoined table.

Table I.

Metal.	Diameter in millimetres.	Observer.	Magnetising force.	Total elongation in fractions of the length.
Soft iron	6·35	Joule	64	·00000562
"	12·7	Mayer	Between 75 and 118	·00000457
Iron........	25·4	Barrett	"Maximum magnetisation"	·00000385
"	2·65	Bidwell	45	·00000450
"	3·65	"	73	·00000389
Nickel......	25·4	Barrett	Unknown	·00000769
"	9 × 0·75	Bidwell	300	·00001000

These figures include the total elongation of the rods, *i.e.*, that due to the permanent as well as to temporary magnetisation.

The magnitude of the effect undoubtedly varies considerably with the quality of the iron employed; that used in my own experiments was ordinary commercial iron wire annealed by being heated red hot and slowly cooled. The permanent elongation was, in both cases, rather more than one-third of the whole. For reasons already given, it was much easier to measure with certainty the temporary than the permanent effect.

By using thinner rods and greater magnetising forces than those previously employed, I have arrived at the curious and interesting fact which it is the main purpose of this paper to describe. If the magnetisation be carried beyond a certain critical point, the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing approximately with the magnetising force. If the force be sufficiently increased a point is arrived at, varying according to the dimensions and quality of the iron, where the original length of the rod is totally unaffected by magnetisation. And if the magnetisation be carried beyond this point, the original length of the rod will be reduced. To take a concrete example: the maximum temporary elongation of my thinnest iron rod occurs when the external magnetising force is about 45; an external force of about 212 has no effect whatever upon the length, which remains exactly the same as when the rod is unmagnetised; a force of about 300 causes the length of the rod to diminish, the amount of the retraction thus produced being about one-half that of the maximum elongation. Here I had exhausted the capability of my battery power—seven Grove cells—but so far I could detect no indication that a limit of retraction was being approached.

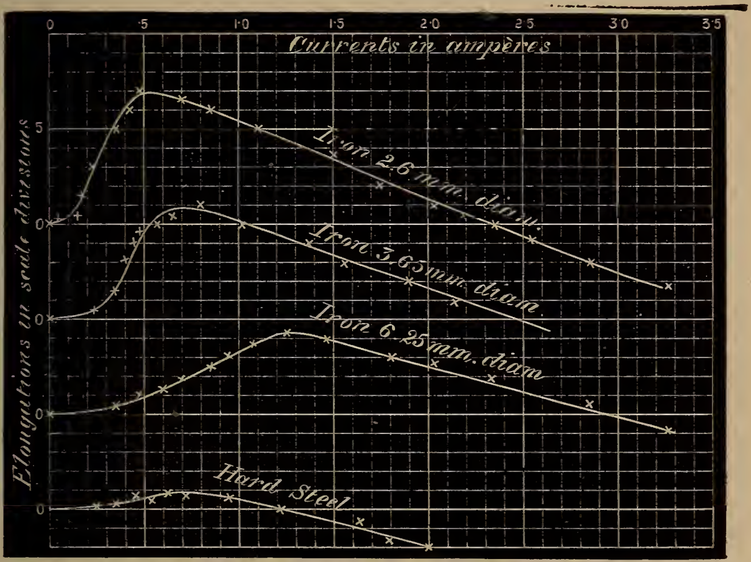
Table II.

Iron.						Hard steel.		Nickel.	
No. 1 diameter=2·65 mm.		No. 2 diameter=3·65 mm.		No. 3 diameter=6·25 mm.					
Magnetising current.	Elongation in scale divisions.	Magnetising current.	Elongation in scale divisions.	Magnetising current.	Elongation in scale divisions.	Magnetising current.	Elongation in scale divisions.	Magnetising current.	Elongation in scale divisions.
0·047	0·25	0·23	0·5	0·34	0·50	0·26	0·12	0·11	− 1·0
0·14	0·5	0·34	1·5	0·49	1·00	0·37	0·25	0·16	− 2·0
0·18	1·5	0·41	3·0	0·60	1·25	0·46	0·50	0·26	− 3·0
0·23	3·0	0·45	4·0	0·70	1·75	0·53	0·50	0·34	− 4·0
0·34	5·0	0·49	4·5	0·86	2·50	0·62	0·87	0·49	− 5·5
0·42	6·0	0·56	5·0	0·95	3·12	0·72	0·87	0·60	− 6·0
0·49	7·0	0·66	5·5	1·08	3·75	0·95	0·62	0·82	− 8·5
0·70	6·5	0·80	6·0	1·24	4·25	1·20	0·00	1·03	−11·0
0·84	6·0	1·01	5·0	1·48	4·00	1·64	−0·75	1·32	−12·0
1·10	5·0	1·38	4·0	1·80	3·00	1·80	−1·50	1·73	−16·0
1·51	3·5	1·68	3·0	2·03	2·50	2·00	−2·00	2·09	−18·0
1·75	2·0	1·89	2·0	2·31	1·75			2·54	−21·0
2·04	1·0	2·16	1·0	2·85	0·50		Mean of two experiments.	2·85	−22·0
2·19	0·5			3·27	−1·00			3·27	−24·0
2·35	0·0								
2·54	−0·75				Mean of two experiments.				
2·85	−2·0								
3·27	−3·25								

I made a systematic course of observations with three iron rods of different thicknesses prepared in the manner described above. All were of ordinary commercial iron annealed in the usual manner. Their respective diameters were 2·65, 3·65, and 6·25 mm., and the length of each was 100 mm. The strength of the successive magnetising currents in ampères and the corresponding temporary elongations in scale divisions are given in Table II. The currents can be approximately expressed as magnetising forces by multiplying by 91·8, and the scale divisions can be reduced to ten-millionths of the length of the rod by multiplying them by 4.

The results are also shown graphically in the first three curves of fig. 2, in which the abscissæ represent the magnetising currents and the ordinates the elongations.

FIG. 2.



The elongations are those due to *temporary* magnetisation only. In order to avoid the uncertainty attached to the elongating effects of permanent magnetism, I always at the beginning of an experiment, and before making an observation, passed through the coil the strongest current at my disposal, thus permanently magnetising the rod to saturation. There appear to be two kinds of residual magnetism. When a strong current has been passed through the coil, the magnetism which remains after the current has ceased to flow is for the most part of a sub-permanent nature. If the rod is undisturbed, the sub-permanent magnetism will remain without material diminution for perhaps half an hour, but in a short time it rapidly falls off. If, however, the rod is shaken, or even if it is removed from the coil with the greatest care, the magnetism which I have called sub-permanent instantly disappears. But, after the destruction of the sub-permanent magnetism, there still remains a small quantity of magnetism of a nature which may properly be called permanent, since it persists for days, or perhaps indefinitely, unless violent measures are resorted to for its removal. I find, by experiments which will be referred to hereafter, that when an iron rod has once been sub-permanently magnetised by a strong current, the intensity of the sub-permanent magnetisation is absolutely unaffected by the action of currents weaker, or not stronger, than the first. For a limited time (say half an hour) currents of varying strength may be passed through the coil, and the *additional* magnetism produced by their action is of a *purely* temporary nature, disappearing completely when the current ceases to flow, and leaving the sub-permanent magnetism exactly where it was before. The elongations referred to in my tables and curves are due to purely temporary magnetisation.

The strength of the magnetising current was varied by means of a box of resistance coils, and was measured by a Helmholtz tangent galvanometer with four separate coils, of Elliott's manufacture. After the first two or three preliminary experiments, no attempt was made to read the galvanometer at the time when the observations of the elongations were made: for, in order to do so, it was found necessary to keep the circuit closed for a period which was sufficiently long to cause the coil to become heated, and confusion was introduced owing to the heat expansion of the rod. A note was made of the resistances successively inserted, and the currents corresponding to the several resistances were afterwards leisurely and carefully determined. It was soon found that the action of the battery was so constant that several elongation experiments might be made on the assumption that the same currents accompanied the same resistances without any sensible error, except perhaps a slight one in the case of the strongest currents; but the estimated currents were from time to time checked by reference to the galvanometer, and when any material variation was observed, a fresh series of galvanometer readings was made.

An examination of the three iron curves discloses the following facts:—In every case the form of the curve for the first part of the ascent is sufficiently nearly parabolic in form to afford confirmation of Joule's law, that the elongation varies up to a certain point as the square of the magnetisation. After passing the maximum, the curve assumes a form which is apparently intended to be a straight line; at all events, no single observation deviates from the straight line by an amount equivalent to more than half a scale division. If this is so, the retraction after the maximum elongation increases with the external magnetising force.

No certain indication of an approach to a limit of retraction is observable in the curves. Stronger magnetising forces would of course show one, and I hope to be able to repeat the experiments with greater battery power.

The maximum elongation is reached by the three rods with magnetising currents which are the same in order of magnitude as the diameters of the rods.

Lastly, it appears from the curves that the amount of maximum elongation is smaller when the diameter of the wire is greater. The successive maxima are 7, 6, and 4·25, and if an error of a quarter of a scale division be allowed, these maxima will be found to be inversely proportional to the square roots of the diameters of the respective wires.

$$\begin{aligned} 7 &\times \sqrt{265} = 112 \\ 6 &\times \sqrt{365} = 114 \\ 4{\cdot}25 &\times \sqrt{625} = 106^{*} \end{aligned}$$

It seems to me difficult to account satisfactorily for this variation of the maximum elongation. It is of course easy to understand why a greater external magnetising force should be required to produce a given intensity of magnetisation in a thick rod than in a thin one. But it is not at first sight at all evident why, when the same magnetisation is produced, the elongation should not be the same in both cases. Possibly my results may be due to a mere accident, such as a difference in the qualities of the three specimens of iron; but their apparent regularity renders such an explanation somewhat improbable.

It seemed extremely desirable that, if possible, a connexion should be established between the point of maximum elongation and some definite phase of the magnetisation of the rod. Much time and labour were spent in endeavours to investigate the magnetisation by a method of induction; but probably owing to the fact that the galvanometer used—one of Elliott's Thomson galvanometers, with the usual astatic system of magnets and an aluminium damping vane—was unsuited for the purpose, no results of any value were obtained. I was more successful in an attempt to measure by a deflection method the relative values of the temporary moments which various magnetising currents produced in the three rods. The coil was placed in a horizontal position, and one of the rods inserted in the tube, where it was supported axially by means of corks at the two ends. A reflecting galvanometer was placed at a suitable distance from it, as determined by preliminary trials, and the height and disposition of the galvanometer were so adjusted that its magnet was on a level with

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* If the last elongation had been 4·5 the product would have been 113.

the axis of the coil, while the direction of the magnet bisected the axis of the coil at right angles. The galvanometer and the coil were connected in circuit with the box of resistance coils, the tangent galvanometer, and the battery, the connexions being so arranged that the electromagnetic actions of the magnetising coil and the galvanometer coil urged the galvanometer needle in opposite directions. The iron rod being temporarily removed from the coil, the galvanometer, which had a resistance of 1400 ohms, was shunted with a few centimetres of German silver wire, and the length of the shunt was adjusted by trial until, when a strong current was passing, the action of the galvanometer coil upon the needle exactly balanced that of the magnetising coil and the connecting wires of the circuit, so that, on depressing the contact key, no movement of the needle occurred except (with the strongest currents) a slight momentary kick due to induction.

The iron rod being replaced inside the coil, a strong current was caused to circulate round it for two or three seconds. The rod was thus sub-permanently magnetised as in the former experiments. The line of light was then brought to zero of the galvanometer scale by means of the controlling magnet. Great care was taken in setting up the scale to place it accurately at right angles to the magnetising coil, and in such a position that the perpendicular upon it from the middle of the galvanometer needle met it exactly at the zero point, a specially made T-square being used for the purpose. The magnetic field in which the galvanometer needle hung was the resultant of those due to the controlling magnet, the horizontal component of the earth's force, and the sub-permanent magnetism of the experimental rod. No attempt was made to determine its strength.

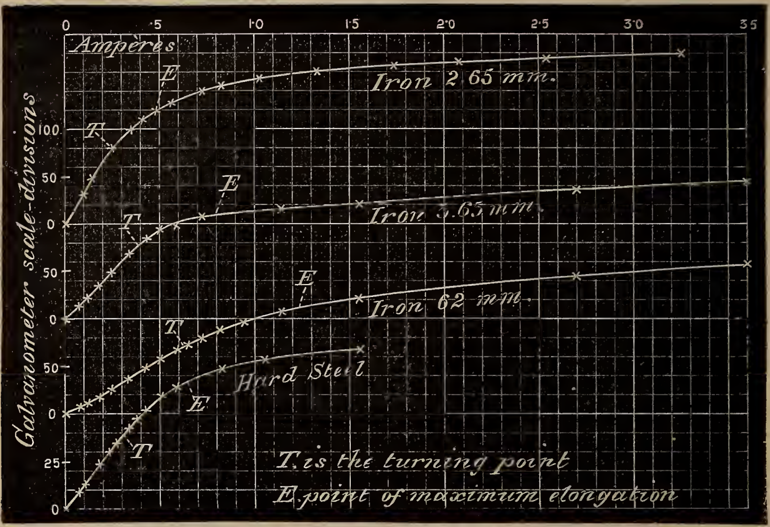
Currents of gradually increasing strength were successively passed through the coil, and the deflections corresponding to the temporary moments of the rod were noted. When the circuit was opened, the spot of light returned in most cases accurately to zero, and the permanent deviations from zero never exceeded two or three divisions, equal to one-fortieth of an inch. The results are given in Table III, and shown graphically in the curves in fig. 3. In each separate experiment the galvanometer deflections are proportional to the temporary moments; but these deflections and the ordinates of the curves are purely arbitrary, and as regards the absolute values of the moments, they give no information, nor are the ordinates of one curve comparable with those of another.

The relative changes in the values of the temporary moments with increasing magnetising forces are, however, clearly shown, and to ascertain the nature of these was the sole object of the experiment. The distance between the galvanometer magnet and the three rods was respectively 25 cm., 36·5 cm., and 53 cm.

Table III.

Iron.						Hard steel.		Nickel.	
No. 1 diameter = 3·65 mm.		No. 2 diameter = 3·65 mm.		No. 3 diameter = 6·25 mm.		Current.	Deflection.	Current.	Deflection.
Current.	Deflection.	Current.	Deflection.	Current.	Deflection.				
0·11	32	0·012	2	0·012	1	0·012	2	0·012	1
0·16	48	0·073	15	0·073	8	0·073	9	0·073	8
0·26	81	0·108	22	0·108	11	0·108	13	0·108	11
0·34	98	0·185	38·5	0·185	19	0·185	23	0·185	19
0·41	110	0·237	50	0·237	26	0·237	30	0·237	27
0·49	121	0·336	69	0·336	37	0·274	35	0·274	31
0·57	129	0·426	85	0·426	48	0·336	42	0·336	40
0·60	133	0·501	94	0·501	58	0·389	47	0·426	52
0·72	140	0·597	99	0·597	66	0·426	51	0·501	60
0·82	145	0·720	108	0·642	73	0·501	59	0·597	67
1·03	153	1·137	116	0·720	79	0·597	64	0·819	80
1·32	160	1·563	124	0·819	87	0·819	74	1·137	90
1·73	167	2·700	137	0·960	96	1·137	79	1·563	98
2·09	171	3·606	146	1·137	106	1·563	84		
2·54	176			1·563	122				
3·27	180			2·700	144				
				3·606	158				

FIG. 3.



The points corresponding to the maximum elongation in the three curves are marked with the letter E. At first sight they seem to possess no particular distinguishing characteristic in common. Indeed the only points in the curves which appear to be marked by any special property are those which are called by Chrystal ("Enc. Brit.") after Wiedemann, the "turning points." Up to these points the temporary moments increase with the magnetising force, or even more rapidly; after the points are passed, the rate of increase in the temporary moments is less than that of the magnetising force. When the curve does not begin to ascend in a straight line, the turning points are found by drawing tangents from the origin: they are indicated in the curves by the letter T,* but it is not easy to determine their positions with perfect accuracy.

From a careful examination of these curves it appears probable that a simple relation does exist between the turning points and the points of maximum elongation, the abscissæ of the points of maximum elongation being almost exactly equal to twice those of the turning points.

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* According to Rowland ("Phil. Mag.," 1873, vol. ii, p. 155), "the temporary magnetism increases continually with the current." This may be strictly true (up to the turning point) for rods or rings having the diameter of those used by Rowland. Thus the curve for my thickest iron rod ascends in a perfectly straight line; but a slight convexity towards the axis of x may be suspected in the medium one, and in the thinnest such convexity is quite evident. It is even more marked in the curve for a wire, 0·77 mm. in diameter, given in fig. 5.

I have also made a great number of experiments with steel. The results obtained appeared at first to be of the most inconsistent character, and it was with difficulty that I finally succeeded in evolving order out of them. The fact clearly is that the point of maximum elongation (when there is one) depends in a very remarkable manner upon the hardness or temper of the steel. Like Joule, I found that a soft steel rod which had been neither annealed nor tempered behaved in very much the same manner as iron, though the effects were smaller. There was a point of maximum elongation which was well defined, but I was not able by any current at my command to produce actual retraction. A rod which was made exceedingly hard by being dropped into cold water when at a bright red heat, had no point of maximum elongation within the limits of my magnetising currents, the temporary elongation continually increasing with increasing magnetisation, and giving no evidence of an approach to a limit. But when the same rod was let down to a yellow temper its behaviour was altogether different. With a very small magnetising force it showed signs of retraction, and the retraction increased with stronger magnetising currents, ultimately becoming very considerable. A rod tempered blue also retracted when magnetised, but the effect did not begin to appear until the magnetising force was much greater than was necessary when the temper was yellow, and after the rod had been still further let down by heating, a measurable *elongation* occurred before the magnetising force was sufficient to cause retraction.

Again, another piece of steel was hardened by raising it to a *dull* red heat and dropping it into cold water. It could easily be marked with a file, and was certainly softer than the last-mentioned rod before it was tempered, though it appeared to be harder than the same rod in the yellow condition. The new hard rod was slightly elongated by feeble magnetisation, and after passing a maximum retracted at about the same rate as iron.

All these various results, which at first sight appeared to be disconnected and inharmonious, point to the following conclusion:—The critical value of the magnetising force for a steel rod diminishes as the hardness becomes greater up to a certain point corresponding to a yellow temper, after which it increases, and, with very hard steel, becomes very high. There is therefore a critical degree of hardness for which the value of the critical magnetising force is a minimum; in steel of a yellow temper the value of the critical magnetising force is lower than in steel which is either softer or harder.

Some careful experiments were made with the hard steel rod last referred to. The results are contained in Table II, and the corresponding curve in fig. 2. As in the case of iron, the rod was first permanently magnetised by a current equal to the strongest subsequently used. The relations of the magnetising force and temporary moment appear in Table III and in the last curve of fig. 3. In this experiment the distance between the steel rod and the galvanometer magnet was 15 cm.

In the light of these experiments I have endeavoured to find an explanation of the anomalous results obtained by Joule and by Mayer with hard steel. It will be remembered that Joule, using gradually increasing currents, found that (after the first current, which produced no effect whatever while it circulated, but was followed by a small elongation when it had ceased) his hard steel bar was slightly elongated both when the current was made and when it was broken, the length of the bar being thus continuously increased. Though I have made many attempts, using steel rods in different conditions, to obtain Joule's results, I have never succeeded in finding a rod which behaved in the manner described by him. Below I give Joule's table, and also a diagram in which I have plotted his results, the abscissæ representing the magnetic intensity of the bar, and the ordinates the corresponding elongations. Both are on an arbitrary scale:—

Table IV (Joule's).

Deflection of galvanometer.	Magnetic intensity.	Elongation of bar.
39° 0′	1·11	0
0	1·36*	0·2
52 35	4·09	1·0
0	2·85	1·3
60 15	5·10	1·8
0	3·52	1·9
69 45	5·91	2·5
0	4·20	2·7

It clearly appears that the elongations due to permanent magnetisation and to permanent + temporary (*i.e.*, total) magnetisation lie upon separate curves. And, since the total curve is below the permanent curve, it follows that the temporary magnetisation has a negative or retracting effect. Taking ordinates equal to the differences between those of the permanent and total curves, I have plotted the curve for temporary magnetisation, which, of course, lies on the negative side of the horizontal axis, and starts from the point representing a magnetic intensity of 1·11. Following the analogy of other experiments, I have continued the curve above the horizontal axis representing this part of it by a dotted line; it is probable that

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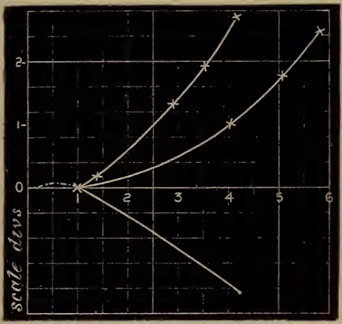
* It is very extraordinary that the "magnetic intensity" of the bar should be greater after the current had been cut off than it was when the current was flowing. Joule makes no reference to the fact.

FIG. 4.



any elongation represented by the dotted portion would be too small to be measurable.

The results of this experiment of Joule's are thus shown to be reconcilable with others, if we may make the assumption that in this particular specimen of steel the elongations due to temporary and permanent magnetisation followed different laws, and that while the critical point of the former occurred at an unusually early stage, that of the latter was not reached within the limits of the experiment. It may be indeed that this is always the case, though under ordinary circumstances the difference is too small to lead to the anomalous result under discussion. Having confined my attention almost entirely to the investigation of temporary effects, I have little experimental evidence to bring forward bearing upon the point.*

Mayer's results may be much more easily accounted for. The fact that his steel rods were invariably shortened by magnetisation (after the first magnetisation, the effect of which varied in different specimens) clearly indicates that his magnetising force exceeded the critical value, which was smaller for the steel bars than for the iron which he had previously used. He apparently never magnetised his

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* Were it not for the proverbial accuracy of Joule's work, a simpler explanation of the anomaly would have suggested itself. The lower of the two curves above the horizontal axis represents the state of things *while a current is passing*, and the fact that this curve does not coincide with the upper one might, perhaps, be accounted for by the "solenoidal suction" which would occur if the rod were not quite symmetrically placed with respect to the coil, or even if it were not perfectly homogeneous throughout. Thus, the apparent elongations when the circuit was broken would be really due to the cessation of the suction, while the elongations indicated when the circuit was closed would be less than those which actually occurred. Each of the vertical divisions in the diagram represents only one thirteen-millionth part of the length of the rod: a very small variation in the pressure of the end of the rod upon its support would, therefore, have a sensible effect.—(February 23, 1886.)

steel with currents of less than the maximum strength, and a smaller magnetising force would perhaps have produced elongation, unless indeed the permanent magnetisation induced by the first current equalled or exceeded the critical value. This was almost certainly the case with his yellow-tempered steel, which was permanently *shortened* by the first magnetisation, while all the other specimens were permanently elongated. These considerations are consistent with all the phenomena exhibited by Mayer's steel bars.

In working with a rod of steel which had been neither annealed nor hardened, I obtained some very curious effects of which I am not at present prepared to offer a complete explanation. I therefore describe the experiments exactly as they were performed, without attempting to account for the results.

Experiment 1.—A current of 2 ampères was passed through the coil, whereupon the rod elongated 3 scale divisions. Without breaking the circuit, the current was reduced by inserting resistance to 0·6 ampère. The rod underwent a further elongation of 3 divisions, making the total elongation equal to 6 divisions. On breaking the circuit the rod retracted 6 divisions, returning to its original length; but when the circuit was again closed, the resistance still being inserted and the current consequently 0·6 ampère, the resulting elongation was only 3 divisions.

It appears therefore that a strong magnetising force subsequently diminished causes a greater temporary elongation than the diminished force is capable of producing if applied in the first place.

Experiment 2.—A current of 2 ampères being passed through the coil, an elongation of 3 scale divisions was produced. The current was reduced to 0·26 ampère, when a further elongation of 1 division occurred. On breaking the current the rod returned to its original length. Once more a current of 0·26 ampère was passed through the coil, but no movement whatever occurred.

From this it appears that the temporary elongation of a steel rod when once produced may be maintained by a magnetising force which is itself too small to cause any perceptible elongation whatever.

Something of the same kind, though in a smaller degree, was observed by Mayer in rods of iron.

Both these experiments were repeated many times, the results being invariably of the same character, and there is no doubt whatever as to the reality of the effects described.

On a small scale, I have repeated some of Joule's experiments with stretched wires, and found, as he did, that when a wire was loaded with a certain weight, the effect of magnetisation was not elongation but retraction. No measurements, however, were attempted, my apparatus not being well adapted for the purpose.

It appeared, upon consideration, that the results of this class of experiments would be brought into perfect harmony with those already described, if it could be shown that the critical value of the magnetising force was lowered when the rod was stretched. There were reasons arising from the nature of my apparatus why I could not attempt to prove this directly; but an indirect method affords strong evidence that this is the case. By the method of deflection already described, it could be easily ascertained whether the position of the "turning point" was affected by stretching a wire. Now, in every case which I have hitherto investigated, it was found that the critical value of the magnetising force was very approximately equal to twice the magnetising force at the turning point. If therefore it should appear that the position of the turning point was affected by stretching, the presumption would be strong that the critical value would be altered to a corresponding extent.

Four deflection experiments were therefore made. In the first the wire, which was of annealed iron 0·77 mm. in diameter, was supported in a horizontal position inside the coil by means of corks; gradually increasing currents were passed through the coil and the galvanometer deflections noted as in former cases. The wire was then removed, and after being demagnetised was replaced inside the coil, and a weight of 7 lbs. was attached to it by means of a cord passing over a pulley. Once more the deflections accompanying increasing currents were noted.

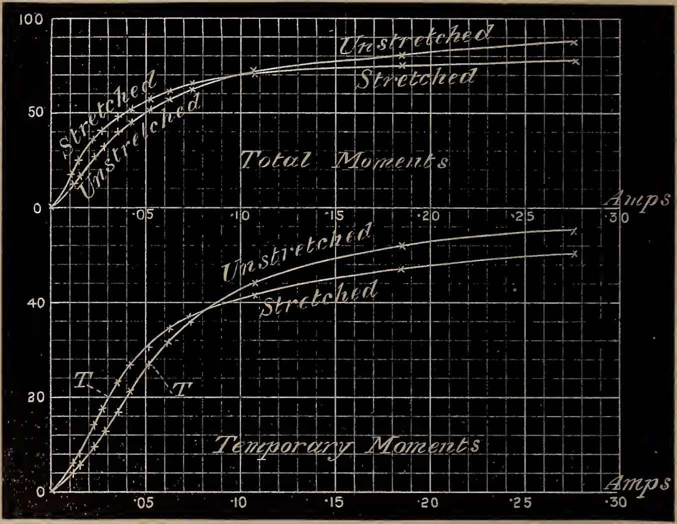
The third and fourth experiments were similar to the first two, except for the fact that the wire was magnetised to saturation before any observations were made. The deflections recorded in the first and second experiments are due therefore to the sum of the permanent

Table V.

Currents.	Magnetometer deflections for total magnetisation.		Magnetometer deflections for temporary magnetisation.	
	Unstretched.	Stretched.	Unstretched.	Stretched.
0·012	12	16	4·5	6·5
0·015	16·5	24	6	8
0·023	25	35	10	15
0·027	31	40	13	18
0·035	38	47	18	23
0·042	44	51	21·5	27
0·051	51	56	27	30·5
0·061	56	59	31	34
0·073	62	64	36	37
0·108	71	70	44	41·5
0·185	80	76	52	47·5
0·274	88·5	79	55	50

and temporary magnetisations, while those in the third and fourth were produced by the temporary magnetisation only. The results are given in Table V and in the subjoined curves (fig. 5). Both series are interesting as affording an illustration of the law which has been fully investigated by Sir William Thomson,[*] that the magnetisation of a wire is at first increased and afterwards diminished by stretching; but the results of the second series only (in which the ordinates represent the temporary moments) are comparable with those of the former experiments.[†]

FIG. 5.



Referring to the curve of temporary magnetisation, it will be seen that the magnetising current at the turning point is reduced by stretching with a weight of 7 lbs. from 0·051 to 0·030: presumably, therefore, the magnetising current for the critical point is at the same time reduced from about 0·102 to 0·060, and a current between these limits would be accompanied by elongation when the wire was unstretched, and by retraction when it was stretched.

For a few experiments made with nickel, a strip was used of the following dimensions:—Length 100 mm., breadth 9 mm., thickness

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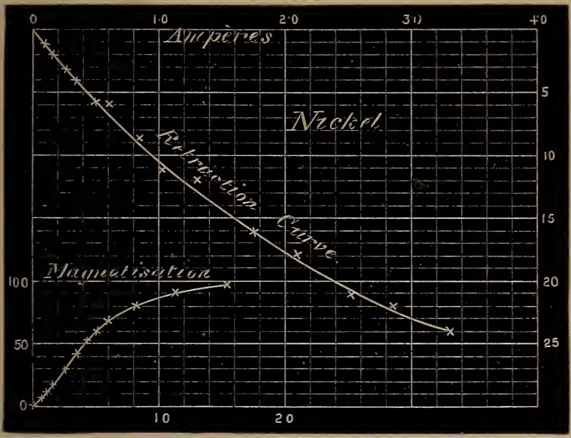
[*] "Phil. Trans.," 1876 and 1879.

[†] It should be noticed that the ordinates are on a different scale in the two diagrams, as may be seen by comparing the figures at the side. The distance between the wire and the centre of the galvanometer magnet was 13 cm. in all the experiments.

0·75 mm. Brass wires were soldered to the ends in the usual manner.

The permanent magnetisation induced by the strongest current appeared not to cause a permanent retraction of more than one scale division. The temporary retractions produced by increasing magnetising forces are given in Table II and in fig. 6. The retractions are of much greater extent than the elongations of iron under similar circumstances, and though the curve affords evidence of an approach to a limit, it is nevertheless clear that a considerable further retraction would have occurred if the current had been increased beyond the power of my battery.

Fig. 6.



I also made a deflection experiment with the nickel, thinking it might possibly be the case that it had no turning point., *i.e.*, that the ratio of the temporary moments to the magnetising forces decreased *ab initio*. It appeared, however, that the turning point was unusually well marked, occurring with a current of 0·042 ampère. The details are given in Table III and fig. 6. The experiment was repeated two or three times with the same result, the nickel having been in each case previously magnetised with a strong current. The distance between the centre of the galvanometer needle and the nickel was 15 cm.

The behaviour of a stretched nickel wire has, I believe, never hitherto been investigated. I therefore made the experiment with a nickel wire 0·5 mm. in diameter, loaded with a weight equivalent to about 2 lbs. The result of magnetisation was a very considerable retraction, but for reasons already referred to I was unable to measure the amount.

I have not for some weeks occupied myself with the investigation of the singular facts described in this paper without from time to time indulging in speculations as to their physical causes. It is, however, evident enough that the investigation is incomplete, and many more experiments, some of them requiring additional apparatus of a special kind, remain to be tried. I hope to return to the subject on a future occasion, and in the meantime refrain from theorising as to the causes of the phenomena.

SUMMARY.

The experiments have not been sufficiently numerous to render generalisation in all cases perfectly safe; but, so far as they go, they indicate the following laws:—*

I. *Iron.*

1. The length of an iron rod is increased by magnetisation up to a certain critical value of the magnetising force, when a maximum elongation is reached.

2. If the critical value of the magnetising force is exceeded, the elongation is diminished, until, with a sufficiently powerful force, the original length of the rod is unaffected, and if the magnetising force is still further increased the rod undergoes retraction.

3. Shortly after the critical point is passed, the elongation diminishes in proportion as the magnetising force increases. The greatest actual retraction hitherto observed was equal to about half the greatest elongation; but there was no indication of a limit, and a stronger magnetising force would have produced further retraction.

4. The value of the external magnetising force corresponding to maximum elongation is nearly equal to twice its value at the "turning point."

Definition.—The turning point in the magnetisation of an iron bar is reached when the temporary moment begins to increase less rapidly than the external magnetising force.

5. The external magnetising force corresponding to the point of maximum elongation is greater for thick rods than for thin rods.

6. The amount of the maximum elongation varies inversely as the square root of the diameter of the rod.

7. The turning point, and therefore presumably the point of maximum elongation, occurs with a smaller magnetising force when the rod is stretched than when it is unstretched.

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* The elongations and magnetisations referred to are temporary only.

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II. *Steel.*

The behaviour of steel varies greatly with the hardness and temper of the metal. More experiments than I have hitherto made would be necessary to establish the general laws with certainty; but my results are consistent with the following conclusions :—

1. In soft steel magnetisation produces elongation, which increases up to a certain value of the magnetising force, and afterwards diminishes. The maximum elongation is less than in the case of iron, and the rate of diminution after the maximum is passed is also less.

2. The critical value of the magnetising force for a steel bar diminishes with increasing hardness of the steel up to a certain point corresponding to a yellow temper, after which it again increases, and with very hard steel becomes very high.

3. In soft steel a strong magnetising force subsequently diminished may cause a greater temporary elongation than the diminished force is capable of producing if applied in the first place.

4. A temporary elongation when once produced in soft steel may be maintained by a magnetising force which is itself too small to originate any perceptible elongation.

III. *Nickel.*

1. Nickel continues to retract with magnetising forces far exceeding those which produce the maximum elongation of iron. The greatest retraction of nickel hitherto observed is more than three times as great as the maximum elongation of iron, and the limit has not yet been reached.

2. A nickel wire stretched by a weight undergoes retraction when magnetised.

February 4, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On Intravascular Clotting." By L. C. WOOLDRIDGE, M.B., D.Sc., Demonstrator of Physiology in Guy's Hospital (from the Brown Institution). Communicated by Professor BURDON SANDERSON, F.R.S. Received January 21, 1886.

Notwithstanding all the work that has been done on the subject of the coagulation of the blood, the definite results which have been obtained as to intravascular clotting are extremely scanty.

I think most physiologists will agree with me in the statement, that no method is known by which one can, at will, produce a complete fibrinous coagulation in the vessels of a living animal. I have found such a method, and one which appears to be infallible in its action.

I have succeeded in obtaining from the testis and thymus gland of the calf, a substance presenting the characters of a proteid, the injection of which in sufficient quantity into the veins of an animal, will cause instant death, owing to widespread intravascular clotting.

In its preparation I proceed in the following manner:—The organ having been finely minced, is mixed with a large quantity of distilled water and allowed to stand for some hours. The liquid is then strained off and subjected to the action of a centrifugal machine so long as any deposit is separated from it. The clear liquid is then made strongly acid with acetic acid, whereupon a bulky precipitate appears, which is collected by the centrifugal machine, and well washed with water acidified with acetic acid.

If this precipitate is dissolved in alkaline salt solution and injected into the circulation, it produces intravascular clotting. If the quantity injected is considerable (1 to 2 grams), it causes instant death in a dog of moderate size with complete thrombosis of the vena porta and its branches. Clots are also found in the right side of the heart and in the pulmonary artery. In a rabbit I found that the injection of 1 gram caused death before the injection was completed. Here there was thrombosis of the portal vein, iliac and renal veins, and of the vena cava and aorta, and clots in both sides of the heart.

When death occurs, the blood which flows from a cut artery fails to coagulate, and when the quantity injected is insufficient to kill, the blood (drawn off after injection) may remain uncoagulated for some days. In either case coagulation of shed blood may be induced by the addition to it of the liquid which has been injected. It therefore appears that the agent which brings about coagulation, *intra venas*, must disappear in the act of coagulation. The shed blood contains only a minute trace of fibrin ferment.

The acetic acid precipitate is soluble* in 0·5 per cent. HCl solution. On digesting this solution at 37°, after the addition of pepsine, a part of it is converted into peptone, but a precipitate appears in the process which is permanent. When the digestive products (peptone and precipitate), having been rendered alkaline, are injected into the circulation, no effect is produced.† There is neither intravascular coagulation, nor is the blood deprived of its power of coagulation; but if fresh acetic acid precipitate be added to the liquid, both effects follow injection. Consequently, the failure of effect when the products of digestion are injected alone, is not due to presence of pepsine or peptone. I have ascertained that the acetic acid precipitate does not cause coagulation of dilute magnesium sulphate plasma, which coagulates readily on the addition of fibrin ferment. The agent, therefore, in producing intravascular coagulation cannot be identified with that body.

II. "A Further Enquiry into a Special Colour-relation between the Larva of *Smerinthus ocellatus* and its Food-plants."

By Edward B. Poulton, M.A., of Jesus and Keble Colleges, Oxford. Communicated by Professor J. S. Burdon Sanderson, F.R.S. Received January 26, 1886.

CONTENTS.

1. *Introductory.*

In my previous paper upon this subject ("Proc. Roy. Soc.," No. 237, 1885, p. 269), I gave an account of some breeding experiments under-

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* As casein is "soluble" in milk.

† The total quantity of peptone is very small.

taken in 1884 in which the larvæ of *Smerinthus ocellatus* were fed upon various food-plants, and the resulting larval colours were carefully compared. I also described the colours of captured larvæ of the same species, and mentioned the trees upon which they had been found. I was extremely anxious to continue the investigation in the following year (1885) in order chiefly to throw further light upon the two following points:—

(1.) By means of experiments and observations I had been enabled to show that the colour of the larva was in most cases affected by the food-plant, but there remained a certain number of exceptions which had to be accounted for. I suggested that these might be explained by supposing that the tendencies towards certain colours which were produced by particular food-plants in one generation became independent larval tendencies in the next, which might modify or overcome the usual effects of the food-plants; and that such transmitted influences augmented as the number of generations upon one food-plant (or others producing similar effects) increased. I was desirous of testing this theory by breeding from moths of which the history in the larval state was accurately known.

(2.) In my last paper I also pointed out that there was considerable evidence for believing that the influence of the food upon the larval colour is not a comparatively simple phytophagic influence, but one which is exceedingly complex, being brought about by the colour of part of the leaf (the part which the larva resembles), acting as a stimulus to some larval sensory surface (presumably the ocelli) and so through the nervous system regulating the amounts and kinds of the vegetal pigments absorbed and made use of, and that of the larval pigments deposited.

I wished to test this theory by feeding the larvæ under such conditions that they could only be affected by the colour from one side of the leaves of their food-plant, and it seemed that the best way of achieving this object was by sewing the leaves together. In the subsequent experiments the edges of the leaves were at first pared with the scissors to make them correspond, but it was afterwards found better to double each single leaf longitudinal and sew together the opposite margins which were thus brought into contact. The intention of the experiment was to compare the larvæ which had been exposed to the colour of the under sides of the leaves only, with those which had been exposed to the upper sides only, and with those which had been fed upon the normal leaves. If the larval colours varied according to these three sets of conditions, it would be quite clear that the larvæ were only influenced by the colour of the leaf-surface, because the leaf-substance eaten (from the edge through its whole thickness) must have been identical in all three cases. I also wished to vary the experiment by feeding some larvæ upon leaves which had been given a different tint artificially by removing the "bloom" from the under surface, and to test whether the ocelli formed the impressionable part of the larvæ by investigating the effect upon colour of covering these organs with some innocuous opaque pigment.

I also wished to further investigate the effect of certain food-plants, about which the evidence was conflicting, and to carefully watch for instances of individual variation among the larvæ from the same batch of eggs and fed upon the same food, to look out for any indications which would throw light upon the red-spotted varieties, and also to further enquire into the periods during which the larvæ are most susceptible to the influence of the food-plant.

As far as these questions could be answered by work in the field, I was very successful, for the larvæ were even more abundant than in the summer of 1884, and I was able to extend my area of observation to Switzerland. I have also been exceedingly glad to be able to reconcile the conflicting evidence given in my last paper. But the breeding experiments did not yield adequate results, considering the immense amount of labour bestowed upon them. In the first place the moths emerged in an unfortunate order—a great many males, and later a great many females. Then of those which emerged together, there was great difficulty in obtaining such a system of pairing as I was desirous of instituting, the result being that I could get no larvæ with a strong hereditary tendency towards the yellowish variety; and these I was most anxious to obtain, because all my bred larvæ in 1884 tended very strongly in the opposite direction. The eggs I obtained in nearly all cases resulted from pairing the moths which came from these bred larvæ (1884). Although disappointed in the pairing of the moths, it seemed likely that the experiments would yield sufficiently convincing results from the very comprehensive scale on which they were conducted, for in July, 1885, I had many hundreds of young larvæ belonging to five different families. After the great labour of bringing this large number through the most delicate period of their lives, and just before the results appeared, the larvæ began to die off in hundreds, so that only seventy-five lived to an age at which trustworthy observations could be made. I can only suggest that this altogether exceptional mortality may have been due to the excessive heat and dryness which prevailed at the time. I do not think that it can have been due to the fact that both parents of the majority of the 1885 larvæ resulted from larvæ belonging to the same batch in 1884, because such interbreeding among moths does not produce injurious effects, at any rate until after it has been continued for many generations. Besides, in one instance, the larvæ were the offspring of parents which came from quite different localities, and these did not succeed any better than the others. But although a very small proportion of the larvæ survived, they were still fairly numerous, and formed a considerable body of evidence bearing upon the questions alluded to above, and giving distinct answers to all of them, except the one which bears upon the time of life during which the larvæ are most susceptible to the influences of the environment, and that which suggests the ocelli as the sensory surface which is influenced.

Before describing the experiments in detail, I wish to express my sincere thanks to my wife for her kind help in the labour of attending to so many larvæ, and in the troublesome work of sewing the leaves together. Mr. G. C. Druce has also kindly supplied me with the branches of certain species of food-plant when I was away from home, and has rendered me invaluable assistance in naming the sallows. Mr. J. G. Baker, of Kew, also kindly named the Swiss sallows, of which specimens were sent to him by Mr. Druce.

2. *Experiments upon the Larvæ of* Smerinthus ocellatus *during* 1885.

The following experiments are arranged in five different series, belonging respectively to five different batches of eggs. There is complete uncertainty as to the male parent (if any) in Series II, and consequently there is some doubt thrown over the male parent in Series III, because in these cases (alone) the same female laid two batches of eggs. The cause of the uncertainty is explained at the beginning of Series II. The series are arranged in a succession which corresponds to an advance in the (presumably) hereditary tendency from the whitish towards the yellowish variety. And so also in each series the different sets of experiments are arranged in an order which corresponds to a similar advance in the tendencies known to be produced by the food-plants, *i.e.*, beginning with apple and ending with *Salix rubra.*

But the order is merely provisional in the case of less definite tendencies, or of plants which are little known as food-plants. It must also be remembered that the difference between the hereditary tendencies of the various series is very small, because of the failure (except in one case in which very few larvæ lived) to obtain any eggs from moths which came from yellowish larvæ.

Series I.

Eggs were laid in June, 1885, by a female moth bred from a larva which had been fed during 1884, for the whole period of larval life upon ordinary apple, and which was a typically whitish-green variety (mentioned in "Proc. Roy. Soc.," No. 237, 1885, p. 300). The eggs were fertilised by a male moth bred from a larva which had been fed upon crab for the whole of its life, and was a similar whitish variety (also mentioned on p. 300). Hence the inherited tendencies must have been strongly towards the whitest variety of this larva.

Out of a large number of larvæ which hatched at the beginning of July, 1885, a very small proportion lived until they were old enough to be of use in the present investigation. A careful examination of the survivors was made on August 12th, with the following results:—

1. *Pyrus Malus* (*var. acerba*).—Five larvæ (including one which was found after escaping, and which almost certainly belonged to this lot) were hatched on July 2nd, and now four were well in the last stage and one was changing its last skin. All five were extreme whitish varieties. Eventually all these larvæ died, but their colour was unchanged, and they were sufficiently advanced to warrant the conclusion that no further alteration would have taken place.

2. *Populus tremula, &c.*—One larva, hatched July 2nd, was now (August 12th) changing its skin for the last time and seemed to be a whitish variety. By August 20th it was well in the last stage and an intermediate variety, and without further change on August 27th, when it was nearly full-fed (ceasing to feed in a day or two). After the first fortnight the larva was fed upon a somewhat similar species of poplar, which I have not yet been able to name with certainty.

3. *Salix babylonica.*—One larva (hatched July 3rd) had now entered upon the last stage, and seemed to be well on the yellowish side of an intermediate variety. This description especially applied to the back, but there was a blueness about the ventral surface and lower part of the sides which is never seen in a true yellowish variety. On August 20th the larva was still on the yellowish side of intermediate, but not to such an extent as that seen in larvæ of Series III, which had been fed upon the same plant. Later, the larva became less yellow, so that by August 27th it was distinctly intermediate, and remained without further change until September 3rd, when it ceased feeding.

4. *Salix amygdalina, July 4th—13th, S. triandra, July 13th—14th, and S. rubra, July 14th, onwards.*—One larva (hatched July 4th—5th) was changing its last skin and apparently whitish. Another larva had died at the beginning of the last stage, and was also whitish. The former was dead by August 20th, so that no results were obtained from these larvæ, except the fact that the tendencies of the food-plants (towards producing the yellowish varieties) had evidently been largely counteracted in these larvæ. This larva is afterwards described as if fed upon *S. rubra*, for the leaves were selected so as to be similar to those of this tree in their effects.

The effects of hereditary influence are certainly seen in the larvæ of this series. The parent larvæ were extreme white varieties, and belonged to a group which evidently inherited a very strong tendency in this direction, as was shown by the comparatively slight effect that followed the use of foods which most powerfully tend to produce yellow varieties. It is certain that more dependence can be placed upon this proof of a larval tendency, than by trusting to the maximum results obtained by the use of food-plants which tend to produce white varieties; because the power of the latter is so great as to afford no means of discriminating between larvæ with different tendencies except when the latter are very exceptionally strong in the direction of yellow. (For the proof of the strong tendencies of the parent larvæ, and an account of the effects of various foods upon them, see "Proc. Roy. Soc.," as above quoted, pp. 298—300.) The larval tendencies in this case were even stronger than in the parents, having been augmented by inheritance from the latter. Crab, which has no power in checking the tendency towards white (I cannot yet believe that it causes white itself) produced the most extreme white varieties in these larvæ as in their parents (No. 1). But *S. rubra* (with other similar foods unavoidably used during absence from home) evidently produced less effect than in the case of the parents (No. 4), and the same is true of *Salix babylonica* (No. 3) if we assume that this plant acts in the same manner as *S. rubra*. No conclusions can be drawn from the effects of *Populus tremula*, &c. (No. 2), because this is, I believe, the only instance yet recorded of the larva feeding upon the food-plant in question.

Series II.

Eggs were laid by a female moth bred from a larva which had been fed during 1884 for the whole of its life upon *Salix viminalis*, and which became an intermediate variety with some tendency towards the whitish side. (The larva was one of those mentioned on p. 300 of the paper already quoted.) In the case of this moth it seemed likely that no fertile eggs would be laid, for coitus did not take place when I placed a male in the same box with it. After this I put several males in the box, but I did not witness any act of coitus, although I watched the moths constantly, and the act lasts for several hours in all the cases which have come under my notice. In the meanwhile the moth kept laying eggs which I put in a box by themselves and carefully labelled. The great majority of these eggs shrivelled up, but to my astonishment a few gave rise to larvæ which are considered under these series. Subsequently to the laying of these mostly infertile eggs I succeeded in artificially inducing coitus, with the result that a large number of fertile eggs were laid, which were kept separate and are considered under the next series. Inasmuch as many males were present in the box with the female, it would be obviously impossible to maintain that the larvæ of this series were parthenogenetically developed, but I may state in favour of such a view that in all other cases the coitus lasted long enough for me to witness it, and that nearly all the eggs behaved like those which were laid by other female moths without coitus. I may add that I always carefully separated the eggs which were laid before and after coitus, and also that when several males were together in the same box with a female, the former were distinguished from one another by small notches in the wings.

The inherited tendency was probably towards the intermediate variety (arguing from the female parent only), because the parent larva was almost intermediate after feeding on *S. viminalis* for its whole life, although the group of larvæ to which it belonged tended strongly towards white.

The larvæ were examined on August 12th, with the following results:—

1. *Salix viminalis.*—Six larvæ (hatched July 10th) of which four were nearly full grown, and very similar, being good whitish varieties, though not so strong as those produced by apple. The two others are younger but apparently similar. By August 20th the four larger ones had all ceased feeding without any change of colour. The two smaller larvæ died.

2. *Salix Smithiana.*—Two larvæ were well in the last stage and were greener than those just described—perhaps intermediate varieties. By August 16th one of these was decidedly intermediate, while on August 20th it was well on the yellowish side of intermediate and very nearly full grown. It ceased feeding without further change on August 27th. The other larva died soon after August 12th. These larvæ were fed for a considerable time upon the upper twigs (bearing large leaves) of the doubtful species of Salix mentioned in the note on p. 301 of the paper quoted above. Such leaves were indistinguishable from those of *S. Smithiana.*

These results are certainly perplexing, for the larvæ upon *S. viminalis* (No. 1) were whiter than the parent larva which was fed upon the same plant (although the former probably represents the real tendency of the food-plant), while the one upon *S. Smithiana* (No. 2) was rather yellower than those which are generally produced by this plant, although the data are insufficient. On the other hand, there is nothing at all startling or violently opposed to the conclusions of the other series in the above results, which in one case are those normal to the food-plant, and in the other differ but slightly from the normal result. It must also be remembered that there is complete uncertainty as to the male parent (if any) of these larvæ.

Series III.

The eggs which produced the larvæ of this series were laid by the female moth described at the beginning of Series II. It was bred from a larva which had been fed upon *Salix viminalis*, and which became an intermediate variety with some tendency towards the whitish side. After laying the eggs which produced the larvæ of the last series, coitus was artificially induced with a male moth, bred from a larva which had been fed for its whole life upon ordinary apple (mentioned at p. 300 of the paper quoted above), and which was a typical whitish variety. Hence the inherited tendencies were probably towards the white variety, somewhat modified in the direction of intermediate. Very many fertile eggs were laid after coitus, in June, 1885, and were hatched about July 10th, and although a large number died, a considerable mass of evidence was forthcoming from the fairly numerous larvæ which survived, and which were divided into nineteen sets of experiments. The results of the examination of these larvæ on various dates are given below:—

1. *Ordinary Apple.*—On August 12th two larvæ (hatched July 12—16) were examined, and were in the fourth stage and *very* white, with a peculiar transparent appearance, which seems to be often caused by this food-plant. On August 20th they were both dead, but there could be no doubt of the tendency of the food-plant in this case.

2. *Ordinary Apple.*—On August 12th four larvæ (hatched July 10th) were examined: three were in the last stage and one in the fourth; all were *very* white. On August 20th all were dead except one, which died on August 27th. There was no doubt about the extreme tendency of the food; but apple seemed extraordinarily fatal in its effects during this last summer.

3. *Ordinary Apple* (*the leaves sewn so as to expose the under sides only*).—One larva (hatched July 10th) was examined August 12th, when it was at the end of the fourth stage and *very* white. On August 20th it had entered the last stage, and was unchanged in colour. On August 27th it was dead. There could be no doubt about the strong tendency of the food, but the unsewn apple leaves produced such a maximum effect that there was no room left for the sewn ones to do more.

4. *Ordinary Apple* (*the leaves sewn so as to expose the upper sides only*).—Three larvæ (hatched July 10th) were examined August 12th, when they were young in the fourth stage and very white. By August 20th they were all dead, and so immature that it is impossible to draw any certain conclusions. It must also be noted that in the case of such broad leaves as apple, there is a constant tendency for the larvæ to expose considerable areas of the under surface by nibbling away part of one side of the leaf.

5. *Crab* (*Pyrus Malus, var. acerba*).—Four larvæ (hatched July 16th) were examined August 12th, when they were very small and apparently tending strongly towards the white variety. On August 16th one had died, and the others were only in the third stage. On August 20th they were still quite small and very white, and on August 27th they were all dead, except one which died soon after. As far as the observations went the larvæ were typically white, but

they were very small. Nevertheless it is improbable that there would have been any change from this strongly marked tendency.

6. *Salix viminalis.*—One larva (hatched July 11th) was well in the last stage when it was examined on August 16th; it was a whitish variety with tendencies towards intermediate. The larva was further examined on August 20th and 27th, and on the last date was slightly on the white side of intermediate. This is the last note upon the larva, which must have been full fed by this time.

7. *Salix viminalis.*—Five larvæ (hatched July 10th) were examined on August 20th, when one had ceased feeding a few days before, three were nearly full fed, while one was small. All were slightly on the white side of intermediate. The larvæ were again examined on August 27th, when only two were still feeding, but were practically mature, and were very slightly on the white side of intermediate. The small one had died. The results are final as regards the other four larvæ.

8. *Salix viminalis* (*the leaves sewn so as to expose the under sides only*).—One larva (hatched July 10th) was examined on August 12th, when it was in the fourth stage, and very white. It was again examined on August 27th, when it had been fed for about a week on the ordinary unsewn leaves of *S. viminalis.* It was well in the fourth stage and *strongly* white. The larva died shortly afterwards, but it is probable that the colour would not have changed.

9. *Salix alba.*—Two larvæ (hatched July 14 and 15) were examined on August 12th, when one was in the fourth stage and the other was well in the last stage. They were intermediate varieties, or perhaps rather on the yellowish side. On August 16th the younger one was dead without further change, and the older larva was examined on August 16th, 20th, and 27th, remaining on the yellowish side of intermediate until its death on the last of the above-mentioned dates.

10. *Salix Smithiana.*—On August 12th four larvæ (hatched July 10th) in the last stage were examined, and were found to be on the white side of intermediate. By August 20th three were dead, and the remaining larva was examined then and on the 27th. On the last date the larva was well in the last stage and slightly on the white side of intermediate, this being the last note taken, and certainly representing the final effect of the food. These larvæ were fed for a considerable time upon the upper twigs (bearing large leaves) of the doubtful species of Salix mentioned in the note on p. 301 of the paper quoted above. Such leaves were indistinguishable from those of *S. Smithiana.*

11. *Salix cinerea.*—Two larvæ (hatched July 10th) were examined August 16th, when one was well in the last stage and one in the third. The former was intermediate, the latter too young for any certain results. On August 20th the younger larva was dead, the older one being still intermediate, while upon August 27th it was

nearly full fed, and slightly upon the yellowish side of intermediate, this being the last note, and giving the final result.

12. *Salix cinerea.*—Three larvæ (hatched July 11th) were tolerably full fed when they were examined on August 16th. One was on the white side and one on the yellow side of intermediate, while the third was a yellowish variety (although not strongly yellowish). It was extremely interesting to note that the latter—the only undoubtedly yellowish larva yet obtained in my breeding experiments in 1884 and 1885—possessed traces of the red spots that occur commonly on the yellowish varieties of *S. ocellatus.* On the first five abdominal segments there was a little local darkening of the green borders to the oblique stripes occupying the position of the upper row of red spots, and in the centre of each dark spot there was an extremely faint tinge of red. There was also a very slight tendency towards the suffusion of the ground colour round the spiracles with a tinge of red. On August 20th the larvæ were as they have been described (except that the whitish intermediate larva was now intermediate), and were practically full fed. The yellowest one was a bright yellow variety (although there was but little yellow on the under surface, so that the larva was not one of the strongest varieties). On August 22nd the yellow variety and the intermediate larva had ceased feeding, while the yellowish intermediate larva became adult about August 25th. There was no further change in the colour of any of the larvæ.

13. *Salix cinerea.*—Three larvæ (hatched July 12th) were well in the last stage when they were examined on August 16th. One was on the white side of intermediate, and two intermediate or slightly on the yellowish side. On August 20th and 27th the larvæ were again examined and had progressed in the direction of the yellowish variety, so that on the latter date—when two had ceased feeding, and the other, though still feeding, was mature—they were all on the yellowish side of intermediate, although only slightly so in one case.

14. *Populus nigra.*—Five larvæ (hatched July 10th) were examined on August 12th, when four were in the last stage and one smaller. They were all whitish, but looked as though they were progressing in the direction of intermediate. On August 16th only two remained alive, one being well in the last stage and a whitish intermediate variety, while the other was whitish, being much smaller and not thriving. On August 27th the large larva was the only one alive and was advanced in the last stage, and a distinct intermediate variety without any tendency in either direction. This represents the final result, as the larva subsequently died without further change.

15. *Salix triandra.*—Eleven larvæ (hatched July 9—12th) — of which three were small, but eight were advanced in the last stage— were examined on August 12th, and were all intermediate varieties, as different as possible from those from the same batch of eggs which

were fed upon apple. On August 16th two of the eight large larvæ were quite clearly, though slightly, on the yellowish side of intermediate. On August 18th four ceased feeding, and on August 23rd three more without change of colour. The other larva and three small ones died.

16. *Salix triandra* (*without the whitish bloom upon the under side of the leaves*).—The bloom was for the first part of the time rubbed off with the moistened fingers, but afterwards a tree was found near the Oxford University parks, of which all the leaves were without the bloom, and the larvæ were fed upon this food for the later part of their lives. The following results afford a very interesting comparison with those given above, following the use of the ordinary leaves of *S. triandra* which presumably tend less towards the yellowish variety of larva than those supplied in the present instance.

Ten larvæ (hatched July 13—14th) were variously advanced in the last stage on August 18th, and on comparing them with those (see above) fed upon ordinary *S. triandra* (most of which were rather older), it appeared that the former would be considerably yellower when they had reached an equal age. There were also other younger larvæ upon the same leaves, of which the tendency could be better estimated at a later date. On August 23rd one had ceased to feed, and was distinctly on the yellowish side of intermediate. On August 27th the small ones had died without any results, while four of the older ones were full fed, and the others dead (although old enough to indicate what their colour would have been). The results were very uniform, all being on the yellowish side of intermediate, while only a small proportion of those fed upon ordinary *S. triandra* were at all beyond the intermediate form.

17. *Salix babylonica.*—Four larvæ (hatched July 10—11th) were well in the last stage on August 12th when they were examined. They had been fed on *S. triandra* for twenty-four hours (July 23rd—24th) because I was travelling and could not get the proper food. On August 12th they were all well on the yellow side of intermediate: they were again examined on August 20th and 27th, when two ceased feeding, one was practically mature, and one had died. The colour remained the same in all cases.

When examining these larvæ at an earlier date (August 3rd), when they were more numerous, I noticed one which was in the fourth stage, and which possessed the upper row of rust-coloured spots which are often found on the yellow varieties of these larvæ. The spots were present on the second thoracic segment (*very* faintly), and upon the first five and the seventh abdominal segment. To my great surprise I observed that the larva was distinctly whitish, and as I was most anxious to prove that such varities can bear the spots I changed its food from *S. babylonica* to apple (August 3rd). The next day the larva ceased feeding before its last ecdysis, and it died on August 20th when advanced in the last stage, and an intermediate variety. Thus it is quite certain that the spots can appear on other than yellowish varieties.

18. *Salix rubra.*—Three larvæ (hatched July 10th) were well in the last stage on August 12th, when they were examined. Like those upon *S. babylonica* they had been fed for one day upon *S. triandra.* One was decidedly on the yellow side of intermediate, one less markedly so, and one was intermediate. On August 20th the two former were decidedly on the yellowish side, and I have a note to the effect that I was sure that they were yellower than the larvæ fed upon this tree last year (1884), and of which an account is given in the paper already alluded to. At this time the two yellower larvæ ceased feeding, while the third was still intermediate, and it ceased feeding about August 25th without further change.

19. *S. rubra.*—One larva (hatched July 10th) which had been fed for one day as above described upon *S. triandra* was examined on August 16th, when it was in the last stage and apparently on the yellowish side of intermediate. On August 20th it was advanced in the last stage and unchanged in colour, and on August 27th it was about full fed and slightly on the yellowish side of intermediate, and there is no doubt that this result was final, for the larva could not have undergone further change when it was so mature, this being the last note I have about it.

Reviewing these sets of experiments and comparing them with those of Series I and II, we find upon the whole considerable evidence for the existence of a hereditary force which influences the larval colour in this species.

Ordinary apple (Nos. 1 and 2) produces a maximum effect, as might be expected from previous experiment and observation. It would probably do so even if there existed a strong hereditary tendency towards yellow, and in this case the transmitted influence deviated but little from the direction of the typical white variety (as indicated by the life histories of the present larvæ). There is no doubt that a similar effect would have been produced in the other two series if the larvæ fed upon this food-plant had lived long enough to enable me to take reliable observations. (As this was not the case, such experiments were not alluded to in either series.) Since ordinary apple produced maximum effect, it was practically certain that the same result would follow the use of leaves which were sewn so as to show the white under sides only (No. 3). No. 4, in which the leaves of apple were sewn so as to show the upper sides only, did not terminate as I should have expected, as far as I could judge of the effects in the immature larvæ. The process of sewing and paring causes injury to the leaves, so that the larvæ did not thrive upon them (being less healthy than they were upon the unsewn leaves, although they were far from healthy upon the latter during the past year). Nevertheless, I hope to succeed in the rearing the larvæ upon such leaves in a more favourable season, and I believe that some deviation from the extrème white variety will result (this conclusion being warranted by the results of other experiments described in the present paper). Nos. 1—4 were not represented in the other two series.

Crab (No. 5) produced white varieties, as far as could be ascertained, acting as it did on the present larvæ and in Series I (No. 1). It was quite as fatal in its effects as ordinary apple, and the larvæ seem to grow more slowly when fed upon it than upon any other plant, as was also noticed in the case of the parent larvæ. Considering the extreme effects following the use of this food in the other cases, it could hardly be expected that there would be any appreciable difference in the larvæ of this series.

Salix viminalis (Nos. 6 and 7) produced exactly the same effects as in the parent larvæ, while the larvæ of Series II (No 1) fed upon the same plant were fairly strong white varieties. This is a very strange contrast considering the parentage of the two series, but the uncertainty of the male parent in Series II prevents much importance from being attached to it.

The effect of *S. viminalis*, sewn so as to show the under sides of the leaves (No. 8), affords a most interesting comparison with the results of Nos. 6 and 7, for the former produced a strong white variety (that is up to the time of its death). Here is a distinct proof of the effect of the colour of one leaf-surface as apart from the leaf-substance eaten by the larva.

Salix alba (No. 9) cannot be compared with any previous experience of my own. I expected it to act like *S. viminalis*, but the larva was rather yellower than those which had fed upon the latter plant. The leaves of *S. alba* vary much in whiteness, the young leaves being far more downy and white than the older ones, so that a different effect is probably produced by the two kinds. There is independent evidence (Mr. Boscher's, which will be alluded to presently) that this food-plant produces white larvæ.

Salix Smithiana (No. 10) produced a larva which when mature was on the whitish side of intermediate. This is probably the normal result of the food which in this case coincided with the hereditary influence. In Series II (No. 2), however, the larvæ fed upon the same food-plant were rather yellower; hence the effects of *S. Smithiana* and *S. viminalis* in the two series were the exact converse of each other—a very perplexing result. At the same time, as already pointed out, there was nothing at all unusual in the results of any of the individual sets of experiments. (See also the notes upon the parentage of the larvæ of these two series.)

Salix cinerea (Nos. 11, 12, and 13) produced very interesting results, varying from a good yellow variety to intermediate, nearly all being upon the yellowish side of the latter. Thus, there is a distinct, though slight, advance upon the effect of this food on the parents in the direction of yellow.

The results obtained from the three larvæ of No. 12 are extremely interesting, showing that individual variation may sometimes play an important part in the colour produced, although the whole of the results of all observations and experiments conducted up to the present time certainly prove that such a factor is generally insignificant, and rarely causes any effects that can be detected. By individual variation I mean the development of a different colour than that which would be produced by the food-plant acting upon a larval tendency which is uniform for nearly all the larvæ from each batch of ova, the latter tendency being probably explicable as the inherited results of previous food-plants for many generations. In other words, I mean breaches in the uniformity, however caused, of the larval tendency, and a study of this and the previous paper upon the same subject, will show that such irregularities are comparatively rare, and especially so when the food-plant itself is known to possess a strong influence in the direction of either extreme of coloration.

Populus nigra (No. 14).—The results of this food-plant, intermediate as far as the evidence went, cannot be compared with any other experience, for this is, I believe, the only instance of the larvæ having been known to eat this food-plant. From the green glabrous undersides of the leaves I should have anticipated a tendency towards yellow, which was only partially verified.

Salix triandra (No. 15).—I was especially anxious to gain abundant evidence as to the effect of this food-plant, because I believed that its tendency was towards yellow. Mr. Boscher described numerous instances of typical white larvæ having been found on it. I have, however, since ascertained that Mr. Boscher was mistaken in his identification, and that the trees upon which he found the whitish larvæ were *Salix alba*, and such a result from the latter food-plant is what I should have anticipated. At the same time, I should wish to point out that the identification of the various species of Salix is immensely difficult, and that I have only been saved from hopeless confusion by the skilled assistance of Mr. G. C. Druce, who has most kindly helped me, and, when necessary, has obtained other opinions, throughout this investigation. During the past year (1885) I have proved by observation in the field (as will be seen) that the effect of *S. triandra* is to produce yellowish varieties, and the same thing is proved by these experiments, considering the hereditary influences. I have therefore verified the prediction upon which I ventured in my last paper (already quoted, p. 306), although at the time when the paper was written, nearly all the evidence seemed to point the other way. The larvæ of this set were intermediate, inclining in some cases to the yellowish side. Hence the effects are the same as those produced by leaves which are known to cause the yellowish varieties as a rule. These results can be compared with no previous experience, as the larva has never been bred upon this tree (as far as I am aware). and I can find no instance recorded of its being found in the field upon this food-plant, except the instances which occurred in 1885 (to be described). These agree with the breeding experiments, as does the result of an experiment given in my last paper (quoted above, p. 303), in which *S. triandra* modified the colour of a whitish larva found upon *S. ferruginea.*

Salix triandra (No. 16, without the whitish bloom on the under sides of the leaves).—These results compare in a very interesting manner with those of the last set, the influence in the direction of yellow being more strongly marked than in the case of the usual leaves of this plant. The numerous larvæ of these two sets were repeatedly placed side by side and compared in the most careful manner, and there could be no doubt that there was a considerable difference in the predominance of yellow, while the larvæ had been subject to exactly the same conditions, except in the one point mentioned above.

Salix babylonica (No. 17).—The larvæ were all well on the yellowish side of intermediate (except the one which was put upon another food when quite young), and this result compares favourably with that of the larvæ of Series I upon the same plant (No. 3), where with the greater hereditary influence towards white the larva became an intermediate variety. In this case also the larvæ of the two series were examined side by side, so that there was no doubt about the difference. This result also compares favourably with the larvæ captured upon this food-plant during 1884.

Salix rubra (Nos. 18 and 19).—These results also compare well with No. 4 of Series I. As far as it was possible to judge from the immature condition of the larvæ in the latter set, the effect was not so yellow as in Series III. The effect produced in the present case was stronger than in the larvæ from the same set as the parents, which were fed upon *S. rubra.* This was to be expected, because the tendency of the latter was very strongly towards the white variety, while in the present instance it was somewhat modified.

Series IV.

Eggs were laid by a female moth, bred from a larva, which had been fed in 1884 for the whole of its life upon *Salix cinerea*, and which became an intermediate variety. (The larva was one of those mentioned on p. 300 of the paper quoted.) The eggs were fertilised by a male moth bred from a larva which had been fed for its whole life upon *Salix rubra* (mentioned at p. 300), and which became a yellowish intermediate variety. The tendencies were thus presumably towards intermediate, or slightly on the yellowish side. A large number of eggs were laid, and nearly all of them hatched successfully, yielding apparently healthy young larvæ, but the most extraordinary mortality prevailed, so that no single larva arrived at maturity, or indeed at an age which would render any conclusions possible (except in the case of very marked colours which were not manifested). This is all the more to be regretted because I had reserved by far the greater part of this lot of larvæ for some experiments which would have conclusively decided some of the points in this difficult problem. A few larvæ were fed upon some of the same food-plants as in the other instances, in order to gain further evidence as to their effect. Thus larvæ were fed upon *Salix cinerea* (in this case it would have been interesting to ascertain the effect of the food upon two generations of larvæ, although the female parent only had been so fed upon *S. cinerea*) and upon *S. Smithiana* (the leaves sewn together so as to expose the under sides only). A few of the larvæ were blinded before they had seen the food-plant, by carefully painting over the ocelli with lamp-black, a lens being used to make certain that all of the ocelli were covered. This was a task of considerable delicacy and difficulty in the small and restless larva, but when once accomplished the larvæ did not seem any the worse, and behaved in every way as the others which were not blinded. As the lamp-black only formed an opaque film over the transparent cuticular covering of the ocelli, and as the former is thrown off at ecdysis, the pigment had to be renewed at the beginning of each fresh stage, and the greatest care was necessary to prevent the larvæ from changing their skins at unexpected times, and thus having the opportunity of seeing the food-plant. Hence any larva which had ceased feeding before ecdysis was isolated and only put back upon the food after it had changed its skin and the pigment had been renewed. Larvæ treated in this way were fed upon the two food-plants which tend most strongly in opposite directions—ordinary apple and *Salix rubra*, and at the same time large numbers of unblinded larvæ from the same batch of eggs were fed upon the same plants. Had the larvæ lived there must have been conclusive evidence as to one obvious theory of the origin of afferent impulses which determine the selection and the use of certain proportions of the mixed vegetal pigments, and the deposition of certain amounts and kinds of true larval pigments—the theory that such impulses are caused by the colour of one or both sides of the leaf acting as a stimulus by means of the ocelli. I am indebted to Professor G. J. Romanes for the suggestion that experiments should be made upon blinded larvæ, while Professor E. Ray Lankester advised me to use some harmless pigment instead of silver nitrate. The lamp-black mixed in the usual way with Mc. Guilp. acted in every way satisfactorily, drying very quickly and being perfectly innocuous, and completely opaque. The possibility of future success was shown by these experiments, for the death of the larvæ was certainly not due to the conditions under which they were placed, as was shown by comparing them with the normal larvæ.

Further experiments were tried upon the larvæ from this batch of eggs, to ascertain if possible the exact periods of larval life which are most sensitive to the influence of the food-plant, as gauged by the persistence of effects after the change to another food-plant which tends in an opposite direction. Most of the larvæ were used in this series of experiments. A large number (forty or fifty) were fed upon ordinary apple, and about an equal number upon *Salix rubra*. At the end of the first stage a certain number (six) were shifted from apple to *S. rubra*, and an equal number from *S. rubra* to apple, and so with each succeeding stage. Thus if the larvæ had lived there would have been the following groups when they were full fed :—

1. Fed upon ordinary apple during stage 1, and *S. rvbra*, stages 2–5.
2. „ „ „ stages 1–2, „ „ 3–5.
3. „ „ „ „ 1–3, „ „ 2–5.
4. „ „ „ „ 1–4, „ stage 5.
5. „ „ „ „ 1–5.

And again,

1. Fed upon *S. rubra* during stage 1, and upon ordinary apple, stages 2–5.
2. „ „ „ stages 1–2, „ „ „ 3–5.
3. „ „ „ „ 1–3, „ „ „ 4–5.
4. „ „ „ „ 1–4, „ „ stage 5.
5. „ „ „ „ 1–5.

I think there is no doubt that a careful comparison of these ten groups (which would in all cases have been kept separate as soon as their food was changed) would have very completely answered the question of which the solution was sought in this series of experiments. I have given an account of these experiments—although they yielded no results owing to the unfortunate and altogether exceptional season—because it is likely that future work on these lines will be successful in throwing some light on this difficult subject, and because it is to be hoped that others may be induced to assist in these investigations.

Series V.

Eggs were laid by a female moth bred from a larva which had been fed during 1884 for the whole period of larval life upon *Salix rubra*, and which was rather on the yellow side of an intermediate variety. (The larva was one of those mentioned in the paper quoted above, p. 300.) The eggs were fertilised by a male moth bred from a yellowish larva found upon *S. rubra* on the River Cherwell August 7th, 1884, and mentioned on p. 301 of the paper quoted above, as the larva in the last stage without the brownish-red spots. The larva had been fed upon apple from August 10th—18th, without causing any change of colour (see pp. 302 and 303). Thus the hereditary tendencies should be towards a rather strong yellowish variety, if they are due to a compromise between the tendencies of the two sexes. A large number of eggs were laid in June, 1885, which hatched at the beginning of July, but there was great mortality among the larvæ in all stages, but especially when they were very young. A careful examination of the few surviving larvæ was made on August 12th, all the others having died before the period at which it was possible to make any trustworthy observations of their colour.

1. *Salix viminalis* (*upper side*).—One larva (hatched July 2nd) had been fed upon the leaves of *S. viminalis*, folded and sewn so that only the upper side was exposed. The larva died August 10th in the last stage, after failing for some time; it was a very green intermediate variety, and although it had very little yellow about it, the contrast with larvæ fed in a normal way upon the leaves of this food-plant was most interesting (although the different parentage must be taken into account).

2. *Salix cinerea.*—Two adult larvæ (hatched July 2nd) had been fed upon this plant for their whole life. One was decidedly but not strongly on the yellowish side of intermediate; the other strictly intermediate. By August 20th both had ceased feeding without further change.

3. *Populus nigra.*—Two larvæ (July 3rd) had not thriven at all. One was in the fourth and one in the third stage; the former evidently tending towards the whitish variety, but they were too young for any certain conclusions, and by August 20th both were dead, without any further results.

The review and comparison of these results is, on the whole, disappointing.

Salix viminalis, upper side only (No. 1).—The result of this experiment was very interesting. The larva was frequently placed side by side with others upon ordinary *S. viminalis*, and the difference was extremely marked. I do not think that too much weight must be attached to hereditary influence in producing this effect, because the other sets of experiments in this series do not prove the influence to be as strong as I should have expected from the colour of the parent larvæ.

Salix cinerea (No. 2).—The effects compare unfavourably with those produced by this food-plant upon the larvæ of Series III (Nos. 11, 12, 13), for the latter were rather more strongly influenced in the direction of yellow, while the hereditary tendency was presumably weaker. At the same time the effect was more marked than in the set of larvæ to which the parents belonged; and there was nothing at all unusual in the results themselves.

Populus nigra (No. 3).—There is little to be said about this result. The larvæ were too young to warrant any conclusion, but they were whitish when they died. At the same time the larvæ of Series III (No. 14), which were fed upon this food-plant were also whitish when young, while those that lived progressed in the direction of yellow; so that the most mature was an intermediate variety at the time of its death. It is probable that the larvæ of Series V may have also changed in the same direction if they had lived.

It is noteworthy that the strong hereditary influence in the direction of yellow, which we should suppose existed in Series V (because of the colour of the parent larvæ), depends chiefly upon the male parent; and how far this element asserts itself in opposition to the other sex is quite unknown in this class of experiment. Indeed, a large number of data of this kind might be valuable in gauging the relative strengths of the sexes in this form of heredity, but the present data are far too limited to be regarded as a serious contribution to this aspect of the subject.

3. *The General Results of the Breeding Experiments.*

It is now necessary to consider how far the questions suggested at the beginning of this paper have received answers from the experiments which have been detailed above.

(1.) With regard to the first question, it is, I think, certain that the larval tendencies towards certain colours are transmitted, as was proved by the fact that the parent larvæ had very strong tendencies towards the whitish variety, while in the next generation only a single yellowish form appeared out of seventy-five larvæ. On the other hand, there was conclusive evidence of the modified tendency towards white in the offspring following the change wrought in the parents by food-plants with strong tendencies. Thus, although food-plants such as *S. rubra* (tending strongly towards yellow) did not produce yellow varieties, yet the larvæ were, as a rule, yellower than in the case of the parents. There was no difference between the parents and offspring in the results of food-plants which tended strongly towards white, these being strong enough to overcome any ordinary hereditary tendencies. The results obtained by comparing the different series together are less conclusive, but it is unfortunate that a really satisfactory number of larvæ was only obtained in one case (Series III), the others being insufficient to afford any very convincing comparison. The comparison between Series I and III was certainly, as far as it went, in favour of a stronger tendency towards white in the former series, such as we should expect from the parentage. Series II is the one about which there is so much obscurity, but its results were rather irregular when compared together and with those of the other series. In Series V we should expect a greater predominance of the yellowish tendency, if the male parent is of equal importance with the female in this respect, but the data were very insufficient.

But it must be clearly understood that the question is really settled, because of the wonderfully uniform results of the comparison between parents and offspring as a whole, in which comparison we are dealing with strong and definite tendencies; while in the case of the offspring we are considering delicate differences between such tendencies, which are obviously much more difficult to detect and need far larger data for their accurate determination.

(2.) As to the second question, I think it may be said that conclusive experimental proof has been afforded of the theory brought forward in my last paper—that the colour of the leaf, and not its substance when eaten, is the agent which influences the larval colours. It seems to me that this is proved by the breeding experiments in Series III, in which the larvæ from the same batch of eggs were whitish intermediate and white after being respectively fed upon *S. viminalis* and upon leaves of the same plant sewn so that only the under sides were visible (Nos. 6, 7, and 8).

On the other hand an intermediate variety was produced by feeding a larva from another batch of eggs upon similar leaves sewn so as to expose the upper sides. (Series V, No. 1.) The same thing is proved by a comparison of two sets of larvæ from the same batch of eggs, fed respectively upon *S. triandra* and upon the leaves of the same plant from the under sides of which the whitish bloom had been removed.

Concerning the food-plants, about which the evidence was conflicting, the experiments have in some cases helped to clear up the difficulties. The greatest of these difficulties concerned *S. viminalis* and *S. triandra*, but in the latter case there really was no confliction of evidence, as Mr. Boscher's white larvæ were found upon the very similar but much whiter *S. alba*. As to *S. viminalis*, the difficulty does not at first sight appear to be cleared up by the breeding experiments, but I will defer its consideration until after detailing my experience with captured larvæ, for what I believe to be the correct solution presented itself to me from the results of this part of the investigation. The experiments upon crab produced exactly the same results as in 1884: this will also be considered later. With regard to other food-plants, the view I previously expressed that *S. Smithiana* tends to produce whitish intermediate varieties, is on the whole supported, and so also in the case of *S. babylonica*, which as I suggested, is similar in its effects to *S. rubra*. The effects of various plants hitherto untried have also been observed as a result of the experiments and work in the field.

As to the occurrence of individual variation in larvæ from the same batch of eggs and fed upon the same food-plants, it is now quite certain that such variation may take place, but any considerable divergence is very exceptional.

Thus in the twenty-three larvæ bred in 1884, there was practically no individual variation, while in 1885 there were only eight instances out of seventy-five larvæ, and in none of these instances did the variation amount to more than a remove of one place from that which contained the largest number of larvæ, and which therefore represented the normal result of the food-plant for each particular experiment. In such a calculation the differences between the larval colours are arranged in five classes, *i.e.*, white, whitish intermediate, intermediate, yellowish intermediate, and yellow. The difference between any two of these is very small, and hence it is seen how entirely insignificant was the amount of individual variation even in the few cases in which it occurred. In one instance only was there a variation on both sides of the normal result, *i.e.*, in Series III, Nos. 11, 12, 13, where seven larvæ fed upon *S. cinerea* became intermediate in one case, yellowish intermediate in five cases, and yellow in one case. Here there is a difference of two places between the extremes, but one larva only varied in each direction, while five remained normal. Thus, although this is by far the most striking instance of individual variation met with in about a hundred bred larvæ in 1884—1885, it is by no means extreme, and cannot alone explain such excessive variations as have been met with in the field out of about an equal number of instances. I refer especially to the instance recorded in my last paper (p. 302), in which a bright yellowish variety was found upon apple, the divergence from the normal result being as wide as possible (five places). Another almost equally striking instance was met with this year (as will be recorded) upon *S. cinerea* in the field, one larva being whitish intermediate and four others yellowish. Here the divergence amounts to four places, and compares in an interesting way with the lesser divergence in the larger number of larvæ bred upon the same plant. A divergence equal to that upon *S. cinerea* was recorded in my last paper (p. 301) upon *S. ferruginea*, one larva being yellowish and three whitish intermediate. It is possible or even likely that considerable divergence is occasionally caused by individual variation, but that this is not the only or indeed the chief explanation of the few instances of extreme divergence recorded, is proved by the fact that such variation only occurs when the probabilities are greatly in favour of correspondingly different hereditary tendencies, and that a much greater uniformity prevails when the larvæ are bred from the same batch of eggs. The former argument is enforced by the fact that the captured divergent larvæ are sometimes of different age (and therefore probably of different parentage) from the normal larvæ upon the same tree. It must be clearly understood that in speaking of these extreme divergences in the field, I am not alluding to such instances as Mr. Boscher's eighteen yellow larvæ upon *S. viminalis*, or my own instances of yellow larvæ upon crab. I believe that these are to be interpreted in another way which will be explained later. There are altogether three factors which determine by their relative predominance the colour of these larvæ: (1) the tendency produced by the food-plant; (2) the hereditary larval tendency; (3) individual variation. (It does not signify for the present purpose whether the third factor is a definite and independent tendency, or merely a variable disturbance of a normal equilibrium between the first and second factors, or an irregular recurrence to the influences of earlier generations.) Of these three factors the third has been shown to be comparatively unimportant, while many extreme exceptions are explicable by the second. But I shall show later that the first factor may also produce variable results in the case of the same food-plant, and it is to such a cause that we must refer the interpretation of the conflicting testimony concerning the effects of *S. viminalis*, &c. At the outset it would be unlikely that the other two factors could have produced the exceptions (upon *S. viminalis*, &c.), because of their number and uniformity upon certain varieties of the food-plant. (See Mr. Meldola's account of Mr. Boscher's captures, pages 241 and 306 of the English translation of Weismann's "Studies in the Theory of Descent," Part II.) I was very interested to find that two of the bred larvæ possessed the red spots. In my last paper I pointed out (p. 309) that the occurrence of the spots upon the yellowish variety only was an "argument against the conclusion that these effects are in any way due to the food-plants." It was, therefore, very satisfactory to find a spotted larva which did not advance beyond the intermediate variety, and which at an earlier stage was even whiter. (Series III, No. 17.) The other instance was in accordance with the observation (which was universal until the above recorded instance appeared) that the spots are always found upon yellow varieties, for out of about a hundred bred larvæ in 1884 and 1885, there was only one yellow variety, and this, with one exception, was the only red spotted larva. But if the spots were always necessarily connected with one variety, this would not prove that there could be no larval colour modifications, depending on the colour of the food-plant (in fact nothing can do away with this conclusion now that it has so firm a basis of experimental proof). There are many reasons for thinking that the ancestral form of the larva was yellow, brightly spotted, and ornamented in other ways which are suggested in the larval ontogeny. (See "Proc. Ent. Soc. London," 1885, Part II, August, pp. 290—296.) The newly hatched larvæ are always brightly yellowish even when fed upon apple. The particular form of protection now gained by the larva, by a resemblance to the foliage of its food-plant, has involved the laying aside of this ornamentation, but some of its features occasionally appear (by reversion), and when this is the case they are associated in nearly all cases with the ancestral ground-colour. It is possible that the differences of ground-colour which are now dependent on the food-plant arose independently, and persisted for a long time as ordinary cases of dimorphism or polymorphism, and that their relation to the colour of the food-plant was determined by natural selection at a much later date. But although the differences may have commenced in this way, they did not probably reach anything like their present condition until they came to depend on the food-plant, for without such a relation the colours would often render the larva conspicuous instead of protecting it.

4. *Observations in the Field upon Larvæ of* S. ocellatus *during* 1885.

The larvæ were very abundant last year and the results were more uniform than in 1884. An account of all the captured larvæ is given below.

August 2nd.—Upon *Salix rubra* in some fields by the River Cherwell, near Oxford, seven nearly full-grown larvæ and one small in the fourth stage, which was a bright yellowish variety. Of the former number four were bright yellowish varieties, and three were well on the yellow side of intermediate, almost good yellow varieties. Also upon *S. cinerea* in the same locality, one nearly adult larva which was a good yellow variety. Also upon a small tree of *S. babylonica* in a garden at Oxford (the same tree upon which seven larvæ were found in 1884; see "Proc. Roy. Soc.," No. 237, 1885, pp. 301 and 302), one nearly full-grown larva decidedly on the yellow side of an intermediate variety but not strongly yellowish.

August 3rd.—Upon *S. viminalis* on the River Cherwell, near Oxford, three larvæ in the last stage (upon the same tree) of which one was nearly adult and rather yellowish, but not more than an intermediate variety; while the other two were much less advanced in the last stage, and were whitish varieties.

August 4th.—Upon *S. rubra* by the Cherwell (as above, August 2), two nearly adult bright yellowish larvæ.

August 9th.—Upon *S. rubra* by the Cherwell (as above), one nearly adult bright yellowish larva. Also upon *S. cinerea* in the same locality, two almost full-fed larvæ which were good yellow varieties. Also upon *S. linearis* in the University Parks, one larva which was perhaps slightly on the yellow side of an intermediate variety.

August 16*th.*—Upon *S. rubra* by the Cherwell (as above), one nearly adult bright yellowish larva. Also upon *S. cinerea* in the same locality two nearly adult larvæ (on the same bush), of which one was slightly on the whitish side of an intermediate variety, while the other was a rather bright yellowish variety. Also upon *S. triandra* in the same locality one almost full-fed bright yellowish larva.

August 23*rd.*—Upon *S. Smithiana* at Binsey upon the Isis, near Oxford, one nearly adult larva on the whitish side of an intermediate variety. Also upon *S. triandra* in the same locality, one nearly adult bright yellowish variety. Also upon *S. triandra* upon the Isis at Medley Weir near Oxford, one bright yellowish larva which had just entered the last stage.

August 25*th.*—Upon *S. rubra* (Cherwell) three small larvæ towards the end of the third stage, all strongly yellowish varieties, but differing somewhat in intensity.

August 30*th.*—Upon *S. triandra* close to the bridge at Ferry Hincksey, near Oxford, one nearly adult larva which was a good yellowish variety, but rather whitish on the back. Also upon ordinary apple in a garden at Oxford one very white variety at the end of the fourth stage (changing its skin).

September 11*th and* 12*th.*—Upon a variety of *S. alba* with small narrow leaves, having smooth greenish under sides; in a dry part of the bed of the river at Visp, Switzerland, two full-fed strong yellow varieties (although not the strongest because of the want of a distinct yellow tinge to the under surface). Both had the very sharply marked and distinct white stripes which are often found on larvæ with this tint of ground-colour. Also September 12th, near the stream which flows through Brigue, Switzerland, three larvæ upon different species of sallow. Upon a variety of *S. alba* very similar to the above (? *S. vitellina*), an adult intermediate variety rather strongly tending towards the yellowish form upon its dorsal surface, and having very distinct white stripes, such as were possessed by the larvæ from Visp. Upon *S. incana*, a yellow variety of the larva, looking as though it would have been very yellow if it had been in a healthy condition. But the larva, which was well in the last stage, was much stunted and in very bad health, having been attacked probably by some parasite, and pierced in twenty-eight places. Also upon *S. alba* (the leaves much like the common English form), a larva which was advanced in the last stage, and an exceedingly white variety—the palest I have ever seen. There was a little yellow on the under side, but it was not at all the tint of the yellowish varieties, and indicated no transition in that direction. The larva did not seem to be very healthy. It possessed in common with all very strong white varieties a distinct trace of the subdorsal line for its whole length, and there was a trace of the darkening for a border to a "ninth stripe" upon the third thoracic segment in front of and parallel with the more distinct and larger "eighth stripe" upon the first abdominal segment.

5. *Experiments upon captured Larvæ.*

Being engaged in the extensive breeding experiments already described, I did not attempt much with the captured larvæ, especially as nearly all of the latter were full-grown when found. The strongly yellowish larva in the fourth stage, found August 2nd on *Salix rubra*, was put upon apple on August 3rd, when it was 24 mm. long. On August 27th it was 51 mm. long, and was much affected by the change of food, being an intermediate variety, or perhaps slightly on the yellowish side of intermediate. It was interesting to note that the change of colour affected the shagreen dots, which became white, having been formerly yellow as in all strongly yellowish varieties.

6. *Conclusions arrived at by the Consideration of the captured Larvæ: The Reconciliation of conflicting Evidence.*

The colours of the captured larvæ were wonderfully uniform for their respective food-plants. *Salix rubra* produced a large number of yellow larvæ, and others which were but little removed from yellow. The larva upon *S. babylonica* resembled these latter. *S. triandra* also produced yellow larvæ, and so with *S. cinerea* (with one exception). There was but little confliction in the results of *S. viminalis*, and *S. Smithiana* produced a normal larva, and the colours of the Swiss larvæ (with the exception of that upon *S. incana*) might have been almost exactly anticipated by investigating the colour of the under sides of the leaves. Thus there are fewer exceptions than in the larvæ captured in 1884, and yet among them was one instance which suggested to me the explanation of those conflicting results which have been the chief obstacle to the complete acceptance of my theory of the colour-relation between food-plant and larva; I mean especially the immense difference between Mr. Boscher's experience (quoted by Mr. Meldola as above referred to) and my own with regard to *S. viminalis*. The larva which suggested the interpretation was the yellowish intermediate variety found August 9th upon *Salix linearis*. I had much wished to find a larva upon this foreign species of Salix, of which there are many fine specimens in the Oxford University Parks; for I had noticed for over a year that the under sides of the leaves were more densely covered with down and whiter than any species of Salix I had ever seen, and even more so than apple. The upper sides of the leaves were dark-green and glossy, and the leaves were narrow, pointed, and very small, and extended at right angles to the twigs. The leaves resembled those of *S. viminalis*, only they were much smaller and whiter underneath. I thought that such a tree must produce the most extreme white varieties, and I was greatly astonished at the colour of the single larva found upon it. Thinking over such a result, I remembered that the one bright yellow variety which I had found upon *S. viminalis* (on August 11, 1884; see p. 301 of the above-mentioned paper) was upon a variety of the latter plant with very small leaves, while the white larvæ were found upon the large-leaved variety which is the common one at Oxford. Shortly afterwards, through the kindness of Mr. Boscher and Mr. William White, I had the opportunity of looking at some twigs of the trees upon which the eighteen spotted yellow larvæ were found (see p. 304 of the former paper, &c.). *The trees were the small-leaved variety,* and Mr. Boscher states that all the yellow larvæ were found upon such food-plants. Then again I remembered that in the case of the very bright yellow variety found upon crab (*var. acerba*) on August 14th, 1884 (see former paper p. 301), the latter tree had very small leaves. Finally, *Salix incana* at Brigue possessed leaves which were very white and downy underneath, but they were very small, and the larva found upon the tree was yellow.

All this convergent evidence suggested the following explanation. The larvæ are only affected by that part of the environment which is so close to them as to be almost or quite in contact; the tint of mature life is (as far as it is caused by the colour of the food-plant) a resultant of the conflicting tints which have formed part of the immediate environment of the larva throughout its life, and the ultimate predominance of one larval tint is due to the relative proportion of the whole larval life during which that tint predominated in the environment. This conclusion is also supported by the breeding experiments and the experiments upon captured larvæ. Such being the case, the ultimate whiteness of a mature larva is largely due to the considerable proportion of its earlier life which is spent upon the white under sides of the leaves. (The young larvæ invariably take up this position.) During this period white is the only colour in its immediate environment, except when it is actually engaged in eating, and so may perhaps be affected to some extent by the colour of the upper sides. But when the larva reaches a certain size and weight, it must in nearly all cases quit this position and retire to the stem, because the leaf is not strong enough to bear it without being dragged into an unnatural position, or because it is too small to form a background for the larval body. Therefore the time at which it retires must chiefly depend upon the size and strength of the leaves. Having once quitted the small leaves the larva does not again rest upon them, because they can be entirely eaten from the stem, whereas the large leaves cannot be reached without venturing upon them, and therefore in the latter case, the chances are in favour of the larva being left upon a partially eaten large leaf during many of the periods of rest, even at an advanced stage. When the larva is smaller and eats much less it remains on the same leaf for many days. But directly the larva rests on the stem the tints of its immediate environment alter, for they are then due to the colours of both sides of the leaves and of the stem itself. The relative predominance of the colours of the two sides of the leaves depends upon the position of the larva and the arrangement of the leaves. But the position of the larva is uniform (except when it is wandering to a fresh twig), the head being always directed towards the apex of the stem. Hence in the case of food-plants whose large leaves regularly droop over from the vertical twigs, or are curved in the usual way with the concavity downwards, the tints of the under side still predominate after the larva has retired to the stem, and they will still form almost the only effective colour as was the case in earlier periods. When, however, the leaves hang irregularly or spread out horizontally from horizontal twigs, the colours of the two sides may be equally important, or may depend (in the latter case) upon the side of twig on which the larva rests.

This explanation of course will apply but little to leaves of which the upper and under surfaces are approximately similar in colour, and accordingly there is very little conflicting testimony from such food-plants (*Salix rubra*, *S. babylonica*, *S. triandra*), and such as there is, is mainly explicable by variations in the other two factors which go to influence larval colour. But even in these plants there is some difference between the colours of the two sides which would have an effect upon the larvæ, as was proved by the experiment in which the bloom was rubbed off in the case of *S. triandra*. But in the case of leaves with strongly white under sides such an explanation accounts for all the conflicting evidence met with in the field (due to this factor and recognised by the uniformity cf results from trees with leaves of a particular size and arrangement). Thus when I expressed the opinion that *Salix viminalis* produces white larvæ I was thinking of our common long-leaved variety. The leaves of this variety often grow 6 inches long near Oxford, and quite three-quarters of an inch wide. Such leaves would retain a larva until the end of the fourth stage, and often far into the last stage. Furthermore the long leaves droop over very regularly from the higher vertical twigs upon which the larvæ are generally found, and so present their under sides to the stem and to a larva resting upon it. On the other hand, the leaves of the other variety are much smaller (from memory I should say that they are often about an inch and a-half long and three-eighths of an inch wide), and often hang in irregular wisps from the more vertical twigs, or droop vertically from the more horizontal branches. Thus such a variety of leaf would retain the larva for a comparatively short time, and after its retirement to the stem the colour of the immediate environment would be as largely due to the upper as the under side. Hence the effect of the narrow and small-leaved *S. viminalis* in producing yellow larvæ, and the exceedingly white-leaved (under sides) *Salix linearis* in producing a yellowish intermediate larva. The influence of *S. cinerea* (in the direction of yellow) is probably in a great measure due to the same facts, for its small leaves are often downy underneath, and are always much whiter than those of *S. rubra*, &c. The much stronger influence of *S. Smithiana* towards white is probably due to its much larger but very similar leaves. It is likely that some of the irregular results referred to other factors may be explained in this way. Thus it was very obvious that the leaves of *S. ferruginea* (?), upon which very differently coloured larvæ were found (p. 301 of the former paper), varied very much in size, those on the lower branches looking like rather narrow leaves of *S. cinerea*, the upper ones being exactly like those of *S. Smithiana;* but in such an isolated case it is not possible to determine certainly which of the factors caused the exception, or whether it was due to a combination of causes. I think it is unlikely that any great difference could be caused by a slight variation of habit in larvæ, *i.e.*, in the period at which different individuals would retire to the stem from leaves of the same size. It is probable that the habit is very uniform, and always leads the larvæ to remain on the leaves as long as the size and strength of the latter will permit them to do so. In *Salix alba* the question is complicated by the fact that after the larva retires (early in this case) to the stem, the whiteness of the environment will partially depend upon nearness to the apices of the twigs, for the upper sides of the young leaves are white as well as the under sides. The exceedingly strong influence of apple is readily explicable by the considerations advanced above; for the leaves are large, broad, and strong, and will take the weight of a larva advanced in the last stage without bending. The single larva found upon apple in 1885 (August 30th) was resting before changing its skin for the last time on the under side of the leaf, and I have often before found the large larvæ in the same position. After the larva retires to the stem the apple leaves form broad curved white surfaces, which everywhere environ the (presumably) sentient part of its body, which is always directed during rest towards the apex of the twig. Upon all the varieties of food-plant, and especially upon apple, the larva tends to rests upon the young and vigorous twigs which stand out from the trees and bear fewer larger leaves at wider intervals, and with more regular arrangement than those upon the older wood below. Thus the larva gets the maximum effect from the under sides of the leaves after it has retired to the stem. This explanation also helps towards clearing up the difficulty about the irregular effect of crab. Although the under sides of the leaves are smooth and green, they are generally of a whitish-green, and I think that a white larva is better protected when resting on the under side of the leaf than a yellow larva would be, although this is often true in the case of trees which are known to produce yellow larvæ. Again the leaves are large as a rule, and so the larvæ are advanced when they rest on the stem, and even then the arrangement of the leaves and the position of the larva cause the under side to contribute most colour to the immediate environment. I have already mentioned that the bright yellow, red-spotted larva captured in 1884 upon crab, *var. acerba*, was upon a tree with exceedingly small leaves. Furthermore, in this variety the leaves are extremely variable in size upon different parts of the tree. But although the conditions mentioned above may have conduced towards the fact that my bred larvæ fed upon this plant became so white, I cannot but think that such a result was largely due to their strong hereditary tendency towards this colour, for the crab cannot compare with ordinary apple in the whiteness of the under sides of the leaves, nor is it in this respect equal to *Salix viminalis*, *Smithiana*, or even *cinerea.* I think that this food-plant more than any other requires further experimental work with larvæ of all varieties of hereditary tendency, but it is very unfortunate that the larvæ do not seem to thrive upon the plant, at any rate in confinement.

Another great difficulty is, I think, completely explained by the above-mentioned consideration. I mean the fact that bred larvæ tending strongly towards white, became intermediate in 1884 and in many cases in 1885 when fed upon the large-leaved variety of *Salix viminalis* (for I have always fed my larvæ upon this variety). In order to obtain the best leaves I have to walk to the Cherwell and take a boat; and as this is not always convenient, I bring home and give to the larvæ a great quantity. The leaves being very long and crowded in the glass cylinders in which the larvæ are kept, their natural arrangement is entirely altered, so that the upper sides are presented to the larvæ to a much greater extent than happens on the tree. The result is to affect the environment of the larvæ upon the leaves as well as those upon the stem, for in the former case the upper sides of other leaves must be often crowded close up to the under side of the one upon which a larva is resting. Furthermore, the leaves do not last so long without withering as upon the trees in the open air, and therefore the larvæ are frequently compelled to wander on to fresh leaves, and in so doing they must be affected by the colour of the upper as well as the under sides. In the future it would be well to breed some larvæ in large cases which would hold the twigs without overcrowding, and would permit the leaves to fall naturally. In the case of apple the arrangement of the leaves has not been disturbed in the cylinders, because I can get the twigs in my garden, and because the leaves are of a more manageable shape.

It is possible that the same causes have helped to produce yellower results than are normal in the breeding experiments with *Salix alba* and *S. Smithiana*, for these leaves have been somewhat disturbed, although not nearly so much as in the case of *S. viminalis.* I do not doubt the validity of this explanation for the latter plant, and the results of experiments are thus satisfactorily interpreted which have been sources of difficulty and uncertainty since the summer of 1884. This explanation also clears up what I felt to be a great difficulty in my former paper when I wrote the words (page 314) "it is only the part of the environment imitated which produces any effect, *e.g.*, the under sides of the leaves in the case of *S. ocellatus*, and yet the environment, of course, includes both surfaces." I have shown above that the effective part of the environment—the immediate environment—does *not* in many cases include both surfaces, but either entirely or chiefly the under surface, *i.e.*, that which is *ipso facto* imitated, and when it does include the other surface for a sufficiently long period, a different effect is produced (in the case of leaves with differently coloured sides). It is therefore obvious that when we speak of the tendency of a plant to produce a certain colour, we mean a tendency from the size and arrangement of the leaves to encourage a larval position in which the effective colour of the environment is only contributed by one leaf surface, or, on the other hand, a tendency to change the larval position into one in which both surfaces may become equally effective, or, again, into one in which either of them may predominate. In this explanation of what is meant by the tendency of a plant, I am, of course, especially referring to those with leaves having white under sides; but it will probably apply to some extent in nearly all cases, for there is always some difference between the two sides of the leaves.

There is one other comparison between the captured and bred larvæ which is a source of difficulty. The exceedingly uniform results upon *Salix rubra* in the field (I have found altogether twenty-two larvæ, of which eighteen were yellow, three yellowish intermediate, and one intermediate) render it more than probable that the plant has possessed an influence sufficiently powerful to reverse a larval tendency in the direction of white (for it is very unlikely that in all the eighteen instances of a maximum result the larva happened to tend in the same direction as the food-plant); and yet in the breeding experiments, out of nine larvæ eight were yellowish intermediate and one intermediate; and thus in no case has the food-plant completely overcome a strong tendency (1884) or a somewhat modified tendency (1885) towards white.

I have thought that part of this difference (also observable in the cases of *S. babylonica* and *S. triandra*) may be due to the fact that the tops of the glass cylinders in which the larvæ are bred, are covered with white muslin, which probably, therefore, produces some slight effect on the larvæ. Again the larvæ in the field are probably affected by the amount of light, and especially direct sunlight, which must brighten the colours of their environment. I have commonly found them on *S. rubra* (as in the other trees) upon the higher younger branches standing out from the trees, and especially during the past summer upon twigs that have been allowed to grow out from the tops of hedgerow sallows; and I have also noticed that they are better protected in these strong lights because of the brightness of their own colour than in the shadows of the lower and more crowded leaves. The bred larvæ have never been so freely exposed to light, and although the small leaves of the food-plant do not become much disarranged (probably hardly any effect would be produced if they were), yet the crowding certainly helps to shade the leaves and to diminish the brightness of their colours. (During the past summer I have kept the larvæ under a north window to protect them as far as possible from the excessive heat.) It is very probable that some of the difference is to be explained in this way, but most of it is no doubt due to the hereditary tendencies of my bred larvæ, which were always towards white, while this tendency is probably less common than the other by the banks of streams (see former paper, p. 310). It will be very interesting in future experiments to breed the larvæ under yellow and under white glass. Next year I hope to be able to make such experiments.

7. *The Whole of the Evidence Summarised.*

I have arranged all the results hitherto obtained in a table which is printed below. The only important omission is the hereditary tendency of the bred larvæ, and this would have rendered the table too complicated, for there were four different series in 1885. It will, however, be remembered that all the larvæ bred in 1884 tended strongly towards white, while nearly all of them in 1885 possessed the same tendency in a slightly modified form. All the numbers without reference marks refer to my own observations or experiments conducted at Oxford; while special marks call attention to the work of other observers, or to my own work in other localities. The 204 instances given below comprise all the cases in which the colour of the larva and the name of the food-plant have been noted, either in breeding or in field observations.

Only in the case of the white varieties captured upon ordinary apple have I ventured to allude to the larvæ under the vague term "very common," because such has been my experience and that of other observers, although no list of instances has been kept, and therefore no number can be quoted. Had I left the space blank it would have conveyed an entirely wrong impression of a very general experience. It must be understood that in the following descriptions

Food-plant.	Description of under sides of leaves.	Size of leaves.	Nature of evidence.	Dates.	Colour of larvæ.					Totals.	Additional notes and general conclusions.
					White.	Whitish intermediate.	Intermediate.	Yellowish intermediate.	Yellow.		
Apple	White pubescent	Large and broad	Breeding experiments	1884	5	..	..	..	..	5	Tends very strongly towards the most typical variety. Only one exception yet known, although this was upon a tree which was in every way normal.
				1885	6	..	..	..	..	6	
				Other dates	3*	..	..	..	..	3	
			Observations in the field	1884	1†	..	..	..	1†	2	
				1885	1	..	..	..	..	1	
				Other dates	Very common						
				Totals	16	..	..	..	1	17	
Apple .... (under sides)	Ditto	Ditto	Breeding experiments	1885	1	..	..	..	..	1	The same effects as the normal leaves.
Apple .... (upper sides)	(Upper sides are dark green and glabrous)	Ditto	Breeding experiments	1885	3	..	..	..	..	3	I am not yet satisfied that the reverse effects may not be produced. The larvæ did not arrive at maturity.

* Mr. E. Boscher's observations and experiments at Twickenham.
† In a garden at Reading.

Crab …… (cultivated)	Whitish-green and glabrous	Ditto ……	Breeding experiments	1884	5	..	..	..	..	5	Very contradictory results. The effect of this plant needs reinvestigation with larvæ inheriting all varieties of tendency. The bred larvæ evidently inherited strong tendencies towards the whitish variety.
			Observations in the field	Other dates	..	..	..	..	2†	2	
				Totals	5	..	..	..	2	7	
Crab (wild) var. *acerba*	Green and glabrous	Broad, very variable in size, but generally smaller than those described above	Breeding experiments	1885	9	..	..	..	..	9	Same conclusions as those given above. The bred larvæ were supplied with leaves of different sizes. The captured larva was upon a very small-leaved tree. The former larvæ inherited strong tendencies towards the whitish variety.
			Observations in the field	1884	..	..	..	..	1	1	
				Totals	9	..	..	..	1	10	
Salix viminalis	White, with dense satiny down	Long and narrow, extremely variable in size	Breeding experiments	1884	..	..	4	..	..	4	The large-leaved varieties tend towards white, the small-leaved forms towards yellow. The bred larvæ were fed upon large leaves, but their tendency was somewhat modified by the displacement from their natural position, which resulted from crushing them into the breeding cages. The nineteen captured yellow larvæ were upon the small-leaved variety: two of the white ones were upon the large-leaved form, and the other two, with the intermediate larva, upon a tree with leaves of an intermediate size.
				1885	4	5	..	..	..	9	
			Observations in the field	1884	1	..	..	..	1	2	
				1885	2	..	1	..	..	3	
				Other dates	1	..	..	..	18* (about)	19	
				Totals	8	5	5	..	19	37	

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* Mr. E. Boscher's observations and experiments at Twickenham.

† In a garden at Reading.

Food-plant.	Description of under sides of leaves.	Size of leaves.	Nature of evidence.	Dates.	Colour of larvæ.					Totals.	Additional notes and general conclusions.
					White.	Whitish intermediate.	Intermediate.	Yellowish intermediate.	Yellow.		
S. viminalis (under sides)	Ditto	Large-leaved varieties were used	Breeding experiments	1885	1	..	..	..	..	1	Probably always towards white.
S. viminalis (upper sides)	(Upper sides are generally dark green, and glabrous)	Ditto	Breeding experiments	1885	..	..	1	..	..	1	Probably always towards yellow.
Salix linearis (Forbes)	Whiter and with denser down than the last	Very small and narrow	Observations in the field	1885	..	..	..	1	..	1	Probably towards intermediate, or either side of it, according to the leaf-surface, which forms most of the immediate environment.
Salix incana	Like *S. viminalis*	Long, narrow, and small	Observations in the field	1885	..	..	..	..	1†	1	Probably towards yellow, like the small-leaved *S. viminalis*.
Salix alba	The young leaves with a glistening silky down, present but less obvious on the older leaves	Moderate size, like those of *S. triandra*, but smaller	Breeding experiments	1885	..	..	..	2	..	2	Evidently strongly towards white, as shown by the 20 instances in the field. The bred larvæ may have been affected by disarranged leaves, and they did not reach maturity.
			Observations in the field	1885 Other dates	1† 19*	.. ..	.. ..	.. ..	.. ..	1 19	
				Totals	20	..	..	2	..	22	

* Mr. E. Boscher's observations and experiments at Twickenham.

† In Switzerland.

Salix alba, ? var. *vitellina*	The whole effect green, with a bluish-white "bloom." Quite smooth and downless	Narrow and small	Observations in the field	1885	..	..	..	1†	..	1	Probably the tendency is generally towards yellow.
Salix alba var. ?	Ditto	Ditto	Observations in the field	1885	..	..	..	..	2†	2	Almost certainly a normal result.
Salix Smithiana	White, with dense satiny down	Large and rather narrow, but vary much in size on same tree	Breeding experiments	1885	..	4	..	1	..	5	Larvæ found on the large leaves. Probably the tendency is towards the white side of intermediate, the other varieties having become more affected by the colour of the upper sides. Leaves of the next tree were sometimes used in breeding these larvæ.
			Observations in the field	1884	..	1	..	1	..	2	
				1885	..	1	..	..	..	1	
				Totals	..	6	..	2	..	8	
Salix ferruginea, Anderson (probably)	Similar to *S. Smithiana*	Similar to *S. Smithiana*	Observations in the field	1884	..	3	..	..	1	4	Same results as with *S. Smithiana.* Mr. J. G. Baker, of Kew, thinks that the tree is *S. Smithiana.*
Salix cinerea	Reddish, glaucous, or ashy, downy sometimes, and the amount of down variable. A very variable species	Broad but small	Breeding experiments	1884	..	..	4	..	..	4	Probably generally towards yellow, but exceptions are to be expected, and are, I think, better protected on this tree because of the "dead" appearance of the generally light coloured under sides.
				1885	..	..	2	6	1	9	
			Observations in the field	1885	..	1	..	..	4	5	
				Other dates	..	..	..	..	3‡	3	
				Totals	..	1	6	6	8	21	

† In Switzerland.

‡ Two at Reading, one at Oxford.

Food-plant.	Description of under sides of leaves.	Size of leaves.	Nature of evidence.	Dates.	Colour of larvæ.					Totals.	Additional notes und general conclusions.
					White.	Whitish intermediate.	Intermediate.	Yellowish intermediate.	Yellow.		
Salix triandra	The whole effect is green, but a whitish "bloom" is present. Smooth and downless	Generally large, but varying, rather narrow	Breeding experiments	1885	..	..	6	2	..	8	Evidently in the direction of yellow.
			Observations in the field	1885	..	..	..	..	4	4	
				Totals	..	..	6	2	4	12	
S. triandra (without bloom)	Ditto, without the bloom	Ditto	Breeding experiments	1885	..	..	..	10	..	10	More strongly than the last in the direction of yellow.
Salix babylonica	Like *S. triandra*, only the "bloom" is bluish-white	Small and narrow	Breeding experiments	1885	..	..	1	4	..	5	Strongly in the direction of yellow.
			Observations in the field	1884 1885	.. ..	.. ..	.. ..	6 1	1 ..	7 1	
				Totals	..	..	1	11	1	13	

Salix rubra	Ditto	Small and rather narrow	Breeding experiments	1884	..	..	..	5	..	5	Ditto.
				1885	..	..	1	3	..	4	
			Observations in the field	1884	..	..	1	..	3	4	
				1885	..	..	..	3	12	15	
				Other dates	..	..	..	..	3	3	
				Totals	..	..	2	11	18	31	
Populus nigra	Light green and glabrous	Large and broad	Breeding experiments	1885	..	..	1	..	..	1	Insufficient evidence. I should expect tendency towards yellow.
									Complete total ..	204	

All are my own observations and experiments except the three numbers marked *. All the unmarked observations and experiments took place at Oxford.

of the under sides of the leaves and their size, I am alluding to the trees upon which the larvæ mentioned in the table were found or were bred. There may be varieties in many cases to which my descriptions would not apply.

8. *Conclusion.*

A glance at the table printed above will at once show the nature and amount of the evidence for the conclusion that the larva of *Smerinthus ocellatus* maintains a colour-relation with the food-plant upon which it was hatched, adjustable within the limits of a single life, and that the predominant colour of the food-plant itself is the stimulus which calls up a corresponding larval colour. This may seem to be a long paper to prove the existence of such a relation for a single species, but it must be remembered that in accepting the conclusion now arrived at, we are admitting the existence of an entirely new resource in the various schemes of larval protection by resemblance to the environment, and one which stands on a very different level from all the others; in these the gradual working of natural selection has finally produced a resemblance—either general or special—to something which is common to all the food-plants of the larva, or to some one or more of them, the larva being less protected upon the remainder; but in this case the same gradual process has finally given the larva a power which is (relatively) immediate in its action, a power which enables the organism itself to answer with corresponding colours the differences which obtain between its various food-plants. And the action differs no less from the superficially similar cases of much more rapid changes in the colour of other organisms (amphibia, fish, &c.) corresponding to the changing colours of their environment, for in such organisms the external colours act as a stimulus which, through a nervous circle, modifies the condition of existing pigments; while in the larva it is the amounts and kinds of vegetal pigments made use of and larval pigments deposited that are affected. The influence, in fact, makes itself felt by affecting the absorption and production of pigments rather than their modification when formed; and such a method of gaining protection is, as far as we yet know, unique in the animal kingdom. And such a power is not confined to the species in which its existence has been to some extent completely proved. There are already proofs that many others can maintain a similar colour-relation (see my former paper, and the references given by Mr. Meldola in his translation to Weismann's "Studies in the Theory of Descent," Part II), and I am sure that careful observation will reveal many slight and protective differences among larvæ of the same species when found upon differently-coloured food-plants, and will prove that this power is not at all uncommon among the great body of lepidopterous larvæ which adopt the methods of protective resemblance. Furthermore, it is very probable, as suggested by Professor Meldola, that the colour of the environment will prove to act as one of the determining causes of the larval colours ultimately assumed by the individuals of dimorphic species (which are generally green and brown in lepidopterous larvæ). To show in what a light this colour-relation appears to Dr. August Weismann (whose essay upon "The Markings of Caterpillars" first induced me to work at these organisms), I quote the following sentences from a letter I received from him after sending him my paper in the "Entomological Society's Transactions," Part I, April, 1884, in which this subject is alluded to :—

"Dagegen verstehe ich nicht ganz, wie sie sich den 'phytophagic character of the ground-colour' entstanden denken. Ich habe augenblicklich mein Buch nicht zur Hand u. kann deshalb die Note von Meldola nicht nachsehen, erinnere mich auch derselben nicht. Sie scheinen zu glauben, dass die *Nahrung* der Raupe bis zu einem gewissen Grad ihre Farbe *direkt* hervorrufe. Ich wäre sehr begierig, einen Beweis dafür kennen zu lernen. Ich kann mir nicht denken, wie dies möglich sein, solle jedoch weiche ich den *Thatsachen !* Ich bin begierig, zu erfahren, ob Sie solche inzwischen gefunden haben."

I venture to hope that the facts spoken of by Professor Weismann are now satisfactorily demonstrated, not as proving the former theory to which he alludes, that the food itself causes the change of colour after it has been eaten by the larva, but as proving the existence of the more subtle form of influence described in the present and in my last paper. At the same time I must express my sense of the great extent to which I am indebted to Professor Meldola, to whom we owe the former theory, for without the most suggestive editorial notes to his translation of Weismann's work, and the experiments undertaken by Mr. Boscher at his request, it is most improbable that the present investigation would ever have been begun.

III. "On the Polarisation of Light by Reflection from the Surface of a Crystal of Iceland Spar." By Sir JOHN CONROY, Bart., M.A., of Keble College, Oxford. Communicated by Professor G. G. STOKES, P.R.S. Received January 27, 1886.

(PLATE 2.)

In the year 1819 Sir David Brewster communicated to the Royal Society ("Phil. Trans.," 1819, p. 145) an account of some experiments he had made on the polarisation of light by reflection from the surface of double refracting substances, and showed that Malus' statement with regard to Iceland spar was incorrect.

Malus said that Iceland spar behaves towards the light it reflects like a common transparent body, and that its polarising angle is about 56° 30′, and that whatever be the angle comprehended between the plane of incidence and the principal section of the crystal, the ray reflected by the first surface is always polarised in the same manner ("Théorie de la Double Refraction," pp. 240, 241).

Some years later Seebeck ("Pogg. Ann.," vol. xxi, p. 290; vol. xxii, p. 196; vol. xxxviii, p. 276; vol. xl, p. 462) made a number of very accurate observations on the same subject, and in 1835 and 1837 Neumann published in "Pogg. Ann.," vol. xl, 497, and vol. xlii, p. 1, an account of further experiments that he had made on the reflection of light by Iceland spar.

He begins his second paper by a brief summary of the results obtained by Brewster and Seebeck. "Brewster found that the angle of complete polarisation for calcspar depends on the position of the reflecting surface relatively to the axis, and upon the position of its principal section to the plane of reflection; he also found that when the reflecting surface is covered with a liquid, the plane of polarisation of the completely polarised ray does not coincide with the plane of reflection, but makes a smaller, or greater, angle with this; when a cleavage-face of calcspar is covered with oil of cassia this deviation may amount to 90°. The knowledge of these phenomena has only been further advanced in recent times. Dr. Seebeck has so followed out, by means of most accurate determinations, the influence of optically uniaxial crystals upon complete polarisation, that the angle of incidence at which this occurs can be determined as accurately beforehand as it can by Brewster's law in the case of uncrystallised bodies. Seebeck also discovered that the deviation of the plane of polarisation from the plane of reflection, which Brewster had observed, also occurs when the ray of light falls directly from air on to the surface of the crystal."

Seebeck's observations having been mainly directed to the determination of the angle of polarisation, Neumann's object was to determine the azimuth of the plane of polarisation of the reflected light. They both assumed, contrary to Fresnel's hypothesis, that the density of the ether in the two media was the same and the elasticity different, and therefore that the plane of vibration coincided with the plane of polarisation, and starting with this assumption succeeded in showing that the observed and calculated results were in close accordance.

Seebeck and Neumann only repeated a portion of Brewster's experiments, and no one except Sir David Brewster appears to have made any determinations of the angles and azimuths of polarisation when the spar was in contact with media other than air.

Professor Stokes very kindly called my attention to these experiments of Sir David Brewster, and pointed out that as they had never been published in detail, and had not been repeated by anyone else, it was desirable that further observations should be made on this subject. The experiments, the results of which I have the honour of submitting to the Royal Society, were undertaken at Professor Stokes' suggestion, and in carrying them out I had the benefit of his advice.

The apparatus used was essentially the same as that employed by Seebeck; the divided circle of the goniometer was, however, horizontal, and not vertical, as in Seebeck's instrument, and the arrangement for keeping the reflected ray constantly in the axis of the observing tube, whilst the angle of incidence was varied, differed from that employed by him; the axes of the stage and of the observing tube were fitted with toothed wheels which geared into a double pinion, the diameters of the wheels being such that the angular velocity of the observing tube was double that of the stage.

The goniometer had a vertical stage in addition to the ordinary horizontal one, which could be moved nearer to and further from the axis of the instrument, and this stage had four adjusting screws, so that the front surface could be placed parallel to the axis of rotation.

A brass plate was clamped to this stage; a short brass tube carrying an annular toothed wheel at one end, and a divided collar at the other, fitted into an aperture at the lower end of this plate, and could be rotated in the plane of the plate by turning a milled head fixed at the end of a rod, which carried a bevel pinion working into the annular wheel.

A brass tube with a collar at one end could be fastened to the annular wheel by four screws passing through holes in the collar, the back of the collar and the inner surface of the wheel being portions of a spherical surface.

The crystal whose reflective power was to be examined was fixed in the inner tube, which was then adjusted by means of the four screws so that the surface of the crystal was in the plane of rotation, and then, by altering the position of the vertical stage, the plane of rotation brought vertically over the axis of the goniometer.

The crystal was cemented into the tube with plaster of Paris; a little lard was rubbed on the edges of the face which was to be exposed, and it was placed on a plate of glass with this surface downwards, and the brass tube, the collar of which had also been greased, placed round it, and centred by means of a marked card placed under the glass, and plaster of Paris poured in. In one of the earlier experiments the crystal was found after a certain number of observations had been made to have become loose, owing to the plaster having shrunk away from the tube; three holes about 2 mm. in diameter were therefore drilled in the sides of the tube, and no further difficulty was experienced from this cause, as the little projections of plaster which filled these holes effectually prevented any movement.

In order to adjust the reflecting surface, a diaphragm with a small hole in it was fitted into the eye end of the observing tube, and a lamp with a flat wick so placed that its image was seen by reflection from the surface of the crystal, two of the adjusting screws of the tube being in the horizontal plane (*i.e.*, the plane of incidence). The tube was then turned through an angle of 180°, and the reflected image brought back half way into the centre of the field by altering the two screws of the tube which were in the plane of incidence, and the remainder of the distance by means of the screws of the stage, which, as well as the observing tube, remained clamped to the horizontal circle whilst this adjustment was being made.

The tube was then turned back to its original position, and the adjustment repeated if necessary; the tube was then turned through 90°, and the second pair of screws altered till the reflected image remained in the centre of the field whilst the crystal was rotated. The adjustment was then examined by means of a simple form of diagonal eye-piece placed in the collimator tube of the goniometer, consisting of a brass tube with a diaphragm at either end, with a small aperture in each, and also in the side of the tube; inside the tube, and opposite the aperture in the side, a piece of microscopical cover-glass was fixed at an angle of 45° with the axis of the tube; the lamp was placed opposite the aperture in the side of the tube, and the vertical stage rotated until the light of the lamp reflected from the thin glass was reflected back by the crystal along the axis of the eye-piece to the observer; the tube holding the crystal was then rotated, and if the spot of light remained visible whilst the tube made a complete rotation, the adjustment was considered to have been correctly made, and the position of the stage was then read on the horizontal circle of the goniometer, and this measurement taken as that of perpendicular incidence.

Several such readings were made, and then the position of the tube and lamp altered, and several more readings made, and the mean of these, which usually were close together, taken as the zero for the angle of incidence.

Two complete series of observations were made with cleavage-faces of Iceland spar in air, water, and tetrachloride of carbon, the water and tetrachloride of carbon being contained by a nearly cylindrical thin glass vessel (a chemical beaker), which stood on the horizontal stage of the goniometer, the tetrachloride being prevented from evaporating by a layer of water floating on its surface.

When the reflection took place in air, a paraffin lamp, with a flat burner placed edgeways (*i.e.*, radially to the goniometer) was used as the source of light; when the crystal was in water or tetrachloride of carbon, its reflecting power was so much diminished that a more intense source of light was necessary, and a magic lantern (a "sciopticon") was used, a black card with a slit 3·5 mm. wide being placed in the slide-holder, and focussed on the surface of the crystal, care being taken in both cases that the direction of the incident light should coincide as nearly as possible with the axis of the collimator tube.

The measurements were made by altering the angle of incidence and the azimuth of the observing Nicol until the light was reduced to a minimum, the position of the crystal remaining fixed.

In order to obtain anything like accurate results with observations of this kind it is necessary to make a large number of determinations and take their mean: it was obvious that there were two ways in which any given number of observations might be grouped, either by making a good many separate determinations for a few positions of the crystal, or by making a few observations at a number of different azimuths; the latter alternative being the one adopted, two readings were made at seventy-two different azimuths of the crystal.

In the first series the observations started from one of the edges of the crystal, the tube containing it being turned through 10° after each pair of readings; after thirty-six pairs of readings the crystal was turned through 6° 20′, and then thirty-six more double readings made at intervals of 10° from each other.

In the second series the observations started from the principal section, and were also made at intervals of 10°; the crystal was turned through 5° after thirty-six observations had been made, and then thirty-six more were made, also at intervals of 10°.

It was thought that by working in this way the results would be more independent of each other, and therefore more trustworthy than if the readings had been made continuously round the whole circle. The position of the crystal was determined by placing a square on the horizontal stage of the goniometer, and rotating the tube carrying the crystal until the edge of the crystal appeared to coincide with the vertical edge of the square, and noting the reading of the divided ring attached to the tube; several such readings were made, and the tube and crystal turned through 180° and several more observations made, and the mean of these taken as the position in which one of the sides of the crystal was vertical (*i.e.*, perpendicular to the plane of incidence); the position of the principal section could then be readily determined as it bisects the obtuse angle, and therefore, that angle being one of 101° 55′, or nearly 102°, forms an angle of about 51° with the adjacent edges.

The position of the crystal in which the principal section was in the plane of incidence and the obtuse summit nearest the observer was considered the zero position; when the principal section was in the plane of incidence and the obtuse summit towards the side from which the light was incident upon it, was therefore azimuth 180°. The crystal was rotated clockwise, and the same direction of rotation was considered the positive direction for the Nicol.

Table I gives the measurements made in this way with a cleavage-face of Iceland spar in air; Tables II and III with the same face in water and carbon tetrachloride. Table IV contains the measurements made with another cleavage-face of the same crystal in water. Tables V, VI, and VII, give the results with a cleavage-face of a second crystal in air, water, and carbon tetrachloride.

Table I.

Iceland Spar in Air.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and $\theta+180°$.
° ′	° ′	° ′	° ′	° ′	° ′	′
+1 0	+0 10	57 20	181 0	+0 22	57 06	+14
7 20	+0 35	57 24	187 20	+0 47	57 09	+15
11 0	+0 30	57 17	191 0	+1 10	57 12	+ 5
17 20	+0 40	57 22	197 20	+1 50	57 16	+ 6
21 0	+0 25	57 24	201 0	+2 05	57 25	− 1
27 20	+0 45	58 0	207 20	+2 35	57 29	+31
31 0	+0 47	57 50	211 0	+3 15	57 59	− 9
37 20	+0 37	58 11	217 20	+3 25	57 59	+12
41 0	+0 40	58 17	221 0	+3 30	58 19	− 2
47 20	+0 47	58 39	227 20	+3 37	58 31	+ 8
51 0	+0 35	58 34	231 0	+4 05	58 34	0
57 20	+0 35	59 02	237 20	+3 42	58 52	+10
61 0	−0 03	58 48	241 0	+3 45	59 03	−15
67 20	−0 10	59 18	247 20	+3 32	59 12	+ 6
71 0	+0 02	59 13	251 0	+3 37	59 15	− 2
77 20	−1 08	59 32	257 20	+3 50	59 42	−10
81 0	−1 25	59 24	261 0	+3 05	59 32	− 6
87 20	−1 43	59 35	267 20	+3 10	59 20	+15
91 0	−1 33	59 49	271 0	+2 45	59 38	+11
97 20	−2 30	59 26	277 20	+2 12	59 33	− 7
101 0	−2 30	59 38	281 0	+2 35	59 45	− 7
107 20	−2 33	59 25	287 20	+1 15	59 23	+ 2
111 0	−2 23	59 11	291 0	+1 47	59 20	− 9
117 20	−2 48	59 06	297 20	+0 52	59 03	+ 3
121 0	−2 58	59 04	301 0	+0 55	58 56	+ 8
127 20	−3 13	58 43	307 20	+0 30	58 43	0
131 0	−3 05	58 37	311 0	+0 05	58 37	0
137 20	−3 0	58 25	317 20	−0 20	58 25	0
141 0	−2 55	58 10	321 0	0	58 17	− 7
147 20	−2 37	57 47	327 20	+0 07	58 06	−19
151 0	−2 20	57 54	331 0	−0 03	57 48	+ 6
157 20	−2 08	57 26	337 20	−0 08	57 54	−28
161 0	−1 33	57 43	341 0	−0 25	57 30	+13
167 20	−0 48	57 22	347 20	−0 06	57 22	0
171 0	−1 0	57 28	351 0	+0 05	57 23	+ 5
177 20	+0 12	57 14	357 20	−0 13	57 27	−13

Table II.

Iceland Spar in Water.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and θ+180°.
+1° 0′	+ 0° 12′	49° 19′	181° 0′	− 0° 03′	49° 18′	+ 1′
7 20	+ 0 40	50 29	187 20	+ 2 0	49 33	+56
11 0	+ 1 22	49 42	191 0	+ 3 02	49 11	+31
17 20	+ 1 55	49 59	197 20	+ 5 17	49 26	+33
21 0	+ 1 10	50 10	201 0	+ 6 37	49 54	+16
27 20	+ 1 12	51 15	207 20	+ 7 55	51 07	+ 8
31 0	+ 0 25	50 30	211 0	+ 7 55	50 06	+24
37 20	+ 1 27	51 30	217 20	+10 27	50 44	+46
41 0	+ 0 42	51 33	221 0	+11 22	51 14	+19
47 20	+ 0 22	52 30	227 20	+12 27	52 28	+ 2
51 0	− 0 23	52 21	231 0	+13 07	52 07	+14
57 20	− 2 20	53 37	237 20	+13 37	53 24	+13
61 0	− 2 48	53 52	241 0	+13 42	53 53	− 1
67 20	− 3 03	54 23	247 20	+13 47	54 50	−27
71 0	− 5 38	54 49	251 0	+13 42	54 52	− 3
77 20	− 8 23	55 56	257 20	+13 40	55 14	+42
81 0	− 7 35	55 32	261 0	+12 52	55 38	− 6
87 20	− 8 40	55 32	267 20	+12 10	56 07	−35
91 0	− 9 33	55 37	271 0	+11 05	56 11	−34
97 20	−10 55	56 05	277 20	+10 15	55 47	+18
101 0	−12 08	56 08	281 0	+ 9 20	55 49	+19
107 20	−13 25	55 34	287 20	+ 7 0	55 41	− 7
111 0	−12 38	54 58	291 0	+ 7 10	55 34	+24
117 20	−14 03	54 07	297 20	+ 4 57	54 13	− 6
121 0	−13 0	54 18	301 0	+ 3 10	53 23	+55
127 20	−12 24	52 41	307 20	+ 1 45	53 44	−63
131 0	−12 13	53 04	311 0	+ 1 52	53 42	−38
137 20	−12 0	53 02	317 20	− 0 10	52 23	+39
141 0	−11 0	51 09	321 0	+ 1 05	51 50	−41
147 20	− 9 10	51 04	327 20	+ 0 10	51 42	−38
151 0	− 8 58	49 52	331 0	+ 0 37	51 12	−80
157 20	− 6 38	49 42	337 20	+ 0 17	50 39	−57
161 0	− 6 10	48 46	341 0	− 0 25	49 53	−67
167 20	− 4 13	50 20	347 20	+ 0 27	49 05	+75
171 0	− 2 45	49 18	351 0	+ 0 45	49 55	−37
177 20	− 1 55	49 10	357 20	− 0 15	49 25	−15

Table III.

Iceland Spar in Tetrachloride of Carbon.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and θ+180°.
+1° 0′	+0° 22′	44° 34′	181° 0′	−0° 43′	46° 39′	−125′
7 20	− 0 18	46 35	187 20	+ 4 37	45 15	+ 80
11 0	+ 0 17	45 46	191 0	+ 5 45	44 20	+ 86
17 20	+ 0 42	45 40	197 20	+ 7 12	45 19	+ 21
21 0	+ 1 05	46 22	201 0	+11 35	45 18	+ 64
27 20	+ 1 37	47 43	207 20	+14 02	46 55	+ 48
31 0	+ 1 55	47 0	211 0	+16 55	47 39	− 39
37 20	+ 0 07	48 21	217 20	+19 15	48 02	+ 19
41 0	+ 0 07	49 34	221 0	+22 40	50 47	− 73
47 20	− 2 38	50 05	227 20	+24 50	51 0	− 55
51 0	− 0 38	50 36	231 0	+26 30	53 03	−147
57 20	− 4 50	53 37	237 20	+27 40	53 51	− 14
61 0	− 7 25	55 39	241 0	+31 12	57 48	−129
67 20	− 8 48	56 10	247 20	+31 22	58 15	−125
71 0	−16 50	60 37	251 0	+33 47	61 19	− 42
77 20	−19 20	59 41	257 20	+32 47	62 35	−174
81 0	−23 10	61 27	261 0	+33 25	64 59	−212
87 20	−26 25	62 40	267 20	+32 20	64 43	−123
91 0	−30 23	64 47	271 0	+31 17	64 11	+ 36
97 20	−28 20	60 50	277 20	+28 02	64 16	−206
101 0	−31 45	64 09	281 0	+26 30	64 49	− 40
107 20	−32 15	63 0	287 20	+20 57	62 02	+ 58
111 0	−32 30	63 15	291 0	+21 37	63 11	+ 4
117 20	−28 38	58 43	297 20	+15 02	57 45	+ 58
121 0	−32 0	62 39	301 0	+13 52	58 27	+252
127 20	−27 45	55 34	307 20	+10 40	58 48	−194
131 0	−29 40	58 34	311 0	+ 5 42	55 47	+167
137 20	−22 13	55 23	317 20	+ 2 50	50 25	+298
141 0	−22 25	52 12	321 0	+ 1 15	52 31	− 19
147 20	−20 35	52 30	327 20	+ 1 25	49 41	+169
151 0	−17 25	51 06	331 0	+ 1 02	49 49	+ 77
157 20	−12 50	44 49	337 20	− 0 19	46 58	−129
161 0	−13 35	45 39	341 0	+ 0 30	48 49	−190
167 20	− 6 35	45 04	347 20	− 0 15	46 22	− 78
171 0	− 5 30	46 12	351 0	− 0 08	47 31	− 79
177 20	− 2 35	44 08	357 20	− 0 53	45 56	−108

Table IV.

Iceland Spar in Water.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and $\theta+180°$.
+ 1°	+ 1° 22′	48° 42′	181°	+ 0° 37′	49° 43′	−61′
11	+ 0 50	49 10	191	+ 4 02	49 25	−15
21	− 0 30	48 43	201	+ 7 17	49 52	−69
31	+ 1 25	50 11	211	+ 9 45	50 35	−24
41	− 0 18	50 46	221	+11 35	51 30	+16
51	− 0 23	52 19	231	+12 47	52 59	−40
61	− 2 43	53 49	241	+13 07	53 45	+ 4
71	− 5 43	54 48	251	+13 42	55 11	−23
81	− 8 0	55 23	261	+11 50	55 16	+ 7
91	−10 28	55 36	271	+11 01	55 24	+12
101	−12 30	55 05	281	+ 8 25	55 25	−20
111	−14 40	54 31	291	+ 5 22	54 20	+11
121	−12 40	53 43	301	+ 3 20	53 29	+14
131	−12 23	51 30	311	+ 2 0	52 10	−40
141	−12 0	51 33	321	+ 1 37	51 48	−15
151	− 7 23	50 15	331	− 1 0	51 15	−60
161	− 3 48	49 35	341	− 0 48	48 48	+47
171	− 1 23	49 19	351	+ 0 22	49 11	+ 8

Table V.

Iceland Spar in Air.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and θ+180°.
+0°	+0° 32′	57° 24′	180°	+0° 37′	56° 46′	+38′
5	+0 45	56 56	185	+1 02	56 22	+34
10	+0 40	57 29	190	+1 20	56 52	+37
15	+0 57	56 57	195	+0 55	56 39	+18
20	+0 57	57 34	200	+2 17	57 10	+24
25	+1 0	57 11	205	+2 57	56 47	+24
30	+0 52	57 54	210	+3 02	57 36	+18
35	+1 12	57 37	215	+3 50	57 11	+26
40	+1 0	58 11	220	+3 35	57 56	+15
45	+0 47	58 03	225	+4 02	57 51	+12
50	+0 22	58 50	230	+3 45	58 10	+40
55	+0 30	58 28	235	+3 57	58 06	+22
60	+0 20	59 19	240	+3 25	58 36	+43
65	0	58 59	245	+4 02	59 19	−20
70	−0 50	59 33	250	+3 30	59 19	+14
75	−0 38	59 04	255	+3 30	58 52	+12
80	−1 05	59 49	260	+3 07	59 15	+34
85	−1 23	59 22	265	+3 20	59 03	+19
90	−1 50	60 07	270	+2 17	59 39	+28
95	−2 05	59 24	275	+2 50	59 0	+24
100	−1 50	59 43	280	+1 42	59 37	+ 6
105	−2 53	59 31	285	+1 42	58 50	+41
110	−2 55	59 36	290	+0 50	59 23	+13
115	−2 53	58 56	295	+0 55	58 50	+ 6
120	−2 58	59 04	300	+0 42	58 34	+30
125	−3 03	58 47	305	+0 17	58 27	+20
130	−3 05	58 22	310	+0 07	58 47	−25
135	−2 55	58 27	315	+0 15	58 01	−34
140	−2 28	57 59	320	−0 15	58 06	− 7
145	−2 23	57 13	325	−0 05	58 37	−80
150	−2 55	57 11	330	−0 25	57 54	−43
155	−1 35	56 46	335	−0 03	57 17	−31
160	−1 20	57 21	340	−0 25	57 21	0
165	−0 43	56 24	345	+0 02	57 05	−41
170	−0 28	56 56	350	0	57 28	−32
175	+0 10	56 22	355	+0 22	56 58	−36

Table VI.

Iceland Spar in Water.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and $\theta+180°$.
°	° ′	° ′	°	° ′	° ′	′
+ 0	− 0 08	48 43	180	+ 0 35	48 25	+18
5	+ 0 17	48 38	185	+ 1 32	48 17	+21
10	+ 0 32	49 04	190	+ 3 0	48 39	+25
15	+ 1 45	48 56	195	+ 5 35	48 43	+13
20	+ 0 50	49 09	200	+ 5 47	49 27	−18
25	+ 1 15	49 32	205	+ 7 47	49 15	+17
30	+ 0 47	50 14	210	+ 9 07	49 47	+27
35	+ 0 55	49 53	215	+ 9 27	50 10	−17
40	+ 0 12	51 14	220	+ 9 40	50 46	+28
45	− 0 13	51 14	225	+11 50	50 57	+17
50	− 0 20	51 33	230	+12 50	52 09	−36
55	− 1 15	51 52	235	+12 12	52 19	−27
60	− 2 15	52 38	240	+13 0	53 05	−27
65	− 3 25	53 43	245	+14 27	53 23	+20
70	− 4 38	53 50	250	+13 15	54 08	−18
75	− 5 45	54 32	255	+12 30	54 28	+ 4
80	− 7 50	54 45	260	+12 07	55 08	−23
85	− 7 13	54 18	265	+10 57	54 55	−37
90	−10 23	55 19	270	+10 32	55 20	− 1
95	− 9 55	54 25	275	+ 9 30	55 34	−69
100	−10 58	54 22	280	+ 9 40	55 40	−78
105	−11 15	53 35	285	+ 7 22	54 45	−70
110	−11 45	54 15	290	+ 6 15	54 07	+ 8
115	−10 58	53 35	295	+ 5 15	54 12	−37
120	−11 43	53 05	300	+ 3 40	52 39	+26
125	−10 43	52 15	305	+ 2 27	52 45	−30
130	−10 58	51 23	310	+ 1 22	52 05	−42
135	−10 55	51 25	315	+ 1 37	52 16	−51
140	−10 08	51 07	320	− 0 30	51 25	−18
145	− 9 0	49 54	325	− 0 33	50 29	−35
150	− 8 48	50 01	330	− 0 13	50 07	− 6
155	− 6 15	49 36	335	+ 0 07	49 55	−19
160	− 5 13	49 13	340	− 0 05	49 26	−13
165	− 3 48	49 02	345	+ 0 10	49 0	+ 2
170	− 2 13	48 38	350	+ 0 15	48 59	−21
175	− 1 0	48 43	355	− 0 05	48 30	+13

Table VII.

Iceland Spar in Tetrachloride of Carbon.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and θ+180°.
+ 0°	− 0° 10′	45° 07′	+180°	+ 0° 30′	44° 44′	+ 23′
5	+ 0 35	45 28	185	+ 2 32	45 05	+ 23
10	+ 0 45	45 55	190	+ 5 17	45 47	+ 8
15	+ 0 55	46 52	195	+ 8 27	45 41	+ 71
20	+ 2 22	46 36	200	+11 45	46 30	+ 6
25	+ 1 20	47 06	205	+14 42	47 26	− 20
30	+ 0 17	48 06	210	+15 57	47 45	+ 21
35	− 0 10	48 38	215	+17 17	48 13	+ 25
40	− 0 23	49 41	220	+21 57	50 17	− 36
45	− 2 0	50 37	225	+24 52	52 17	−100
50	− 3 25	52 14	230	+26 07	53 19	− 65
55	− 5 58	53 41	235	+26 45	54 12	− 31
60	− 6 55	54 30	240	+31 20	57 26	−176
65	−14 43	58 23	245	+33 07	58 32	− 9
70	−15 45	60 08	250	+30 45	59 48	+ 20
75	−20 55	60 18	255	+29 30	59 31	+ 47
80	−22 28	60 43	260	+34 15	65 43	−300
85	−26 25	62 13	265	+32 32	63 24	− 71
90	−25 28	59 42	270	+29 05	63 18	−216
95	−29 42	61 05	275	+25 52	61 52	− 47
100	−31 10	61 58	280	+26 47	64 47	−169
105	−32 13	61 24	285	+22 07	60 32	+ 52
110	−28 13	58 07	290	+17 15	58 57	− 50
115	−28 08	56 47	295	+15 42	58 07	− 80
120	−30 50	57 17	300	+10 32	57 04	+ 13
125	−27 50	56 13	305	+ 9 47	55 47	+ 26
130	−22 53	51 59	310	+ 7 05	54 09	−130
135	−21 30	51 04	315	+ 3 32	51 09	− 5
140	−21 40	49 23	320	+ 1 42	50 38	− 75
145	−18 58	49 15	325	+ 0 25	48 40	+ 35
150	−15 15	47 08	330	− 0 05	48 12	− 64
155	−12 35	47 30	335	− 1 03	47 11	+ 19
160	−10 43	46 15	340	− 0 05	47 25	− 70
165	− 8 18	46 12	345	− 0 10	45 48	+ 24
170	− 4 58	45 16	350	+ 0 05	46 02	− 46
175	− 2 30	45 11	355	+ 0 10	45 06	+ 5

It had been intended to make similar measurements with artificial surfaces cut perpendicular and parallel to the axis of the crystal, and three pieces of Iceland spar cut respectively parallel to a natural face, and perpendicular and parallel to the axis, and all polished with "whiting" were obtained.

Seebeck states ("Pogg. Ann.," vol. xxi, 290) that Iceland spar polished with rouge or putty powder differs in its optical properties from the natural substance, but that an artificial surface polished with chalk behaves very nearly, if not exactly, like a natural one.

Seebeck's measurements were all made with the crystal in air, and as the changes in the azimuth of the plane of polarisation, and in the value of the polarising angle, for different azimuths of the crystal, when such is the case are small, it seemed desirable before making any measurements with the artificial surfaces cut perpendicular and parallel to the axis, to make some determinations with an artificial surface parallel to a natural face of the crystal when the crystal was immersed in water; this was accordingly done.

Contrary to the orders that had been given, the edges of the plate were cut away by the optician who polished it, and it was therefore impossible to determine the position of the principal section in the same manner as that previously employed with the other crystals. The results were therefore plotted, the divisions of the ring carrying the crystal being taken as abscissæ and the azimuths of the plane of polarisation of the reflected light as ordinates. It was assumed that the curve for the artificial face would be similar to that for the natural face, and the points at which the curve cut the x axis were taken as indicating the position of the principal section, and the azimuths of the crystal thus determined. Table VIII gives the results:—

Table VIII.

Artificial Surface of Iceland Spar in Water.

Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Azimuth of the principal section of the crystal.	Azimuth of the plane of polarisation.	Angle of polarisation.	Difference in the values of the polarising angle at θ and $\theta+180°$.
5°	+0° 32′	46° 47′	185°	+0° 57′	47° 0′	−13′
15	+1 37	46 35	195	+2 42	46 40	− 5
25	+2 12	47 37	205	+4 55	47 12	+25
35	+3 05	47 10	215	+6 40	47 36	−26
45	+3 0	48 48	225	+7 25	48 26	+22
55	+3 02	48 47	235	+8 45	48 50	− 3
65	+0 47	50 13	245	+9 27	49 59	+14
75	+0 02	50 11	255	+8 55	50 06	+ 5
85	−3 33	51 37	265	+7 45	51 06	+31
95	−4 13	50 38	275	+4 10	50 46	− 8
105	−6 38	50 26	285	+2 17	50 39	−13
115	−7 20	49 44	295	+1 22	50 37	−53
125	−8 0	49 53	305	−0 35	50 05	−12
135	−7 10	48 37	315	−1 28	48 55	−18
145	−7 15	48 41	325	−2 58	48 34	+ 7
155	−4 38	47 41	335	−2 08	47 07	+34
165	−3 38	47 23	345	−1 35	47 13	+10
175	−1 30	46 21	355	−0 08	46 02	+19

These results differ considerably from those obtained previously with a natural face in water (Tables II, IV, and VI), and it therefore did not appear worth while to make any further experiments with artificial surfaces, as it seemed certain that the results would be untrustworthy.

The difference between the results obtained with this artificial surface and with a natural surface of the crystal is too great to be explained by supposing that the artificial surface was not cut absolutely parallel to the direction of the cleavage, and must therefore be attributed to some change produced by the polishing, possibly due to the pressure employed (conf. Seebeck, "Pogg. Ann.," vol. xx, 1830, 27).

The values of the azimuths and angles of polarisation given in Tables I, II, III, IV, V, VI, VII, and VIII, were plotted on sectional paper; the azimuths of the principal section of the crystal being taken as abscissæ, and the azimuths of the Nicol, and the angles of polarisation −40°, as the ordinates for the two sets of curves.

In order to draw the smooth curves, a piece of plate-glass, rather smaller than the drawing paper, was mounted in a soft-wood frame, so that one surface of the glass was flush with the wood, the sectional paper upon which the observations had been plotted was fixed to the wood with drawing pins, and a sheet of ordinary drawing paper placed over it, and fastened in the same manner. This glass drawing board was then placed in front of a lamp, and smooth curves drawn by eye in the ordinary manner.

Professor Stokes pointed out to me that the experimental results which had been obtained were well suited for reduction by means of the harmonic analysis, and not only explained the method but himself reduced the first set of observations made with a cleavage-face in water. All the observations were accordingly reduced by this method; the determinations made at azimuths 1°, 11°, &c., and at 7° 20′, 17° 20′, &c., in the one series, and at 0°, 10°, &c., and at 5°, 15°, &c., in the second series, being kept separate.

Owing to the fact that the principal section of the crystal is a plane of symmetry, the periodic series for the development of the azimuths of the planes of polarisation can contain sines only, and that for the polarising angles cosines only, including the constant term; therefore the coefficients of the cosines in the former case, and of the sines in the latter, were not calculated, except with the observations made with the artificial surface; it seemed possible that the process of polishing might occasion some want of symmetry, and that therefore it was desirable to calculate the values of the coefficients for both sines and cosines.

The observations in one set only having started at zero azimuth, in the other three there was a small correction to be made in the coefficients for the error thus produced; this was done by multiplying the coefficients by the secants of 1°, 2°, 3°, 4°, for the orders 1, 2, 3, and 4, in the first set; by the secants of 2° 40′, 5° 20′, 8°, and 10° 40′ in the second, and by the secants of 5°, 10°, 15°, and 20° in the fourth set. This correction only exceeded 1′ in sixteen cases, and attained its maximum value in the second series of observations made in carbon tetrachloride, where in the case of the coefficients of $\sin 2\theta$, $\sin 3\theta$, and $\cos 2\theta$ it amounted to 9′ and 8′ respectively.

Table IX gives the results and their means.

Table IX.

Iceland Spar in Air. Azimuths.

SERIES I.

Obs. at 1°, 10°, &c.	$23' - 2°\ 14' \sin\theta + 1°\ 47' \sin 2\theta \ldots\ldots\ldots + 4' \sin 4\theta.$
„ −2° 40′, 7° 20′, &c.	$21 - 2\ 11 \sin\theta + 1\ 48 \sin 2\theta + 1' \sin 3\theta - 2 \sin 4\theta.$

SERIES II.

„ 0°, 10°, &c.	$20 - 2\ 02 \sin\theta + 1\ 51 \sin 2\theta \ldots\ldots\ldots + 5 \sin 4\theta.$
„ 5°, 15°, &c.	$31 - 2\ 12 \sin\theta + 1\ 52 \sin 2\theta + 7' \sin 3\theta - 4 \sin 4\theta.$
Mean (A)	$24 - 2\ 10 \sin\theta + 1\ 49 \sin 2\theta + 2' \sin 3\theta + 1 \sin 4\theta.$

Iceland Spar in Air. Polarising Angles.

SERIES I.

Obs. at 1°, 10°, &c.	$58°\ 25' - 2' \cos\theta - 1°\ 12' \cos 2\theta + 1' \cos 3\theta + 1' \cos 4\theta.$
„ −2° 40′, 7° 20′, &c.	$58\ 25 + 7 \cos\theta - 1\ 10 \cos 2\theta + 1 \cos 3\theta - 1 \cos 4\theta.$

SERIES II.

„ 0°, 10°, &c.	$58\ 21 + 14 \cos\theta - 1\ 20 \cos 2\theta + 1 \cos 3\theta + 7 \cos 4\theta.$
„ 5°, 15°, &c.	$57\ 56 + 13 \cos\theta - 1\ 20 \cos 2\theta + 5 \cos 3\theta + 1 \cos 4\theta.$
Mean (B)	$58\ 17 + 8 \cos\theta - 1\ 15 \cos 2\theta + 2 \cos 3\theta + 2 \cos 4\theta.$

Iceland Spar in Water. Azimuths.

SERIES I.

Obs. at 1°, 10°, &c.	$26' - 9°\ 47' \sin\theta + 5°\ 31' \sin 2\theta + 45' \sin 3\theta - 14' \sin 4\theta.$
„ −2° 40′, 7° 20′, &c.	$23 - 9\ 42 \sin\theta + 5\ 48 \sin 2\theta + 59 \sin 3\theta - 1 \sin 4\theta.$

SERIES II.

„ 0°, 10°, &c.	$25 - 9\ 11 \sin\theta + 5\ 17 \sin 2\theta + 55 \sin 3\theta - 8 \sin 4\theta.$
„ 5°, 15°, &c.	$41 - 8\ 55 \sin\theta + 5\ 15 \sin 2\theta + 38 \sin 3\theta - 4 \sin 4\theta.$

SERIES III.

„ 1°, 11°, &c.	$23 - 9\ 39 \sin\theta + 5\ 36 \sin 2\theta + 41 \sin 3\theta - 22 \sin 4\theta.$
Mean (C)	$27 - 9\ 27 \sin\theta + 5\ 29 \sin 2\theta + 47 \sin 3\theta - 10 \sin 4\theta.$

Iceland Spar in Water. Polarising Angles.

SERIES I.

Obs. at 1°, 10°, &c.	$52°\ 21' + 16' \cos\theta - 3°\ 23' \cos 2\theta + 3' \cos 3\theta + 17' \cos 4\theta.$
„ −2° 40′, 7° 20′, &c.	$52\ 34 + 9 \cos\theta - 3\ 08 \cos 2\theta - 1 \cos 3\theta + 10 \cos 4\theta.$

SERIES II.

Obs. at 0°, 10°, &c. .... $51^\circ\ 40' + 5' \cos\theta - 3^\circ\ 11' \cos 2\theta + 4' \cos 3\theta - 17' \cos 4\theta$.
" 5°, 15°, &c. .... $51\ 32 + 7 \cos\theta - 3\ 12 \cos 2\theta - 7 \cos 3\theta + 10 \cos 4\theta$.

SERIES III.

" 1°, 11°, &c. .... $52\ 05 - 11 \cos\theta - 3\ 16 \cos 2\theta - 5 \cos 3\theta + 13 \cos 4\theta$.

Mean (D)......... $52\ 02 - 5 \cos\theta - 3\ 14 \cos 2\theta - 1 \cos 3\theta + 13 \cos 4\theta$.

Iceland Spar in Tetrachloride of Carbon. Azimuths.

SERIES I.

Obs. at 1°, 11°, &c........ $39' - 25^\circ\ 03' \sin\theta + 11^\circ\ 16' \sin 2\theta + 4^\circ\ 28' \sin 3\theta + 11' \sin 4\theta$.
" −2° 40′, 7° 20′, &c. $50 - 23\ 31 \sin\theta + 10\ 11 \sin 2\theta + 4\ 0 \sin 3\theta - 44 \sin 4\theta$.

SERIES II.

" 0°, 10°, &c. .... $56 - 23\ 26 \sin\theta + 10\ 17 \sin 2\theta + 4\ 18 \sin 3\theta - 49 \sin 4\theta$.
" 5°, 15°, &c. .... $28 - 23\ 07 \sin\theta + 9\ 55 \sin 2\theta + 4\ 23 \sin 3\theta - 12 \sin 4\theta$.

Mean (E)......... $43 - 23\ 47 \sin\theta + 10\ 25 \sin 2\theta + 4\ 17 \sin 3\theta - 24 \sin 4\theta$.

Iceland Spar in Tetrachloride of Carbon. Polarising Angles.

SERIES I.

Obs. at 1°, 11°, &c........ $54^\circ\ 05' - 17' \cos\theta - 9^\circ\ 40' \cos 2\theta + 52' \cos 3\theta + 1^\circ\ 18' \cos 4\theta$.
" −2° 40′, 7° 20′, &c. $53\ 01 + 4 \cos\theta - 8\ 45 \cos 2\theta + 36 \cos 3\theta + 59 \cos 4\theta$.

SERIES II.

" 0°, 10°, &c. .... $52\ 50 + 12 \cos\theta - 8\ 41 \cos 2\theta + 12 \cos 3\theta + 1\ 23 \cos 4\theta$.
" 5°, 15°, &c. .... $52\ 41 - 3 \cos\theta - 8\ 29 \cos 2\theta + 7 \cos 3\theta + 1\ 10 \cos 4\theta$.

Mean (F)......... $53\ 09 - 1 \cos\theta - 8\ 54 \cos 2\theta + 27 \cos 3\theta + 1\ 12 \cos 4\theta$.

Artificial Surface of Iceland Spar in Water. Azimuths.

(G.).... $28' - 3^\circ\ 52' \sin\theta - 9' \cos\theta + 5^\circ\ 11' \sin 2\theta + 11' \cos 2\theta + 33' \sin 3\theta + 7' \cos 3\theta - 21' \sin 4\theta - 4 \cos 4\theta$.

Artificial Surface of Iceland Spar in Water. Polarising Angles.

(H.) ...... $48^\circ\ 53' - 1' \sin\theta - 1' \cos\theta + 4' \sin 2\theta - 2^\circ\ 09' \cos 2\theta + 4' \sin 3\theta - 8' \cos 3\theta$ ...... $+ 1 \cos 4\theta$.

Brewster, in his paper in the "Phil. Trans." for 1819, p. 158, says, "in any given surface when A and A″ are the maximum and minimum polarising angles, viz., in the azimuths of 0° and 90°, the polarising angle A′ at any intermediate aximuth α, may be found by the formula $A' = A + \sin^2\alpha(A'' - A)$."

This expression is the same as that given by the harmonic reduction of the observations set forth in the preceding pages, if we assume that the smaller terms are due to errors of observation, as in that case the expression for the polarising angle in air (B) becomes $58^\circ\ 17' - 1^\circ\ 15' \cos 2\theta$.

Calling the coefficient of $\cos 2\theta$ x, and the minimum value of the polarising angle A, this is $(A+x)-x\cos 2\theta$, which is identical with Brewster's expression, since $A''-A$ is the same as $2x$.

Brewster states that for a rhomboidal surface of calcareous spar $A''-A$ 138′, whereas the harmonic reduction gives the value as 150′, which perhaps, considering the nature of the determinations, is as close an agreement as could be well expected.

Brewster's formula also appears to hold good for the case of Iceland spar in water, as the harmonic series for the value of the polarising angle (D) may be taken as $52^\circ\ 02' - 3^\circ\ 14' \cos 2\theta$. But with the spar in tetrachloride of carbon the agreement no longer holds, as the coefficient of $\cos 4\theta$ becomes too large to be neglected, being 1° 12′. The determinations made in this strongly refracting liquid were less satisfactory than the others, as is shown by the figures in Tables III and VII, but there is hardly sufficient ground for assuming that the value of the coefficient of $\cos 4\theta$ is merely due to errors of observation.

The experiments of which an account had been given confirm the accuracy of Brewster's observations made with a surface of Iceland spar in contact with media other than air, and show moreover that, as Seebeck pointed out, the change in the value of the azimuth of the plane of polarisation of the reflected light also occurs, though to a far less extent, when the crystal is in air, and further, as the refractive index of the medium increases, the change in both these values is greatly augmented.

The harmonic analysis affords a means of expressing, approximately at least, both these changes as functions of the azimuth of the principal section of the crystal, and further shows that when the crystal is in air or water, Brewster's formula for the angle of polarisation expresses the facts of the case.

The constant term in the expression for the azimuth of the plane of polarisation of the reflected light being due partly to errors of observation and partly to the index error of the Nicol, and, for the reason stated by Professor Stokes in the note he has done me the honour of appending to this paper, the coefficients of the cosines of odd multiples of θ in the expressions for the angles of polarisation being probably due to inaccuracies in the determination, it seems best to omit these terms (which at any rate are extremely small), so that we obtain as the final result the following approximate expressions in the several cases.

Azimuths of the Plane of Polarisation of Light Polarised by Reflection.

Cleavage surf. in air.	$-\ 2^\circ\ 10' \sin\theta + 1^\circ\ 49' \sin 2\theta + 0^\circ\ 2' \sin 3\theta + 0^\circ\ 1' \sin 4\theta.$
Ditto, in water	$-\ 9^\circ\ 27' \sin\theta + 5^\circ\ 29' \sin 2\theta + 0^\circ\ 47' \sin 3\theta - 0^\circ\ 10' \sin 4\theta.$
Ditto, in CCl_4......	$-23^\circ\ 47' \sin\theta + 10^\circ\ 25' \sin 2\theta + 4^\circ\ 17' \sin 3\theta - 0^\circ\ 24' \sin 4\theta.$
Artificial surf. in water	$-\ 3^\circ\ 52' \sin\theta + 5^\circ\ 11' \sin 2\theta + 0^\circ\ 33' \sin 3\theta - 0^\circ\ 21' \sin 4\theta.$

Polarising Angles.

Cleavage surface in air	$58^\circ\ 17' - 1^\circ\ 15' \cos 2\theta + 0^\circ\ 2' \cos 4\theta$.
Ditto, in water	$52^\circ\ 2' - 3^\circ\ 14' \cos 2\theta + 0^\circ\ 13' \cos 4\theta$.
Ditto, in CCl_4	$53^\circ\ 9' - 8^\circ\ 54' \cos 2\theta + 1^\circ\ 12' \cos 4\theta$.
Artificial surface in water........	$48^\circ\ 53' - 2^\circ\ 9' \cos 2\theta + 0^\circ\ 1' \cos 4\theta$.

From these expressions the values of the ordinates of the curves representing the phenomena were calculated, and Plates I and II give the curves as plotted from the values so obtained.

These curves correspond very closely with the smooth curves drawn from the points given by the observations, the values of the ordinates for those portions of the curve corresponding to azimuths 0—40°, and 320—360°, being rather greater than the values given by the smooth eye-drawn curve. The curves for the artificial surface in water (G and H) show clearly, when compared with the corresponding curves for the natural surface (C and D), how greatly these two surfaces differed in their optical behaviour.

In conclusion I must express my thanks to Professor Stokes for his advice and assistance, and for all the trouble he has taken with reference to the determinations of which an account is given in this paper.

Note by Professor STOKES, P.R.S.

On inspecting these numbers we may remark :—

1. The coefficients of $\sin 4\theta$ in the expressions for the azimuths are in all cases so small that they can hardly be said to emerge from errors of observation. Since, however, there is no reason to suppose that such a term does not exist, the coefficients may as well be retained, as being somewhat more probable than zero would be.

2. Brewster found that the polarising angles were the same for any two azimuths differing by 180°, and MacCullagh afterwards deduced this result from theoretical considerations. If we assume this law as exact, the harmonic expression for the polarising angle will contain no terms involving cosines of odd multiples of θ. Now with one doubtful exception the coefficients in the above expressions are insensibly small. The single exception, where a coefficient has at first sight the appearance of being real though small, is that of the term involving $\cos 3\theta$ for the observations in tetrachloride of carbon. The observations with this liquid were the most uncertain, probably from the feebleness of reflection arising from its high refractive index. If the differences of the polarising angles for azimuths of the principal plane differing by 180° be examined, it will be seen that a coefficient amounting in the mean to only 0° 27′, and subject to a mean error from set to set of 17′, can have little claim to be regarded as real.

Conroy.

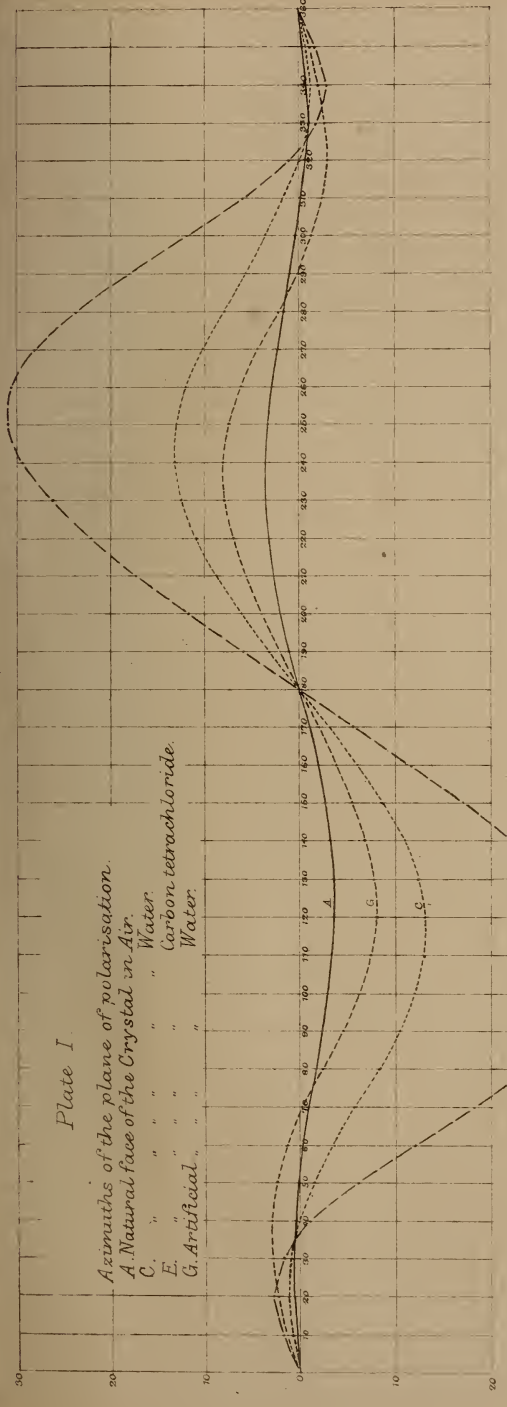


Plate I.

Azimuths of the plane of polarisation.

A. Natural face of the Crystal in Air.

C. " " " " " Water.

E. " " " " " Carbon tetrachloride.

G. Artificial " " " " Water.

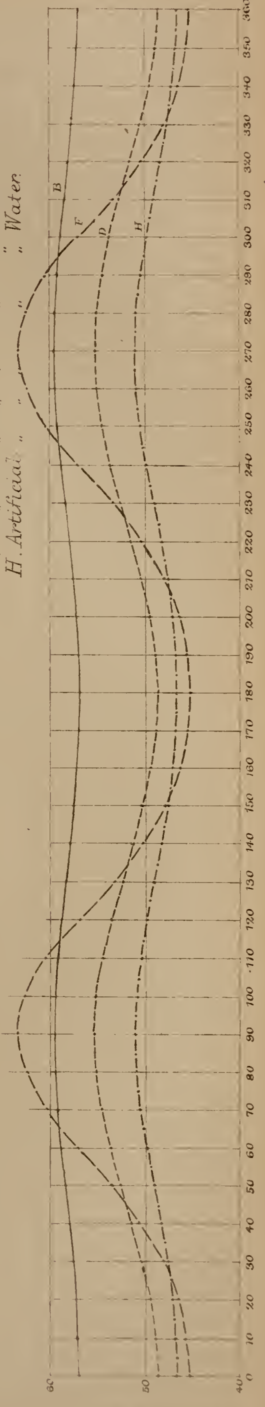


Plate II.

Polarising Angles.

B. Natural face of the Crystal in Air.

D. " " " " " Water.

F. " " " " " Carbon tetrachloride.

H. Artificial " " " " Water.

West, Newman & Co. lith.

It seems best therefore in this, as well as other cases, to omit the terms involving cosines of odd multiples of θ.

3. As regards the observations with the artificial surface in water, the coefficients of the cosines in the expression for the azimuths and of the sines in the expression for the polarising angles are insensibly small, indicating no introduction of asymmetry with respect to the principal plane arising from the process of polishing. The coefficients of the cosines of odd multiples of θ in the second expression are also insensible. The constant term in the first expression, representing (on the assumption of symmetry with respect to the principal plane) the index error of the circle carrying the Nicol, agrees almost exactly with those obtained for the cleavage surfaces in air and water.

It would seem best then to omit those terms which we have reason to think are really *nil*, and which the observations show to be at any rate extremely small, and to exhibit the final result accordingly.

February 11, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Theory of Lubrication and its Application to Mr. Beauchamp Tower's Experiments, including an Experimental Determination of the Viscosity of Olive Oil." By Professor OSBORNE REYNOLDS, LL.D., F.R.S. Received December 29, 1885.

(Abstract.)

Lubrication, or the action of oils and other viscous fluids to diminish friction and wear between solid surfaces, does not appear to have hitherto formed a subject for theoretical treatment. Such treatment may have been prevented by the obscurity of the physical actions involved, which belong to a class as yet but little known, namely, the boundary or surface actions of fluids; but the absence of such treatment has also been owing to the want of any general laws revealed by experiment.

The subject is of such fundamental importance in practical mechanics, and the opportunities of observation so frequent, that it

may well be a matter of surprise that any general laws should have for so long escaped detection.

Besides the general experience obtained, the friction of lubricated surfaces has been the subject of much experimental investigation by able and careful experimenters; but although in many cases empirical laws have been propounded, these fail for the most part to agree with each other and with the more general experience.

The most recent investigation is that of Mr. Tower, undertaken at the instance of the Institute of Mechanical Engineers. Mr. Tower's first report was published in November, 1883, and his second in 1884 ("Proc. Inst. Mechanical Engineers").

In these reports Mr. Tower, making no attempt to formulate, states the results of experiments apparently conducted with extreme care, and under very various and well-chosen circumstances. Those of the results which were obtained under the ordinary conditions of lubrication so far agree with the results of previous investigators as to show a want of any regularity. But one of the causes of this want of regularity, viz., irregularity in the supply of lubricant, appears to have occurred to Mr. Tower early in his investigation, and led him to include amongst his experiments the unusual circumstance of surfaces completely immersed in an oil-bath. This was very fortunate, for not only do the results so obtained show a great degree of regularity, but while making these experiments he was accidentally led to observe a phenomenon which, taken with the results of his experiments, amounts to a crucial proof that in these experiments with the oil-bath the surfaces were completely and continuously separated by a film of oil; this film being maintained by the motion of the journal, although the pressure of the oil at the crown of the bearing was shown by actual measurement to be as much as 625 lbs. per square inch above the pressure in the oil-bath.

These results with the oil-bath are very important, notwithstanding that the condition is not common in practice. They show that with perfect lubrication a definite law of the variation of the friction with the pressure and the velocity holds for a particular journal and brass. This strongly implies that the irregularity previously found was due to imperfect lubrication. Mr. Tower has brought this out. Substituting for the bath an oily pad of tow pressed against the free part of the journal, and making it so slightly greasy that it was barely perceptible to the touch, he again found considerable regularity in the results. These were, however, very different from those with the bath. Then with intermediate lubrication he obtained intermediate results, of which he says: "Indeed, the results, generally speaking, were so uncertain and irregular that they may be summed up in a few words. The friction depends on the quantity and uniform distribution of the oil, and may be anything between the oil-bath results and seizing, according to the perfection or imperfection of the lubrication."

On reading Mr. Tower's first report, it occurred to the author that in the case of the oil-bath the film of oil might be sufficiently thick for the unknown boundary actions to disappear, in which case the results would be deducible from the equations of hydrodynamics.

Mr. Tower appears to have considered this, for he remarks that, according to the theory of fluid friction the resistance would be as the square of the velocity, whereas in his results it does not increase according to this law.

Considering how very general the law of resistance as the square of the speed is with fluids, there is nothing remarkable in it being assumed to hold in such a case. But the study of the behaviour of fluid in very narrow channels, and particularly the recent determination by the author of the critical velocity at which the law changes from that of the square of the velocity to that of the simple ratio, shows that with such highly viscous fluids as oils, such small spaces as those existing between the journal and its bearing, and such limited velocities as that of the surface of the journal, the resistance would vary, *cæteris paribus*, as the velocity. And further, the thickness of the oil film would not be uniform, and might be affected by the velocity, and as the resistance would vary, *cæteris paribus*, inversely as the thickness of the film, the velocity might exert in this way a secondary influence on the resistance; and further still, the resistance would depend on the viscosity (commonly called the body) of the oil, and this depends on the temperature. But as Mr. Tower had been careful to make all his experiments in the same series with something at a temperature of 90° F. (he does not state precisely what), it did not at first appear that there could be any considerable temperature effect in his results.

The application of hydrodynamical equations for viscous fluids to circumstances similar to those of a journal and a brass in an oil-bath, in so far as they are known, at once led to an equation* between the variation of pressure over the surface and the velocity, which appeared to explain the existence of the film of oil at high pressure.

This equation was mentioned in a paper read before Section A at the British Association, at Montreal, 1884. It also appears from a paragraph in the Presidential Address (p. 14, Brit. Assoc. Rep., 1884)

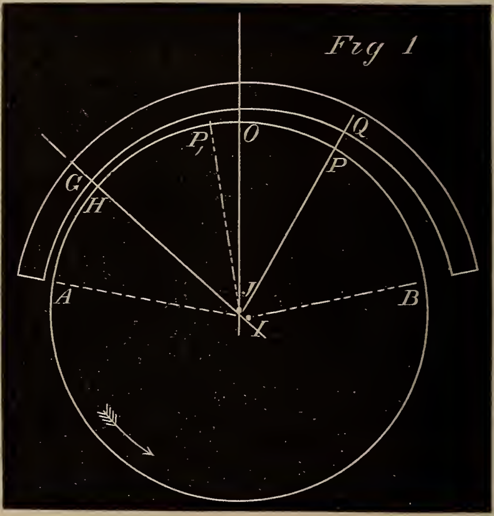
No. of equation in the paper.

* $$\frac{dp}{dx}=\frac{6\mu U(h-h_1)}{h^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

in which p is the intensity of pressure, μ coefficient of viscosity, x the direction of motion, h the interval between the journal and the brass, h_1 being the value of h for which the pressure is a maximum, U the surface velocity in the direction of x.

that Prof. Stokes and Lord Rayleigh had simultaneously arrived at a similar result. At that time the author had no idea of attempting the integration of this equation. On subsequent consideration, however, it appeared that the equation might be so transformed* as

FIG. 1.



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* If the journal and brass are both of circular section, as in fig. 1, and R is the radius of the journal, R + a radius of brass, J the centre of the journal, I the centre of the brass, JI $= ca$, HG the shortest distance across the film, IO the line of loads through the middle of the brass, A the extremity of the brass on the off side, B on the on side, P_1 the point of greatest pressure,

Putting

$$\begin{aligned} \text{OIH} &= \phi_0 - \frac{\pi}{2} \\ \text{OIP}_1 &= \phi_1 \\ \text{OIP} &= \theta \\ h &= a\{1 + c\sin(\theta - \phi_0)\} \\ h_1 &= a\{1 + c\sin(\phi_1 - \phi_0)\} \end{aligned}$$

the equation (31) becomes

$$\frac{dp}{d\theta} = \frac{6\text{R}\mu c\{\sin(\theta - \phi_0) - \sin(\phi_1 - \phi_0)\}}{a^2\{1 + c\sin(\theta - \phi_0)\}^3} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (48)$$

If $\frac{a}{\text{R}}$ is small. This equation, which is at once integrable when c is small, has been integrated by approximation when c is as large as 0·5.

The friction is given by an equation

$$f = -\tfrac{1}{2}\text{R}\frac{dp}{d\theta}a\{c\sin(\theta - \phi_0)\} - \mu\frac{\text{U}_1 - \text{U}_0}{a\{1 + c\sin\theta - \phi_0\}} \quad . \quad . \quad . \quad . \qquad (49)$$

This is also approximately integrated up to $c = 0·5$.

to be approximately integrated by considering certain quantities small, and the theoretical results thus definitely compared with the experimental.

The result of this comparison was to show that with a particular journal and brass the mean thickness of the film would be sensibly constant for all but extreme values of load divided by the viscosity, and hence if the coefficient of viscosity were constant the resistance would increase approximately as the speed.

As this was not in accordance with Mr. Tower's experiments, in which the resistance increased at a much slower rate, it appeared that either the boundary actions became sensible, or that there must be a rise in the temperature of the oil which had escaped the thermometer used to measure the temperature of the journal.

That there would be some excess of temperature in the oil film on which all the work of overcoming friction is spent is certain, and after carefully considering the means of escape of this heat, it appeared probable that there would be a difference of several degrees between the oil-bath and the film of oil.

This increase of temperature would be attended with a diminution of viscosity, so that as the resistance and temperature increased with the velocity there would be a diminution of viscosity, which would cause the increase of the resistance with the velocity to be less than the simple ratio.

In order to obtain a quantitative estimate of these secondary effects, it was necessary to know the exact relation between the viscosity of the oil and the temperature. For this purpose an experimental determination was made of the viscosity of olive oil at different temperatures as compared with the known viscosity of water. From the result of these experiments an empirical formula has been deduced

ϕ_0 and ϕ_1 and c have to be determined from the conditions of equilibrium, which are

$$\int_{-\theta_1}^{\theta_1} \{p \sin\theta - f \cos\theta\} dp = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

$$\int_{-\theta_1}^{\theta} \{p \cos\theta + f \sin\theta\} d\theta = \frac{\mathrm{L}}{\mathrm{R}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

$$\int_{-\theta}^{\theta} f d\theta = \frac{\mathrm{M}}{\mathrm{R}^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

where $2\theta_1$ is the angle subtended by the brass, L the load, and M the moment of friction.

The solution of these equations may be accomplished when c is small and has been approximately accomplished for particular values of c up to 0·5, the boundary conditions as regards p being

$$\theta = \pm\theta_1 \qquad p = p_0,$$

whence substituting the values of ϕ_1, ϕ_0, c in (48) and (49), and integrating, the values of the friction and values of the pressure are obtained.

for the viscosity of olive oil at all temperatures between 60° and 120° F.*

Besides the effect on μ the temperature might, owing to the different expansion of brass and iron, produce a sensible effect on the small difference a in the radii of the brass and journal, *i.e.*, on the mean thickness of the film, E was taken for the coefficient of this effect, and since, owing to the elasticity of the material, the radius would probably alter slightly with the load, m was taken as a coefficient for this effect, whence a is given by an equation† in terms of a_0, its value with no load and a temperature zero.

Substituting these values in the equations, the values of the pressure and friction deduced from the equations are functions of the temperature, which may be then assumed, so as to bring the calculated results into accord with the experimental.

There was, however, another method of arriving, if not at the actual temperatures, at a law connecting them with the frictions, loads, and velocities. For the rise in temperature was caused by the work spent in overcoming friction, while the heat thus generated had to be carried or conducted away from the oil film. Consideration of this work and the means of escape gave another equation between the rise of temperature, the friction, and velocity.‡

The values of the constants in this equation can only be roughly surmised from these experiments, without determining them by substituting the experimental values of f, U, and T, as previously determined, but it was then found that the experiments with the lower loads gave remarkably consistent values for A, B, E, m, and a_0, which was also treated as arbitrary. In proceeding to the higher loads for which the values of c were greater, the agreement between the calculated and experimental results was not so close, and the divergence increased as c increased. On careful examination, however, it appeared that this discordance would be removed if the experimental frictions were all reduced 20 per cent. This implied that 20 per cent. of the actual friction arose from sources which did not affect the pressure of the film of oil; such a source would be the friction of the ends of the brass against flanges on the shaft commonly

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* An inch being unit of length, a pound unit of force, and a second unit of time, for olive oil

$$\mu = 0{\cdot}00004737 e^{-0{\cdot}0221\mathrm{T}} \quad \ldots\ldots\ldots \quad (8)$$

† $$a = (a_0 + m\mathrm{L}) e^{\mathrm{ET}} \quad \ldots\ldots\ldots \quad (117)$$

‡ $$f = \left(\mathrm{A} + \frac{\mathrm{B}}{\mathrm{U}}\right)\mathrm{T} + \mathrm{EAT}^2 \quad \ldots\ldots\ldots \quad (120)$$

A + ET represents the rate at which the mechanical equivalent of heat is carried away per unit rise of temperature; B represents the rate at which it is conducted away.

used to keep the brass in its place, or by any irregularity in the longitudinal section of the journal or brass. Although no direct reference is made to such flanges in Mr. Tower's reports, it is such a common custom to neck the shaft to form the journal that there is great probability of the flanges being used. A coefficient n has therefore been introduced into the theory, which includes both the effect of necking and of irregularity in longitudinal section. Giving n the value 1·25, the calculated results came into accordance with all Mr. Tower's results for olive oil, the difference being such as might well be attributed to experimental inaccuracy, and this both as regards the frictions measured with one brass, No. 1, and the distribution of the pressure round the journal with another, No. 2.

Not only does the theory thus afford an explanation of the very novel phenomena of the pressure in the oil film, but it also shows, what does not appear in the experiments, how the various circumstances under which the experiments have been made affect the results.

Two circumstances in particular which are brought out as principal circumstances by the theory seem to have hitherto entirely escaped notice, even that of Mr. Tower.

One of these is a, the difference in the radii of the journal and of the brass or bearing. It is well known that the fitting between the journal and its bearing produces a great effect on the carrying power of the journal, but this fitting is supposed to be rather a matter of smoothness of surface than a degree of difference in radii. The radius of the bearing must always be as much larger than that of the journal as is necessary to secure an easy fit, but more than this does not seem to have been suggested.

It now appears from this theory that if viscosity were constant the friction would be inversely proportional to the difference in the radii of the bearing and journal, and this although the arc of contact is less than the semi-circumference; and taking temperature into account it appears from the comparison of the theoretical frictions with the experiment on brass No. 1, that the difference in the radii at 70° F. was

$$a=0{\cdot}00077 \text{ (inch)},$$

and comparing the theoretical pressures with those measured with brass No. 2,

$$a=0{\cdot}00084 \text{ (inch)},$$

or the difference was 9 per cent. greater in the case of brass No. 2.

These two brasses were probably both bedded to the journal in the same way, and had neither been subjected to any great amount of wear, so that there is nothing surprising in their being so nearly the same fit. It would be extremely interesting to find how far under continuous running prolonged wear tends to preserve this fit. Mr. Tower's experiments afford only slight indication of this. It does appear, however, that the brass expanded with an increase of temperature, and that its radius increases as the load increases in a very definite manner.

Another circumstance brought out by this theory, and remarked on both by Lord Rayleigh and the author at Montreal, but not before suspected is, that the point of nearest approach of the journal to the brass is not by any means in the line of load, and what is still more contrary to common supposition, it is on the *off*[*] side of this line.

This point H moves as the ratio of load to velocity increases; when this ratio is zero, the point H coincides with 0, then as the load increases it moves away to the left, till it reaches a maximum distance $\frac{1}{2}\pi - \phi_0$, being nearly $-\frac{\pi}{2}$. The load is still small, smaller than anything in Mr. Tower's experiments, even with the highest velocities. For further increase of load, H returns towards 0, or $\frac{1}{2}\pi - \phi_0$ increases with the largest loads and smallest velocities to which the theory has been applied; this angle is about 40°. With a fairly loaded journal well lubricated it would thus seem that the point of nearest approach of brass to journal, *i.e.*, the centre of wear, would be about the middle of the off side of the brass.

This circumstance, the reason of which is rendered perfectly clear by the conditions of equilibrium, at once explains a singular phenomenon, incidentally pointed out by Mr. Tower, viz., that the journal having been run in one direction for some time, and carrying its load without heating, on being reversed began to heat again, and this after many repetitions always heating on reversal, although eventually this tendency nearly disappeared. Mr. Tower's suggested explanation appears to the author as too hypothetical to be satisfactory, even in default of any other; and particularly as this is an effect which would necessarily follow in accordance with the theory, so long as there is wear. For the centre of wear, being on the *off* side of the line of loads, this wear will tend to preserve or diminish the radius of the brass on the *off* side, and enlarge it on the *on* side, a change which will, if anything, improve the condition for producing oil pressure while running in this direction, but which will damage the condition on which the production of pressure in the film depends when the journal is reversed and the late *off* side becomes the new *on* side. That with a well-worn surface there should be sufficient wear to produce this

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[*] *On* and *off* sides are used by Mr. Tower to express respectively the sides of approach and recession, as B and A, fig. 1, p. 194, the arrow indicating the direction of motion.

result, with such slight amounts of using as those in Mr. Tower's experiments before reversal, seems doubtful, but supposing the brass new, and the surface more or less unequal, the wear for some time would be considerable, even after the initial tendency to heat had disappeared. Hence it is not surprising that the effect should have eventually seemed to disappear.

The circumstances which determine the greatest load which a bearing will carry with complete lubrication, *i.e.*, with the oil film continuous between brass and journal throughout the entire arc, are definitely shown in the theory, so long as the brass has a circular section.

As the ratio of the load to velocity increases JI or c increases, and the point H approaches 0, when c reaches the value 0·5, which makes $GH = a(1-c) = 0{\cdot}5a$, the pressure of the oil in the film is everywhere greater than at A and B, the pressure in the bath, but for a further increase in the load the pressure falls near A on the *off* side, the fall will cause the pressure to become less than that of the atmosphere, or if sufficient to become absolutely negative, until discontinuity or rupture of the oil film occurs. The film will then only extend between brass and journal over a portion of the whole arc, and a smaller portion as the load increases or velocity diminishes, *i.e.*, as c increases. Thus since the amount of negative pressure which the oil will bear depends on circumstances which are uncertain, the limit of the safe load for complete lubrication is that which causes the least separating distance to be half the difference in radii of the brass and journal.

The rupture of the oil does not take place at the point of nearest approach, but on the *off* side of this, and will only extend up to a point P_2 definitely shown in this theory, which is at the same distance on the *off* side of H as P_1 is on the *on* side. Hence after this rupture the brass may still be in equilibrium, entirely separated from the journal, and the question as to whether it will carry a greater load without descending on to the journal will depend on the relative values of $\frac{a}{R}$, and on the smallness of the velocity. The condition then becomes the same as that for imperfect lubrication, and except in the case of a being very small, theory shows that the ultimate limit to the load will be the same with the oil-bath and with partial lubrication as Mr. Tower found it to be.

This much may be inferred without effecting the integrations for imperfect lubrications; could these be effected, the theory would be as applicable to partial lubrication as it has been to complete lubrication, *i.e.*, a sufficient supply of oil. And as it is, sufficient may be seen to show that with any supply of oil, however insufficient for complete lubrication, the brass will still be completely separated from the journal, although the supporting film of oil will not touch the brass except over a limited area, and it is shown by general reasoning that in the one extreme, when the oil is very limited, the friction increases directly as the load, and is independent of the velocity, while in the other, where the oil is abundant, the circumstances are those of the oil-bath.

The effect of the limited length of bearings, and the escape of the oil at the ends, is also apparent in the equations.

Although in the main the present investigation has been directed to the circumstances of Mr. Tower's experiments, namely, a cylindrical journal revolving in a cylindrical brass, the main object has been to establish a general and complete theory based on the hydrodynamical equations for viscous fluids. Hence it has been thought necessary to proceed from the general equations, and to deduce the equations of lubrication in a general form, from which the particular form for application has been obtained. It has been found necessary also to consider somewhat generally the characters of fluid friction and viscosity.

The property of viscosity has been discussed in Section II of the paper, which section also contains the account of the experimental investigation as to the viscosity of olive oil. The general theory deduced from the hydrodynamical equations for viscous fluids with methods of application, including two cases besides the cylindrical journal in which the equations become completely integrable, viz., two plane surfaces of elliptical shape approaching, and one plane sliding over another not quite parallel plane surface, is given in Sections IV, V, VI, and VII.

The physical considerations of the effect of the heat generated are discussed in Section VIII.

As there are some circumstances which cannot be taken into account in the definite reasoning, particularly as regards incomplete lubrication, besides which, as the definite reasoning tends to obscure the more immediate purpose of the investigation, a preliminary discussion of the problem presented by lubrication, illustrated by aid of graphic methods, has been introduced as Section III.

Finally, the definite application to Mr. Tower's experiments is given in Section IX, which concludes as follows:—

The experiments to which the theory has been definitely applied may be taken to include all Mr. Tower's experiments with the 4-inch journal and oil-bath, in which the number of revolutions per minute was between 100 and 450, and the nominal loads in pounds per square inch between 100 and 415. The other experiments with the oil-bath were with loads from 415 till the journal seized at 520, 573, or 625, and a set of experiments with brass No. 2 at 20 revolutions per minute. All these experiments were under extreme conditions, for which by the theory c was so great as to render lubrication incomplete, and preclude the application of the theory without further integrations.

The theory has, therefore, been tested by experiments throughout the extreme range of circumstances to which the particular integrations undertaken are applicable, and the results, which in many cases check one another, are consistent throughout.

The agreement of the experimental results with the particular equations obtained on the assumption that the brass as well as the journal are truly circular, must be attributed to the same causes as the great regularity presented by the experimental results themselves.

Fundamental amongst these causes is, as Mr. Tower has pointed out, the perfect supply of lubricant obtained with the oil-bath. But nearly as important must have been the truth with which the brasses were first fitted to the journal, the smallness of the subsequent wear and the variety of the conditions as to magnitude of load, speed, and direction of motion.

That a brass in continuous use should preserve a circular section with a constant radius requires either that there should be no wear at all, or that the wear at any point P should be proportional to sin (90°—POH).

Experience shows that there is wear in ordinary practice, and even in Mr. Tower's experiments, there seems to have been some wear. In these experiments, however, there is every reason to suppose that the wear would have been approximately proportional to $c\sin(\phi_0-\theta)=c\sin(90°-\text{POH})$, because this represents the approach of the brass to the journal within the mean distance a, for all points except those at which it is negative, at these there would be either no wear at all or a slight positive wear. So long, then, as the journal ran in one direction only, the wear would tend to preserve the radius and true circular form of that portion of the arc from A to F (fig. 1, note *), altering the radius at F, and enlarging it from F to B. On reversal, however, A and F change sides, and hence alternate motion in both directions would preserve the radius constant all over the brass.

The experience emphasised by Mr. Tower, that the journal, after running for some time in one direction, would not run at first in the other, strongly bears out this conclusion. Hence it follows that had the journal been continuously run in one direction, the condition of lubrication, as shown by the distribution of oil pressure round the journal, would have been modified, the pressure falling between 0 and B on the *on* side of the journal, a conclusion which is borne out by the fact that in the experiments with brass No. 2, which was run for some time continuously in one direction, the pressure measured on the *on* side is somewhat below that calculated on the assumption of circular form, although the agreement is close for the other four points.

When the surfaces are completely separated by oil it is difficult to see what can cause wear. But there is generally metallic contact at starting, and hence abrasions, which will introduce metallic particles into the oil (blacken it), these particles will be more or less carried round and round with the journal, causing wear and increasing the number of metallic particles and the viscosity of the oil. Thus the rate of wear would depend on the metallic particles in the oil, the values of c, $\frac{1}{a}$, and the velocity of the journal, and hence would render the greatest velocity of the journal at which the maximum load with a large value of c could be carried, small; a conclusion which seems to be confirmed by Mr. Tower's experiments with brass No. 2 at twenty revolutions a minute.

In cases such as engine bearings the wear causes the radius of curvature of the brass continually to increase, and hence a and c must continually increase with wear. But, in order to apply the theory to such cases, the change in the direction of the load (or the velocity of approach of the surfaces) would have to be taken into account.

That the circumstances of Mr. Tower's experiments are not those of ordinary practice, and hence that the particular equations deduced in order to apply the theory definitely to these experiments do not apply to ordinary cases, does not show that the general theory as given in the general equation could not be applied to ordinary cases were the conditions sufficiently known.

These experiments of Mr. Tower have afforded the means of verifying the theory for a particular case, and hence have so far established its truth as applicable to all cases for which the integrations can be effected.

The circumstances expressed by—

$$\mu \frac{\mathrm{L}}{\mathrm{U}} \frac{a}{\mathrm{R}} c_1 \phi_0 \phi_1 n_1 m c' \mathrm{AEB},$$

which are shown by the theory to be, together with the supply of lubricant, the principal circumstances on which lubrication depends, although not the same in other cases, will still be the principal circumstances, and indicate the conditions to be fulfilled in order to secure good lubrication.

The verification of the equations for viscous fluids under such extreme circumstances affords a severe test of the truth and completeness of the assumptions on which these equations are founded; and the result of the whole research is to point to a conclusion (important to natural philosophy) that not only in cases of intentional lubrication, but wherever hard surfaces under pressure slide over each other without abrasion, they are separated by a film of some foreign matter, whether perceptible or not; and that the question as to whether the action can be continuous or not turns on whether the motion tends to preserve the foreign matter between the surfaces at the points of pressure, as in the almost if not quite unique case of the revolving journal, or tends to remove it, and sweep it on one side, as is the action of all backward and forward rubbing with continuous pressure.

The fact that a little grease will enable any surfaces to slide for a time has tended doubtless to obscure the action of the revolving journal to maintain the oil between the surfaces at the point of pressure, and yet, although only now understood, it is this action that has alone rendered machinery or even carriages possible. The only other self-acting system of lubrication is that of reciprocating motion with intermittent pressure and separation of the surfaces to draw the oil back or to draw a fresh supply. This is important in certain machinery, as in the steam-engine, and is as fundamental to animal mechanism as is the continuous lubricating action of the journal to mechanical contrivances.

II. "The Electrical Phenomena accompanying the Process of Secretion in the Salivary Glands of the Dog and Cat." By W. MADDOCK BAYLISS, B.Sc., and J. ROSE BRADFORD, B.Sc., Senior Demonstrator of Anatomy in University College, London (from the Physiological Laboratory of University College). Communicated by E. A. SCHÄFER, F.R.S. Received February 4, 1886.

(Abstract.)

The glands examined were the submaxillary and parotid of the dog and cat, and in all of these we have determined that the process of secretion is accompanied by definite electrical changes; as, however, the submaxillary gland both in the dog and cat has been more thoroughly examined than the parotid, the present communication is confined almost entirely to the former.

The chorda tympani and sympathetic nerves were exposed in the usual manner, divided, and the peripheral ends arranged for stimulation, a canula being placed in Wharton's duct. The submaxillary gland having been exposed was led off in the following manner. One non-polarisable electrode was placed on the superficial or cutaneous aspect of the gland, and the second electrode so arranged in the wound as to touch the deep surface of the gland as close to the hilus as possible without pressing on the duct.

A Thomson galvanometer of high resistance was used.

Electrical Condition during Rest.

Dog.—The cutaneous surface of the gland is in the great majority of cases negative to the hilus, both when examined as above described and also when the gland is removed from the animal and led off.

In four experiments amongst twenty-four, the outer surface of the gland was positive. In two cases the outer surface was at first positive, but subsequently became negative, and in one case it was at first negative but subsequently became positive.

The electromotive force of the current of rest varies very much both in different cases and in the same case at different times; thus in the former case it may vary from $\frac{1}{10}$ volt to $\frac{1}{500}$ volt, but owing to a variety of structures (muscles, &c.) being unavoidably injured in the preparation, not much stress can be laid on this point.

Cat.—Out of twenty experiments on the submaxillary gland, in fifteen the surface of the gland was positive to the hilus, in three the surface of the gland was negative, in one the surface was at first negative and subsequently became positive, and in one the surface was at first positive and subsequently became negative to the hilus.

Hence, although a corresponding amount of injury is inflicted on the tissues in the case of the cat as in the dog, yet on the whole the resting current is opposite in its sign in the two cases.

Excitatory Changes.

Dog. Chorda Tympani.—On throwing an induction current into the chorda tympani, a very well-marked deflection of the galvanometer is *always* observed of a sign indicating that the outer surface of the gland becomes *negative* to the hilus. Although in different dogs the amount of this deflection varies, yet never have we failed to obtain it.

Frequently this variation is not the sole one observed, its course being interrupted by a second deflection showing the outer surface of the gland to become positive. This second variation, however, is by no means always observed, and more especially it is not seen if the first or main phase is very large, being then indicated only by a slight temporary arrest in the deflection caused by the first phase. The latent period of the variation is short, being about 0·37″, as measured by the capillary electrometer. The deflection quickly reaches a maximum and begins to diminish before the cessation of the excitation, returning quickly towards zero, but as a rule leaving a slight after-effect.

Atropine, in doses of 5—10 mgrms., abolishes the main phase of the chorda variation in from 2—3 minutes from its injection into the pleura. In those cases in which this phase only had been observed, frequently after such a dose of atropine the second phase (*i.e.*, outer surface of gland positive to hilus) is seen on excitation of the chorda, although previously not detected, owing to the magnitude and rapidity of the deflection caused by the first or main phase.

This second phase is more refractory towards atropine than the main phase, although ultimately abolished by it in large doses.

Excitation of Sympathetic causes well-marked changes of potential in the gland structures which are very different to those produced on excitation of the chorda; the latter have a very short latent period, are readily abolished by atropine, and are of such a sign as to cause the outer surface of the gland to become negative, occasionally followed by the outer surface becoming positive.

Excitation of the sympathetic, however, produces after a very long latent period an electrical effect very refractory as regards the action of atropine on it, and of such a sign that the outer surface of the gland becomes *positive* to the hilus.

Further, the course of the variation is very slow, and its amplitude is much less than that of the chorda variation. Thus in one case on excitation of the sympathetic a deflection of 62 was obtained, the chorda giving a deflection of 140 with $\frac{1}{9}$ shunt.

Atropine in small doses has apparently no effect on the sympathetic variation, but in large doses, 40—100 mgrms., it is not without effect, at first producing great lengthening of the latent period, and then steadily diminishing the amplitude of the variation, although after even 100 mgrms. a slight variation, *i.e.*, 10—15 divisions, is still perceptible.

Cat. Chorda tympani.—In the cat, excitation of the chorda causes an electrical variation of such a sign that the outer surface of the gland becomes negative to the hilus, but whereas in the dog, a second phase was on the whole not observed in the majority of cases, in the cat a second phase is usually present, and very frequently is greater in amount than the first phase. Further, in a few cases, the first phase (*i.e.*, outer surface of gland negative) was very small indeed, *i.e.*, less than 20 divisions, and in one case it was absent, the chorda giving a pure second phase. These varieties observed in the variations are largely dependent on the nature of the accompanying secretion.

In these cases in which the first phase was large, the secretion was very watery, and if the secretion obtained was viscid the electrical variation consisted of a small first phase and a large second phase.

Atropine in doses of 2—20 mgrms. abolishes the first phase of the chorda variation, leaving the second phase, as in the dog, and this second phase requires a larger dose to abolish it, *i.e.*, 20—40 mgrms.

Excitation of the sympathetic in the cat produces an electrical effect resembling more the chorda effect of the cat than the sympathetic effect of the dog. Thus the usual effect is a deflection similar to the chorda effect of the cat, *i.e.*, diphasic, but with this difference, that the first phase is usually larger than the second phase, and not as in chorda excitation, the second larger than the first. This variation is obtained if the accompanying secretion be watery in character, but if, as occasionally happens, it be viscid, then the second phase is larger, and the first phase smaller in amount.

Atropine in small doses abolishes the first phase, and in doses of 10—40 mgrms. the second phase, thus showing a very great difference between its action on the sympathetic variation in the cat and dog respectively.

Thus to sum up our results:—

In the submaxillary of the dog excitation of the chorda produces a copious slightly viscid secretion, and the electrical effect consists of a large first phase, the second phase being small, and although not always observed is probably always present.

In the cat a similar excitation produces a copious viscid secretion, and the electrical effect is diphasic, the second phase being usually the larger.

In the dog, excitation of the sympathetic produces a scanty viscid secretion, and the electrical effect consists of a pure second phase.

In the cat, excitation of the sympathetic produces a very copious and but slightly viscid secretion, and the electrical effect is diphasic, the first phase being usually the larger.

In the parotid the results obtained are similar to those in the submaxillary.

In the dog excitation of the tympanic plexus causes the surface of the gland to become negative to the hilus, and the variation is readily abolished by atropine. Excitation of the sympathetic causes the surface of the gland to become positive to the hilus, and the variation is not readily abolished by atropine.

February 18, 1886.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "Observations on the Radiation of Light and Heat from Bright and Black Incandescent Surfaces." By MORTIMER EVANS, M.Inst.C.E., F.R.A.S. Communicated by Lord RAYLEIGH, M.A., D.C.L., F.R.S. Received February 3, 1886.

In the course of an investigation into the nature of carbon filaments, such as are ordinarily used in the construction of incandescent lamps, my attention was arrested by certain variations in the amount of light emitted from filaments which were, to the best of my belief, of similar nature and composition, but which, when tested under precisely similar conditions, gave the most anomalous results. I was also struck with changes which occurred to a greater or less degree in the light yielded by certain lamps when re-tested subsequent to a shock of over-incandescence, or long continued hard running at a high temperature; the light yielded after this occurrence (indeed the light yielded by any lamps that had been much used) I found to be invariably lessened both in quantity and brightness, and to require a considerable increase in the energy supplied to it to produce from the same filament the light it originally gave. After seeking vainly to account for these irregularities from structural differences in the carbon filaments themselves, and after testing and re-testing many carbons made in a variety of ways, both by myself and others, it occurred to me that the composition or structure of the carbon itself, of which the filaments were made, might have really little to do with the discrepancies and changes I had noticed.

All the carbons I had tried gave in turn the most irregular results, and although some of these were porous, and some dense and compact, the light emitted from any one of them per unit of surface for each unit of electrical energy supplied to it was very varied and uncertain, and did not appear to follow any condition of the porosity or denseness of the filament itself.

All the carbons in turn gave the same light per unit of surface when raised to the same incandescence, but the energy required to produce this light, or raise the filament to this incandescence, varied sometimes in a remarkable way. At times a filament was found which, with 2 watts or volt ampères passing through it, would yield the light of the standard candle. And again, with other filaments it sometimes occurred that no less than 5 volt ampères were required to produce this light.

On collating a number of these observations, and comparing the filaments themselves with their various testings, I noticed, I thought, some difference in the outward appearance of those filaments which had tested well and those that had required any large amount of energy to give a satisfactory light, and, following up this idea, I soon became convinced that it was to this surface appearance or condition that the whole question of economical light giving or otherwise might be traced. All the filaments, it appeared, whose surfaces were of a dull black required the larger amounts of energy to yield the usual unit of light, while from those filaments with even moderately bright surfaces the light of the standard candle could be obtained for an expenditure of energy surprisingly less. To ascertain with greater certainty if this idea were correct, I prepared a number of carbons made from a vegetable fibre which, though yielding a somewhat porous carbon, was strong and uniform in texture.

Having selected two filaments as like each other as the eye could determine, and having ascertained by careful measurement that they were both of exactly the same length and diameter, and therefore of equal surface, I subjected each carbon in turn to the action of an electrical current in a hydrocarbon atmosphere, so regulating the current as to maintain the carbon filament at a white heat in the vapour until a sufficient deposit of carbon upon its surface was obtained.

To provide for the deposit of carbon upon the one filament being as dull a black as possible, I used for the depositing medium an atmosphere of ordinary coal-gas drawn from a domestic burner. A large glass jar was filled with this, and a constant current of the cold gas kept circulating through it during the deposit, and the resulting surface was all that could be desired. It had all the appearance of being coated with lampblack, but the coating was quite permanent, and did not brush off, or even soil the fingers in handling.

On the other of these two filaments I now deposited carbon of a bright silvery appearance in marked contrast with the dull black of that just described, and this deposit I found I could readily effect by using as the depositing atmosphere the very hot vapour of almost any hydrocarbon having a high boiling point, though from the porous nature of the carbons I was using I did not get the surface as brilliant as I subsequently obtained it from smoother carbons.

This filament was then mounted on platinum electrodes, as the other had been, enclosed in a similar glass globe, and exhausted of air, the vacuum being carried to about the $\frac{1}{1000000}$ of an atmosphere.

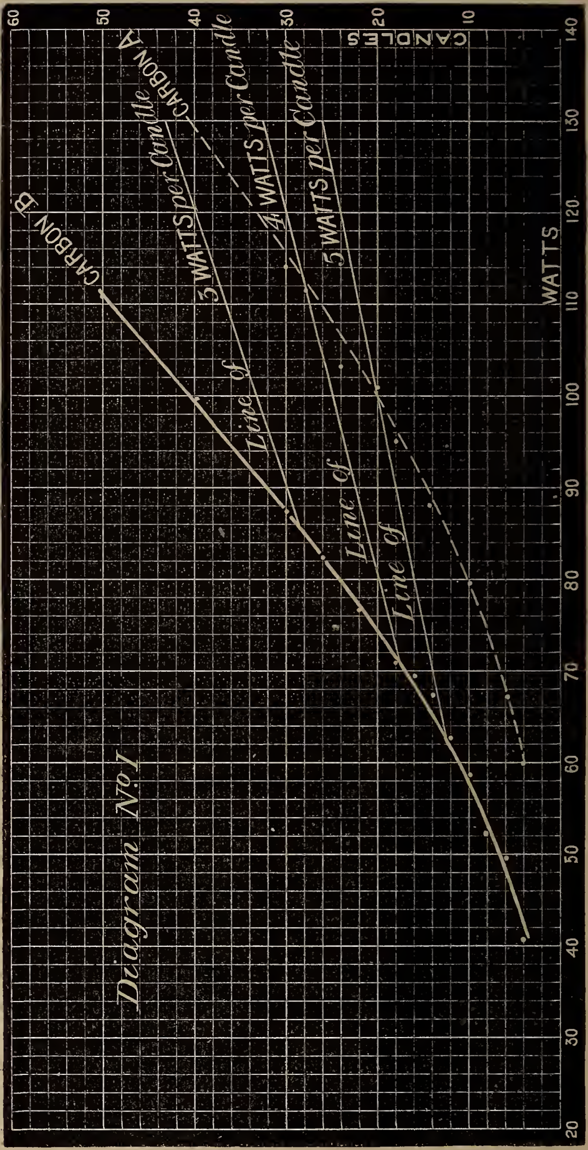
The remeasurement of these carbon filaments subsequent to the reduction of the carbon on their surfaces, showed no perceptible increase in their diameter, the deposit of carbon which had been added being in all likelihood less than the ten thousandth of an inch in thickness, and the surface areas of each filament still remained practically equal in all respects.

Having now two carbon filaments with which a comparative test might be made, and in which the conditions were in all respects identical, except in that of surface condition or polish, the one being like soot and the other like silver, I passed a series of known electrical currents through each in turn, registering the light produced against a standard candle burning 120 grains per hour in a good photometer provided with a sliding screen.

In Table No. I, Carbon A, are shown the testings of the blackened filament, and in Table II, Carbon B, those of the filament which was made bright. In diagram No. 1 may be seen the plotting of these results and their relative curves. The dotted curve marked Carbon A shows the testings of the black filament, and the curve marked with a plain hard white line, Carbon B, gives the testings of the filament which was bright. The horizontal divisions in the diagram give the watts or volt ampères of energy passing through the filament, and the perpendiculars mark the corresponding candle powers. From these tests it may be noticed that with two carbon filaments identical in all respects but in that of surface polish or brightness, the blackened filament required no less than 100 watts to keep its surface at an incandescence yielding 20 candles, whilst the filament with the bright surface was kept at the same incandescence, and gave an equal light with 74 watts only, also that each filament when consuming an energy of 4 watts per candle, that which was blackened required no less than 113 watts of energy to effect this (besides having its surface incandescence strained to yield 28 candles), while the bright filament with 71 watts only effected the same economy, viz., 4 watts only per candle, and had to give from its surface only 17¾ candles.

These results satisfied me that the condition of the carbon surface was wholly the cause of the large differences shown by these curves, and I determined therefore to carry out a more extended series of tests with carbons about which I knew nothing.

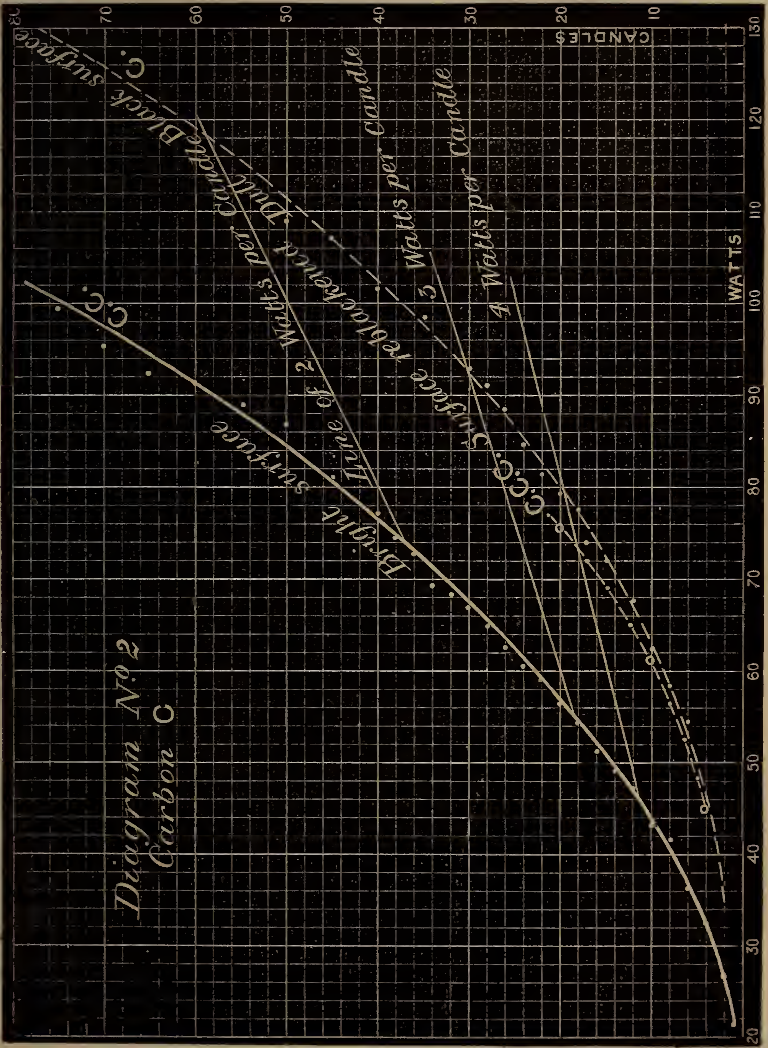
For this purpose, therefore, I procured two carbons of foreign manufacture, but by whom made I did not know. The following were their chief characteristics. The carbons were very nearly square in section, and appeared before carbonising to have been sliced from some homogeneous material like parchment paper.

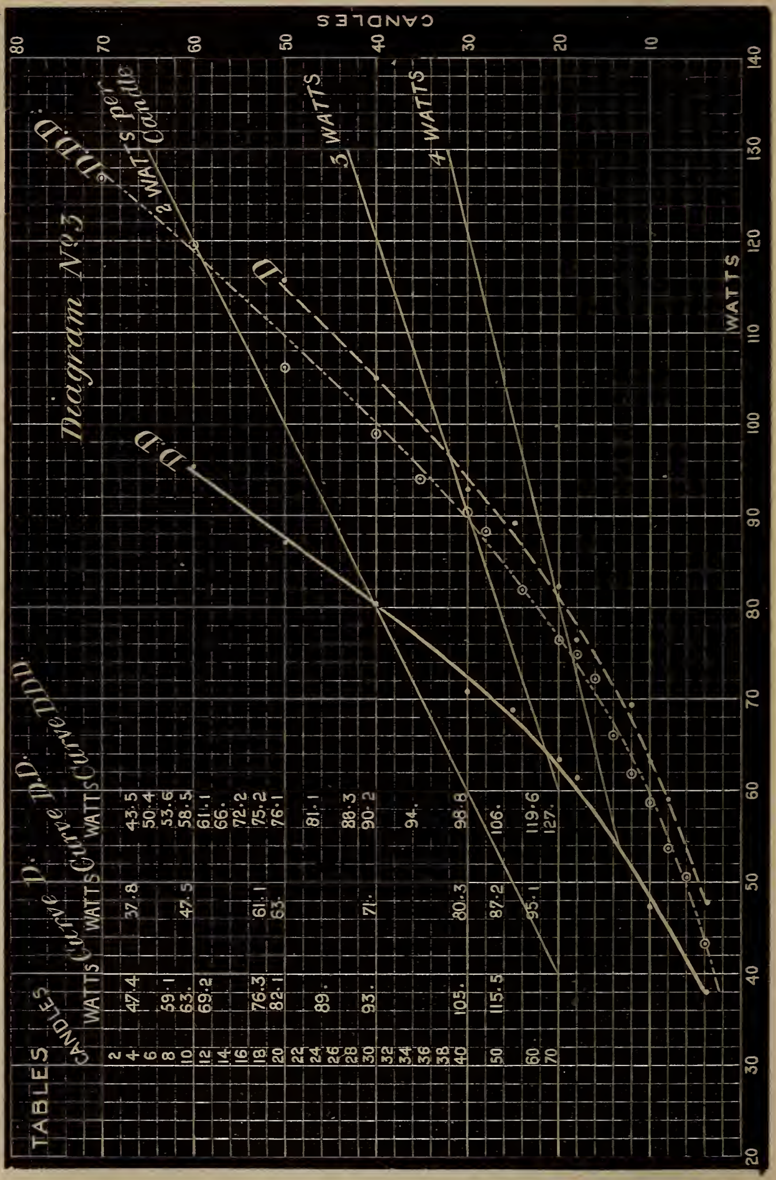


They were both, I found from careful measurements, of precisely the same dimensions and surface area, and each presented the same dull dead black surface; the carbon itself appeared exceedingly dense and hard. Testing these filaments as I procured them, I placed them in bulbs and exhausted as before.

Tables III and VI give the photometric tests of these carbons as I procured them, and diagrams 2 and 3 show the plotting of these tests. I have marked these filaments C and D, the black dotted line in diagram 2 being the curve for filament C, and the black dotted line in diagram 3 that for filament D.

In these two curves the extreme uniformity with which these carbons tested is worthy of notice, the one giving nineteen and a half candles and the other twenty for the eighty watts supplied to each,





TABLES

CANDLES	WATTS Curve D.	WATTS Curve D.D.	WATTS Curve D.D.D.
2			
4	47.4	37.8	43.5
6			50.4
8	59.1		53.6
10	63.	47.5	58.5
12	69.2		61.1
14			66.
16			72.2
18	76.3	61.1	75.2
20	82.1	63.	76.1
22			
24	89.		81.1
26			
28			88.3
30	93.	71.	90.2
32			
34			94.
36			
38			
40	105.	80.3	98.8
50	115.5	87.2	106.
60		95.1	119.6
70			127.

which not only well inside any error of observation, but is in great measure a sufficient proof of the extreme equality of the areas of their radiating surfaces.

I now dismounted these filaments and subjected them to incandescence in the hot hydrocarbon vapour as before with carbon B. The result was highly satisfactory, as they both took a surface much brighter than carbon B had done. Again remounting them and exhausting, they were placed in the photometer as before. The results are given in Tables IV and VII, and the curves from these tables are shown by the bright hard lines CC and DD, diagrams 2 and 3. These curves appear fully to bear out the assumption arrived at in the former tests, Tables I and II, and the improvement in light radiation per unit of energy is especially marked in the case of filament C, where it may be noticed that at 2 watts of energy per candle of light the same filament in its black condition was strained to yielding sixty candles nearly, while in its polished state it had only to yield thirty-seven, and still be as economical in its electrical energy as before. The same filament at 3 watts per candle, when black, had to give off 31 candles, equal to 270 candles per square inch of its surface, while in its polished state it required to give only 18 candles to equal 3 watts per candle, and its surface was strained only to the extent of 155 candles per square inch. It is certain, therefore, that its lasting power with its surface bright would be many times greater at the foregoing expenditure of energy than in its black condition.

As the filaments were still unbroken and appeared capable of yet another test, I resolved to attempt the reblackening of them, and to ascertain if possible if the test-curve under these conditions would again revert to its former position, but I had now to reblacken over the bright surface which could not be removed. The filaments were again successfully dismounted, and with some difficulty again reblackened over their former polished surfaces.

They were now tested, as is shown in Tables V and VIII, and the corresponding curves are given in diagrams 2 and 3, marked CCC, DDD. The recession of the curves was in both cases nearly complete, any difference being fully accounted for by the incomplete reblackening of the carbon surface.

In carrying out these experiments I much regret not having made the necessary arrangements for simultaneously testing both the heat and light emitted from each filament in its blackened and bright condition. I have little doubt the loss of efficiency when black was due to the energy supplied being radiated in large quantities as heat waves from the blackened surfaces, which these surfaces when bright would not permit. This radiation of heat, however, which had not been converted into light by emission from a bright surface was abundantly manifested in the handling of the lamps. The incandescent globe containing the bright filament could at all times be readily held in the hand even when giving its maximum of light, while the heat radiated from the filament when its surface was blackened was most intense, and not only caused at times severe burns, but occasionally would even char the little paper labels attached to the glass.

I also regret not having dissipated by a powerful current the black carbon deposited over the bright surface in the last case, as an increase of economy in working and light-giving should have resulted, and this would have been the more interesting as ordinarily it is just the other way; as soon as by overheating or long use the polished surfaces of our best lamps are injured, so surely is there an increased waste of energy, and hence the extreme difficulty of preserving lamps made for use as standards for any long period.

From these results it appears probable that the attainment of economical high E.M.F. lamps of ordinary sizes may be very difficult, as what would be a high E.M.F. lamp with a black surface would be a low E.M.F. lamp were this black surface made bright; the energy required being less, both the E.M.F. and current would have to fall. The desire, therefore, for high E.M.F. lamps should be met not by a supply of black wasteful filaments, but more properly by economical lamps of greatly increased size and candle power, or by lamps of a smaller candle power run two or more in series.

DIAGRAM 1.

	Carbon A. Table I.			*Carbon B.* Table II.		
Candles.	Volts.	Amp.	Watts.	Volts.	Amp.	Watts.
4	53·5	1·12	60	38·5	1·05	40·4
6	56	1·2	67·2	42	1·18	49·5
8	..	..	..	43	1·23	52·4
10	60·5	1·32	80	45	1·30	58·5
12	..	..	..	46·5	1·35	62·5
14	63	1·4	88·2	48	1·41	67·6
16	..	..	..	48·5	1·43	69·3
18	65	1·46	94·9	49	1·45	71·0
20	66·5	1·52	101			
22	..	..	..	50·5	1·52	76·7
24	67	1·54	103			
25						
26	..	..	..	52	1·58	82·1
28	69	1·62	111·7			
30	69	1·65	113·8	53	1·65	87·7
35						
40	74	1·80	133·2	56	1·78	99·7
45						
50	..	..	..	58·5	1·9	101
55	77	1·95	181			
60	..	..	..	62	2·05	127

DIAGRAM 2.

Carbon C.

	Curve C. Table III.			C.C. Table IV.			C.C.C. Table V.		
Candles.	Volts.	Amp.	Watts.	Volts.	Amp.	Watts.	Volts.	Amp.	Watts.
4	45	0·86	38·7	34	0·95	32·7	39	1·16	45·2
6	52·5	1·04	54·6	35	1·02	36·2			
8	54	1·08	58·3	38	1·08	41·0			
10	56	1·12	62·7	39	1·12	43·7	44·5	1·38	61·4
12	58	1·17	67·8	40	1·17	47·3			
14	59	1·22	71·9	41	1·20	49·2			
16	60	1 23	73·8	42	1·22	51·2			
18	61	1·27	77·4	43	1·26	54			
20	62	1·28	79·7	44	1·28	56·3	49·5	1·53	75·7
22	63	1·3	81·9	44·5	1·33	59			
24	64	1·32	84·5	45	1·35	60·7			
26	65	1·36	88·4	46	1·36	62·5			
28	66	1·38	91·0	46·8	1·40	65·4			
30	66·5	1·40	93	47	1·47	67·2			
35	68	1·46	99·2						
40	69	1·48	102	49·5	1·54	76·2			
45	70·5	1·52	107	50	1·60	80·8			
50	71	1·54	109	52	1·67	86·8			
55	72	1·58	113	52·5	1·70	89·1			
60	73·5	1·62	119	52·8	1·73	91·3			

DIAGRAM 3.

Carbon D.

	Curve D. Table VI.			D.D. Table VII.			D.D.D. Table VIII.		
Candles.	Volts.	Amp.	Watts.	Volts.	Amp.	Watts.	Volts.	Amp.	Watts.
4	46·5	1·02	47·4	37·3	1	37·8	34	1·28	43·5
6	..	..	..	..	..	..	36	1·4	50·4
8	51	1·16	59·1	..	..	..	37	1·45	53·6
10	52·5	1·20	63	42	1·13	47·8	38·5	1·52	58·5
12	54·5	1·27	69·2	..	..	..	40	1·54	61·6
14	..	..	..	..	..	..	40·5	1·62	66
16	..	..	..	..	..	..	42	1·72	72·2
18	57	1·34	76·3	47	1·30	61·1	43	1·75	75·2
20	58·3	1·40	82·1	47·8	1·32	63	43	1·77	76·1
22									
24	..	..	..	..	..	..	44·5	1·84	81·8
25	61	1·46	89·0						
26									
28	..	..	..	..	..		46	1·92	88·3
30	62	1·50	93	50	1·42	71	46·5	1·94	90·2
35	..	..	..	..	..	..	47	2	94
40	65	1·62	105	52·5	1·53	80·3	48	2·06	98·8
45									
50	68	1·70	115	54·2	1·60	87·2	50	2·12	106
55									
60				57	1·67	95·2	52	2·3	119·6

II. "On a Thermopile and Galvanometer combined." By Professor GEORGE FORBES, M.A. Communicated by Lord RAYLEIGH, M.A., D.C.L., Sec. R.S. Received February 4, 1886.

The author has lately made use of a special form of thermopile and galvanometer combined, which is very sensitive for the measurement of radiation. The apparatus is especially suitable for use as a line-thermopile.

The first experiments were made with two half tubes, one of antimony the other of bismuth, soldered together so as to make a short tube about 2 cm. external diameter, the walls being 2 mm. thick, and the length of tube about 2 or $2\frac{1}{2}$ cm. The sides of the tube where the junctions of the metals occur were then filed flat, so as to present a thin wall to receive the radiations and to enable it to rise in temperature more rapidly, and also more uniformly throughout the thickness of this wall. This tube was lamp-blacked. A Thomson's mirror and magnets (by J. White, of Glasgow), in its usual brass cell, but with an insulated coating, was then inserted in the tube, and the whole apparatus mounted inside a brass cube with a brass cone at one side to throw the radiations upon one junction, and with a circular hole facing the mirror. This apparatus when properly adjusted with a lamp and scale was found to be very sensitive. It had been the intention of the author to use a telescope in place of a lamp, but the radiations of the lamp were found to give rise to no inconvenience.

Let us compare the probable sensitiveness of this instrument, say, with a line-thermopile of the ordinary construction of 20 pairs, forming a line of the same length as the tube, the double length of a pair of antimony and bismuth in the line-thermopile being equal to the circumference of the tube. Let E be the E.M.F. of one junction, and let R be the resistance of one pair in the line-thermopile, and let R′ be the resistance of the galvanometer used with the line-thermopile. The total resistance of the line-thermopile is 20R, and that of the tubular one is $\frac{R}{20}$, and the currents in the line and tubular thermopiles respectively are $\frac{20E}{20R+R'}$ and $\frac{20E}{R}$. If the galvanometer were specially constructed to match the line-thermopile, R′ would be made equal to 20R, and the current would be $\frac{E}{2}$, or one-fortieth of the current in the tubular thermopile and galvanometer combined. Thus it would require forty turns of wire in the low-resistance galvanometer, if these coils occupied the same space as the metal of the tubular thermopile, to equal the sensitiveness of the latter, and a larger number if it occupied a greater space. On the whole, it seems probable that by specially designing a galvanometer to match the line-thermopile the arrangement would be about as sensitive as the new instrument in the form hitherto described, but the simplicity and cheapness of construction of the latter commends it.

The next apparatus was made according to the following instructions:—

Take a wedge whose distance from the apex to the base is about 6 cm., the base of the triangular section of the wedge being about 3 cm., and the width of the wedge 6 cm. The wedge is half of antimony and half of bismuth, the division being made by the medial plane perpendicular to the three rectangular faces of the wedge cut off the apex of the wedge by a plane parallel to the base of the wedge, and exposing a surface of 1½ cm. width. This is the surface which receives the radiations, and it is lamp-blacked. A hole about 1 cm. diameter is now drilled (or it is better to file it out before the two metals are soldered together) through the two sides of the wedge, so as to leave only a thin wall along the junction of the metals at the surface which receives the radiations. A Thomson cell with suspended mirror and magnet is inserted in this hole and the instrument is complete, and ready to be placed inside the brass box with cone already described.

The resistance of this cell is very low and its sensitiveness thereby increased. Moreover this type has a great advantage in the fact that the mass of metal acts as a damper upon the vibrations of the magnet, and so we have a dead-beat instrument.

The diameter of the cone to receive radiations at its mouth was 5 cm. A candle at a distance of 30 cm. from the mouth of the cone gave a deflection of 52 divisions, a reading being easily made correct to one division. A duplex lamp burning paraffin oil at a distance of 1½ metres gave a deflection of 60 divisions.

The author takes this opportunity to describe a method of carrying the delicate Thomson cells without danger of breaking the silk fibre suspension. The cell consists externally of a brass tube. A horse-shoe magnet is obtained with the distance between its legs small compared with the diameter of the above-mentioned tube. The tube is placed so as to rest on the inner edges of the legs of the magnet, with the mirror over the poles of the magnet, the mirror magnets having their poles over poles of opposite name of the horse-shoe magnet, and with the silk fibre next to the magnet. The mutual magnetic attraction takes the tension off the silk fibre and holds the mirror fixed in position, and the fibre cannot be broken by a blow given to the apparatus.

Fig. 1 is a sketch of the first arrangement in the form of a tube.

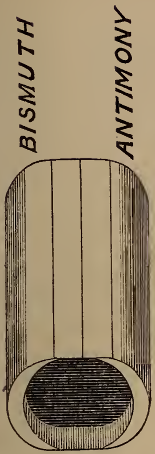


FIG 1.

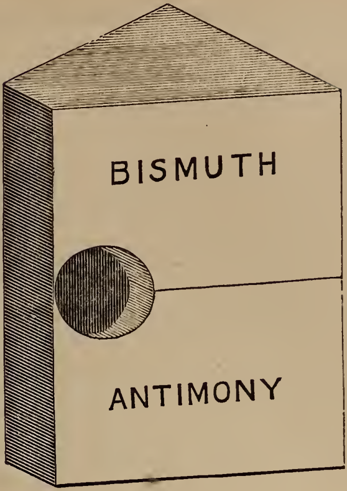


FIG. 2.

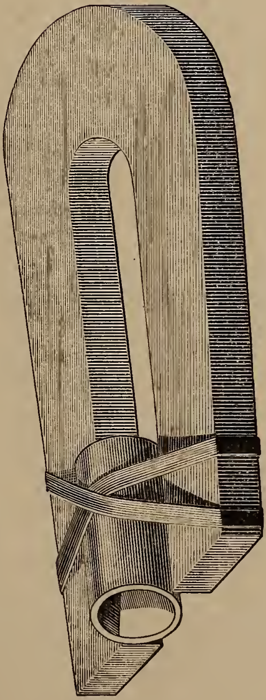


FIG. 3.

Fig. 2 is a sketch of the low-resistance combination, showing the hole into which the Thomson cell is inserted.

Fig. 3 shows the portable arrangement to prevent fracture of the silk suspension.

February 25, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Comparison between Apparent Inequalities of Short Period in Sun-spot Areas and in Diurnal Declination-ranges at Toronto and at Prague. By BALFOUR STEWART, M.A., LL.D., F.R.S., and WILLIAM LANT CARPENTER, B.A., B.Sc. Received February 17. Read February 25, 1886.

1. In a report to the Solar Physics Committee ("Proc. Roy. Soc.," vol. 37, p. 290, 1884) we discussed the relations between certain apparent Inequalities of short periods in sun-spot areas on the one hand and diurnal temperature-ranges at Toronto and at Kew of corresponding periods on the other.

In the present communication we proceed to discuss the connexion between the same solar Inequalities and the diurnal declination-ranges at Toronto and at Prague.

For the Toronto declination-ranges we are indebted to the kindness of the Science and Art Department, South Kensington, and of Mr. Carpmael, Director of the Toronto Observatory, through whom we have received daily values (excluding Sundays) of the diurnal range of magnetic declination at Toronto extending from 1856 to 1879 inclusive, and thus forming a series of 24 years.

Each number is the difference in scale-divisions of the declinometer between the greatest eastern and the greatest western deflection of the declination magnet on each day, as observed at the hours 6 A.M., 8 A.M., 2 P.M., 4 P.M., 10 P.M., and midnight of Toronto mean time, one scale-division of the instrument being equal to $0'{\cdot}72$ nearly. It is probable that such differences represent very nearly the true diurnal range.

Disturbances appear to be violent at Toronto, and we have rejected a few of the most disturbed observations, embracing those which denote ranges above forty scale-divisions, or 28′·8. Although this rejection has been made, it must not be supposed that the remainder are entirely undisturbed, but only that they are freed from the excessive influence of the most violent disturbances.

We have extracted the Prague ranges from the published records of that Observatory, and we have not found it necessary to exclude disturbances except in one or two very marked cases. The Prague ranges are derived from observations made at 6 A.M., 10 A.M., 2 P.M., and 10 P.M., hours which are common to the whole series, and there is reason to believe that the ranges thus deduced are not greatly different from those which would have been obtained from an hourly series of observations.

2. The declination-ranges of the present paper have been reduced exactly in the same manner as the temperature-ranges of our previous report ("Proc. Roy. Soc.," May 1, 1884, vol. 37, p. 290). It is therefore unnecessary to discuss the method of reduction, this having been already done at considerable length.

We proceed consequently at once to consider—

Results of Comparison around 24 *Days.*

3. *Comparison as to Duration of Period.*—This is given in the following table, in which the sun-spot and Toronto temperature columns are transcribed from our former paper for the purpose of comparison. The sums in these columns are those of 36 years. The Prague declination columns exhibit likewise sums of 36 years, while the Toronto declination columns exhibit sums of 24 years. As in our last paper, to save space we have divided each individual sum by 100; that is to say, we have dismissed the two right hand figures.

We have inclosed in brackets the positions of all sufficiently well-defined maximum Inequalities of sun-spots, of Toronto temperature-ranges, and of Prague declination-ranges. But inasmuch as the Toronto declination-ranges only extend over 24 years, we have merely exhibited the numbers without brackets, believing these to be of inferior accuracy.

Before the table is examined it may be well for the reader to be reminded that the sun-spot areas extend from 1832 to 1867 inclusive, thus embracing 36 years; that the Toronto temperature and the Prague declination-ranges extend from 1844 to 1879 inclusive, thus embracing 36 years; while the Toronto declination-ranges extend from 1856 to 1879 inclusive, thus embracing 24 years. It thus appears that the Toronto temperature and the Prague declination-ranges are for the same 36 years, 24 of which they have in common with the sun-spot series. On the other hand, the Toronto declination

Table I.—Apparent Inequalities around 24 days (SS = Sun-spots; TT = Toronto Temperature-ranges; PD = Prague Declination-ranges; TD = Toronto Declination-ranges).

Period.	SS.	TT.	PD.	TD.	Period.	SS.	TT.	PD.	TD.	Period.	SS.	TT.	PD.	TD.
− 48	94	27	35	31	− 15	131	(76)	47	19	+ 17	241	61	80	31
− 47	98	29	32	28	− 14	162	71	50	22	+ 18	276	71	(82)	25
− 46	104	48	27	31	− 13	236	66	55	17	+ 19	(292)	78	79	17
− 45	131	(50)	41	32	− 12	228	58	53	20	+ 20	241	(81)	65	12
− 44	162	41	50	35	− 11	251	51	59	23	+ 21	227	75	55	17
− 43	214	38	(61)	39	− 10	248	48	(59)	29	+ 22	220	64	41	23
− 42	233	37	51	39	− 9	256	41	57	33	+ 23	151	48	32	35
− 41	258	38	45	38	− 8	(257)	34	56	35	+ 24	146	43	36	40
− 40	(288)	45	33	43	− 7	233	29	48	39	+ 25	141	52	41	43
− 39	255	45	33	44	− 6	232	40	43	40	+ 26	(155)	64	55	47
− 38	216	(46)	36	46	− 5	225	52	35	40	+ 27	150	(68)	62	46
− 37	175	42	36	53	− 4	117	(65)	29	50	+ 28	143	67	(63)	46
− 36	111	42	38	58	− 3	165	64	34	52	+ 29	153	61	49	46
− 35	93	48	51	63	− 2	179	45	41	53	+ 30	176	59	44	48
− 34	107	47	60	67	− 1	267	35	55	50	+ 31	223	55	37	50
− 33	149	(50)	(64)	70	0	264	37	55	52	+ 32	277	44	26	49
− 32	192	46	62	71	+ 1	272	42	57	55	+ 33	321	(48)	22	53
− 31	221	33	51	64	+ 2	(303)	42	56	47	+ 34	(332)	46	21	55
− 30	(236)	18	43	63	+ 3	259	(49)	58	49	+ 35	332	43	36	56
− 29	233	24	35	59	+ 4	225	43	(61)	50	+ 36	322	39	48	58
− 28	221	26	30	48	+ 5	208	48	44	39	+ 37	294	45	(61)	59
− 27	189	29	39	43	+ 6	203	49	46	37	+ 38	243	56	59	60
− 26	179	30	52	37	+ 7	172	57	(47)	36	+ 39	197	65	51	60
− 25	176	43	61	24	+ 8	169	(66)	35	27	+ 40	175	(68)	50	60
− 24	163	56	70	19	+ 9	189	63	36	30	+ 41	239	47	(68)	66
− 23	165	63	(75)	15	+ 10	202	55	36	31	+ 42	286	30	67	67
− 22	115	65	66	22	+ 11	204	45	39	28	+ 43	332	22	58	67
− 21	114	66	69	27	+ 12	(241)	32	41	28	+ 44	(365)	27	(63)	70
− 20	103	69	63	31	+ 13	214	19	49	27	+ 45	362	35	45	67
− 19	160	70	52	23	+ 14	178	23	63	32	+ 46	345	42	29	62
− 18	(179)	67	52	25	+ 15	174	31	71	32	+ 47	283	47	40	67
− 17	174	67	52	27	+ 16	209	42	(76)	30	+ 48	269	47	33	60
− 16	104	74	41	17	..	..	..	..	..	..	..	..	..	..

series of 24 years has its 24 years in common with the Prague series, but only 12 years in common with the sun-spot series.

Confining our comparisons in period to sun-spots, Toronto temperature, and Prague declination-ranges, it will be seen that on the whole the positions of maximum apparent Inequality for sun-spots are near those for Toronto temperature and Prague declination. It may be desirable here to repeat the remark which we made in our previous communication, that while this likeness cannot be considered as conclusively proving a connexion, it is nevertheless the sort of similarity which might be expected to exist between phenomena physically connected, but which contain so many apparent Inequalities, and these so near together, that our series of observations is not sufficiently extensive to enable us to eliminate their influence upon each other, or to allow us to ascertain their true positions.

We may likewise remark that in our opinion there is not a greater correspondence between sun-spots and declination-ranges than between sun-spots and temperature-ranges.

4. *Comparison in Phase.*—For this purpose we have treated the Toronto declination and the Prague declination Inequalities *exactly* in the way in which we treated the temperature-range Inequalities of our previous paper, so that the Inequalities of the following table (Table II) are quite comparable with those of our previous paper; they are indeed virtually the same Inequalities. The only difference is that we have in Table II set for calculation in each case from the corresponding sun-spot minimum, which seems to be the most convenient starting point when comparing together Inequalities such as those of this table, which as a rule have only one prominent maximum in their period. It thus appears that here the settings have been arranged by strictly *celestial* considerations. If, therefore, there is no connexion between these terrestrial and solar Inequalities, the declination-range maxima should be distributed impartially up and down the table without any other than chance grouping together. Their behaviour is, however, very different from this—the maxima being comparatively closely grouped together about a position a couple of days after the corresponding sun-spot maximum.

5. *Constancy of Type in the various Inequalities.*—There is a very considerable constancy of type in the declination Inequalities which, as already stated, have only one prominent maximum. Nevertheless, as will be seen both from Table II and from the diagram which accompanies this paper, there is a tendency to duplicity of phase in the terrestrial that is entirely wanting in the solar Inequalities.

Table II.—Comparison of Sun-spot and Declination Inequalities around 24 Days.*

Symbol.	Period in Days.	Date of first number (in January, 1832).
24_{-39}	23·7862	January 1 or January 25.
24_{-33}	23·8193	,, 5 ,, 29.
24_{+18}	24·0985	,, 20
24_{+33}	24·1807	,, 17
24_{+40}	24·2190	,, 21

Toronto declination.						Sun-spots.						Prague declination.					
− 39	− 33	+ 18	+ 33	+ 40	Mean.	− 39	− 33	+ 18	+ 33	+ 40	Mean.	− 39	− 33	+ 18	+ 33	+ 40	Mean.
−114	−176	− 47	− 19	−131	− 97	−574	−539	−554	−421	−690	−556	− 99	− 80	−205	− 37	− 60	− 96
−101	−221	− 52	− 23	−131	−106	−526	−472	−499	−398	−639	−507	−103	− 85	−154	− 54	−128	−105
− 90	−261	− 64	− 46	−132	−119	−436	−360	−401	−372	−549	−424	− 88	− 81	−107	− 46	−178	−100
− 84	−275	− 79	− 66	−136	−128	−303	−234	−260	−340	−428	−313	− 62	− 56	− 60	− 24	−198	− 80
− 90	−221	− 29	− 69	−117	−105	−156	−111	− 94	−280	−289	−186	− 17	− 16	− 7	− 14	−190	− 49
− 87	−128	+ 36	− 31	− 78	− 58	− 9	+ 13	+ 66	−179	−139	− 50	+ 28	+ 26	+ 58	− 15	−146	− 10
− 75	− 6	+ 95	+ 20	− 25	+ 2	+125	+137	+206	− 28	+ 20	+ 92	+ 43	+ 52	+121	− 24	− 67	+ 25
− 37	+110	+108	+ 66	+ 41	+ 58	+233	+249	+314	+162	+198	+231	+ 56	+ 55	+154	− 29	+ 21	+ 52
− 1	+162	+ 71	+119	+ 88	+ 88	+331	+325	+381	+350	+365	+350	+ 49	+ 41	+159	− 31	+ 74	+ 58
+ 58	+174	+ 18	+155	+129	+107	+394	+349	+413	+505	+514	+435	+ 37	+ 19	+118	− 7	+ 98	+ 53
+121	+151	− 33	+182	+161	+116	+422	+339	+427	+606	+614	+482	+ 40	+ 30	+ 78	+ 19	+113	+ 56
+158	+111	− 32	+205	+173	+123	+412	+328	+431	+637	+643	+490	+ 35	+ 58	+ 54	+ 41	+114	+ 60
+194	+102	− 12	+217	+199	+140	+364	+330	+438	+615	+623	+474	+ 42	+109	+ 49	+ 68	+134	+ 80
+193	+110	+ 19	+184	+211	+143	+326	+350	+436	+558	+574	+449	+ 56	+153	+ 76	+ 64	+146	+ 99
+153	+139	+ 67	+115	+200	+135	+306	+354	+402	+464	+515	+408	+ 54	+143	+104	+ 57	+114	+ 94
+110	+174	+ 80	+ 49	+170	+117	+292	+330	+324	+344	+450	+348	+ 59	+ 99	+115	+ 46	+ 72	+ 78
+ 66	+176	+ 71	− 70	+ 99	+ 68	+273	+259	+196	+182	+364	+255	+ 61	+ 28	+114	+ 27	+ 2	+ 46
+ 11	+158	+ 35	−149	+ 26	+ 16	+203	+148	+ 45	− 11	+235	+124	+ 47	− 40	+ 93	+ 10	− 41	+ 14
− 10	+112	− 11	−188	− 61	− 32	+ 87	+ 21	− 98	−199	+ 61	− 26	+ 22	− 76	+ 38	− 9	− 30	− 11
− 24	+ 30	− 41	−221	−120	− 75	− 66	− 99	−225	−359	−148	−179	− 7	− 92	− 23	− 10	− 6	− 27
− 57	− 45	− 57	−179	−139	− 95	−237	−228	−330	−447	−362	−321	− 38	− 82	−110	− 13	+ 39	− 40
− 74	− 94	− 56	−136	−158	−104	−389	−351	−425	−475	−533	−435	− 65	− 72	−193	+ 3	+ 71	− 51
− 97	−135	− 52	− 85	−143	−102	−504	−461	−498	−470	−648	−516	− 70	− 68	−232	+ 4	+ 49	− 63
−123	−147	− 35	− 30	−126	− 92	−568	−536	−545	−444	−703	−559	− 80	− 65	−240	− 26	− 3	− 83

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* In this table all the Inequalities are aggregated for 12 years, and have all been equalised in the same manner.

Table III.—Apparent Inequalities around 26 days (SS = Sun-spots; TT = Toronto Temperature-ranges; PD = Prague Declination-ranges; TD = Toronto Declination-ranges).

N.B.—In this table two or three more numbers of SS and TT are bracketed than in the corresponding tables of our previous paper.

Period.	SS.	TT.	PD.	TD.	Period.	SS.	TT.	PD.	TD.	Period.	SS.	TT.	PD.	TD.
− 48	(456)	53	58	50	− 16	327	31	59	51	+ 16	496	57	(90)	28
− 47	453	60	(60)	55	− 15	394	40	66	48	+ 17	280	63	75	40
− 46	451	59	56	53	− 14	456	54	68	46	+ 18	345	54	70	45
− 45	407	56	56	55	− 13	429	50	80	45	+ 19	280	41	66	48
− 44	321	61	55	57	− 12	459	68	93	51	+ 20	234	42	60	53
− 43	273	68	44	44	− 11	(472)	(80)	101	58	+ 21	247	47	55	50
− 42	271	63	54	45	− 10	345	68	(105)	63	+ 22	234	48	50	44
− 41	259	69	61	44	− 9	320	63	103	68	+ 23	290	58	41	47
− 40	180	48	70	53	− 8	266	68	105	69	+ 24	304	57	39	45
− 39	168	39	75	52	− 7	124	67	(117)	78	+ 25	(340)	51	38	40
− 38	167	60	86	54	− 6	128	63	114	78	+ 26	322	37	36	48
− 37	149	59	106	47	− 5	159	(68)	106	73	+ 27	321	47	35	48
− 36	168	(60)	118	43	− 4	231	55	95	76	+ 28	315	73	38	50
− 35	205	51	(124)	47	− 3	284	49	79	73	+ 29	230	(88)	47	49
− 34	276	48	119	47	− 2	(340)	37	60	68	+ 30	218	61	55	48
− 33	296	43	111	51	− 1	304	44	49	66	+ 31	179	52	60	48
− 32	317	28	105	49	0	319	44	47	62	+ 32	140	(70)	70	48
− 31	384	58	101	33	+ 1	320	43	56	63	+ 33	115	68	(71)	45
− 30	382	60	85	41	+ 2	213	49	58	50	+ 34	168	55	66	42
− 29	(410)	(62)	65	49	+ 3	218	(51)	(67)	48	+ 35	211	43	66	41
− 28	387	58	57	56	+ 4	(232)	48	66	46	+ 36	269	44	56	40
− 27	373	50	59	58	+ 5	137	45	59	40	+ 37	320	55	45	37
− 26	350	40	63	54	+ 6	181	44	54	43	+ 38	411	66	50	44
− 25	351	44	67	55	+ 7	212	43	51	47	+ 39	493	69	45	46
− 24	332	53	72	51	+ 8	200	57	38	36	+ 40	552	62	45	46
− 23	287	51	68	53	+ 9	238	78	34	38	+ 41	619	73	43	50
− 22	(337)	(72)	(77)	60	+ 10	308	(84)	44	40	+ 42	632	(73)	49	45
− 21	297	71	69	56	+ 11	358	70	59	31	+ 43	(637)	70	(56)	41
− 20	240	59	64	49	+ 12	425	(80)	66	33	+ 44	596	47	43	41
− 19	297	56	62	53	+ 13	478	74	74	34	+ 45	559	42	42	41
− 18	264	43	55	55	+ 14	481	69	80	29	+ 46	510	..	39	39
− 17	292	36	52	54	+ 15	(503)	58	87	28	+ 47	374	..	47	41
..	..	..	..	..	..	..	..	..	..	+ 48	..	..	57	44

Results of Comparison around 26 Days.

6. *Comparison as to Duration of Period.*—This is exhibited in Table III, which is precisely analogous to Table I. The same remarks, too, are applicable to both tables, and it will be observed that here, as in the former table, the positions of maximum Inequality for sun-spots, are, on the whole, near those for Toronto temperature and Prague declination. Nor is there, in our opinion, a greater correspondence between sun-spots and declination-ranges than between sun-spots and temperature-ranges.

7. *Comparison in Phase.*—This comparison is exhibited in Table IV, which is precisely analogous to Table VI of our previous communication, except that here we have introduced the Inequality −52, which we had omitted from Table VI, because the Toronto Inequality was not sufficiently near the type. It will be noticed from Table IV, that at least as far as regards the Toronto declination, the constancy of phase is not so evident as for the Inequalities around 24 days. It will likewise be remarked, that while the chief Toronto declination maximum, like that for Inequalities around 24 days, follows a little after the sun-spot maximum, the chief Prague declination maximum decidedly precedes the other two. It thus appears that the similarity in time of maximum between the two declination stations which holds for Inequalities around 24 days (Table II) does not hold for Inequalities around 26 days.

Broadly speaking, in both cases there are appearances of duplicity of phase, but in the case of Toronto the same maximum has remained the predominant one in both tables, while in the case of Prague the predominant maximum for the 24-day Inequalities has become the subsidiary maximum for those around 26 days.

8. In attempted explanation of this we would in the first place desire to repeat the remark we made in our previous communication, namely, that there are two possible kinds of periodicity with regard to sun-spots, and that it is not necessary to regard the Inequalities around 24 days and those around 26 days as perfectly similar phenomena. Again, as regards the evidence we gave in a footnote to that communication, tending to show that the Inequalities around 26 days might denote the synodic periods with respect to the earth of those around 24 days, this evidence is, we find, borne out by the declination results. We prefer, however, to wait until we have accumulated more information before we venture to discuss this important subject. Meanwhile we shall content ourselves with remarking that the similarity between the two stations, Toronto and Prague, for the one set of magnetic Inequalities, and their dissimilarity for the other, is at first sight in favour of the theory of a physical difference of some sort between the two. We

Table IV.—Comparison of Sun-spot and Declination Inequalities around 26 Days.*

Symbol.	Period in days.	Date of first number (in January, 1832).
26_{-52}	25·6915	January 22.
26_{-29}	25·8279	,, 16.
26_{-11}	25·9347	,, 7.
26_{+10}	26·0593	,, 17.
26_{+42}	26·2492	,, 7.

Toronto declination.						Sun-spots.						Prague declination.					
− 52	− 29	− 11	+ 10	+ 42	Mean.	− 52	− 29	− 11	+ 10	+ 42	Mean.	− 52	− 29	− 11	+ 10	+ 42	Mean.
−125	+ 50	−176	+138	− 82	−39	− 977	−818	−887	− 852	−1036	−914	−175	−143	−176	+ 62	+ 4	− 86
−123	0	−121	+131	− 15	−26	−1103	−763	−855	− 862	− 996	−916	−152	−146	−132	+100	+ 55	− 55
− 82	− 58	− 49	+111	+ 41	− 7	− 805	−659	−779	− 789	− 926	−792	−107	−155	− 27	+124	+102	− 12
− 31	−100	+ 10	+ 46	+ 53	− 4	− 507	−502	−603	− 665	− 791	−614	− 47	−121	+138	+111	+131	+ 42
− 32	−141	+ 62	− 40	+ 22	−26	− 229	−308	−361	− 499	− 555	−390	+ 35	− 36	+266	+ 73	+101	+ 88
− 35	−170	+108	−129	− 27	−51	+ 51	−101	− 81	− 282	− 273	−137	+140	+ 73	+332	+ 48	+ 64	+131
− 34	−143	+167	−153	− 69	−46	+ 334	+ 91	+141	− 28	− 11	+105	+194	+121	+298	+ 12	− 16	+122
+ 1	− 74	+186	−123	−120	−26	+ 310	+286	+294	+ 253	+ 208	+270	+186	+103	+212	+ 21	− 58	+ 93
− 4	+ 31	+156	− 51	−170	− 8	+ 383	+463	+372	+ 537	+ 427	+436	+158	+ 54	+126	+ 25	−102	+ 52
+ 12	+117	+ 66	− 21	−163	+ 2	+ 389	+599	+414	+ 806	+ 699	+581	+157	+ 53	+ 85	+ 66	− 55	+ 61
+ 46	+166	− 28	− 4	−107	+15	+ 486	+661	+441	+1007	+ 981	+715	+148	+ 79	+ 87	+ 51	− 13	+ 71
+119	+153	− 85	− 14	− 32	+28	+ 604	+659	+466	+1107	+1248	+817	+107	+114	+ 67	+ 5	+ 21	+ 63
+177	+100	− 96	− 6	− 35	+28	+ 721	+637	+512	+1097	+1404	+874	+ 59	+105	+ 15	− 63	− 33	+ 17
+209	+ 38	− 58	+ 6	− 60	+27	+ 796	+586	+588	+1015	+1412	+879	+ 21	+ 67	− 33	− 71	− 59	− 15
+203	+ 11	− 27	+ 35	− 50	+34	+ 560	+560	+687	+ 868	+1258	+787	+ 14	+ 37	− 35	− 38	− 11	− 7
+158	− 6	+ 11	+ 71	+ 22	+51	+ 384	+514	+771	+ 662	+1013	+669	− 22	+ 32	− 27	− 20	+ 68	+ 6
+ 97	− 23	+ 54	+ 72	+122	+64	+ 558	+475	+767	+ 381	+ 752	+587	− 24	+ 68	− 57	− 25	+109	+ 14
+ 26	− 76	+110	+ 18	+160	+48	+ 491	+383	+645	+ 85	+ 486	+418	− 71	+ 60	−126	− 49	+ 64	− 24
− 35	− 90	+140	− 47	+190	+32	+ 551	+273	+422	− 159	+ 197	+257	− 81	+ 45	−145	− 37	+ 18	− 40
− 66	− 85	+119	− 95	+165	+ 8	+ 365	+116	+188	− 313	− 123	+ 47	−106	+ 2	−109	− 19	− 21	− 51
− 73	− 29	+ 60	− 87	+153	+ 5	+ 169	− 68	− 28	− 408	− 455	−158	− 48	− 2	− 60	− 23	− 51	− 37
− 53	+ 16	− 8	− 58	+104	+ 0	− 131	−296	−238	− 473	− 748	−377	− 21	− 14	− 75	− 65	− 74	− 50
− 78	+ 54	− 84	− 6	+ 52	−12	− 526	−504	−469	− 517	− 960	−595	− 3	− 37	−136	−102	− 85	− 73
− 81	+ 77	−140	+ 29	+ 2	−23	− 867	−679	−687	− 562	−1062	−771	− 68	− 87	−161	−112	− 72	−100
− 98	+ 87	−184	+ 74	− 66	−37	− 931	−778	−833	− 644	−1084	−854	−118	−129	−167	− 61	− 52	−105
− 98	+ 95	−193	+103	− 90	−37	−1076	−827	−887	− 765	−1065	−924	−176	−143	−160	− 13	− 35	−105

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* In this Table all the Inequalities are aggregates for 12 years, and have all been equalised in the same manner.

have used the words *at first sight*, because, apart altogether from the comparatively small number of the Inequalities discussed, there is a strictly terrestrial consideration which we must not lose sight of.

It is well known to all magneticians that we have not as yet arrived at any wholly satisfactory method of separating between the disturbed and the undisturbed magnetic observations, and the results now exhibited have unquestionably been deduced from observations which include a good many disturbances. Now under these circumstances the effects of disturbances would only disappear from our results on the hypothesis that such effects have no reference whatever to the periodicities of which we have been treating—that they are, in fact, non-periodic—so that they will become eliminated in a sufficiently extensive series of observations. But we have much reason to suppose that this is not the case, for the observations of Professor Loomis and of Mr. John Allan Broun would seem to indicate that short-period Inequalities of sun-spots occasion terrestrial magnetic disturbances, which follow closely on the celestial phenomena, so that a maximum of sun-spots is quickly followed by a maximum of disturbance. Now in the preceding tables we have discussed some of the most prominent solar Inequalities in connexion with their magnetic effects, and doubtless the result we have obtained is a composite one, its components being an Inequality of solar diurnal declination-range (undisturbed), and an Inequality of disturbance declination-range. We may add that Toronto is a station where the disturbance is great, and also that the sun-spot Inequalities around 26 days are greater than those around 24 days.

Attempted Elimination of Disturbances.

9. All these considerations point to the necessity of eliminating as much as possible the effect of disturbances before we venture to discuss our results. We have attempted to do this in the following manner :—

First of all, we would remind the reader that the Inequalities around 24 and 26 days that we have been dealing with are most probably not *all* the Inequalities around these periods, but only the larger specimens of them.

We remarked in our previous communication that observations founded on sun-spots might present the same variety of period, when treated as we have treated them, which they presented when treated in another way by Carrington, who found that the spots in one solar latitude had a different period of rotation from those in another. If there be any truth in this remark, we might expect that the few solar Inequalities which we have exhibited are only the most prominent members of a comparatively large series, packed, it may be, so closely together that we cannot disentangle them completely by our limited series of observations. Now it is probable that magnetic disturbances would limit themselves in great measure to the especially large solar Inequalities, so that if we could find some method of treating not merely the *larger* but *all* the Inequalities, we might probably rid ourselves to a considerable extent of the influence of disturbance. But by our method we have the means of doing this. We possess for each element, for each period altogether over 100 series, representing Inequalities extending from −52 to +52 of our notation.

Furthermore, we have the same series of 24 years common to Toronto declination, Kew temperature, and Prague declination, and it is with this common series that we have made a comparison as follows. The Kew temperature Inequalities have virtually only one maximum and one minimum, and we have selected all those in which it is possible to ascertain accurately the position of the maximum, that is to say, all those which are according to type. Now let the Toronto and Prague declination Inequalities be set in all cases so as to start from the maximum of the corresponding Kew temperature Inequality, using of course for this purpose not the whole 36 years of Prague observations, but only 24 of these. We are thus comparing 24 years of simultaneous declination records at Toronto and at Prague, the setting being in each case from the maximum of the corresponding Kew temperature record for the same 24 years.

In this comparison all the Inequalities, great and small, may be imagined as made use of, and the influence of disturbance eliminated at least to a great extent.

10. The results of this process are exhibited in the following table, and they may be at once compared with those given in Tables II and IV. For the purpose of this comparison we have transferred the starting points of the modified Inequalities to the solar minimum, so as to make them comparable with those of the previous tables. We can easily make the change from the knowledge derived from our previous paper that the Kew temperature maximum is about 2 days before the solar maximum.

The Toronto declination Inequality for 24 days is not greatly altered by the modified process.

In the Prague declination Inequality for 24 days the modification produced causes the two maxima to be more clearly separated from one another.

In both of these Inequalities as modified, the great maximum is not long after the solar maximum.

If we turn next to the Inequalities around 26 days, we find that for Toronto the subsidiary maximum of Table IV becomes when modified the predominant one, and the prominent maximum of Table IV the subsidiary one, while there is no striking alteration in the Prague Inequality.

Table V.—Modified Values with Disturbances supposed to be Reduced.

Toronto declination, 24 days.	Prague declination, 24 days.	Toronto declination, 26 days.	Prague declination, 26 days.
−31	− 7	+38	+60
−29	− 9	+37	+64
−22	−11	+39	+63
−12	−12	+37	+71
− 7	− 9	+36	+61
+ 1	− 1	+23	+50
+ 9	+ 3	+ 5	+30
+17	+ 5	−10	+28
+29	+ 5	−19	+32
+36	− 4	−19	+30
+41	− 9	−26	+16
+44	−12	−31	−15
+40	−13	−43	−40
+37	+ 1	−41	−59
+29	+17	−29	−55
+14	+29	−12	−38
− 1	+36	+ 3	−16
−17	+33	+ 7	− 6
−30	+18	+ 1	− 8
−32	+ 1	−17	−36
−31	−12	−29	−67
−27	−20	−24	−98
−27	−18	− 4	−84
−31	−11	+16	−44
..	..	+29	+10
..	..	+33	+51

Thus the result has been to do away with that want of similarity between the Toronto and Prague 26-day Inequalities which appeared in Table IV, and to substitute two series in which the predominant maximum of the one is near in position to that of the other, and the subsidiary maximum of the one near in position to that of the other.

Nevertheless, the predominant maxima of the 24-day Inequalities agree most nearly in position with the subsidiary maxima of the 26-day Inequalities. In fine, the Inequalities around 26 days are different from those around 24 days in much the same way for both stations.

11. It appears to us that these results are in favour of there being some physical difference between the Inequalities around 24 days and those around 26 days, or at least we may use this as a working hypothesis. Professor Stokes has suggested that an outbreak of solar

activity would probably alter the quality as well as the quantity of the solar rays, so as to bring in a greater proportion of those which are absorbed in the upper regions of the atmosphere. We might probably thus expect a set of terrestrial actions following promptly after the solar outbreak. This is similar to what we have more especially in the magnetic Inequalities around 24 days.

On the other hand, if the Inequalities around 26 days are due to the earth's being placed in a favourable position for receiving the solar influence, we shall have a state of things physically different from that which we imagine to characterise the Inequalities around 24 days, and in our ignorance of the exact way in which the sun influences the magnetism of the earth, we cannot assert that the Inequality produced in the one case will be necessarily the same as that produced in the other.

Apparent Progress of Magnetic Weather.

12. In order to prevent ambiguity, it is desirable to define what we mean by the apparent progress of magnetic weather. If a particular state of declination diurnal range—a maximum for instance—be found to occur at Prague four days after it occurs at Toronto, and if there is reason to believe that this difference in time depends upon the distance between the stations, we should characterise the phenomenon by terming it an apparent progress of magnetic weather from west to east. But this phrase must not be regarded as implying any theoretical explanation of the observed fact, or as asserting that it is an actual progress of matter in the direction from west to east which gives rise to the phenomenon.

It is obvious that if such a progression exists it will be most readily seen in the undisturbed observations, for it is one of the characteristics of a disturbance to occur simultaneously or nearly so at stations far apart, while it is another characteristic to exalt the daily range. Hence if disturbances possess periodicity, the maxima of their periods might be expected to occur simultaneously or nearly so at stations far apart. Magnetical weather is, however, something different from disturbances, and denotes, as we have used the term, a particular state or value of undisturbed diurnal magnetic range, just as a particular state or value of diurnal temperature-range may be said to denote a particular kind of meteorological weather. Again, in certain preliminary investigations evidence has been given by one of us tending to show that there is possibly a progress of magnetic weather from west to east. But it is clear that in making a comparison of this nature not only must we get rid of disturbances as much as possible, but we must likewise limit our comparison to Inequalities of the same type or nearly so. Now both of these conditions are possessed by the eries of Table V, for in the first place we may imagine that they are nearly freed from disturbance, and in the next place the two series are very much alike in type.

13. In order to compare the Inequalities of Table V we may consider the Prague series as stationary and the Toronto as movable, and take the algebraic addition of the two series in various relative positions. For instance, Toronto pulled backwards one or two divisions (days) to the left; both together; Toronto pushed forward 1, 2, 3, 4, 5, &c., divisions to the right. The algebraic sum of the two will give the greatest range when the corresponding phases of the two Inequalities are most nearly together.

The following is the result obtained by this method of comparison:—

Table VI.

24-day Inequalities.	Joint area of both.
Toronto 2 to left	594
,, 1 ,,	676
Together	738
Toronto 1 to right	776
,, 2 ,,	804
,, 3 ,,	796
,, 4 ,,	794
,, 5 ,,	794
,, 6 ,,	792
,, 7 ,,	770
,, 8 ,,	742
,, 9 ,,	726
,, 10 ,,	694

26-day Inequalities.	Joint area of both.
Toronto 2 to left	1322
,, 1 ,,	1448
Together	1560
Toronto 1 to right	1628
,, 2 ,,	1658
,, 3 ,,	1670
,, 4 ,,	1642
,, 5 ,,	1574
,, 6 ,,	1532
,, 7 ,,	1500

For the 24-day Inequalities the position of maximum area is somewhat undecided, the numbers bracketed being practically the same. On the whole we may consider that the middle point of this region, which denotes "Toronto 4 to the right," expresses the nearest coincidence in phase.

For the 26-day Inequalities the maximum is when Toronto is pushed three divisions to the right. We may therefore state that as far as this comparison is concerned, a given phase occurs at Toronto three or four days before it occurs at Prague. In this preliminary investigation no account has been taken of the difference in longitude between the two stations as affecting the strict simultaneity of the diurnal ranges.

Comparison between Temperature-ranges and Declination-ranges.

14. The Toronto temperature-ranges and the Prague declination-ranges are for the same series of 36 years, and if we compare together the corresponding Inequalities of these ranges as given in Tables I and III, we obtain the following result by taking the sums:—

Toronto Temperature-range, 24 days.	Prague Declination-range, 24 days.	Toronto Temperature-range, 26 days.	Prague Declination-range, 26 days.
4735	4795	5293	6316

We may conclude from this comparison that, as treated by our method, the declination-ranges and temperature-ranges exhibit Inequalities pretty much of the same magnitude. There is a slight excess of the declination over the temperature for the 26-day Inequalities, but these, being larger, may possibly be influenced by the results of disturbance to a greater extent than those around 24 days. Disturbance would doubtless increase the range.

Again, while both kinds of Inequalities are very much of the same size, the results of this and of our previous paper lead us to conclude that the one set of Inequalities does not exhibit a closer correspondence with sun-spots than the other, so that as far as our experience goes there is no reason for saying that for short-period solar Inequalities the terrestrial result is more marked in magnetism than in meteorology.

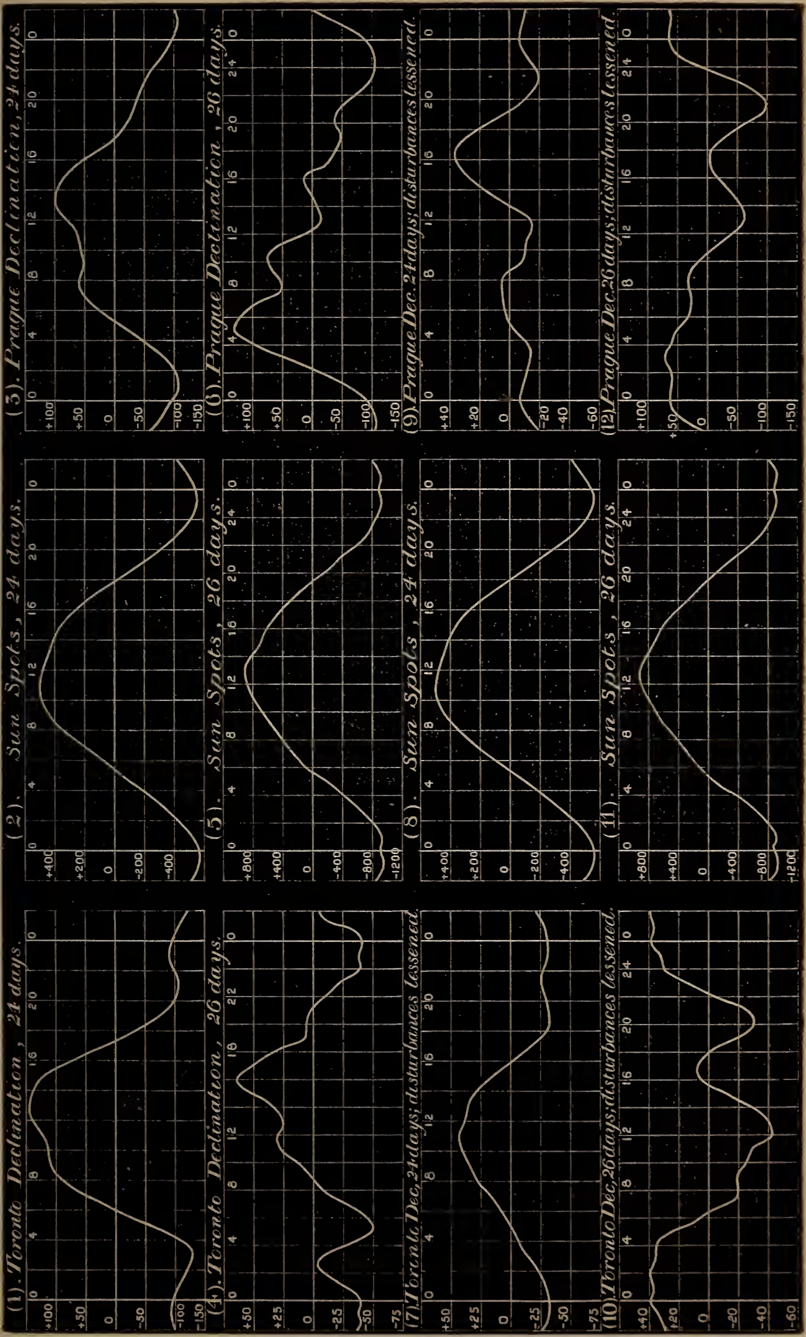
15. It is perhaps worth while to exhibit the connexion between the temperature-range and the declination-range Inequalities in the following manner (p. 234).

We have already (Art. 9) mentioned how the Kew temperature-ranges were used by us for setting the Inequalities whose mean result is given in Table V. Now if there be no perceptible physical relation between temperature-range and declination-range, the declination-range Inequalities set by this method should have their corresponding phases distributed at random impartially up and down the paper. In Table VII we have exhibited the individual series representing Prague declination-ranges around 26 days that have been set by this method, only in order to save space we have grouped them into threes (with due regard to phase). It will, we think, be seen from this table that, with comparatively few exceptions, *minus* numbers are grouped together in the upper part of the table, and *plus* numbers in the lower.

The result is thus, in our opinion, in favour of that hypothesis which asserts a physical relationship between the two Inequalities.

Table VII.—Individual Prague Declination Inequalities (26 Days) set by Kew Temperature Maxima.

−51	−48	−42	−36	−27	−24	−21	−18	−15	−12	+0	+3	+6	+9	+12	+15	+18	+21	+24	+36	+42	+45	Sum.
+ 51	− 14	+ 148	−2503	− 738	+ 74	− 616	− 419	− 337	+ 36	−1759	−1101	− 777	+ 109	+1055	+ 207	+ 662	+ 966	+ 992	+ 897	+728	+300	− 2039
− 135	− 219	+ 447	−2255	− 927	− 72	− 715	− 746	− 356	+ 97	−1613	−1086	− 668	+ 144	+ 448	− 254	+ 281	+ 777	+ 388	+ 428	+641	+ 18	− 5377
− 565	− 314	+ 531	−1946	− 949	− 407	− 891	− 988	− 139	− 165	− 917	− 829	− 170	+ 824	+ 28	− 175	− 240	+ 414	− 467	− 79	+ 89	−445	− 7800
− 385	− 186	+ 833	−1130	− 185	− 651	− 985	−1304	− 267	− 686	− 710	− 638	+ 462	+1028	− 75	− 219	− 600	+ 30	− 709	+ 47	−348	−675	− 7353
− 553	− 49	+1003	− 324	+ 43	− 785	− 942	−1494	− 692	− 762	− 666	− 186	+1057	+ 951	+ 45	+ 47	− 411	− 246	− 380	+ 649	−843	−505	− 5039
− 619	+ 320	+ 974	+ 400	+ 210	− 388	− 694	−1564	−1553	− 619	− 559	+ 241	+1312	+ 298	+ 99	+ 88	− 155	− 457	+ 31	+1005	−565	+ 2	− 2183
−1107	+ 444	+1086	+ 731	− 296	− 23	− 431	−1306	−1944	− 286	+ 6	+ 692	+1054	− 72	+ 146	− 83	− 78	− 401	+ 83	+ 579	− 47	+548	− 705
−1168	+ 284	+1193	+1079	− 317	+ 373	− 150	− 933	−2024	− 498	+ 494	+ 889	+ 511	− 170	+ 218	− 607	− 787	− 260	+ 59	− 464	+521	+719	− 1038
−1293	− 297	+1104	+1574	− 430	+ 215	− 444	− 684	−1451	− 808	+ 478	+ 813	− 21	− 302	− 198	−1366	−1610	− 199	+ 321	−1006	+322	+445	− 4837
−1077	− 924	+ 635	+1880	− 370	− 16	− 375	− 572	− 827	−1123	+ 243	+ 809	− 312	− 368	− 674	−1791	−2272	− 678	+ 179	−1287	+ 40	− 20	− 8900
− 919	−1394	− 76	+1585	− 648	− 307	− 326	− 201	− 340	−1355	− 2	+ 431	− 379	− 603	−1393	−2116	−2132	−1162	− 263	−1146	+ 10	−112	−12848
− 605	−1660	− 155	+ 915	− 367	− 158	+ 477	+ 517	− 129	−1425	+ 405	+ 357	− 227	− 539	−1372	−1899	−1491	−1750	−1063	−1149	+153	+ 40	−11125
− 178	−1427	+ 59	+ 842	+ 258	+ 50	+ 673	+1422	+ 119	−1505	+ 693	+ 71	− 22	− 599	−1341	−1304	− 734	−1325	− 944	−1001	+243	+257	− 5694
+ 376	−1115	+ 629	+1011	+ 563	+ 292	+ 964	+1700	+ 590	−1156	+1201	+ 290	− 83	− 606	− 934	− 482	− 7	− 878	− 447	− 714	+138	+150	+ 1472
+1034	− 447	+ 478	+1287	+ 345	+ 463	+ 708	+1498	+1082	− 459	+ 853	− 2	− 295	− 687	− 815	+ 121	+ 754	+ 204	+ 225	− 347	+327	+323	+ 6650
+1199	− 158	+ 50	+ 651	− 341	+ 833	+ 686	+ 813	+1353	+ 546	+ 869	+ 49	− 552	− 760	− 789	+ 349	+1454	+ 645	+ 224	+ 138	+429	+263	+ 7951
+1048	+ 194	− 643	+ 173	− 307	+1285	+ 343	+ 633	+1326	+1391	+ 839	+ 133	− 456	− 804	− 765	+ 432	+1703	+1124	+ 68	+ 327	+177	+282	+ 8503
+ 808	+ 320	− 809	− 146	+ 50	+1211	+ 367	+ 605	+1085	+1895	+1238	+ 678	− 332	− 711	− 595	+ 594	+1313	+ 988	+ 83	+ 439	−401	−331	+ 8349
+ 954	+ 690	− 802	+ 213	+ 482	+ 424	+ 590	+ 700	+ 940	+1865	+1284	+ 754	− 2	− 497	− 99	+ 843	+ 749	+ 955	+ 151	+ 449	−821	−510	+ 9312
+ 829	+ 969	− 833	+ 608	+ 372	− 701	+ 996	+ 561	+ 917	+1542	+ 667	+ 374	+ 220	+ 104	+ 158	+1059	+ 59	+ 716	+ 164	+ 822	−830	−633	+ 8140
+ 458	+1044	− 947	+ 739	+ 60	−1152	+1135	+ 485	+ 888	+1079	+ 70	− 248	+ 482	+ 694	+ 580	+1159	− 148	+ 343	− 195	− 750	−611	−163	+ 6502
+ 124	+ 917	−1207	+ 386	+ 165	− 836	+ 773	+ 562	+ 722	+ 824	− 435	− 404	+ 382	+1010	+ 780	+1119	− 15	− 225	− 340	+ 272	−466	− 79	+ 4029
+ 146	+ 835	−1274	− 428	+ 725	− 75	+ 306	+ 527	+ 482	+ 691	− 307	− 402	+ 180	+ 852	+1274	+1215	+ 596	− 580	− 273	− 384	−251	− 15	+ 3840
+ 528	+ 893	−1295	−1192	+1279	+ 160	− 269	+ 342	+ 410	+ 581	− 344	− 278	− 227	+ 430	+1364	+1224	+1047	− 254	+ 238	− 341	+109	−144	+ 4261
+ 490	+ 795	− 895	−1913	+1021	+ 178	− 539	+ 55	+ 212	+ 248	− 632	− 560	− 423	+ 280	+1513	+1166	+1117	+ 341	+ 742	+ 282	+449	+ 29	+ 3956
+ 559	+ 499	− 234	−2237	+ 302	+ 13	− 641	− 209	− 67	+ 52	−1396	− 847	− 714	− 6	+1342	+ 673	+ 945	+ 912	+1133	+ 834	+807	+256	+ 1973


(1). Toronto Declination, 24 days.
0 4 8 12 16 20 0
+100 +50 0 -50 -100 -150
(2). Sun Spots, 24 days.
0 4 8 12 16 20 0
+400 +200 0 -200 -400
(3). Prague Declination, 24 days.
0 4 8 12 16 20 0
+100 +50 0 -50 -100 -150
(4). Toronto Declination, 26 days.
0 4 8 12 16 22 0
+50 +25 0 -25 -50 -75
(5). Sun Spots, 26 days.
0 4 8 12 16 20 24 0
+800 +400 0 -400 -800 -1200
(6). Prague Declination, 26 days.
0 4 8 12 16 20 24 0
+100 +50 0 -50 -100 -150
(7). Toronto Dec. 24 days; disturbances lessened.
0 4 8 12 16 20 0
+50 +25 0 -25 -50 -75
(8). Sun Spots, 24 days.
0 4 8 12 16 20 0
+400 +200 0 -200 -400
(9). Prague Dec. 24 days; disturbances lessened.
0 4 8 12 16 20 0
+40 +20 0 -20 -40 -60
(10). Toronto Dec. 26 days; disturbances lessened.
0 4 8 12 16 20 24 0
+40 +20 0 -20 -40 -60
(11). Sun Spots, 26 days.
0 4 8 12 16 20 24 0
+800 +400 0 -400 -800 -1200
(12). Prague Dec. 26 days; disturbances lessened.
0 4 8 12 16 20 24 0
+100 +50 0 -50 -100 -150

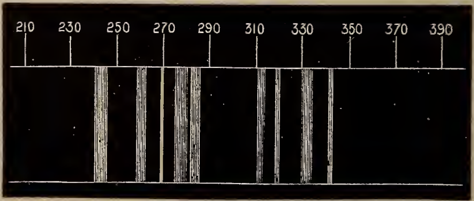
II. "On Radiant Matter Spectroscopy: Note on the Earth $Y\alpha$." By WILLIAM CROOKES, F.R.S. Received February 18, 1886.

Among the samarskite earths which concentrate towards the middle of the fractionations there is one (or a group) which presents in the radiant matter tube a well marked phosphorescent spectrum differing from those I have already described.

The measurements of the bands and lines are given below:—

Scale of spectroscope.	λ.	$\frac{1}{\lambda^2}$	Remarks.
10·325°	6446	2407	Approximate centre of a red band shaded off on the least refrangible side.
10·310	6415	2430	Somewhat sharp edge of the red band.
10·185	6189	2611	Approximate centre of a very faint orange band.
10·130	6094	2693	A sharp narrow orange-red line.
10·050	5970	2806	Approximate centre of a narrow bright orange band. (Between this line and 2693 is a fainter semi-continuous orange band.)
9·840	5676	3104	Approximate centre of a narrow bright green band.
9·790	5613	3174	Approximate centre of a narrow green band, not quite so bright as 3104.
9·690	5495	3312	Approximate centre of a bright green band, wider than the other three green bands.
9·610	5406	3422	Approximate centre of a narrow bright green band.

The accompanying figure gives the spectrum drawn to the $\frac{1}{\lambda^2}$ scale.



The earth giving the above spectrum, when sufficiently purified, presents all the characteristics of the earth discovered by Marignac, and provisionally called by him $Y\alpha$.[*] Through the kindness of M. de

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[*] "Comptes rendus," xc, p. 899.

Marignac I have been enabled to compare a specimen of $Y\alpha$ of his own preparation with the earth described above. The two earths agree in their chemical characteristics, and their phosphorescent spectra are practically identical.

No name has yet been given to this earth, as M. de Marignac appears to be in some doubt whether it is not identical with J. Lawrence Smith's earth mosandra.* A specimen of mosandra prepared by J. Lawrence Smith, and sent me by M. de Marignac, gave a phosphorescent spectrum showing that it was compound, and that yttria was one of its constituents.

March 4, 1886.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Atkinson, Prof. Edmund, Ph.D.
Bidwell, Shelford, M.A.
Bosanquet, Robert Halford Macdowall, M.A.
Boys, Charles Vernon, A.R.S.M.
Buchanan, John Young, M.A.
Burdett, Henry Charles, F.S.S.
Buzzard, Thomas, M.D.
Cameron, Sir Charles Alexander, M.D.
Cash, J. Theodore, M.D.
Claudet, Frederic, F.C.S.
Colenso, William, F.L.S.
Corfield, Prof. William Henry, M.D.
Curtis, Arthur Hill, D.Sc.
Davis, James William, F.G.S.
Denton, John Bailey, M.I.C.E.
Dixon, Harold B., M.A.
Douglass, Sir James Nicholas, M.I.C.E.
Ewart, Professor J. Cossar, M.D.
Ewing, Professor J. A., B.Sc.
Festing, Edward Robert, Major-General, R.E.
Forbes, Professor George, M.A.
Forsyth, Andrew Russell, M.A.
Foster, Professor Balthazar Walter, F.R.C.P.
Galloway, William.
Gowers, William Richard, M.D.
Green, Professor A. H., M.A.
Hinde, George Jennings, Ph.D.
Horsley, Prof. Victor, F.R.C.S.
Latham, Peter Wallwork, M.D.
Lewis, Timothy Richards, M.B., Surgeon-Major, A.M.D.
MacGillivray, Paul Howard, M.A.
Manson, Patrick, M.D.
Meldola, Raphael, F.R.A.S.
Milne, Professor John, F.G.S.
Moxon, Walter, M.D.
Ord, William Miller, M.D.

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* "Comptes rendus," lxxxvii, p. 145; lxxxvii, p. 831; lxxxix, p. 480.

Palmer, Henry Spencer, Colonel R.E.
Pickard-Cambridge, Rev. Octavius, M.A.
Poynting, Prof. John Henry, B.Sc.
Pritchard, Urban, M.D.
Pye-Smith, Philip H., M.D.
Ramsay, Professor William, Ph.D.
Rodwell, George F., F.R.A.S.
Russell, Henry Chamberlaine, B.A.
Sanders, Alfred, F.L.S.
Sedgwick, Adam, M.A.
Snelus, George James, F.C.S.
Sollas, Professor William Johnson, D.Sc.
Stevenson, Thomas, M.D.
Tate, Professor Ralph, F.G.S.
Teale, Thomas Pridgin, F.R.C.S.
Tenison-Woods, Rev. Julian E., M.A.
Tidy, Prof. Charles Meymott, M.B.
Tonge, Morris, M.D.
Topley, William, F.G.S.
Unwin, Prof. W. Cawthorne, B.Sc.
Warington, Robert, F.C.S.
Wharton, William James Lloyd, Captain R.N.
Whitaker, William, B.A.
White, William Henry.
Wilde, Henry.
Wright, Professor Edward Perceval, M.A.

The Bakerian Lecture was then delivered as follows:—

I. The Bakerian Lecture.—"Colour Photometry." By Captain Abney, R.E., F.R.S., and Major-General Festing, R.E.

(Abstract.)

One of the authors of this paper has already communicated to the Physical Society of London ("Phil. Mag.," 1885) a method by which a patch of monochromatic light could be thrown on a screen. This formed the starting point of the present investigation, which was to ascertain whether it was practicable to compare with each other the intensity of lights of different colours.

The authors describe various plans they adopted to effect this purpose, and finally found that by placing a rod in front of the patch of monochromatic light, and of a candle by casting another shadow, the intensities of the two lights could be compared by what they term an oscillation method. It is known that on each side of the yellow of the spectrum the luminosity more or less rapidly decreases. By placing a candle at such a distance from the screen that the luminosity of the two shadows appears as approximately equal, it is easy to oscillate the card carrying the slit through which the monochromatic rays of the spectrum pass. (The slit is in the focus of the lens which helps to form the spectrum.) The shadow of the rod cast by the candle can thus be made to appear alternately "too light" or "too dark" in comparison with the shadow of the rod cast by the parts of the spectrum falling on the screen. By a rapid oscillation the position of equality of the two shadows can be distinguished with great exactness. The authors describe their method of fixing the position of the rays employed and the source of light with which the spectrum is formed. They also enter into details as to the comparison light, the receiving screen, and the comparative value of the light as seen by them respectively. The curve of the intensity of the arc light spectrum, as seen by their eyes, which they call the normal curve, is then described. The question as to the effect of an alteration of the colour of the comparison light is then discussed, as is the effect of the brightness of the spectrum.

The next point touched upon is as to the value of mixed light as compared with its components. It is found that the following law holds good, viz.: *that "the sum of the intensities of two or more colours is equal to the intensity of the same rays when mixed."* This law is applied to Hering's theory of colour.

The authors next state that with the majority of people the curve of luminosity of the spectrum is identical with the normal curve, but that in some cases slight differences may be observed, of which one example is given. Such slight deficiency does not constitute colour-blindness, since the want of appreciation of any colour is but very partial. They next describe observations made by four colour-blind persons, and show that there is a remarkable divergence in their curves from the normal. The deficiency curves are shown, from which it appears that two of the observers are totally blind to red, whilst the other two are partially so. They then show that such observers would not give a true value for any light which is not of identically the same colour as the comparison light they might employ. It also appears that the intensity of illumination felt by a colour-blind is really less than that perceived by a normal-eyed person.

Two examples of the normal curve for sunlight are then given, one taken on a day in July by the method of separating close lines by means of varying illumination, and the other in November, by the method adopted by the authors. Their results are compared with Vierordt's curve, obtained by extinguishing colour with white light.

In order to ascertain the effect of the turbidity of a medium through which light passes (for instance sunlight), the authors compared the intensity of the spectrum after passing through clear water and turbid water, and found that the absorption agreed with Lord Rayleigh's theoretical deductions that $I' = I_0 \epsilon^{-kx\lambda^{-4}}$, where I' is the intensity after passing through a turbid medium, I_0 the intensity after passing through clear water, x the thickness of the turbid layer, k a constant independent of λ, λ being the wave length.

The authors conclude their paper with a discussion of the intensity of incandescence of carbon electrically heated.

March 11, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity—*continued.* The Internal Friction of Metals." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received February 18, 1886.

(Abstract.)

An abstract of a paper on this subject has been already published,* but the paper itself was withdrawn for the purpose of revision. The fresh experiments which have been for this purpose instituted during the last year were made with improved apparatus, and the coefficient of viscosity of air redetermined, with a view of enabling the author to make more accurate correction for the effect of the resistance of the air.† These more recent experiments on the loss of energy of a torsionally vibrating wire, besides confirming the results of the older ones, as far as the latter have been published, have furnished, more or less in addition, the following facts relating to the internal molecular friction of metals:—

The proportionate diminution of amplitude is independent of the amplitude, provided the deformations produced do not exceed a certain limit. This limit varies with the nature of the metal, and is for nickel very low.

The logarithmic decrement of amplitude increases with the length of the vibration-period, but in a less proportion than the latter, and in a diminishing ratio. The amount of increase of the logarithmic decrement, attending on a given increase through a given range of the vibration-period, varies with the nature of the metal, and with those metals which possess comparatively small internal friction becomes almost insensible. It follows as a consequence that the

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* "Proc. Roy. Soc.," vol. 38, p. 42.

† An abstracted account of this redetermination was read before the Royal Society, January 14, 1886.

internal friction of metals differs from the viscosity of fluids, for in cases of damping by the latter the logarithmic decrement is *inversely* as the length of the vibration-period.

Permanent molecular strain resulting from loading not carried to a sufficient extent to produce sensible permanent extension, diminishes the internal friction, and increases the torsional elasticity.

Considerable permanent longitudinal extension and permanent torsion produce increase of internal friction and diminution of torsional elasticity. The effect of torsion is much greater than that of extension, and the increase of internal friction is much greater than the decrease of torsional elasticity. As a consequence wire-drawing, where we have permanent extension and torsion combined, sometimes increases enormously the internal friction; in fact in the case of six different metals, it was found that by careful annealing the internal friction was decreased from *one-half to one-thirtieth* of the original amount of friction of the metals in the hard-drawn condition. Almost equally remarkable is the effect of rapid fluctuations of temperature, even through ranges of only one or two degrees centigrade, in increasing the internal friction.

The internal friction of a metal wire, whether in the hard-drawn or annealed condition, is temporarily decreased, and the torsional elasticity is temporarily increased by loading not carried beyond a certain limit, beyond this limit both the friction and the elasticity become independent of the load.

The "fatigue of elasticity," discovered by Sir William Thomson in metal wires when vibrating torsionally, is not felt, provided the deformations produced do not exceed a certain limit, depending upon the nature of the metal. The above-mentioned limit is extraordinarily low for nickel, so low, indeed, that it is difficult to avoid "elastic fatigue" with this metal. This last consideration, and others founded on the results of experiments on the effects of stress on the physical properties of nickel, tend to show that the molecules of this metal are comparatively easily rotated about their axes.

The author agrees with Prof. G. Wiedemann, that the loss of energy due to internal friction in a torsionally vibrating wire is mainly due to the to-and-fro rotation of the molecules about their axes; any cause, therefore, which increases the molecular rotatory elasticity diminishes the internal friction, and conversely. The author has, by various means, succeeded in bringing down the internal friction to such an extent that, in the case of one wire, it would have required upwards of 15,000 vibrations to diminish the amplitude to one-half of its initial value, provided the vibrations had been executed *in vacuo.*

The molecules of a metal tend to creep into such positions as will ensure a maximum molecular rotatory elasticity, and they can be assisted in doing so by agitations effected either by thermal or mechanical agency; hence—

Rest after suspension, aided by oscillations at intervals, diminishes the internal friction of a wire which has been recently suspended, or which after a long suspension has been subjected to considerable molecular agitation by either mechanical or thermal agency.

On the contrary, when a maximum molecular rotatory elasticity has been reached, molecular agitation, if carried beyond a certain limit, diminishes the elasticity; hence the results of "fatigue of elasticity;" and hence—

Mechanical shocks and rapid fluctuations of temperature beyond certain limits may considerably increase the internal friction, and, though to a much less extent, diminish the torsional elasticity.

The logarithmic decrement is independent of both the length and diameter of the wire.

II. "On Systems of Circles and Spheres." By R. LACHLAN, B.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. CAYLEY, F.R.S. Received February 23, 1886.

(Abstract.)

This memoir is an attempt to develop the ideas contained in two papers to be found in the volume of Clifford's Mathematical Papers (Macmillan, 1882), viz., "On Power Coordinates" (pp. 546—555), and "On the Powers of Spheres" (pp. 332—336); the date of the former is stated to be 1866, and of the latter 1868, but the editor explains (see p. xxii, and note, p. 332) that though these papers probably contain the substance of a paper read to the London Mathematical Society, February 27, 1868, "On Circles and Spheres" ("Proc. L. M. S.," vol. ii, p. 61), they were probably not written out before 1876. It is possible, therefore, that Clifford may be indebted to Darboux for the conception of the "*power* of two circles," or spheres, as an extension of Steiner's use of the "*power* of a point with respect to a circle. Darboux was the first to give the definition of the power of two circles, in a paper "Sur les Relations entre les Groupes de Points, de Cercles, et de Sphères" ("Annales de l'École Normale Supérieure," vol. i, p. 323, 1872), in which some theorems analogous to the fundamental theorem of this memoir are proved.

This memoir is divided into three Parts: Part I consists of the discussion of systems of circles in one plane; Part II of systems of circles on the surface of a sphere; and Part III of systems of spheres.

The power of two circles is defined to be the square of the distance between their centres less the sum of the squares of their radii.

Denoting the power of two circles (1, 2) by $\pi_{1,2}$, it is proved that the power of any five circles (1, 2, 3, 4, 5) with respect to any other circles (6, 7, 8, 9, 10) are connected by the relation—

$$\begin{vmatrix} \pi_{1,6}, & \pi_{1,7}, & \pi_{1,8}, & \pi_{1,9}, & \pi_{1,10} \\ \pi_{2,6}, & \pi_{2,7}, & \pi_{2,8}, & \pi_{2,9}, & \pi_{2,10} \\ \pi_{3,6}, & \pi_{3,7}, & \pi_{3,8}, & \pi_{3,9}, & \pi_{3,10} \\ \pi_{4,6}, & \pi_{4,7}, & \pi_{4,8}, & \pi_{4,9}, & \pi_{4,10} \\ \pi_{5,6}, & \pi_{5,7}, & \pi_{5,8}, & \pi_{5,9}, & \pi_{5,10} \end{vmatrix} = 0$$

which may be conveniently written:—

$$\pi\binom{1,\ 2,\ 3,\ 4,\ 5}{6,\ 7,\ 8,\ 9,\ 10}=0.$$

This is the fundamental theorem of the paper; it is shown that if the power of a straight line and a circle be defined as the perpendicular from the centre of the circle on the straight line, and the power of two straight lines as the cosine of the angle between them: then the theorem is true if any of the circles of either system be replaced by points, straight lines, or the line at infinity.

Several special systems of circles are then discussed, the most remarkable perhaps being the case when the circles (1, 2, 3, 4) being given, the circles (5, 6, 7, 8) are orthogonal to the former taken three at a time; then (x, y), denoting any other circles, the equation—

$$\pi\binom{x,\ 1,\ 2,\ 3,\ 4}{y,\ 5,\ 6,\ 7,\ 8}=0$$

becomes $$\pi_{x,y}=\frac{\pi_{x,5}\cdot\pi_{y,1}}{\pi_{1,5}}+\frac{\pi_{x,6}\cdot\pi_{y,2}}{\pi_{2,6}}+\frac{\pi_{x,7}\cdot\pi_{y,3}}{\pi_{3,7}}+\frac{\pi_{x,8}\cdot\pi_{y,4}}{\pi_{4,8}}$$

and as a particular case when the two circles (x, y) are replaced by the line at an infinity, we have

$$\frac{1}{\pi_{1,5}}+\frac{1}{\pi_{2,6}}+\frac{1}{\pi_{3,7}}+\frac{1}{\pi_{4,8}}=0.$$

The general theorem is then applied to prove some properties of circles connected with three circles; a formula is given for the radius of a circle which passes through three of the points of intersection of three given circles; the eight circles which can be drawn to touch three circles are shown to be each touched by four of eight other circles, called Dr. Hart's circles, these arrange themselves in pairs; if ρ, ρ' be the radii of a pair of Dr. Hart's circles, and R, R' the radii of the corresponding pair of the eight circles passing through the points of intersection of the given circles, it is shown that

$$\frac{1}{\rho}-\frac{1}{\rho'}=2\left(\frac{1}{R}-\frac{1}{R'}\right).$$

If (1, 2, 3, 4) denote any system of circles not having a common orthogonal circle, then defining the "power-coordinates" of a point as any multiples of its powers with respect to the system of reference (1, 2, 3, 4), it is deduced from the equation

$$\pi\begin{pmatrix}1, & 2, & 3, & 4, & 5\\ 1, & 2, & 3, & 4, & 6\end{pmatrix}=0,$$

that the coordinates of any point must satisfy a non-homogeneous linear relation, and a homogeneous quadric relation called the absolute. Also the equation of the first degree in power-coordinates represents a circle, unless it be satisfied by the coordinates of the line at infinity, and then it represents a straight line.

The equation of the second degree is shown to represent a bicircular quartic, or a circular cubic, some general properties are proved, and then the curves are classified. It is shown that the equation may be reduced to one of the forms

$$ax^2+by^2+cz^2+dw^2=0 \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(A)}$$

the absolute being $x^2+y^2+z^2+w^2=0$;

or $$ax^2+by^2+cz^2=0 \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(B)}$$

$$ax^2+2fyz \quad =0 \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(C)}$$

the absolute being $x^2+y^2-4zw=0$.

The different curves are then discussed in detail, there being nine species in all, three in each group (A), (B), or (C).

Part II contains merely the extension of the results of Part I to spherical geometry; the power of two circles on a sphere is defined to be the product of $\tan r$, $\tan r'$, $\cos \omega$, where r, r', are the radii, ω their angle of intersection; the power of a small circle radius r, and a great circle is, however, defined as $\tan r \cos \omega$; and of two great circles as $\cos \omega$.

The fundamental theorem is as before

$$\pi\begin{pmatrix}1, & 2, & 3, & 4, & 5\\ 6, & 7, & 8, & 9, & 10\end{pmatrix}=0,$$

connecting the powers of two systems of circles.

Consequently the results obtained previously are extended with but slight modification.

In Part III the method of Part I is applied to spheres; it is proved at once that the powers of any systems of spheres must satisfy the relation

$$\pi\begin{pmatrix}1, & 2, & 3, & 4, & 5, & 6\\ 7, & 8, & 9, & 10, & 11, & 12\end{pmatrix}=0,$$

and any of the spheres may be replaced by planes, or the plane at infinity.

Several results obtained in Part I are easily extended, with one exception; there are eight pairs of spheres which touch four given spheres, but except in very special cases no spheres exist analogous to Dr. Hart's circles.

The discussion of the equation of the first degree in power-coordinates is much the same as that in Part I. The reduction, however, of the general equation of the second degree is more complicated; there are four distinct forms to which the equation may be reduced.

$$ax^2+by^2+cz^2+dw^2+ev^2=0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \ (\alpha)$$

the equation of the absolute being

$$x^2+y^2+z^2+w^2+v^2=0;$$

this is the general cyclide, of either the fourth or third order; if $d=e$, it has two cnic-nodes, and if $b=c$, $d=e$, it has four cnic-nodes; but in this case the sphere $x=0$ must be imaginary.

$$ax^2+by^2+cz^2+dw^2=0 \ \ldots\ldots\ldots\ldots\ldots\ldots\ldots \ (\beta)$$

the equation of the absolute being

$$x^2+y^2+z^2-4wv=0.$$

This is the general case of a cyclide having one cnic-node, if $b=c$ it has three nodes; the former case is the inverse of a central quadric, the latter the inverse of a central quadric of revolution: the spheres x, y, z are real in this case.

$$ax^2+by^2+2hzw=0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (\gamma)$$

the equation of the absolute being

$$x^2+y^2+z^2-4wv=0.$$

This represents a cyclide having two principal spheres and a binode; if a or $b=0$ the node is a unode.

$$ax^2+2hyz+dw^2=0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (\delta)$$

the equation of the absolute being

$$x^2+y^2+z^2-4wv=0.$$

This represents a cyclide having only one principal sphere; and a cnic-node; which becomes a binode when $a=0$, and a unode when $h=0$.

The different species of cyclides are then briefly discussed in detail.

III. "Effects of Stress and Magnetisation on the Thermoelectric Quality of Iron." By Professor J. A. Ewing, B.Sc., University College, Dundee. Communicated by Sir William Thomson, F.R.S. Received February 24, 1886.

(Abstract.)

This paper comprises a revised version of one submitted to the Royal Society in 1881, under the title "Effects of Stress on the Thermoelectric Quality of Metals, Part I,"* along with much new matter. It deals principally with the cyclic changes of thermoelectric quality which an iron wire undergoes when exposed to cyclic variations of stress (described in the abstract of the former paper), and with the relations of these changes of thermoelectric quality to the changes of magnetism which also occur as an effect of stress. Stress was applied by exposing the wire to longitudinal pull by means of loads. The changes both of thermoelectric quality and of magnetism exhibit that tendency to lag behind the changes of stress to which in a previous paper† the author gave the name *hysteresis*, and the effects are sufficiently similar in regard to the two qualities to suggest that the changes of thermoelectric quality occur as secondary effects of changes of magnetism. To examine whether this is the case, simultaneous measurements of the magnetic and thermoelectric effects of stress in an iron wire were made, and also independent observations of the thermoelectric effects of magnetisation, without change of stress. A comparison of these made it clear that stress causes change in thermoelectric quality of iron directly, and not as a secondary effect of magnetisation. If the wire be completely demagnetised to begin with, and kept clear of all magnetisation during the application and removal of stress, the presence of hysteresis is not less marked than before. Experiments are given to show how the thermoelectric effects of stress are modified by the existence of more or less magnetisation in the wire; and conversely, how the thermoelectric effects of magnetism are modified by the existence of more or less constant stress. The influence of vibration in destroying the effects of hysteresis is investigated, and also the result of exposing the wire to the process of demagnetising by repeated rapid reversals of a continuously diminishing magnetising force, and it is shown that this process acts in the same way as vibration in destroying the effects of hysteresis. Residual effects of hysteresis are studied, as, for example, the difference which presents itself when a wire is magnetised after having

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* Published in abstract in "Proc. Roy. Soc.," No. 214, 1881.

† "Proc. Roy. Soc.," No. 216, 1881, p. 22.

been loaded strongly and then unloaded down to a certain constant state of stress, and, on the other hand, when the same state of stress has been produced by simply increasing the load ; and it is shown that these residual effects are wiped out by vibration or by demagnetising by reversals. With regard to the effect of stress on thermoelectric quality, it is shown that if a somewhat soft wire be more and more strongly magnetised, these effects become more and more similar to those which are found when the wire is hard drawn, but not magnetised. A few experiments were made with wires of silver, copper, lead, magnesium, and German silver, but in none of these was hysteresis of thermoelectric quality with regard to load discovered.

Special attention is directed to a peculiar feature in the curves by means of which the experimental results are exhibited. In curves showing the relation of thermoelectric electromotive force to load, it is shown that any reversal from loading to unloading, or *vice versâ*, causes an inflection in the curve, the first effect of the new process being to continue the kind of change that was going on before. That this is not due to any mechanical disturbance which the loading or unloading produces, is shown by the fact that it occurs in an equally marked way after the molecules have been brought to a condition of stable equilibrium by vibrating the wire before beginning to load or unload. It is suggested that the effects of hysteresis, described in the paper, have a possible relation to the properties which Professor Osborne Reynolds has recently shown to be possessed by granular media.

The experiments described in the paper are closely connected with those which were communicated in January, 1885, under the title "Experimental Researches in Magnetism," and are now being published by the Society. They were conducted in the Physical Laboratory of the University of Tokio, in 1881–3, partly with the help of one of the author's Japanese students, Mr. P. S. Sakai. The results are given graphically, and are for the most part reduced to absolute measure.

March 18, 1886.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Relationship of the Activity of Vesuvius to Certain Meteorological and Astronomical Phenomena." By Dr. H. J. JOHNSTON-LAVIS. Communicated by Professor JUDD, F.R.S. Received February 26, 1886.

(Abstract).

The determination of the relations, if any such exist, between volcanic activity and certain astronomical or meteorological phenomena, cannot fail to throw much light upon the vexed question of the solid or liquid condition of the earth's interior. M. Perrey, as the result of his careful catalogue of earthquake phenomena, believed himself to have proved that these could be shown to have certain maxima and minima, which correspond with positions of the moon in relation to the earth and sun; there are many considerations which point to the conclusion that great and sudden changes in barometric pressure may be followed by outbursts of volcanic violence; and, finally, if the eruptions of volcanoes, as many geologists believe, are due to water percolating from the surface to a heated magma, rainfall must have no inconsiderable influence in determining the periods of their occurrence.

The author of the paper has made use of the opportunity of a residence in the neighbourhood of Vesuvius, to chronicle, according to a scale devised by himself, the varying quantities of vapour emitted from the crater during its usual quiet and continued (Strombolian) stage of eruption; the period of every new outflow of lava, or of any increase in the flow of lava was also noted. These observations having been carried on daily for a period of one year and nine months —from October, 1883, to June, 1885—were recorded in tabular form side by side with the moon's quadratures and position in her orbit; with these are also arranged the daily records of the height of the

barometer, and the amount of rainfall supplied to the author by Professor Brioschi of the Capodimonte Observatory.

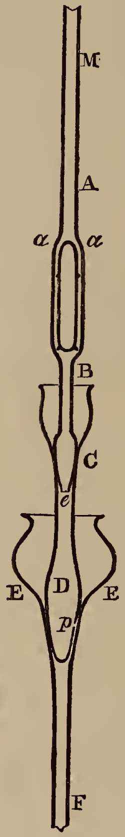
From the discussion of these tables, it is concluded by the author that there is a striking relationship between the curves which mark sudden changes in atmospheric pressure and those which indicate distinct variations in the volcanic activity. As regards the relation of changes in volcanic activity with the lunar positions, the author speaks with greater doubt, the period over which the observations have extended being insufficient to justify definite conclusions; but he believes that his observations point to distinct tidal influences as affecting the liquid magma beneath the volcano.

II. "On an Apparatus for connecting and disconnecting a Receiver under Exhaustion by a Mercurial Pump." By J. T. BOTTOMLEY, M.A., F.R.S.E. Communicated by Sir WILLIAM THOMSON, F.R.S. Received March 1, 1886.

In experimental work with vacua, and especially with the high vacua given by the Sprengel pump, a connecting tap has often been much wished for which would enable the experimenter to remove a piece of apparatus from the pump for examination or preliminary experiment, and afterwards to reapply it to the pump without discharging the vacuum. So far as I am aware nothing satisfactory has hitherto been suggested. The ground glass stopcocks now made by some of the German and English glass workers are undoubtedly very highly finished; but sooner or later, even with the best of them, the air begins to work its way round the grinding marks, in spite of lubricants, and, worse than this, when the apparatus under exhaustion has been removed from the pump and gauges, there is no way of knowing whether or not the air is leaking in round the interstices of the ground glass stopcock.

To meet this difficulty, I have recently constructed a mercurial vacuum tap, which is certainly impervious to air, and which will, I think, be found to work easily and conveniently. In constructing it I have taken advantage of a tap described by Mr. C. H. Gimingham ("Proc. Roy. Soc.," No. 176, 1876), by means of which a piece of apparatus may be disconnected from the pump without discharging the vacuum of the pump; and thus by means of the complete tap, which I proceed to describe, the apparatus under experiment can be separated from the pump and replaced without either the pump or the apparatus being discharged.

The tap consists of three parts. AB is a tube containing a glass float, of which the upper end is conical, and ground very carefully at *aa* to fit a conical opening to the upgoing spirit-bore tube AM; and at M the apparatus which is to be exhausted would be blown on. At C there is an ordinary cup and stopper, ground to a very perfect fit, and the joint at C is made perfectly air-tight in the usual way by pouring mercury into the cup. At the lower extremity of the part CD is a stopper closed at the bottom, but with a fine hole drilled at *p*; and in the tube of the cup EE there is a fine groove cut, which reaches half way up the ground part of the tube and stopper to *p*; but above *p* there is a sufficient length of grinding to make a perfect joint.* When the hole *p* is turned round to meet the groove, there is communication through and through the tap, that is to say, from the



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* This cup and stopper form Mr. Gimingham's ingenious tap.

pump below F to the apparatus attached to M; but when the opening *p* is turned away from the groove the pump is cut off.

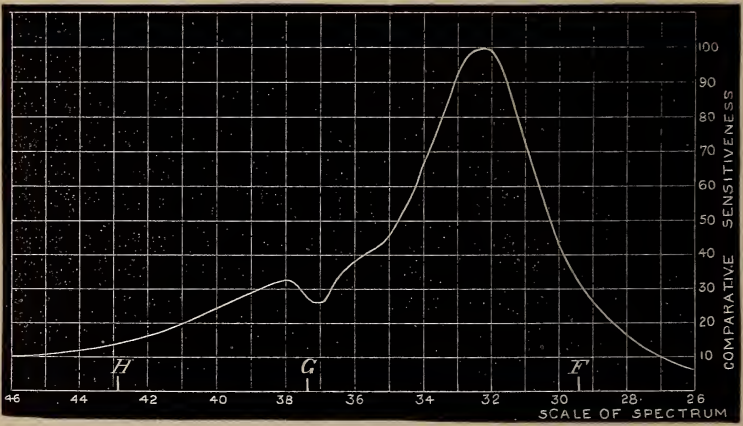
Suppose now that above *p* there is a vacuum, and that *p* is turned round so as to cut off the pump. Let the stopper at C be cautiously raised. Mercury flows from the cup C, and in the first place fills up the space below, and fresh mercury must be supplied to the cup and the supply kept up. The whole of the lower part of the space being filled, the mercury rises in the tube CB, lifts the glass float, and closes the opening *aa* with great pressure. To hold up the stopper at C during the flowing in of the mercury requires considerable force with an opening at C of an ordinary size; but as soon as the whole space from *aa* down to the bottom of D has been filled, the part of this force which is due to air pressure vanishes, and the stopper may be separated from C safely. The mercury in the tube AB does not drop out, as the orifice at *e* is very small; and thus there is nothing to prevent the apparatus under exhaustion being handled in any way that may be desired.

When the apparatus is to be reconnected with the pump, it is only necessary to replace the stopper in the cup C, and turn the hole *p* round to meet its groove. The mercury in the tube AB then drops into the pump. The float falls into its lowest position, and everything is once more as it was before the removal of the apparatus from the pump.

III. "Comparative Effects of different parts of the Spectrum on Silver Salts." By Captain W. de W. Abney, R.E., F.R.S. Received March 2, 1886.

In 1881 I communicated to the Royal Society ("Proc. Roy. Soc.," vol. 33) the results of a research I had made on the comparative effects of different parts of the spectrum on the haloïd salts of silver, and I pointed out that a mixture of iodide and chloride, and iodide and bromide of silver gave rise to a very curious photographic spectrum, a minimum of action taking place at G, the point where the iodide is mostly affected, two maxima consequently occurring. I also gave some theoretical reasons why this should be. About a year afterwards Herr Schumann, of Leipzic, called in question this result, as applied to bromo-iodide of silver, when the two salts were formed simultaneously, *i.e.*, when mixtures in water of soluble bromides and iodides were precipitated together by silver nitrate. He subsequently found that a mixture of the two salts after separate precipitation did give rise to a double maximum. Now my own experiments showed that in either case such double maxima existed, but perhaps they were more marked when the salts were precipitated separately. The only method at that time available to distinguish the maxima was by the appearance of a negative photograph of the spectrum impressed upon it, and hence there was a liability to be deceived, since densities in deposit which are nearly alike are apt to be over looked.

I utilised my method ("Phil. Mag.," 1885) of obtaining patches of monochromatic light from the spectrum, in examining afresh different salts of silver as regards sensitiveness to different rays. The experiments were conducted in the following manner:—A sensitometer, designed by Mr. Spurge, was brought into use ("Photographic Journal," 1882, vol. vi). This consists of a series of small chambers, about 1 cm. square in section, and 2 cm. deep. Below these chambers is a sheet of brass, punctured as shown in the figure, each such puncture corresponding with the square chamber.



Numbers are also punctured in the brass triangle, to correspond to the order of intensity in which the light is admitted to each chamber. Below this brass plate can be placed a sensitive plate to be tested. The tops of the chambers are also closed by a brass plate, in which holes of different diameters are punctured. The area of each hole is $\sqrt[3]{2}$ that of the next, and the total number of chambers is 30. It will be thus seen that the difference in light from an equally illuminated surface admitted to the first and last holes is immense.

To obtain a surface equally illuminated two sheets of finely ground glass were used, one placed about one-eighth of an inch from the holes, and the other about a centimetre away from the first. It was found that when the outside ground glass was illuminated by a candle about 3 feet away, the light shading every part of the bottom of each chamber was for all practical purposes uniform. A patch of monochromatic solar light from one part of the spectrum was then thrown on the ground glass, and an exposure of 30 seconds given to a plate in contact with the brass punctured plate at the bottom of the chambers. Another portion of the spectrum was next thrown on a fresh sensitive surface, and a similar operation carried out, and so on till the whole of the range of the spectrum had been utilised. In each set of experiments it is scarcely needful to remark the same batch of plates was employed. All the plates were developed together for the same length of time, and the number of the chamber noted where no photographic action was visible. Thus if No. 8 showed a trace of photographic action, and No. 9 showed none, No. 9 was taken as a measure. All these numbers were then tabulated, and the admitted light calculated.

Another series of experiments were then conducted precisely as before, the length of exposure being varied, and the numbers observed were again tabulated and compared with the first set. A third series was then taken, and a mean of the results taken. The plates were next fixed and the numbers read, and the light again calculated, with the result that the mean corresponded with the first mean. As a final check, each set of plates were printed on uniformly sensitised paper, and the gradations obtained by the method described in my Treatise on Photography (Longmans). The results obtained were almost identical with the first means. Various salts of silver and combinations of salts were tried, but I need only give one, which is that which has been disputed. The figure gives a graphic description of the results obtained. This series of plates was prepared with a mixture of 6 per cent. of iodide, and 94 per cent. of bromide of silver, and the two were precipitated together. It was somewhat difficult in a photograph of the spectrum, containing but little iodide, to be sure of this dip at G, owing to the occurrence of Fraunhöfer lines. The method adopted brings the dip clearly into view. It might be thought that the strong band of lines near G produced it, but such is not the case, as pure bromide of silver without any admixture of iodide did not show it, and the one maximum of sensitiveness it had lay nearer G.

In the mixed salt which was experimented upon we thus still get two maxima, though the percentage of iodide and bromide is but small. The same line of argument which was applied in the paper I have already referred to as to the cause of this dip near G, still therefore applies.

Table of Intensities.

Scale No.	Intensity.
26	7 ·0
28	14 ·3
30	43 ·0
31	75 ·0
32	100 ·0
33	95 ·0
34	66 ·5
35	43 ·0
36	37 ·5
37	25 ·0
38	33 ·5
39	28 ·5
40	25 ·0
41	20 ·0
42	18 ·0
44	12 ·5
46	10 ·0

IV. "On the Properties of Matter in the Gaseous and Liquid States under various conditions of Temperature and Pressure." By the late THOMAS ANDREWS, M.D., LL.D., F.R.S. Communicated by the PRESIDENT. Received February 7, 1886.

(Abstract.)

The following are the general conclusions to which this inquiry has led:—

1. The law of gaseous mixtures, as enunciated by Dalton, is largely deviated from in the case of mixtures of nitrogen and carbonic acid at high pressures, and is probably only strictly true when applied to mixtures of gases in the so-called perfect state.

2. The critical point of temperature is lowered by admixture with a permanent gas.

3. When carbonic acid gas and nitrogen diffuse into each other at high pressures, the volume of the mixture is increased.

4. In a mixture of liquid carbonic acid and nitrogen at temperatures not greatly below the critical point, the liquid surface loses its curvature, and is effaced by the application of pressure alone, while at lower temperatures the nitrogen is absorbed in the ordinary way, and the curvature of the liquid surface is preserved so long as any portion of the gas is visible.

March 25, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Abstract of Paper upon the Minute Anatomy of the Brachial Plexus." By W. P. Herringham, M.B., M.R.C.P., Communicated by W. S. Savory, F.R.S. Received March 8, 1886.

The paper is based upon 55 dissections, 32 fœtal and 23 adult.

The *posterior thoracic* is formed by the 5th, 6th, and usually the 7th. The 5th supplies the first two digitations, the 6th the next two, the 6th and 7th the lower five, or if there is no 7th, the 5th may supply three, and join the 6th for the remainder.

The *suprascapular* is given off from the 5th, with or without a minute fibre from the 6th.

The *anterior thoracics* are formed usually by the 6th, the 7th, the 8th, and the 9th. The 6th and 7th form the external, supplying the upper part of the pectoralis major, the 7th gives the communicating branch which supplies the middle, and the union of this with the internal from the 8th and 9th, supplies the lower part of the muscle. The minor is supplied by the 7th, 8th, and 9th.

The *coraco brachialis* is supplied by the 7th.

The rest of the *musculo cutaneous* is formed by the 5th and 6th. Both nerves enter the biceps and brachialis anticus. The cutaneous branch is mostly from the 6th, slightly also from the 5th.

The *median* is formed by the 6th, 7th, 8th, and 9th.

The 6th supplies the pronator teres, flexor carpi radialis, superficial thenar muscles, and radial finger, or fingers.

The 7th supplies the flexor sublimis, occasionally the anterior interosseous, the palmar cutaneous, and the finger next the 6th.

The 8th supplies the flexor sublimis, the anterior interosseous, and the fingers inside the 7th.

The 9th supplies the anterior interosseous, and usually ends there.

The *ulnar* is formed by the 8th and 9th ; the muscles in the forearm are supplied by both, those in the hand by the 8th. The 9th supplies the cutaneous branches in front, the 8th the dorsal branch.

The *internal* and *lesser internal cutaneous* are usually supplied by the 9th, the former occasionally by the 8th as well.

The posterior branches:—

The *subscapularis* is supplied by branches from the 5th and 6th only; the *teres major* by the 6th, often with a twig from the 7th; the *latissimus dorsi* by the 7th, often with a twig from the 8th.

The *circumflex* is formed by the 5th and 6th. The latter is not traced to the teres minor. Both go to the deltoid. The cutaneous branch is formed by the 5th alone, or by both.

The *musculospiral* is formed by the 6th, 7th, and 8th; sometimes the 5th, and rarely the 9th, send branches to it.

The triceps is supplied by the 7th and 8th. The long head usually by the 8th, the inner head by the 7th and 8th, and the outer by the 7th. The 6th sometimes runs to the outer head.

The internal cutaneous branch comes from the 8th. The short external cutaneous springs from the 6th, the long varies round the 7th. The brachialis anticus, supinator longus, and supinator brevis are supplied by the 6th.

The extensor carpi radialis longor and brevior are supplied by the 6th or 7th, usually the latter.

The radial is supplied by the 6th alone, or by the 6th and 7th.

The posterior interosseous is usually from the 7th alone, sometimes with aid from the 8th.

The nerves, both sensory and motor, are shown to obey the following law:—

I. *Any given fibre may alter its position relative to the vertebral column, but will maintain its position relative to other fibres.*

An exceptional case is detailed in exemplifying this law.

The muscles are classed in a table, according to their motor nerve supply.

The system of the motor supply appears to be not according to use, but according to position, morphological not functional, and obeys the following law, composed of three rules:—

II. A. *Of two muscles, or of two parts of a muscle, that which is nearer the head end of the body tends to be supplied by the higher, that which is nearer the tail end by the lower nerve.*

B. *Of two muscles, that which is nearer the long axis of the body tends to be supplied by the higher, that which is nearer the periphery by the lower nerve.*

C. *Of two muscles, that which is nearer the surface tends to be supplied by the higher, that which is further from it by the lower nerve.*

These rules are applied in detail.

The system of the sensory supply is examined in detail. It is shown to follow a law composed of two rules :—

III. A. *Of two spots on the skin, that which is nearer the preaxial border tends to be supplied by the higher nerve.*

B. *Of two spots in the preaxial area, the lower tends to be supplied by the lower nerve, and of two spots in the postaxial area, the lower tends to be supplied by the higher nerve.*

It is shown that this is the case with all membranes stretched into a sheath by something pushing out into them, and the epiblastic layer of the epidermis is compared to such a membrane, pushed into a tubal sheath by the developing mesoblast.

A note is added showing that other observers have reached similar results by other methods, and notably that Forgue has formulated laws for the motor nerves of the monkey, identical with those laid down in the present paper.

II. "On the Changes produced by Magnetisation in the Length of Iron Wires under Tension." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Professor F. GUTHRIE, F.R.S. Received March 10, 1886.

In a paper communicated to the Royal Society about a year ago,* I discussed the results of certain experiments made by Joule in relation to "the Effects of Magnetism upon the dimensions of Iron and Steel Bars."†

It is well known that the length of an iron rod is in general slightly increased by magnetisation. Joule enunciated the law that the elongation is proportional in a given bar to the square of the magnetic intensity, and that it ceases to increase after the iron is fully saturated.‡ My own experiments, made with a greater range of magnetising forces and with thinner rods than those used by Joule, show that if the magnetising current is gradually increased after the so-called saturation point of the iron has been reached, the elongation, instead of remaining at a maximum, is diminished, until when the current has attained a certain strength, the original length of the rod is unaltered, and if this strength be exceeded, actual *retraction* is produced.

Joule also found that when the experiment was performed upon an iron wire stretched by a weight, the magnetic extension was in all

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* "On the Changes produced by Magnetisation in the Length of Rods of Iron, Steel, and Nickel." "Proc. Roy. Soc.," vol. 40, p. 109.

† "Phil. Mag." [3], vol. xxx, pp. 76, 225, and the Phys. Soc.'s Reprint of Joule's Scientific Papers, p. 235.

‡ Reprint, pp. 245, 255.

cases diminished, and if the weight were considerable, magnetisation caused retraction instead of elongation. From these facts he appears to have formed the conclusion that, under a certain critical tension, (differing for different specimens of iron, but independent of the magnetising force) magnetisation would produce no effect whatever upon the dimensions of the wire. In one of his experiments* a certain iron wire loaded with a weight of 408 lbs. was found to be slightly elongated when magnetised; the weight was then increased to 740 lbs. with the result that magnetisation was accompanied by a slight retraction. In both cases the magnetising currents varied over a considerable range, and the smaller ones were without any visible effect. Commenting upon these results, Joule conjectured that "with a tension of about 600 lbs. [which number I suppose is selected as being roughly the mean of 408 and 740] the effect on the dimensions of the wire would cease altogether in the limits of the electric currents employed in the above experiments."†

In reference to this surmise‡ I ventured the following remark:—"If he had actually made the experiment, he would perhaps have found that the length of the wire was increased by a weak current, that a current of medium strength would have had no effect whatever, and that one of his stronger currents would have caused the wire to retract." I had, in fact, reason to believe that the effect of tension was to diminish the "critical magnetising force" (which produces maximum elongation) so that the retraction which is found to occur in all iron rods when a sufficient magnetising force is employed, is observed with smaller magnetising currents when the rod is stretched than when it is free,§ but want of suitable apparatus prevented me from submitting this idea to the test of direct experiment.

I have lately modified the instrument, which is described in my former paper, in such a manner that it can be used for observing the effects of magnetisation upon rods and wires under traction. The working part of it is shown in diagrammatic section in the annexed figure. The coil CC contains 876 turns of copper wire, 1·22 mm. in diameter, wound in 12 layers on a brass tube with boxwood ends. To the lower end A of the tube is fitted a brass plug or stopper, having

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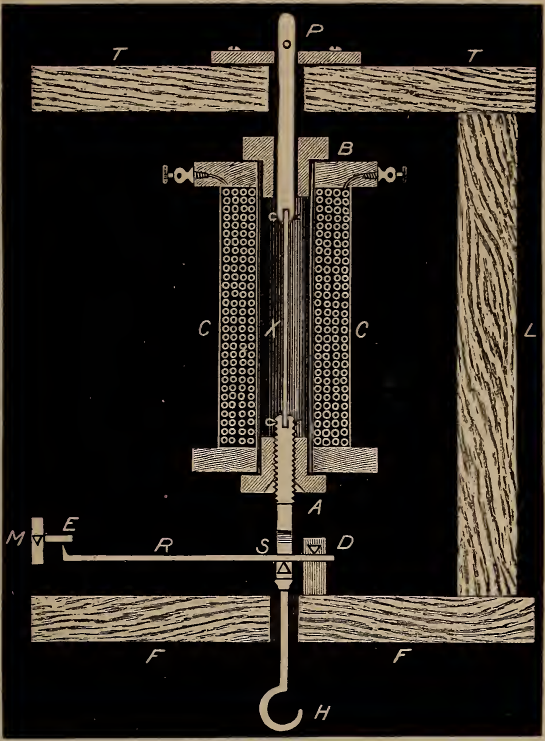
* Reprint, p. 254.

† These currents produced deflections ranging from 6° to 58° on his tangent galvanometer, which "consisted of a circle of thick copper wire one foot in diameter, and a needle half an inch long furnished with an index."

‡ Joule's conjecture is sometimes quoted as if it were an experimental fact. See Chrystal's article on Magnetism, "Enc. Brit.," vol. xv, p. 269.

§ My belief was principally founded upon the fact that while the critical magnetising force appeared in all the cases which I had examined to be about twice that corresponding to the "turning-point" in the magnetisation curve, the turning-point itself occurred at an earlier stage when the wire was stretched than when it was unstretched.



an axial hole drilled through it, which is tapped to receive a screwed brass rod terminating in a stirrup S. The bottom of the stirrup is formed like a knife-edge with the edge uppermost, and beneath it is fixed a hook H, from which weights may be suspended. A second perforated stopper B is fitted to the upper end of the tube; the hole through this is left smooth and freely admits a brass rod, which is suspended by means of a pin at P from a thick brass plate attached to the mahogany table T. The height of P can be varied within small limits by means of a fine screw adjustment, not shown. The table T is attached to the base-board F of the instrument by three stout legs, only one of which, L, appears in the diagram. The wire under experiment, X, is clamped at its two ends between slits in the ends of the brass rods P and S, and thus supports the coil in an upright position. By turning the screwed plug A the position of the wire X may be so adjusted that its middle point shall coincide with that of the axis of the coil.* The knife-edge of the stirrup acts upon the brass lever R, one end of which abuts upon a fixed fulcrum D, while the other

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* Exact coincidence is not essential.

actuates a short arm E attached to the back of a small circular mirror M; the mirror is capable of turning about its horizontal diameter upon knife-edges, resting upon brass planes not shown in the figure. By means of a lantern illuminated by a lime-light, the image of a horizontal wire is, after reflection from a mirror, projected upon a distant vertical scale; a very slight deflection of the mirror causes a considerable movement of the image. The dimensions are as follows:— The distance SD = 10 mm., SE = 170 mm., ME = 7 mm.; the distance from the mirror to the scale = 6400 mm., each scale division = 0·64 mm., and the length of the experimental rod between the clamps = 100 mm. The movement of the focussed image through one scale division therefore indicates a difference of about one five-millionth part* in the length of the rod. The mirror is very accurately worked, and is silvered upon its outer surface; the lens used for the projection is a compound achromatic of high quality, and it is easy to read with accuracy to a half or even a quarter of a scale division.

The magnetising coil is 11·5 cm. long between the boxwood ends; its external diameter is 5·2 cm. and internal diameter 1·9 cm. A current of C ampères produces at its centre a field of about 92 C units.

It will be seen from the above description that the wire under examination sustains the whole weight of the magnetising coil as well as that of the lever R. In order to ascertain the amount of the tension thus produced, the brass rod P was suspended from a hook beneath one pan of a large balance, and it was found that in order to maintain the lever R in a horizontal position it was necessary to place weights amounting to slightly more than 3 lbs. in the other scale pan. In all experiments with this apparatus, therefore, the iron wire is stretched by a minimum initial load of 3 lbs. For some reasons this is a disadvantage, and the arrangement in question was not adopted until many experiments had made it evident that by no other method was it possible to avoid with certainty the source of error introduced by the electromagnetic action commonly known as solenoidal suction between the coil and the wire. When the coil is fixed independently of the wire, the smallest trace of this action produces upon the lever an effect which is enormously exaggerated in the deflection of the image upon the scale. The wire may be placed as accurately as it is possible to do so by measurement, with its middle point in the centre of the coil; but a change in the stretching weight will at once displace it to a small but material extent; and even if the geometrical coincidence were perfect, a slight want of uniformity in the physical qualities of the wire would still render the objectionable action possible. Under ordinary circumstances the

* More exactly 0·00000020588.

disturbance thus introduced would of course be altogether insensible; but in making measurements in which a hundred-thousandth of a millimetre is a considerable quantity, it is far from negligible, as indeed was sufficiently proved by the inconsistency of the results obtained in some of my earlier experiments when the wire was free and the coil attached to the table T.* After the coil had been suspended upon the wire all such inconsistency at once disappeared, for no interaction between the two could then produce any external effect.

Since the apparatus was not calculated to bear any very heavy weight it was necessary to use wires of small sectional area. Thin wires moreover possess an advantage in becoming more strongly magnetised by a given current than thick wires of the same length.

The results of a series of experiments are presented in a synoptical form in the subjoined Table. Four specimens of iron were used. The first was a wire of commercial iron, 1·2 mm. in diameter, which had been softened by heating in a gas flame; the second was a strip of annealed charcoal iron, 5·5 mm. wide and 0·55 mm. thick, its sectional area being about 3 mm.; the third was a piece of hard unannealed wire, 2·6 mm. in diameter; and the last was a wire of very pure soft iron, 3·25 mm. in diameter, which had been carefully annealed. These were successively fixed in the apparatus, and loaded with weights varying from 3 lbs.—that of the coil and lever alone—to a total of 14 lbs. While under the influence of each load, four observations were made in the case of each wire: (1) A determination was attempted of the smallest magnetising current which sensibly affected the length of the wire in the direction of elongation or retraction. (2) The current producing *maximum* elongation (if any), and the extent of such maximum elongation were found. (3) A determination was made of the critical current which was without effect upon the original length of the wire, *i.e.*, the current of such strength that a weaker one would cause elongation and a stronger one retraction. (4) The retraction produced by a fixed current of 1·6 ampère was measured.

The first operation, that of finding the smallest current which produced a sensible deflection, was not easy to perform satisfactorily. Small differences in the disposition of the lever and mirror might

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* The same source of error troubled me much in the experiments described in my former paper until I adopted a similar method of avoiding it. The apparatus used by Joule was far larger, more massive, and presumably less delicate than mine. In the instrument employed in his stretching experiments the lever alone without any additional weight produced a tension in the wire of 80 lbs. Errors arising from solenoidal suction would therefore be less sensible, but it is difficult to believe that some of his results were not affected by them, especially in the case of the experiment (No. 8) on hard steel described at p. 245 of the Reprint, which I believe no one has succeeded in repeating.

Table.

	Commercial iron wire, diam. 1·2 mm.				Charcoal iron strip, section 3 mm.				Hard wire, diam. 2·6 mm.		Soft wire, diam. 3·25 mm.		
Stretching load	3 lbs.	7 lbs.	10 lbs.	14 lbs.	3 lbs.	7 lbs.	10 lbs.	14 lbs.	3 lbs.	14 lbs.	3 lbs.	14 lbs.	
Smallest current producing sensible elongation	0·043	0·064	0·084	..	0·033	0·020	0·029	0·064	0·12	0·15	0·064	0·033	Ampères.
Current producing maximum elongation..............	0·49	0·39	0·23	..	0·44	0·33	0·27	0·15	0·70	0·58	0·70	0·58	
Current by which original length is unaffected.	0·99	0·73	0·47	0·23	1·30	0·99	0·77	0·53	0·99	0·94	1·24	1·09	
Maximum elongation in scale divisions	2	1·5	0·5	..	10	6	4	1	2·5	2·5	6·5	4·5	Scale divisions.
Retraction with current of 1·6 ampère	6	9·5	11	11	9	15	18	20	11	11	8	11	

The magnetic field at the centre of the coil = current × 92.
One scale division = one five-millionth part of the length of the wire.

well cause variations in the readiness with which the arrangement would respond to a movement equivalent to less than one-tenth of a scale division. Nevertheless it is clear in spite of one or two discrepancies that a greater magnetising force is necessary to cause a sensible elongation when the load is great than when it is small. In one case, that of the thin wire under a load of 14 lbs., there was no evidence of any elongation. It is probable, judging by analogy, that a maximum elongation, too small, however, for the instrument to detect, would occur with a current of about 0·12 ampère. Whether any load, however great, would render the preliminary elongation of the wire too small to be measured by an ideally perfect instrument is uncertain.

The second determination could be made with far greater accuracy. But the load had the effect of flattening the apex of the elongation curve in such a manner that the actual maximum was not so sharply defined as in the case of free rods.

The third determination, that of the magnetising current under the influence of which the original length of the wire was unaltered, was susceptible of great accuracy, and was the most important for the purpose of the present investigation.

The measurement of the amount of retraction caused by a given strong current was also perfectly easy and certain.

The figures recorded in the table disclose the following facts:—

1. The effects produced by magnetisation upon the length of an iron wire stretched by a weight, are in general of the same character as those which have been shown in my former paper to occur in the case of a free iron rod. Under the influence of a gradually increasing magnetising force such a wire is at first elongated (unless the load be very great), then it returns to its original length, and finally it contracts.

2. The maximum elongation diminishes as the load increases according to a law which seems to vary with different qualities of iron. If the ratio of the weight to the sectional area of the wire exceeds a certain limit, the maximum elongation (if any) is so small that the instrument fails to detect it.

3. The retraction due to a given magnetising force is greater with heavy than with light loads.

4. Both maximum elongation and neutrality (*i.e.*, absence of both elongation and retraction) occur with smaller magnetising currents when the load is heavy than when it is light; retraction, therefore, begins at an earlier stage. Thus the anticipation expressed in my former paper is justified.

5. The effects both of elongation and of retraction are, as might be expected, greater for thin than for thick wires, and for soft than for hard iron.

Addition, April 3rd, 1886.

It would be difficult for anyone who has not actually seen the apparatus above described to appreciate its extreme delicacy, and the accuracy with which it is capable of measuring such minute quantities as would commonly be regarded as infinitesimal.* It has been suggested to me that greater value would be attached to the experimental results contained in the present and former papers if the manner in which they were arrived at were described in greater detail, and a few of the actual scale readings given in full.

In the case of the twelve series of observations to which the table given in this paper relates, my method of proceeding was as follows:—The iron wire having been placed in position and loaded with a weight, a short time was allowed for the apparatus to attain a nearly steady temperature. The reflected image of the indicating wire (which I will call the index) was then, by means of the fine screw adjustment, brought upon the upper half of the scale, the zero point of which was in the middle, and the index, which, owing to small variations of temperature†, was rarely absolutely at rest, was watched until its upper edge nearly coincided with one of the scale divisions. The number of this division was noted and recorded from my dictation, and at the instant when exact coincidence occurred, a contact key was depressed, which caused a current of 1·6 ampère to pass through the coil. The number of the scale division nearest to which the index was deflected was again noted and recorded as before; and if the point which the index reached happened to be exactly midway between two divisions, the reading was recorded to half a scale division. When the deflections were small, the readings were taken to the nearest half scale division; but this, though easy enough, was in general considered to be a needless refinement.

The next course was to find by a tentative method the strengths of the three currents which respectively produced—(1) the first sensible elongation; (2) the maximum elongation; (3) neither elongation nor retraction; and in order to do this, the resistance in the circuit was varied by means of a large set of coils, and a succession of currents of different strengths (perhaps from twenty to fifty in number, or sometimes even more) were caused to pass through the apparatus.‡

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* The instrument was exhibited in action at the Soirée of the Royal Society, May, 1885.

† Taking the coefficient of expansion of iron to be 0·0000122 per degree C., the heat elongation due to a rise of temperature of one degree would produce a deflection of sixty-one scale divisions; but in addition to the iron there was a somewhat greater length of brass, and if this shared in the heat expansion, a total deflection of more than 150 scale divisions per degree would be produced.

‡ It is of course understood that the circuit was actually closed by the key only

These currents having been determined, the final step was to repeat the first observation of the retraction produced by the fixed current of 1·6 ampère, and thus to check the accuracy of the experiment. The subtractions of the readings were then made, and if there had been a difference of more than one scale division between the preliminary and the final result, it was my intention to repeat the series of observations. This, however, was not found necessary in a single instance. The actual figures as recorded in the note book are given above, the differences being the numbers which appear in the last line of the table in the paper. The agreement between the preliminary and the final readings, when a number of experiments had

Retraction with Current of 1·6 Ampère.*

	Iron wire 1·2 mm.			
Preliminary readings . {	121 127 } 6	96 105 } 9	26 37 } 11	44 55 } 11
Final readings....... {	101 107 } 6	108 118 } 10	22 33 } 11	25 36 } 11

	Iron strip.			
Preliminary readings . {	57 66 } 9	49 64 } 15	78 96 } 18	10 30 } 20
Final readings....... {	61 70 } 9	28 43 } 15	47 65 } 18	−2 18 } 20

	Hard iron.		Soft iron.	
Preliminary readings . {	90 101 } 11	129 140 } 11	60 68 } 8	65 76 } 11
Final readings....... {	92 103 } 11	135 136 } 11	70 78 } 8	36 47 } 11

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at the moment of making an observation, and for a period of not more than half a second at a time.

* In eighteen pairs of observations with iron there was exact agreement sixteen times and a difference of one scale division twice. With nickel the deflections were much larger, sometimes exceeding 100 divisions, and the agreement was not so close; but the discrepancy did not exceed two divisions.

intervened between them, is very remarkable, and could only have been attained, however perfect the instrument, by the method of observation which has been described, unless indeed the readings had been taken to fractions of a scale division.

In the course of my experience in working with the instrument, I have naturally become acquainted with a number of little devices, difficult to describe, which would give me an advantage over a novice in its use. But I believe that any competent manipulator would find it quite easy to obtain uniform and consistent results with it.

The efficiency of the apparatus is due partly to the perfection of the optical arrangements and partly to the fact that in the moving parts unnecessary lightness has not been acquired at the expense of sufficient massiveness and rigidity.

III. "Remarks on the Cloaca and on the Copulatory Organs of the Amniota." By Dr. Gadow. Communicated by Professor M. Foster, Sec. R.S. Received March 11, 1886.

(Abstract.)

The sphincter muscles of the anus of Crocodilia are differentiations of the postpelvic portion of the system of the m. rectus abdominis rather than of the true caudal muscles.

The copulatory muscles of the Carinatæ are derived from the m. sphincter ani solely, whilst in the Ratitæ they are also differentiations of muscles which are still attached to the pelvis, and are, therefore, skeleto-genital.

The mammalian sphincter ani does not take a share in the muscle supply of the copulatory organ, and thus exhibits a difference from Birds and Lizards.

Distinctly copulatory muscles in the Mammalia are derived from skeletal and from non-striped muscles. In this respect the Mammalia agree with Crocodilia and Chelonia.

Then the author describes the nerve-supply of the cloacal region in Crocodilia.

Third Chapter.—The modifications of the cloaca in the various chief groups of Amniota: *Crocodilia*, Lizards, Snakes, *Hatteria*, Birds, Tortoises, Mammals.

Lizards and snakes together represent a special type.

Hatteria comes nearest the Amphibia, or the embryonic condition of Sauropsida; bears, however, resemblance to the Lizards.

Chelonia represent a type somewhat intermediate between that of the Ratitæ and Crocodilia and that of the Monotremata, at the same time bearing slight resemblance to that of the Sauria.

Then the anal sacs or cloacal bladders of the Chelonia are critically discussed with reference to experiments on their being able to take in water.

Then follows a discussion of the peritoneal canals.

The cloacal and copulatory organs of the Chelonia lead with comparatively slight modifications to the Monotremata, from which again a continuity of stages up to the highest Placentalia can be traced.

The whole cloaca of the Amniota consists originally, either permanently or in the embryo only, of three successive chambers which may be distinguished as follows:—

I. The Proctodæum (termed thus by Professor Lankester). It is the outermost anal chamber of epiblastic origin. With its derivatives: (1) Bursa Fabricii in birds; (2) various hedonic glands in most Amniota; (3) the copulatory organs, the at least partly epiblastic nature of which is indicated by the frequently developed horny armament of the glans, by the various sebaceous glands, and as shown in this paper by its development.

II. The Urodæum, from *οὖρον* and *δαιον*. Hypoblastic. This is the middle chamber or primitive cloaca, into which open the urinogenital ducts, and through which pass the fæces. With its differentiations: (1) urinary bladder, ventral; (2) anal sacs in Tortoises, dorsal.

III. The Coprodæum, from *κόπρος* and *δαιον*. This is the innermost cloacal chamber.

The Urodæum is the oldest portion of the whole cloaca, then follows the Proctodæum, and, lastly, the Coprodæum has secondarily assumed cloacal functions.

The various modifications of these three chambers, their function, and the gradual separation of fæces, urine, and genital products have been discussed in the third chapter, and are summarily explained in a table.

A short note on the presence of Muellerian ducts in the males, and of Wolffian ducts in the females of young Crocodilia.

Lastly, general conclusions regarding the phylogenetic development and the homologies of the copulatory organs of the Amniota.

IV. "Electrolytic Conduction in relation to Molecular Composition, Valency and the nature of Chemical Change: being an Attempt to apply a Theory of 'Residual Affinity.'" By Henry E. Armstrong, Ph.D., F.R.S., Professor of Chemistry, City and Guilds of London Central Institution. Received March 11, 1886.

In my recent address to the Chemical Section of the British Association at Aberdeen, I have specially called attention to the "affinity" of *negative* elements—chlorine, oxygen, sulphur, &c.—for negative elements; and I have sought to show that the formation of so-called *molecular compounds* is largely, if not entirely, an outcome of this peculiarity of negative elements. I have also ventured to suggest "that in electrolysing solutions, the friction arising from the attraction of the ions for each other is perhaps diminished, not by the mere mechanical interposition of the *neutral* molecules of the solvent—in the manner suggested by F. Kohlrausch—but by the actual attraction exercised by these molecules upon the negative ion in virtue of the affinities of the negative radicles." In this passage I but vaguely hinted at a modification of the current theory of electrolysis which had occurred to me; as further consideration of the question, especially of Ostwald's electrochemical studies, has strengthened my views, I am led to think that it may be justifiable to submit them for discussion.

It is usual to divide bodies into three classes according to the mode in which they are acted on by an electromotive force: metals forming one class, electrolytes a second, and dielectrics a third. In making this division, perhaps the fact is not sufficiently borne in mind that some compounds—silver chloride, for example—are *per se* electrolytes, while others—such as hydrogen chloride and water—are *individually* dielectrics, but behave as electrolytes when conjoined. On this account, it appears to me desirable to distinguish between—

(*a*) *Metals.*

(*b*) *Simple* electrolytes—compounds, like silver chloride, which in the pure state are electrolytes.

(*c*) *Pseudo*-dielectrics—compounds like water, hydrogen chloride and sulphuric acid, which behave as dielectrics when *pure*, but as electrolytes when mixed with other members of their own class. Conducting mixtures of members of this class may conveniently be termed *composite electrolytes*.

(*d*) Dielectrics.

Simple Electrolytes.

It is undoubtedly a fact that only a limited number of binary compounds are simple electrolytes; and it is especially noteworthy that, with the single doubtful exception of liquefied ammonia, no hydrogen compound—whether binary or of more complex composition—can be classed with the simple electrolytes. Indeed, all the simple electrolytes with which we are acquainted are either compounds such as the *metallic* chlorides, or *metallic* salts—nitrates, sulphates, &c. Including metallic chlorides and their congeners and the corresponding oxides and hydroxides among salts—regarding water as an acid, in fact—and denying the title of salts—hydrogen salts—to the acids, Hittorf's proposition ("Wied. Ann.," 1878, 4, p. 374): "Electrolyte sind Salze" may be safely upheld. But only some of the binary metallic salts are electrolytes: beryllium chloride, for example, belongs to the class of "pseudo-dielectrics" (Nilson and Petterson, "Wied. Ann.," 1878, 4, p. 565; Humpidge, "Phil. Trans.," 1883, p. 604); and in the case of those elements which readily form two classes of salts—so-called *ous* or *proto*-salts and *ic* or *per*-salts, the *ous* compounds alone appear to be electrolytes.

It is highly remarkable that whereas fused silver chloride is easily decomposed on passage of a current of low electromotive force, hydrogen chloride is a "pseudo-dielectric" which forms when coupled with the "pseudo-dielectric" water a readily conducting "composite electrolyte;" while mercuric chloride conducts with great difficulty—possibly not at all when pure—not only in the fused state, but even when coupled with water. No explanation of these facts seems to be afforded by thermochemical data.*

The consideration of these and other similar cases, I think, can

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* The following numbers are given by Thomsen as representing the amounts of heat developed in the formation of the specified chlorides in the state of aggregation in which they exist under ordinary conditions (2 × 35·4 grams of chlorine being in each case used in the production of the chloride):—

Hydrogen chloride	44,000	units (gram ° C.).
Silver ,,	58,760	,,
Mercuric ,,	63,160	,,
Stannic ,,	63,625	,,
Stannous ,,	80,790	,,
Lead ,,	82,770	,,

Only three of the chlorides in this list are simple electrolytes. As much more heat is developed in the formation of two of these three—stannous and lead chlorides—than in the case of any of the others, it would appear probable *à priori* that these would be the most stable; obviously, therefore, the study of the heats of formation throws no light on differences in electrical behaviour such as are manifest between hydrogen, mercuric and stannic chlorides, on the one hand, and silver, stannous and lead chlorides on the other.

but lead to one conclusion: that electrolysability is conditioned both by the nature of the elements in the compound and its molecular structure. I have put forward the hypothesis in my address—"that among metallic compounds, only those are electrolytes which contain more than a single atom of metal in their molecules." The mere presence of two or more associated atoms of metal in the molecule, however, probably does not constitute a compound an electrolyte; and although the hypothesis may be applicable to the majority of simple electrolytes, it certainly does not appear to include all the facts, and it does not serve to explain why certain salts are electrolytes while others are not.

The remarkable difference in the electrical behaviour of two compounds of the same element, such as stannous chloride, in which the ratio of tin to chlorine atoms is as 1 to 2, and stannic chloride, in which $Sn : Cl = 1 : 4$—the one being a simple electrolyte; the other a pseudo-dielectric, if indeed it be not a dielectric—would appear almost to justify the conclusion that in the case of per-salts such as stannic chloride the metal is, as it were, enveloped in a non-conducting sheath of the negative radicle. But whether this be so or not, if—as appears to be the case—all simple electrolytes are *metallic* compounds, and if only proto-salts are electrolytes, may it not be that electric conduction in simple electrolytes is of the nature of ordinary metallic conduction, differing from it only in the circumstance that the compound is decomposed as a consequence of the passage of the current?

This would lead to the conception of an electrolyte as being a metallic compound of such elements, and so constituted, that electric conduction may take place through its mass in a manner similar to that in which it takes place through a mass of metal: in fact through the agency of its metallic atoms. On this view, it is essential that the metallic atoms in the molecules comprising a mass of an electrolyte should be in proximity—as they probably are in proto-salts, but not in many per-salts. The conductivity of two-metal alloys is in many cases much less than that of either of the contained metals: for example, the conductivity of the alloy $SnCu_4$ is about $\frac{1}{4}$th that of tin and about $\frac{1}{30}$th that of copper. The specific conductivity of metals may, therefore, be much reduced by association with one another; and this being the case, it appears probable that the specific conductivity of a metal would be still more reduced by association with a non-metal, and that if the metal were one of low specific conductivity, it might thus practically become altogether deprived of conducting power: perhaps the "exceptional" behaviour of mercuric and beryllium chlorides is to be explained by considerations such as these.

To discuss such questions at all satisfactorily, however, we require to know much more of the electrical behaviour of *pure* fused salts;

it is surprising how little accurate knowledge we possess on this subject.

Composite Electrolytes.

I assume it to be admitted that neither water nor liquid hydrogen chloride, for example, is an electrolyte, although an aqueous solution of hydrogen chloride conducts freely and is electrolysed by an electromotive force of but little more than a volt.

The theory put forward by Clausius in 1857 in explanation of electrolysis is well stated in Clerk Maxwell's "Elementary Treatise on Electricity" (p. 104), in the following words:—

"According to the theory of molecular motion, every molecule of the fluid is moving in an exceedingly irregular manner, being driven first one way and then another by the impacts of other molecules which are also in a state of agitation. The encounters of the molecules take place with various degrees of violence, and it is probable that even at low temperatures some of the encounters are so violent that one or both of the compound molecules are split up into their constituents. Each of these constituent molecules then knocks about among the rest till it meets with another molecule of the opposite kind, and unites with it to form a new molecule of the compound. In every compound, therefore, a certain proportion of the molecules at any instant are broken up into their constituent atoms. Now, Clausius supposes that it is on the constituent molecules in their intervals of freedom that the electromotive force acts, deflecting them slightly from the paths they would otherwise have followed and causing the positive constituents to travel, on the whole, more in the positive than in the negative direction and the negative constituents more in the negative direction than in the positive. The electromotive force, therefore, does not produce the disruptions and reunions of the molecules, but finding these disruptions and reunions already going on, it influences the motions of the constituents during their intervals of freedom. The higher the temperature, the greater the molecular agitation, and the more numerous are the free constituents: hence the conductivity of electrolytes increases as the temperature rises."

This theory has been widely accepted by physicists; but it appears to me that, on careful consideration of the evidence, and especially of recent exact observations on conditions of chemical change, it must be admitted, as I have elsewhere contended (B. A. Address), that proof is altogether wanting of the existence of a condition such as is postulated by Clausius. Moreover, it has been shown by Hittorf that cuprous and silver sulphides, and by F. Kohlrausch that silver iodide, all undergo electrolysis in the *solid* state; the partisans of the dissociation hypothesis would, I presume, scarcely contend that it is easily applicable to such cases as these. It also does not appear to afford any explanation of the *abrupt* change in conductivity which occurs in solid silver iodide and sulphide as the temperature is raised (see p. 280); nor of the peculiar variation in conductivity on diluting sulphuric acid with water (see p. 282).

Again, I venture to think that the conductivity of a *mixture* of compounds which themselves have little or no conducting power is accounted for in but an unsatisfactory and insufficient manner by the hypothesis put forward by F. Kohlrausch ("Pogg. Ann.," 1876, 159, p. 233); there appears to be *far too great a difference* in the behaviour of the pure compounds, water and liquid hydrogen chloride, for example, and of a mixture—no decomposition apparently of either *compound* being effected by any electromotive force short of that which produces disruptive discharge, although the *mixture* of the two will not withstand an electromotive force of little more than a volt. Influenced by these considerations, I am led to conclude that there is no satisfactory evidence that the constituents of the electrolyte are either free prior to the action of the electromotive force, or are primarily set free by the effect produced by the electromotive force upon either member *separately* of the *composite* electrolyte; but that an additional influence comes into play, viz., that of the one member of the composite electrolyte upon the other while both are under the influence of the electromotive force. This influence, I imagine, is exerted by the negative radicle of the one member of the composite electrolyte upon the negative radicle of the other member. Assuming, for example, that in a solution of hydrogen chloride in water the oxygen atom of the water molecule is straining at the chlorine atom of the hydrogen chloride molecule, if when subjected to the influence of an electromotive force the molecules are caused to flow past each other —the phenomena of electric endosmose may be held to afford evidence that in composite electrolytes the molecules are thus set in motion —it is conceivable that this influence, superadded to that of the electromotive force upon the electrolyte, may bring about the disruption of the molecule and conduction: in short, that a state may be induced such as Clausius considers is the state prior to the action of the electromotive force.

A large amount of most valuable information on the connexion of dilution and electrical conduction in aqueous solutions has been recently published by Arrhenius, Bouty, F. Kohlrausch and Ostwald. In his most recent paper, Ostwald ("Journal für praktische Chemie," 1885, 32, p. 300) has given the results of his determinations of the *molecular conductivity* m* in the case of no less than about 120 different

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* $m = kv$, k being the specific conductivity as ordinarily defined, and v the volume of the solution, *i.e.*, the number of litres containing the formula weight in grams of the acid. His results are expressed in arbitrary units.

acids: it appears to me that many—indeed all—of his observations afford most distinct evidence in favour of the view I have expressed above. The general result of his investigation is that the molecular conductivity increases with dilution: in other words, that the dissolved substance exercises a greater specific effect, finally attaining a maximum; it then diminishes, but he believes this to be due to impurities in the water, especially to neutralisation of the acid by traces of ammonium carbonate. The maximum, he appears to think, would be the same for all acids if the dilution could only be pushed far enough: in the case of monobasic acids it is about 90 (arbitrary units); it is twice this in the case of dibasic, thrice in the case of tribasic, and so on.

I will quote first his results in the case of solutions of hydrogen chloride, bromide, iodide, fluoride and silicon fluoride.

Table I.

v.	HCl.	HBr.	HI.	HF.	H_2SiF_6.
2	77·9	80·4	80·4	..	47·81
4	80·9	83·4	83·2	6·54	57·29
8	83·6	85·1	84·9	7·59	62·20
16	85·4	86·6	86·4	10·00	67·08
32	87·0	87·9	87·6	13·14	71·52
64	88·1	88·9	88·7	17·38	75·61
128	88·7	89·4	89·4	23·11	79·22
256	89·2	89·6	89·7	30·30	83·39
512	89·6	89·7	89·7	39·11	91·62
1024	89·5	89·5	89·3	49·49	109·5
2048	89·5	88·9	89·0	59·56	144·0
4096	88·6	87·6	87·8	69·42	187·1
8192	..	..	..	..	226·6
16384	..	..	..	..	258·6
32768	..	..	..	..	282·6

It will be observed that hydrogen chloride, bromide and iodide practically behave alike; the numbers for the chloride are, however, slightly lower than those for the bromide and iodide, and the maximum is not reached quite so soon in the case of the chloride. Hydrogen fluoride is altogether different: its molecular conductivity is exceedingly low to begin with, and is considerably below the maximum even when $v=4096$. But I would call special attention to the numbers for hydrogen silicon fluoride, which is commonly regarded as a dibasic acid: at first, as Ostwald says, it behaves as a monobasic acid of moderate strength—iodic acid, for example; but the maximum for monobasic acids being exceeded, the molecular conductivity increases more and more rapidly, ultimately exceeding the treble value, 270. It must be supposed that it undergoes decomposition in accordance with the equation—

$$H_2SiF_6+2H_2O=SiO_2+6HF.$$

The noteworthy point is *the large excess of water* required to initiate this change: when $v=16$ the solution contains less than 1 per cent. H_2SiF_6, and at this point, according to Ostwald, decomposition probably begins, but that it is far from complete even when a very much larger excess is present is evident from the fact that the maximum when $v = 32,768$ is 282 and not above 400.

Now it is well known that hydrogen chloride, bromide and iodide are, practically speaking, perfect gases under ordinary circumstances: in other words, masses of these gases would mainly consist of molecules such as are represented by the formulæ HCl, HBr and HI. It has been proved, however, by Mallet that hydrogen fluoride at temperatures near to its boiling point mainly consists of molecules of the formula H_2F_2. In the aqueous solution the molecules would be brought more closely together, and therefore it is probable that, even in the case of hydrogen chloride, bromide and iodide, a certain proportion of more complex molecules would result; the relatively high boiling point of hydrogen fluoride (19·4°) renders it probable that in the liquid state this compound would at least partially consist of molecules more complex even than is represented by the formula H_2F_2. On the hypothesis put forward in this paper, the influence exercised by the one member of the composite electrolyte upon the other member during electrolysis is at all events mainly exercised by their respective negative radicles, and the extent of the influence thus mutually exerted by these radicles would depend on the extent to which they are still possessed of "residual affinity." If the hydrogen chloride, bromide and iodide are present chiefly as simple molecules, they should exert, *ab initio*, almost the full effect which they are capable of exerting; and the chief effect of dilution being to decompose the more complex molecules, conductivity should increase to but a slight extent if the extent to which simplification can take place be but small. On the other hand, if owing to the formation of molecular aggregates the residual affinity be more or less exhausted, the initial conductivity will be low, and it will increase on dilution only in proportion as these aggregates become broken up.

It appears to me that the behaviour of the four hydrides under discussion is absolutely in accordance with these requirements of the hypothesis. Even the difference between hydrogen chloride and hydrogen bromide and iodide is not without its bearing. The determinations of the density of chlorine at various temperatures by Ludwig, and of the density of bromine by Jahn ("Monatshefte für Chemie," 1882, p. 176), have shown that it is necessary to raise the temperature considerably higher above the boiling point in order to reduce the density to the theoretical value in the case of chlorine than in the case of bromine: in other words, there is a greater tendency in chlorine to form aggregates more complex than those of the formula

Cl_2 than there is in bromine to form aggregates more complex than those of the formula Br_2. It is, therefore, not unlikely that the chlorine in HCl has more "residual affinity" than the bromine in HBr,* and if so the aqueous solution of the former would have a lower initial conductivity than one of equivalent strength of hydrogen bromide, and the maximum would be obtained only on greater dilution; which is precisely the case.

The evidence afforded by the oxy-acids derived from the halogens appears to me to be equally striking. The following are Ostwald's numbers, including those for nitric acid.

Table II.

v.	Nitric acid.	Chloric acid.	Perchloric acid.	Bromic acid.	Iodic acid.	Periodic acid.
2	77·9	77·9	79·1	..	42·57	
4	80·4	80·2	82·2	..	50·56	23·71
8	82·8	82·3	84·6	..	59·00	30·59
16	84·9	84·0	86·2	..	66·3	39·49
32	86·3	85·3	88·1	79·4	72·3	49·23
64	87·4	86·4	89·2	81·7	76·9	59·48
128	88·2	87·9	89·7	84·1	80·2	69·06
256	88·4	88·7	89·9	86·1	81·8	76·70
512	88·8	88·7	89·8	87·4	83·0	82·59
1024	88·9	88·6	89·8	88·4	83·1	85·38
2048	88·2	87·3	89·3	89·0	82·9	87·95
4096	86·6	85·7	87·8	88·8	81·8	86·62

It will be observed that nitric, chloric and perchloric acids differ but little; that bromic acid has a considerably lower initial conductivity, and does not attain the maximum so soon; that iodic acid differs still more; and that the behaviour of periodic acid is

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* On other grounds also there is reason to believe that hydrogen chloride differs more from hydrogen bromide or iodide than either of these differs from the other: thus less heat is developed on dissolving hydrogen chloride in water than on dissolution of equivalent quantities of the bromide or iodide, the numbers given by Thomsen being—

HCl, $400H_2O = 17{,}300$ units.
HBr, $400H_2O = 19{,}200$ „
HI, $400H_2O = 19{,}200$ „

The solution of hydrogen chloride which distils unchanged at 112° at the ordinary pressure has approximately the composition represented by the formula $HCl \cdot 8H_2O$; whereas the corresponding solutions of hydrogen bromide and iodide boil at 125° and 127°, and their composition is approximately represented by the formulæ $HBr \cdot 5H_2O$ and $HI \cdot 5{\cdot}5H_2O$. A solution of hydrogen fluoride approximately of the composition $HF \cdot 2H_2O$ distils unchanged at 120°.

altogether peculiar—being that of a polybasic acid, it may be added. Ostwald regards it as most surprising—"*in hohem Grade befremdlich*" —that periodic acid should be much weaker than iodic acid, and that the latter should be considerably inferior to iodhydric acid. To my mind, their behaviour is absolutely what might be expected of these acids. Although the molecules in liquid nitric, chloric and perchloric acids are probably not of the simple composition represented by the formulæ HNO_3, $HClO_3$, and $HClO_4$ respectively, the chemical behaviour of these acids does not indicate any great difference between them; owing, however, to the accumulation of oxygen atoms, perchloric acid may be expected to exercise a somewhat greater influence than chloric acid, as it actually does. Chemists are agreed that bromine has less affinity for oxygen than chlorine; hence it may be inferred that the oxygen in bromic acid would have greater residual affinity than the oxygen in chloric acid, and that, therefore, bromic acid would form complex aggregates more readily than chloric acid, and consequently have less influence in electrolysis than chloric acid. This is true in a much greater degree of iodic, and still more of periodic acid:* it is well known that the former not only yields salts of the type $M'IO_3$, but also acid salts such as KHI_2O_6; and that periodic acid forms a series of very complex salts.

The acids of phosphorus form another interesting series:—

Table III.

v.	H_3PO_2.	H_3PO_3.	H_3PO_4.
2	30·89	28·63	14·22
4	37·91	34·29	17·00
8	45·81	41·14	21·26
16	54·13	49·09	27·09
32	62·10	56·96	34·41
64	69·06	64·52	43·05
128	74·05	70·21	53·11
256	77·84	74·54	61·8
512	79·92	77·57	69·9
1024	81·00	79·11	75·4
2048	81·39	79·75	79·0
4096	80·48	79·07	79·8

These numbers afford to my mind the clearest possible evidence that we are dealing with complex molecules. It is especially note-

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* The existence of a stable oxide of the formula I_2O_5, as well as thermochemical data, have been interpreted as evidence that iodine has a greater affinity for oxygen than even chlorine. I am inclined to take the contrary view, however, and to regard the stability of the oxide I_2O_5 as due less to the high affinity of iodine for oxygen than to its low affinity for itself and the high affinity of oxygen for oxygen.

worthy that the maximum never exceeds that of the monobasic acids[*] even in the case of phosphoric acid, which is universally regarded as a tribasic acid, and that the *monobasic* hypophosphorus acid is the strongest and the *tribasic* phosphoric acid is the weakest. In very dilute solution phosphoric acid has less influence than even acetic acid, according to Kohlrausch.

It may be well also to quote Ostwald's numbers for sulphurous, selenious, sulphuric and selenic acids.

Table IV.

v.	Sulphurous acid.	Selenious acid.	Sulphuric acid.	Selenic acid.
2	..	7·63	92·7	97·3
4	19·19	9·73	96·4	103·2
8	25·43	12·70	100·6	109·9
16	32·79	16·60	107·4	117·7
32	41·60	21·73	116·3	127·0
64	50·1	28·24	127·3	138·3
128	58·9	36·15	139·2	148·7
256	66·5	45·11	150·6	157·9
512	72·5	54·27	160·9	164·4
1024	77·1	62·79	169·1	169·7
2048	80·4	69·40	174·4	173·4
4096	83·6	73·58	177·1	174·4
8192	..	..	176·9	173·4

It will be observed that sulphuric and selenic acids are nearly alike in behaviour, the latter being somewhat more active in concentrated solutions; it is noteworthy that of all the polybasic acids studied by Ostwald, these are the only two containing a *single* negative radicle (SO_4,SeO_4) which exhibit a conductivity in excess of that which characterises the monobasic acids.[†]

The numbers obtained for sulphurous and selenious acids are deserving of study. Sulphur dioxide is far from being a perfect gas under ordinary conditions; in the liquid state it is probably rich in

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[*] Ostwald appears to be of the opinion that if the dilution could be carried far enough, a maximum conductivity $= n \cdot 90$ would eventually be attained in the case of every *n*-basic acid. It appears to me that neither do his numbers warrant this—and those here under discussion are an especially good illustration—nor is it likely to be the case on my hypothesis.

[†] Ostwald infers from the great increase in molecular conductivity that the manner in which the acid is electrolysed varies with the strength of the solution; he supposes that in more concentrated solutions sulphuric acid is resolved into H and HSO_4, and that both atoms of hydrogen are split off only as the solution becomes more diluted. This appears to me to be altogether improbable.

aggregates of SO_2 molecules, and these may be to a large extent conserved in concentrated aqueous solutions. But the main explanation of the variation in conductivity on dilution must be looked for, I think, in the peculiar relation which sulphur dioxide manifests to water; it is more than probable that the initial interaction involves the formation of a *hydrate*, $(SO_2)_x(OH_2)_y$, and that from this on dilution is formed sulphurous acid, $SO(OH)_2$, and perhaps also "sulphonic acid," $H{\cdot}SO_3H$. Taking into account the properties of selenious oxide, Ostwald's results appear to me in this case again to lead to but the one conclusion, that conductivity increases in consequence of the specific influence of the fundamental molecule of the compound making itself more and more felt as by dilution it becomes more and more disentangled from its fellows.

The behaviour of solutions of neutral metallic salts on dilution is very similar to that of acids; abundant proof of this is afforded especially by F. Kohlrausch's refined measurements, of which an account has recently been published ("Wied. Ann.," 1886, 26, p. 162). I venture to think that a similar explanation to that above given for oxides will apply to salts; and also that the low molecular conductivities of salts as compared with corresponding acids may be regarded as confirmatory of my hypothesis. I think we must admit that the metals generally have less affinity than hydrogen for negative radicles; if this be granted, we have at once an explanation of the fact that metallic salts are mostly fixed solids, few of which are more than moderately soluble in water while many are very difficultly soluble or insoluble, whereas the corresponding acids are mostly volatile and readily soluble in water, if not miscible with it in all proportions. The affinity of the negative radicles being less exhausted by union with metals than with hydrogen, the fundamental molecules of salts are more prone to unite together to form complex aggregates.

Arrhenius, who has studied the electrical behaviour of solutions of a number of salts,* attributes the change observed in molecular conductivity on dilution—as I have done—to molecular changes; but his deductions are all based on the acceptance of the Williamson-Clausius hypothesis of dissociation.

My hypothesis would also account for the increase in conductivity

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* "Bihang till Kongl. Svenska Vetenskaps-Akademiens Handlingar." Attonde Bandet. Häfte 2. Stockholm, 1884. Arrhenius, S.: "Recherches sur la Conductibilité Galvanique des Électrolytes. I. La Conductibilité Galvanique des Solutions Aqueuses extrêmement diluées, determinée au moyen des Depolarisateurs." 63 pp. II. "Théorie Chimique des Électrolytes." 89 pp. Although aware of his work from Ostwald's reference to it, I was unable to study his memoir until after this paper had been elaborated. Ostwald's quotations, moreover, did not enable me to realise the importance which Arrhenius attaches to the occurrence of molecular simplification and changes in composition on dilution.

in composite electrolytes with rise of temperature. It is true that as temperature rises the influence which individual molecules exert upon each other would be lessened; but on the other hand, the complex aggregates would become more and more completely resolved into their fundamental molecules, the velocity of molecular motion would increase, and the tendency of the constituent atoms to remain united would be lessened. From this point of view the determination of the coefficient of change of conductivity with temperature in the case of substances whose molecular conductivity increases considerably on dilution in comparison with allied compounds which exhibit only a slight variation in molecular conductivity on dilution affords an interesting subject for investigation. F. Kohlrausch has already pointed out ("Pogg. Ann.," 1875, 154, p. 236) that in the case of all neutral salts, "der Einfluss der Temperatur auf das Leitungsvermögen mit wachsender Verdünnung sich Anfangswerthen nähert, die zwischen engen Gränzenliegen," and the experiments of F. Kohlrausch and Nippoldt on solutions of sulphuric acid (*ibid.*, 1869, 138, p. 286) show that the resistance diminishes to a much greater extent forequal increments of temperature in concentrated than in dilute solutions. Thus:—

Table V.

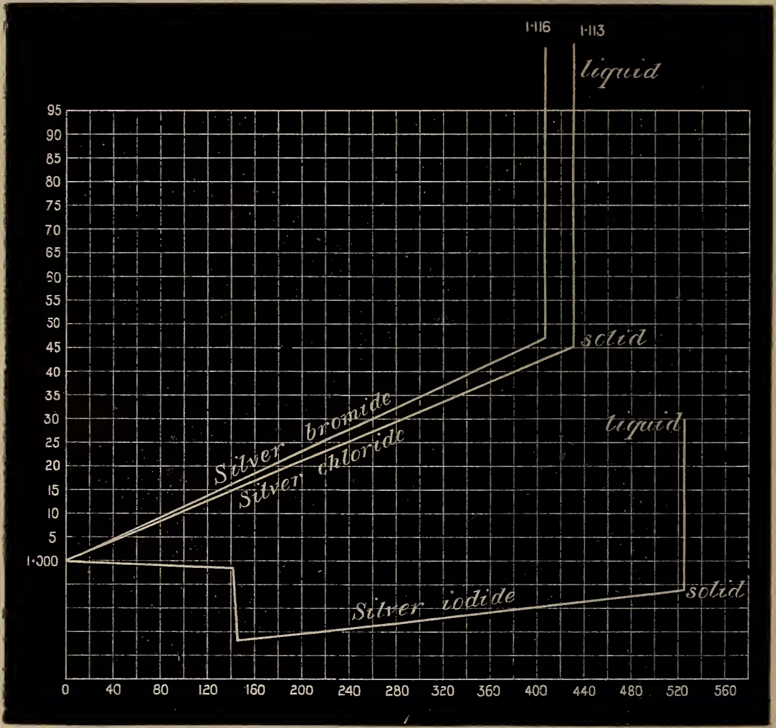
Percentage of sulphuric acid.	Resistance (Mercury=1.)	Percentage increment of conductivity for 1° C.
0·2	465,100	0·47
8·3	34,530	0·653
14·2	18,946	0·646
20·2	14,990	0·799
28·0	13,133	1·317
35·2	13,132	1·259
41·5	14,286	1·410
46·0	15,762	1·674
50·4	17,726	1·582
55·2	20,796	1·417
60·3	25,574	1·794

As concentrated solutions would be richer in complex aggregates than dilute solutions, these results are in entire accordance with my hypothesis: it does not appear to me that they can be satisfactorily interpreted in terms of the dissociation hypothesis.

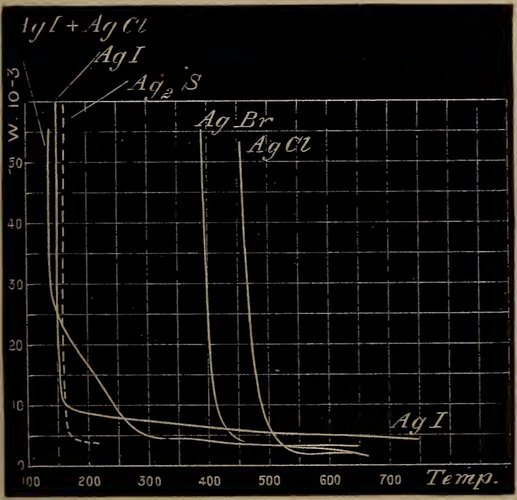
In cases where the influence of the one member of the composite electrolyte upon the other is but slight, it may happen that the effect of temperature in diminishing this influence will outweigh that due to molecular simplification, and that, in consequence, conductivity will diminish with rise of temperature; a mixture of alcohol and ether would appear to furnish an example of this kind; according to Pfeiffer's recent observations ("Wied. Ann.," 1886, 26, p. 226), such a mixture behaves as a metallic conductor of very high resistance.

The increase in conductivity of graphite and gas-retort carbon on heating, and the effect of light on the conductivity of (? impure) selenium and some other substances (Shelford Bidwell, "Phys. Soc. Proc.," 7, p. 129, 256), appear to me to be also explicable on the assumption that in all these cases we are dealing with composite electrolytes.

If any further proof be needed of an intimate connexion between molecular composition and electrolytic conduction, it is most conclusively afforded, I think, by the observations of W. Kohlrausch on chloride, bromide and iodide of silver ("Wied. Ann.," 1882, 17, p. 642), which are exhibited in the accompanying curves. In the fused state, these compounds are better conductors than the most highly-conducting mixture of sulphuric acid and water, which of all liquids



is the best conductor at ordinary temperatures. On reference to the curves, it will be seen that the resistance of both silver chloride and bromide suddenly increases when the change from the fused to the solid state sets in; but that no such change takes place in the case of the iodide. Silver iodide fuses at 527° according to Rodwell, but at about 540° according to Kohlrausch; its electrical resistance increases only gradually after it has become solid, and remains almost a linear function of the temperature during an interval of 400°, until suddenly at near 150° it increases enormously, this change taking place at the moment when according to Rodwell ("Phil. Trans.," 1882, p. 1133) it passes from the transparent, plastic, amorphous solid to the opaque, brittle, crystalline state, the volume increasing considerably as shown by the annexed curve. Kohlrausch has proved most conclusively that



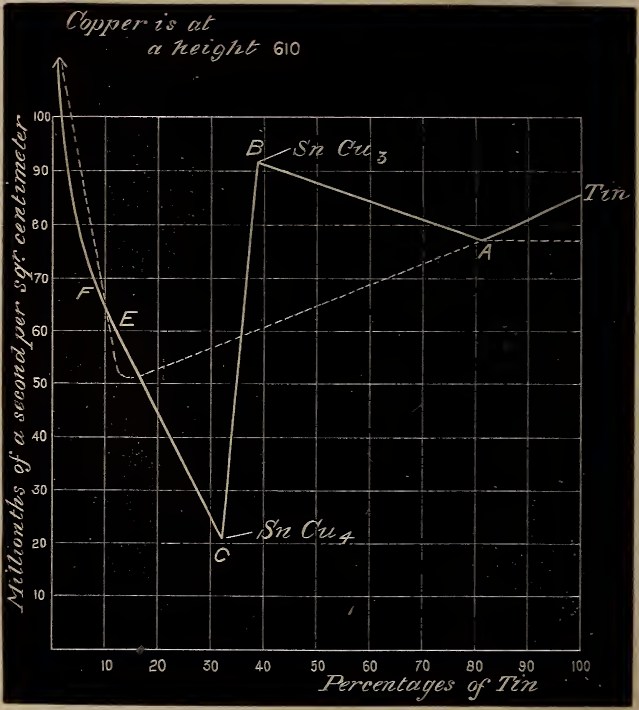
the solid iodide may undergo electrolysis. It would seem that almost immediately after solidification in the case of silver chloride and bromide practically the whole mass consists of complex aggregates so constituted as to be exceedingly bad conductors, but that such aggregates are formed much less readily by silver iodide.

Metallic Conduction.

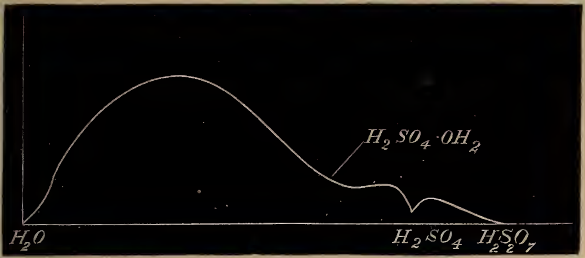
I do not propose in any way to discuss metallic conduction, but merely to call attention to some of the analogies between it and electrolytic conduction.

It is conceivable, and it would appear probable from the fairly regular manner in which the electrical resistance of most pure metals decreases as the temperature falls, the coefficients of change being practically very nearly the same in all cases, that the increase in resistance as temperature rises is mainly due to the increase in molecular inter-distances. As a rule, resistance increases on the passage of a metal from the solid to the liquid state, but there are noteworthy exceptions from which it would appear probable that even in pure metals conductivity to some extent depends on molecular composition: thus the conductivity of bismuth increases at the moment of fusion from 0·43 to 0·73 of that of mercury at 21°, and that of antimony from 0·59 to 0·84 (L. de la Rive, "Compt. rend.," 1863, 57, p. 691); it is well known that bismuth contracts considerably on fusion, and this is probably also the case with antimony. Again, according to Bouty and Cailletet (*ibid.*, 1885, 100, p. 1188), the resistance of mercury decreases at the point of solidification in the ratio 4·08 : 1; this is a remarkable increase in conductivity, and it is difficult to believe that it is wholly due to mere contraction of volume.

That the behaviour of alloys is worthy of far more attention than it has hitherto received appears most clearly from the few data at disposal. As being the most instructive instance, I append the curve



given by Professor Lodge as representing the specific conductivities of the copper-tin alloys ("Phys. Soc. Proc.," 1879–80, 3, p. 158). He examined five alloys, containing respectively 80·8, 38·2, 31·7, 12·6, and 9·7 per cent. by weight of tin, which were prepared by Professor Chandler Roberts; the dotted curve represents the results obtained by Matthiessen, who did not examine any alloy between those containing 16·4 and 85·1 volume per cent. of copper. The comparison of Professor Lodge's curve with that given by F. Kohlrausch for mixtures of sulphuric acid and water—which I also append—appears to



me to be in the highest degree suggestive. In the case of the latter, it will be observed that, starting from SO_3 on the one side and H_2O on the other, minima occur at points on the curve corresponding to compounds of the formula H_2SO_4 and $H_2SO_4 \cdot H_2O$; it is, however, well known that such compounds are unobtainable at ordinary temperatures, and it is highly probable that if the *pure* compounds could be examined, the minima would touch the base line, as in the case of water.* The point of maximum conductivity does not correspond to any known hydrate, but as I have elsewhere remarked it is almost coincident with that of maximum heat evolution on mixing sulphuric acid and water, and it is therefore doubtless the point at which the maximum chemical change occurs. On reference to the alloy curve, it is seen that the addition of quite a small amount of tin to copper produces a very marked effect just as does the addition of a small amount of water to sulphuric acid, the effect being, however, to diminish conductivity in the one case but to increase it in the other; after the addition of only a moderate amount of tin, a

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* The acids richer than H_2SO_4 in SO_3 have been examined by W. Kohlrausch ("Wied. Ann.," 1882, xvii, p. 69). It is especially noteworthy, as I said when reading this paper, that the hydrate $H_2S_2O_7$ is a much worse conductor than either of the hydrates H_2SO_4 or $H_2SO_4 \cdot OH_2$, and that the former of these conducts less readily than the latter, and in this connexion to remember that the compound $H_2S_2O_7$ is the most definite and easily obtained in a crystalline shape, and that the hydrate $H_2SO_4 \cdot OH_2$ is the least definite of the three: the evidence that conductivity depends on absence of homogeneity is overwhelming in this case.—[May 26, 1886.]

pronounced minimum is reached, corresponding to the pronounced maximum attained on addition of a moderate amount of water to sulphuric acid, and at a point moreover corresponding to a definite compound, $SnCu_4$; a further slight addition of tin develops another minimum less pronounced than the first, but also corresponding to a definite compound, $SnCu_3$; the curve then falls slightly and exhibits a third minimum, its course being analogous to that of the sulphuric acid and water curve near to sulphuric acid. If one or the other curve be inverted, the general similarity in form is especially striking. It is obviously important that alloys intermediate between those studied should be examined; a comparison of Lodge's with Matthiessen's curve shows how much may be missed; this remark applies to alloys generally. Whatever may be the explanation,* it appears to me to be clear than in alloys as in composite electrolytes the constituent members of the system influence each other, and thus mutually contribute to the final result. The marked diminution in the conductivity of copper produced by very small quantities of oxygen, of phosphorus or of the metalloid arsenic is well known. It would appear probable that this is in some way due to the occurrence of an electrolytic change, which at least in part is opposed in direction to that taking place in the pure metal during conduction.†

Valency—Chemical Change.

Notwithstanding the fierce controversy which has been waged between the advocates of the doctrine of fixed valency and the advocates of the doctrine of varying valency, our views on the subject are still in an unfortunate degree unsatisfactory and indefinite. Even those—and they probably form a large majority—who regard valency as a variable, dependent both upon the nature of the associated radicles and the conditions—especially as to temperature—under which these are placed, often hesitate to attribute a valency sufficiently high to account for every case of combination; in fact both parties agree in distinguishing "atomic" from "molecular" compounds, and differ only as to where the line shall be drawn.

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* It is very remarkable that not only do the heat conductivity and the induction balance curves for the tin-copper alloys correspond (Chandler Roberts, "Phys. Soc. Proc.," 3, p. 156), but that the curves given by Thurston as representing the strength of these alloys ("Materials of Engineering," Part III, p. 412) also exhibit a marked similarity to the electrical conductivity curves.

† The change produced in gold by a very small quantity of lead is most astonishing: its conductivity is reduced almost to that of lead and it becomes as brittle as glass. It is difficult to understand this change unless it be that opportunity is given for the gold itself to assume a different molecular state, owing to continuity becoming disturbed. The effect produced appears to be strictly comparable with that observed on lowering the temperature of silver iodide from above about 150°, and in the passage of liquid water at 0° into ice.

It is difficult to over-estimate the importance of the theory of valency: its application has led to an enormous extension of our knowledge of carbon compounds especially, and it has furnished us with a simple and consistent system of classifying the mighty host of these bodies; but on the other hand, it may be questioned whether it has not led us away from the search into the nature of chemical change, and even if the introduction of the terms saturated and unsaturated has not had a directly pernicious effect. The almost universal disregard of molecular composition as an important factor in chemical change in the case of solids and liquids, and the popular tendency to overlook the fact that our formulæ of such bodies are purely empirical expressions, has undoubtedly exercised a prejudicial influence.

No known compounds *are saturated*—if any were, such would be incapable, I imagine, of directly taking part in any interaction, and in their case decomposition would necessarily be a precedent change. The paraffins are apparently of all bodies the most inert and the most nearly saturated, and next to them comes hydrogen—the unsaturated character of which is displayed in interactions such as occur at atmospheric temperatures between it and platinum and palladium, and when it displaces silver from silver nitrate or certain of the platinum metals from their salts. One of the most striking instances perhaps of popular error in this respect is water, which is always regarded as a saturated compound, although its entire behaviour and especially its physical properties characterise the molecule H_2O, I think, as that of an eminently unsaturated compound: I fail to see how otherwise we are to explain the high surface tension and high specific heat of liquid water, its high heat of vaporisation, and its imperfectly gaseous behaviour up to temperatures considerably above its boiling point, let alone its great solvent power and its tendency to form hydrates with a multitude of compounds—*especially oxygenated* compounds, be it added.

The theory was brought most prominently under the notice of chemists by Helmholtz in the last Faraday lecture that electricity, like matter, is as it were atomic, and that each unit of affinity or valency in our compounds is associated with an equivalent of electricity—positive or negative; that the atoms cling to their electric charges and that these charges cling to each other. Thus barely stated, this theory does not appear to take into account the fact that the *fundamental* molecules even of so-called atomic compounds *are never saturated*, but more or less readily unite with other molecules to form molecular compounds—molecular aggregates; and unless the application of the theory to explain the existence of such compounds can be made clear, chemists must, I think, decline to accept it.*

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* It is noteworthy that Clerk Maxwell ("Electricity and Magnetism," 1873, vol. i, p. 313), when speaking of the theory of molecular charges, says, "This theory of

There is, however, a most significant passage in Helmholtz' paper, in which it is pointed out that (in a Daniell's cell) the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom, with different force: are we to seek for an explanation in this direction? The impression which the facts make upon the mind of the chemist certainly is, 1, that no two different atoms have equivalent affinities; and, 2, that affinity is a variable depending on the nature of the associated elements: but owing to the recognised complexity of nearly all cases of chemical change, it is difficult to draw any very definite conclusion on this point.

If, however, the nature and properties of so-called molecular compounds generally be considered, and if an attempt be made to form any conception of their constitution, one striking fact is noticeable, viz., that the *metals* in them apparently retain the properties which they exhibited in the parent atomic compounds. Every one knows the marked difference in properties of ferrous as contrasted with ferric salts: they differ not only in chemical behaviour, but also in their physical properties, and are readily distinguishable by their colour. The properties of the ferrous molecular compounds, however, are those of the simple ferrous compounds: ferrous potassium chloride, for example, $Fe_2Cl_4{\cdot}Cl_2K_2$, is a green salt much like ferrous sulphate. Facts such as these have led me to suggest that in such cases the formation of the molecular compound is due to the attraction of the negative element of the one "atomic" compound by the negative element of the other, the metal having no influence except that the amount of affinity of which the negative element is possessed depends on the nature of the metal with which it is associated. It would in fact appear that hydrogen and the metals generally may be regarded as the analogues of the C_nH_{2n+1} and C_nH_{2n-7} hydrocarbon radicles, and that their compounds with negative elements may be likened to unsaturated hydrocarbons of the form $C_nH_{2n+1}{\cdot}CH{\cdot}CH_2$. We know that whenever such a compound enters into combination, the C_nH_{2n+1} radicle takes no part in the change, combination of whatever kind being effected by means of the unsaturated radicle $CH{\cdot}CH_2$ with which it is associated. I do not

molecular charges—he uses the expression molecular in the sense that the chemist uses the term atomic—may serve as a method by which we may remember a good many facts about electrolysis. It is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric currents, and so become independent of these provisional theories." And later (p. 315): "While electrolysis fully establishes the close relationship between electrical phenomena and those of chemical combination, the fact that every chemical compound is not an electrolyte shows that chemical combination is a process of a higher order of complexity than any purely electrical phenomenon."

mean to contend that the metals are fully neutralised in their compounds, but merely that as a rule they behave as though they were saturated just as do the C_nH_{2n-7} radicles derived from the benzenes. There can be little doubt that an absolute distinction must be drawn between hydrogen and the metals on the one hand, and the non-metals on the other. Regarding the facts in the light of our knowledge of carbon compounds, it is difficult to resist the conclusion that the differences observed are due to differences in structure of the stuffs of which the elements as we know them are composed, the which differences condition perhaps a different distribution of the electric charge or its equivalent, in the case of each element.

In the earlier part of this paper I have ascribed the influence which the one set of molecules of the composite electrolyte exercise upon the other during electrolysis to the existence of "residual affinity." I believe this view also to apply to the explanation of the occurrence of chemical change. To quote the words of Arrhenius, "L'activité électrolytique se confonde avec l'activité chimique." Several pregnant examples of this have already been given by Ostwald (" J. pr. Chem.," 1884, 30, p. 93).

The investigation of the nature of chemical change has assumed an altogether different aspect since the publication of Mr. H. B. Dixon's inquiry into the conditions of chemical change in gases ("Phil Trans.," 1884, p. 617; see also " Chem. Soc. Trans.," 1886, p. 94). Mr. Dixon has clearly proved that it is impossible to explode a mixture of carbonic oxide and oxygen, and that the change $2CO + O_2 = 2CO_2$ is effected when sparks are passed across the tube containing the gaseous mixture *only in the path of the discharge.* If traces of water be present *explosion* takes place, the velocity of propagation of the explosive wave increasing with the amount of water up to a certain maximum. These results completely dispose of the popular explanation of such changes, viz., that the molecules in the path of the discharge undergo *dissociation,* that the dissimilar atoms thus liberated then combine together, and that as the heat developed in their union causes the dissociation of yet other molecules, change gradually extends throughout the mass.* Mr. Dixon's experiments have not only shown that the propagation of change is dependent on the presence of a third body, but that this third body must bear a certain relation to those with which it is associated; CO_2, CS_2, C_2N_2, CCl_4, SO_2, and N_2O were found by him to have no action, and only water—or bodies which formed water under the conditions of the experiment—were found capable of determining the explosion. There is an obvious difference

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* I am not to be understood to imply that dissociation does not take place in the path of the discharge; on the contrary, for all the facts appear to me to indicate that conduction and electrolysis are inseparable phenomena in gases as in liquids." (Comp. Schuster, "Proc. Roy. Soc.," 1884, p. 317.)

in constitution between water and the bodies found incapable of determining explosion: the former being a compound of a positive with a negative element, the latter being all compounds of two negative elements; and if it be permissible to generalise from this single instance, it may hence be stated, that in order that interaction shall take place in cases such as that under consideration, it is not only necessary that the elements of the "catalyst" shall be *divisible** between the interacting substances—the elements of CO_2 are obviously as divisible between CO and O_2 as are those of H_2O—but that the catalyst shall consist of a positive and a negative and not of two negative radicals. On this view, it is possible to understand that water itself may act as the catalyst in determining the formation of water at high temperatures from hydrogen and oxygen.†

In the case discussed (the oxidation of carbon monoxide), interaction takes place at a very high temperature, and therefore—since high temperature may be regarded as the equivalent of high electromotive force—under conditions under which the catalyst water is probably a simple electrolyte. The behaviour of sulphur dioxide in presence of oxygen and water is instructive as being a case of an analogous interaction occurring at a low temperature. From a most carefully conducted series of experiments by Mr. Dixon ("Journ. of Gas Lighting," 1881, 37, p. 704), it appears that not only does sulphur dioxide not undergo change in contact with dry oxygen, but that it even resists oxidation if water vapour be present and at a temperature of 100°; as is well known, however, oxidation takes place—but only *slowly*—when an aqueous solution of sulphur dioxide is in contact with oxygen. In this case, in the gaseous mixture the water apparently is not under such conditions that it can act as a simple electrolyte, or even form a composite electrolyte, and action only takes place when the conditions become such that a composite electrolyte can be formed; Ostwald's observations may be held to prove, I imagine, that a very imperfect composite electrolyte results on dissolving sulphur dioxide in water, and in accordance with this is the fact that the aqueous solution is but *slowly* oxidised.‡

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* Mr. Dixon's experiments appear to prove that during the interaction of carbonic oxide and oxygen in presence of water an actual division of the elements of the water molecules takes place between the carbonic oxide and oxygen molecules, and hence that the water does not exercise a mere contact action.

† When this question first came under discussion at the Chemical Society, I said that I looked forward to the time when probably it would be found that a mixture of pure hydrogen and oxygen was inexplosive, like one of pure carbonic oxide and oxygen. I was then still under the influence of current opinion and regarded water as a saturated compound, and had not yet realised the important function of "residual affinity" in such changes.

‡ The behaviour here described of sulphur dioxide appears to me to furnish another argument adverse to the dissociation hypothesis, as oxidation takes place under the conditions least favourable to the occurrence of dissociation.

If a clear distinction can be drawn—as I suppose it can—between simple and composite electrolytes, the presence of a member of the latter class will probably be found to be essential to the occurrence of many interactions taking place at moderate temperatures; thus the oxidation of iron, which is generally supposed to take place only in moist air, is doubtless dependent, not merely on the presence of *water*, but of *impure* water—of water rendered conducting by association with foreign matters; similarly it may be expected that zinc will be found to have no action on water even when associated with a less positive metal, and it would doubtless have no action on sulphuric acid, if such a compound were obtainable in a pure state; but a mixture of sulphuric acid and water readily dissolves it, as the two together form a composite electrolyte of comparatively low resistance.

It can scarcely be doubted that when our elements or compounds are resolved into their ultimate atoms, these atoms are capable of *directly* uniting, and that no catalyst is then required. But if this be the case, and if, as I suppose, the atoms rarely *saturate* each other, the *direct* union of compounds should also be possible in cases in which there is considerable residual affinity; such union would not involve a separation from each other of the constituent elements of one or both of the interacting bodies such as takes place in the changes previously considered. Union of two molecules having taken place; the elements of the interacting bodies having thus been brought into intimate association: it is very probable that in many cases intramolecular change will then supervene, resulting sometimes in mere atomic redistribution, at other times in the resolution of the complex molecule into simpler molecules. I am inclined to think that the majority of so-called double decompositions are thus brought about.

The union of sulphuric anhydride with water to form sulphuric acid, and of sulphuric acid with water to form hydrates, are doubtless cases of this kind. The formation of the hydrate $SO_3{\cdot}OH_2$ must be supposed to be immediately followed by the occurrence of atomic redistribution if we accept the current view that sulphuric acid is a hydroxide of the formula $SO_2(OH)_2$; whether after the formation of the hydrate $H_2SO_4{\cdot}OH_2$ has taken place atomic redistribution in like manner supervenes is a moot point; the large amount of heat developed by the interaction of water and sulphuric acid is, however, specially noteworthy.

Sodium hydroxide is universally regarded as the analogue of water: is its action on sulphuric acid analogous to that of water? I certainly am inclined to hold that it is, and that in the first instance an aggregate, $NaHO{\cdot}SO_4H_2$, results, owing to the attraction of the oxygen of the hydroxide by the oxygenated radicle of the acid: atomic redistribution thereupon takes place, and either the molecule is resolved into two others, water and sodium hydrogen sulphate, or a new compound is formed, which is easily resolvable into these latter. Moreover, I am inclined to attribute this change and the consequent displacement of the hydrogen in the acid by the sodium, not to the fact that sodium has a greater affinity than hydrogen has for SO_4, but to the tendency of hydrogen to displace the sodium in sodium hydroxide and to form water. I do not contend that in such a case as that quoted direct interaction will take place between the substances as we know them in the solid state; these may consist of comparatively inert complex aggregates which require to be resolved into simpler molecules either by dissolution or by application of heat. In other words, the presence of water may be necessary, not because it is essential to have an electrolyte present, but because the occurrence of both molecular interaction and electrolytic conduction depends on identical molecular and intermolecular conditions. The chemical interaction takes place entirely independently of the water molecules, and these latter serve only to separate and keep apart the fundamental molecules of which the interacting bodies are composed.

No final decision for or against the view here put forward can well be arrived at except by the study of the behaviour of gaseous bodies such as ammonia and hydrogen chloride, for example; if proof can be given that these compounds are capable of directly uniting without the intervention of any third body, a most important step will have been made.

Other cases deserving of study are the conversion of nitric oxide into nitric peroxide, oxidation by means of ozone, and the action of metals such as sodium on water. As the formation of nitric peroxide involves the prior separation of oxygen-atoms from oxygen-atoms, and not merely the combination of two molecules, it is not improbable that interaction between nitric oxide and oxygen molecules will only take place in the presence of a catalyst. But it is to be borne in mind that both nitric oxide and ozone are bodies which are capable of interacting with molecules of their own kind, and that considerable heat is thereby developed; and it is conceivable that such bodies being possessed of high residual affinity may directly enter into combination with others which have but little residual affinity. As regards the action of sodium on water, the difference in behaviour with dilute sulphuric acid between moderately pure zinc and very nearly pure zinc is so marked that the vigorous action between sodium and water cannot be held to prove much, as no special care is ever taken to prepare sodium pure; the question whether the affinity of the oxygen in water for sodium is sufficient to cause their direct association and consequent interaction is an interesting one for experimental inquiry, although it would be very difficult to make the experiment properly.

One other application of the theory dwelt on in this paper remains to be mentioned. It is now well established that on exploding gaseous mixtures within a closed chamber, the maximum theoretical temperature is never reached; and this has hitherto always been explained as due to the occurrence of dissociation whereby the change is retarded. If in a mixture, say, of carbonic oxide, oxygen and water gases, the three kinds of molecules act together in the manner I have supposed, it is probable that the extent to which they mutually influence each other would vary with the temperature, and that it would tend to diminish above a certain temperature; if such were the case, change would be retarded in the manner in which it appears to be in explosions within closed chambers. Dissociation undoubtedly does take place in many cases, but there is now a considerable amount of evidence on record to show that the bounding surfaces exercise a most important influence; this is usually not sufficiently taken into account.

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April 1, 1886.

Professor STOKES, D.C.L., President, in the Chair.

Dr. John Francis Julius von Haast (elected 1867) was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Correction to the Equilibrium Theory of Tides for the Continents." I. By G. H. DARWIN, LL.D., F.R.S., Fellow of Trinity College, and Plumian Professor in the University of Cambridge. II. By H. H. TURNER, B.A., Fellow of Trinity College, Cambridge. Received March 12, 1886.

I.

In the equilibrium theory of the tides, as worked out by Newton and Bernouilli, it is assumed that the figure of the ocean is at each instant one of equilibrium.

But Sir William Thomson has pointed out that, when portions of the globe are occupied by land, the law of rise and fall of water given in the usual solution cannot be satisfied by a constant volume of water.*

In Part I of this paper Sir William Thomson's work is placed in a new light, which renders the conclusions more easily intelligible, and Part II contains the numerical calculations necessary to apply the results to the case of the earth.

If m, r, z be the moon's mass, radius vector, and zenith distance; g mean gravity; ρ the earth's mean density; σ the density of water; a the earth's radius; and $\mathfrak{h}$ the height of tide; then, considering only the lunar influence, the solution of the equilibrium theory for an ocean-covered globe is—

$$\frac{\mathfrak{h}}{a}=\frac{3m}{2gr^3}\frac{1}{(1-\frac{3}{5}\sigma/\rho)}(\cos^2 z-\tfrac{1}{3}) \quad \ldots\ldots\ldots\ldots \quad (1)$$

---

* Thomson and Tait's "Nat. Phil.," 1883, § 808.

This equilibrium law would still hold good when the ocean is interrupted by continents, if water were appropriately supplied to or exhausted from the sea as the earth rotates.

Since when water is supplied or exhausted the height of water will rise or fall everywhere to the same extent, it follows that the rise and fall of tide, according to the revised equilibrium theory, must be given by—

$$\frac{\mathfrak{h}}{a}=\frac{3ma}{2gr^3}\frac{1}{1-\frac{3}{5}\sigma/\rho}(\cos^2 z-\tfrac{1}{3})-\alpha \quad \ldots\ldots\ldots\ (2)$$

where α is a constant all over the earth for each position of the moon relatively to the earth, but varies for different positions.

Let Q be the fraction of the earth's surface which is occupied by sea; let λ be the latitude and l the longitude of any point; and let ds stand for $\cos\lambda d\lambda dl$, an element of solid angle. Then we have—

$$4\pi Q=\iint ds$$

integrated all over the oceanic area.

The quantity of water which must be subtracted from the sea, so as to depress the sea level everywhere by $a\alpha$, is $4\pi a^3\alpha Q$; and the quantity required to raise it by the variable height $\frac{3ma^2}{2gr^3}\frac{\cos^2 z-\frac{1}{3}}{1-\frac{3}{5}\sigma/\rho}$ is the integral of this function, taken all over the ocean. But since the volume of water must be constant, continuity demands that—

$$\alpha=\frac{3ma}{2g(1-\frac{3}{5}\sigma/\rho)r^3}\cdot\frac{1}{4\pi Q}\iint(\cos^2 z-\tfrac{1}{3})ds \quad \ldots\ldots\ (3)$$

integrated all over the ocean.

On substituting this value of α in (2) we shall obtain the law of rise and fall.

Now if λ, l be the latitude and W. longitude of the place of observation; h the Greenwich westward hour-angle of the moon at the time and place of observation; and δ the moon's declination, it is well known that—

$$\begin{aligned}\cos^2 z-\tfrac{1}{3}=\tfrac{1}{2}\cos^2\lambda\cos^2\delta\cos 2(h-l)+\sin 2\lambda\sin\delta\cos\delta\cos(h-l)\\+\tfrac{3}{2}(\tfrac{1}{3}-\sin^2\delta)(\tfrac{1}{3}-\sin^2\lambda) \quad \ldots\ldots\ldots\ (4)\end{aligned}$$

We have next to introduce (4) under the double integral sign of (3), and integrate over the ocean.

To express the result conveniently, let—

$$\frac{1}{4\pi Q}\iint \cos^2\lambda \cos 2l\,ds = \cos^2\lambda_2 \cos 2l_2,\ \frac{1}{4\pi Q}\iint \cos^2\lambda \sin 2l\,ds = \cos^2\lambda_2 \sin 2l_2,$$

$$\frac{1}{4\pi Q}\iint \sin 2\lambda \cos l\,ds = \sin 2\lambda_1 \cos l_1,\ \frac{1}{4\pi Q}\iint \sin 2\lambda \sin l\,ds = \sin 2\lambda_1 \sin l_1,$$

$$\frac{1}{4\pi Q}\iint (\tfrac{3}{2}\sin^2\lambda - \tfrac{1}{2})\,ds = \tfrac{3}{2}\sin^2\lambda_0 - \tfrac{1}{2} \quad \ldots\ldots\ldots \quad (5)$$

the integrals being taken over the oceanic area.

These five integrals are called by Sir William Thomson $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, but by introducing the five auxiliary latitudes and longitudes, λ_2, l_2, λ_1, l_1, λ_0 we shall find for the conclusions an easily intelligible physical interpretation.

It may be well to observe that (5) necessarily give real values to the auxiliaries. For consider the first integral as a sample :—

Every element of $\iint \cos^2\lambda \cos 2l\,ds$ is, whether positive or negative, necessarily numerically less than the corresponding element of $4\pi Q$, and therefore, even if all the elements of the former integral were taken with the same sign, $(4\pi Q)^{-1}\iint \cos^2\lambda \cos 2l\,ds$ would be numerically less than unity, and *à fortiori* in the actual case it is numerically less than unity.

Now using (5) in obtaining the value of $\iint(\cos^2 z - \frac{1}{3})ds$, and substituting in (3), we have—

$$\frac{\mathfrak{h}}{a} \div \frac{3ma}{2g(1-\frac{3}{5}\sigma/\rho)r^3} = \tfrac{1}{2}\cos^2\delta[\cos^2\lambda \cos 2(h-l) - \cos^2\lambda_2 \cos 2(h-l_2)]$$
$$+ \sin 2\delta[\sin\lambda \cos\lambda \cos(h-l) - \sin\lambda_1 \cos\lambda_1 \cos(h-l_1)]$$
$$+ \tfrac{3}{2}(\tfrac{1}{3} - \sin^2\delta)(\sin^2\lambda_0 - \sin^2\lambda) \quad \ldots\ldots\ldots\ldots \quad (6)$$

The first term of (6) gives the semi-diurnal tide, the second the diurnal, and the third the tide of long period.

The meaning of the result is clear. The latitude and longitude λ_2, l_2 is a certain definite spot on the earth's surface which has reference to the semi-diurnal tide. Similarly λ_1, l_1 is another definite spot which has reference to the diurnal tide; and λ_0 is a definite parallel of latitude which has reference to the tide of long period.

From inspection we see that at the point λ_2, l_2 the semi-diurnal tide is evanescent, and that at the point λ_2, $l_2+90°$ there is doubled tide, as compared with the uncorrected equilibrium theory. At the place λ_1, l_1 the diurnal tide is evanescent, and at $-\lambda_1$, l_1 there is doubled diurnal tide.

In the latitude λ_0 the long period tide is evanescent, and in latitude (sometimes imaginary) arc $\sin\sqrt{\{\frac{2}{3}-\sin^2\lambda_0\}}$ there is doubled long period tide.

Many or all of these points may fall on continents, so that the evanescence or doubling may only apply to the algebraical expressions, which are, unlike the sea, continuous over the whole globe. But now let us consider more precisely what the points are.

It is obvious that the latitude and longitude λ_2 and l_2, being derived from expressions for $\cos^2\lambda_2 \cos 2l_2$ and $\cos^2\lambda_2 \sin 2l_2$, really correspond with four points whose latitudes and longitudes are—

$$\lambda_2,\ l_2;\ -\lambda_2,\ l_2;\ \lambda_2,\ l_2+180°;\ -\lambda_2,\ l_2+180°.$$

Thus there are four points of evanescent semi-diurnal tide, situated on a single great circle or meridian, in equal latitudes N. and S., and antipodal two and two. Corresponding to these four, there are four points of doubled semi-diurnal tide, whose latitudes and longitudes are—

$$\lambda_2,\ l_2+90°;\ -\lambda_2,\ l_2+90°;\ \lambda_2,\ l_2+270°;\ \lambda_2,\ l_2+270°,$$

and these also are on a single great circle or meridian, at right angles to the former great circle, and are in the same latitudes N. and S. as are the places of evanescence, and are antipodal two and two.

Passing now to the case of the diurnal tide we see that λ_1, l_1, being derived from expressions for $\sin 2\lambda_1 \cos l_1$ and $\sin 2\lambda_1 \sin l_1$, really correspond with four points whose latitudes and longitudes are—

$$\lambda_1,\ l_1;\ -\lambda_1,\ l_1+180°;\ 90°-\lambda_1,\ l_1;\ -90°+\lambda_1,\ l_1+180°.$$

Thus there are four points of evanescent diurnal tide, situated on a single great circle or meridian, two of them are in one quadrant in complemental latitudes, and antipodal to them are the two others. Corresponding to these four there are four points of doubled diurnal tide lying in the same great circle or meridian, and situated similarly with regard to the S. pole as are the points of evanescence with regard to the N. pole; their latitudes and longitudes are—

$$-\lambda_1,\ l_1;\ \lambda_1,\ l_1+180°;\ -90°+\lambda_1,\ l_1;\ 90°-\lambda_1.\ l_1+180°.$$

Lastly, in the case of the long period tide, it is obvious that the latitude λ_0 is either N. or S., and that there are two parallels of latitude of evanescent tide. In case $\sin^2\lambda_0$ is less than $\frac{2}{3}$, or λ_0 less than 54° 44′, there are two parallels of latitude of doubled tide of long period in latitude $\frac{2}{3}$ arc sin $\sqrt{\{\frac{2}{3}-\sin^2\lambda_0\}}$.

From a consideration of the integrals, it appears that as the continents diminish towards vanishing, the four points of evanescent and the four points of doubled semi-diurnal tide close in to the pole, two of each going to the N. pole, and two going to the S. pole; also one of the points of evanescent and one of doubled diurnal tide go to the N. pole, a second pair of points of evanescence and of doubling go to the S. pole, a third pair of points of evanescence and of doubling coalesce on the equator, and a fourth pair coalesce at the antipodes of the third pair; lastly, in the case of the tides of long period the circles of evanescent tide tend to coalesce with the circles of doubled tide, in latitudes 35° 16′ N. and S.

We are now in a position to state the results of Thomson's corrected theory by comparison with Bernouilli's theory.

Consider the semi-diurnal tide on an ocean-covered globe, then at the four points on a single meridian great circle which correspond to the points of evanescence on the partially covered globe, the tide has the same height; and at any point on the partially covered globe the semi-diurnal tide is the excess (interpreted algebraically) of the tide at the corresponding point on the ocean-covered globe above that at the four points.

A similar statement holds good for the diurnal and tides of long period.

By laborious quadratures Mr. Turner has evaluated in Part II the five definite integrals on which the corrections to the equilibrium theory, as applied to the earth, depend.

The values found show that the points of evanescent semi-diurnal tide are only distant about 9° from the N. and S. poles; and that of the four points of evanescent diurnal tide two are close to the equator, one close to the N. pole, and the other close to the S. pole; lastly, that the latitudes of evanescent tide of long period are 34° N. and S., and are thus but little affected by the land.

Thus in all cases the points of evanescence are situated near the places where the tides vanish when there is no land. It follows, therefore, that the correction to the equilibrium theory for land is of no importance.

G. H. D.

II.

For the evaluation of the five definite integrals, called by Sir William Thomson $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, and represented in the present paper by functions of the latitudes and longitudes λ_0, λ_1, λ_2, and l_1, l_2, respectively similar in form to the functions of the "running" latitude and longitude to be integrated, it is necessary to assume some redistribution of the land on the earth's surface, differing as little as possible from the real distribution, and yet with a coast line amenable to mathematical treatment. The integrals are to be taken over the whole ocean, but since the value of any of them taken over the whole sphere is zero, the part of any due to the sea is equal to the part due to the land with its sign changed; and since there is less land than sea, it will be more convenient to integrate over the land, and then change the sign.

Unless specially mentioned, we shall hereafter assume that the integration is taken over the land.

The last of the integrals has already been evaluated by Professor Darwin,* with an approximate coast line, which follows parallels of latitude and longitude alternately.

His distribution of land is given in the following table :—

N. lat.	W. long.	E. long.
Lat. 80° to 90°	20° to 50°.	
70 „ 80	22° to 55° : 85° to 115°.	55° to 60° : 90° to 110°.
60 „ 70	35° to 52° : 65° to 80° : 90° to 165°.	10° to 180°.
50 „ 60	0° to 6° : 60° to 78° : 90° to 130°.	10° to 140° : 155° to 160°.
40 „ 50	0° to 5° : 65° to 123°.	0° to 135°.
30 „ 40	0° to 8° : 78° to 120°.	0° to 120° : 135° to 138°.
20 „ 30	0° to 15° : 80° to 82° : 97° to 110°.	0° to 118°.
10 „ 20	0° to 17° : 87° to 95°.	0° to 50° : 75° to 85° : 95° to 108° : 122° to 125°.
0 „ 10	53° to 78°.	0° to 48° : 98° to 105° : 112° to 117°.
S. lat.	**W. long.**	**E. long.**
0° to 10°	37° to 80°	12° to 40° : 110° to 130°.
10 „ 20	37 „ 74	12° to 38° : 45° to 50° : 126° to 144°.
20 „ 30	45 „ 71	15° to 33° : 115° to 151°.
30 „ 40	55 „ 73	20° to 23° : 132° to 140°.
40 „ 50	65 „ 73	170° to 172°.
50 „ 60	67 „ 72	———
60 „ 70	55 „ 65	120° to 130°.
70 „ 80	about 20° of longitude.	
80 „ 90	„ 180° „	

N.B.—*The Mediterranean, being approximately a lake, is treated as land.*

The limits of the 20° and 180° of longitude between S. latitudes 70° and 90° are not specified. For the evaluation of the last integral this is not necessary, for restricting

$$\iint (3\sin^2\lambda - 1)\cos\lambda\, d\lambda\, dl$$

to a representative portion of the land bounded by parallels λ_1 and λ_2, l_1 and l_2, we get $-\frac{1}{4}(l_2 - l_1)\Big[\sin\lambda + \sin 3\lambda\Big]_{\lambda_1}^{\lambda_2} \times \frac{\pi}{180}$; and similarly for Q; so that if t_1 and t_2 be the number of degrees of longitude N. and S.

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* Thomson and Tait's "Nat. Phil.," 1883, § 808.

of the equator respectively between latitudes λ_1 and λ_2, the last of the integrals becomes

$$\frac{\Sigma\frac{1}{4}(t_1+t_2)\Big[\sin\lambda+\sin 3\lambda\Big]_{\lambda_1}^{\lambda_2}}{720-\Sigma(t_1+t_2)\Big[\sin\lambda\Big]_{\lambda_1}^{\lambda_2}}.$$

But for (*e.g.*)

$$\int_{\lambda_1}^{\lambda_2}\int_{l_1}^{l_2}\cos^2\lambda\cos 2l\cos\lambda\, d\lambda\, dl$$
$$=\frac{1}{24}\Big[9\sin\lambda+\sin 3\lambda\Big]_{\lambda_1}^{\lambda_2}\Big[\sin 2l\Big]_{l_1}^{l_2}$$

the actual limits l_2 and l_1 must be given, and not merely their difference.

It is, however, obvious, on inspection of these integrals, that the land in high latitudes affects them but little; and we shall not lose much by neglecting entirely the Antarctic continent in their evaluation.

This evaluation is reduced by the above process to a series of multiplications, and on performing them the following values of $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, and Q are obtained on the two hypotheses.

(1.) That there is as much Antarctic land as is given in the schedule, which is, however, only taken into account in the last integral $\mathfrak{E}$, and the common denominator $4\pi Q$ of each.

(2.) That there is no land between S. latitude 80° and the pole.

The value of Q is given in terms of the whole surface, and represents the fraction of that surface occupied by land; it must be remembered that the Mediterranean Sea is treated as land. Professor Darwin quotes Rigaud's estimate[*] as 0·266:—

	1st hypothesis.	2nd hypothesis.
$\mathfrak{A}$	+0·03023	+0·03008
$\mathfrak{B}$	+0·00539	+0·00537
$\mathfrak{C}$	−0·01975	−0·01965
$\mathfrak{D}$	+0·02910	+0·02895
$\mathfrak{E}$	−0·01520	−0·00486
Q	0·283	0·278

These results for $\mathfrak{E}$ and Q have already been given by Professor Darwin in "Thomson and Tait's Natural Philosophy," and I have found them correct.

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[*] "Trans. Cam. Phil. Soc.," vol. vi.

We then find for the set of latitudes and longitudes of evanescent tide :—

Nature of tide.		1st hypothesis.	2nd hypothesis.
Long period	lat. λ_0	34° 39′ N.	35° 4′ N.
Diurnal	lat. λ_1 long. l_1	1° 0′ S. 55 50 E.	1° 0′ S. 55 50 E.
Semi-diurnal	lat. λ_2 long. l_2	79° 54′ N. 5 3 W.	79° 56′ N. 5 4 W.

The other points of evanescence are of course easily derivable from these, as shown in the first part of this paper.

As a slightly closer approximation to truth, I have calculated these integrals on another supposition. There are cases where lines satisfying the equations

$$l=\text{const. or } \lambda=\text{const.}$$

diverge somewhat widely from the actual coast line, but a line

$$\pm al\pm b\lambda=\text{const.}$$

(where a and b are small integers) can be found following it more faithfully. An approximate coast line of the land on the earth is defined in the following schedule, west longitudes and north latitudes being considered positive.

Limits of longitude (l).	Equation.	Limits of latitude (λ).
+ 20° to + 10°	$-\lambda=l-40$	+20° to +30°
——	$l=10$	+30 ,, +40
+ 10 ,, − 23	$-\lambda=l-50$	+40 ,, +73
− 23 ,, +120	$\lambda=73$	——
——	$l=120$	+73 ,, +80
+120 ,, + 20	$\lambda=80$	——
——	$l=20$	+80 ,, +70
+ 20 ,, + 50	$-3\lambda=l-230$	+70 ,, +60
+ 50 ,, + 70	$\lambda=l+10$	+60 ,, +80
+ 70 ,, + 80	$\lambda=80$	——
+ 80 ,, + 50	$\lambda=l$	+80 ,, +50
+ 50 ,, + 90	$-2\lambda=l-150$	+50 ,, +30
+ 90 ,, +100	$\lambda=30$	——
+100 ,, + 80	$\lambda=l-70$	+30 ,, +10
+ 80 ,, + 70	$\lambda=10$	——
+ 70 ,, + 30	$2\lambda=l-50$	+10 ,, −10
+ 30 ,, + 73	$-\lambda=l-20$	−10 ,, −53
——	$l=73$	−53 ,, −14

Limits of longitude (l).	Equation.	Limits of latitude (λ).
+ 78° to + 80°	$\lambda=2l-160$	−14° to 0°
+ 80 „ +140	$\lambda=l-80$	0 „ +60
+140 „ −150	$\lambda=60$	——
−150 „ −100	$-\lambda=l+90$	+60 „ +10
−100 „ − 90	$+\lambda=l+110$	+10 „ +20
− 90 „ − 80	$-\lambda=l+70$	+20 „ +10
− 80 „ − 65	$\lambda=l+90$	+10 „ +25
− 65 „ − 40	$-\lambda=l+40$	+25 „ 0
——	$l=-40$	0 „ −20
− 40 „ − 20	$-\lambda=l+60$	−20 „ −40
− 20 „ − $8\frac{3}{4}$	$\lambda=4l+40$	−40 „ + 5
− $8\frac{3}{4}$ „ + $12\frac{1}{2}$	$\lambda=5$	——
+ $12\frac{1}{2}$ „ + 20	$\lambda=2l-20$	+ 5 „ +20

New Guinea.

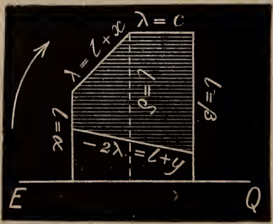
−130 to −150	$2\lambda=l+130$	0 to −10
−150 „ −140	$\lambda=-10$	——
−140 „ −130	$\lambda=l+130$	−10 „ 0

Australia.

−140 to −150	$\lambda=l+130$	−10 to −20
——	$l=-150$	−20 „ −35
−150 „ −115	$\lambda=-35$	——
——	$l=-115$	−35 „ $-22\frac{1}{2}$
−115 „ −140	$-2\lambda=l+160$	$-22\frac{1}{2}$ „ −10

It will be seen that it is only rarely necessary to depart from the forms of equation $\pm\lambda=l+x$ and the two original forms $\lambda=$const. $l=$const. to represent the coast line with considerable accuracy. There are still left one or two outlying portions, of which mention will be made later.

Now supposing we are to find the value of the first integral for the portion of land indicated by the shaded portion of the diagram, E, Q being the equator:



the equations to its boundaries being written at the side of each.

We have

$$\iint \cos^3\lambda d\lambda \cos 2l dl = \frac{1}{12}\int \Big[9\sin\lambda + \sin 3\lambda\Big]_{\lambda_1}^{\lambda_2} \cos 2l dl$$

$$= \frac{1}{12}\int_{\alpha}^{\delta} \{9\sin(l+x) + \sin 3(l+x)\} \cos 2l dl$$

$$+ \int_{\delta}^{\beta} (9\sin c + \sin 3c) \cos 2l dl$$

$$+ \int_{\beta}^{\alpha} - \{9\sin\tfrac{1}{2}(l+y) + \sin\tfrac{3}{2}(l+y)\} \cos 2l dl.$$

We may thus simply travel round the boundary omitting the places where λ=constant: being careful to go round all the pieces of land in the same direction. If we suppose $l=\alpha$ to be the meridian of Greenwich, and the land to be in the northern hemisphere, the direction indicated above is the wrong one for obtaining the value of the integrals over the land, for the longitudes increase to the left; but by following this direction we shall obtain the values over the sea as is in reality required.

The result of integration has, of course, a different form for each form of the relation between l and λ representing the boundary. In computing the numerical values of the integrals, it is convenient to consider together all the parts of the boundary represented by similar equations.

Below are given as representative the forms which the numerator of the first integral $\mathfrak{A}$ assumes for different forms of the boundary, the quantities within square brackets being taken within limits.

Form.		Value of Integral.
$\mp\lambda = l+x$	....	$\pm\frac{1}{24}[\frac{1}{5}\cos(5l+3x) + 3\cos(3l+x) + \cos(l+3x) - 9\cos(-l+x)]$
$\lambda = x$	....	$+\frac{1}{24}(9\sin x + \sin 3x)[\sin 2l]$
$l = x$	....	Zero
$\lambda = 2l+x$	....	$-\frac{1}{24}[\frac{9}{4}\cos(4l+x) - 9l\sin x + \frac{1}{8}\cos(8l+3x) + \frac{1}{4}\cos(4l+3x)]$
$\lambda = 4l+x$	....	$-\frac{1}{24}[\frac{3}{2}\cos(6l+x) + \frac{9}{2}\cos(2l+x) + \frac{1}{14}\cos(14l+3x) + \frac{1}{10}\cos(10l+3x)]$
$\mp 2\lambda = l+x$	....	$\pm\frac{1}{24}[\frac{18}{5}\cos\frac{1}{2}(5l+x) - 6\cos\frac{1}{2}(-3l+x) + \frac{2}{7}\cos\frac{1}{2}(7l+3x) - 2\sin\frac{1}{2}(-l+3x)]$
$-3\lambda = l+x$	....	$+\frac{1}{24}[\frac{27}{7}\cos\frac{1}{3}(7l+x) - \frac{27}{5}\cos\frac{1}{3}(-5l+x) + \frac{1}{3}\cos(3l+x) - \cos(-l+x)]$

Evaluating these integrals on this supposition, we obtain

	1st hypothesis.	2nd hypothesis.
$\mathfrak{A}$	$+0{\cdot}02119$	$+0{\cdot}02110$
$\mathfrak{B}$	$+0{\cdot}00778$	$+0{\cdot}00775$
$\mathfrak{C}$	$-0{\cdot}01890$	$-0{\cdot}01882$
$\mathfrak{D}$	$+0{\cdot}03159$	$+0{\cdot}03128$
$\mathfrak{E}$	$-0{\cdot}04364$	$-0{\cdot}03319$
Q	$0{\cdot}283$	$0{\cdot}278$

It will be noticed that the values of Q are exactly the same as before.

From these we deduce

Nature of tide.		1st hypothesis.	2nd hypothesis.
Long period	lat. λ_0	33° 29′ N.	33° 55′ N.
Diurnal	lat. λ_1 long. l_1	1° 3′ S. 59 7 E.	1° 3′ S. 58 58 E.
Semi-diurnal	lat. λ_2 long. l_2	81° 22′ N. 10 5 W.	81° 23′ N. 10 5 W.

The agreement of these values of the quantities with the values calculated on the previous supposition is not quite so close as I anticipated, but it should be remarked that the numerators of the quantities $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$ are the differences of positive and negative quantities of very much greater magnitude, as becomes obvious on proceeding to the numerical calculation; and thus a comparatively small change in one of the large compensating quantities, due to large tracts of land in different portions of the globe, affects the integrals a considerable extent.

In this connexion I was led to investigate the effect of counting various small islands and promontories as sea, or small bays and straits as land. For instance, a portion of sea in the neighbourhood of Behring's Straits is included as land, and a corresponding correction must be applied to the integrals. This correction I have *estimated* as follows:—The area of the sea is estimated in square degrees, by drawing lines on a large map corresponding to each degree of latitude and longitude and counting the squares covered by sea, fractions of a square to one decimal place being included, though the tenths have been neglected in the concluded sum. This area has then been multiplied by the value of (say) $\cos^3\lambda \cos 2l$

for the approximate centre of gravity of the portion, to find an approximate value of the integral $\iint \cos^3\lambda \cos 2l \cos \lambda dl$ over its surface.

By drawing the assumed coast line on a map, it will become obvious that such corrections may be applied for the following portions, defined by the latitude and longitude of their centres of gravity; remarking that when there is a portion of land which may be fairly considered to compensate a portion of sea in the immediate neighbourhood, no correction has been applied. For instance, it would be seen that part of the Kamschatkan Peninsula is excluded from the coast line, and part of the Sea of Okhotsk is included; but these will produce nearly equal effects on the integrals in opposite directions, and are thus left out of consideration.

Area in square degrees.	Longitude.	Latitude.
+160	+172°	+64°
+240	+150	+71½
+166	+ 85	+60
+ 80	+ 60	+52
+ 68	+ 85	+ 9
− 20	+ 75	+21
− 69	+ 55	+ 4
+ 43	+ 34	−11
+ 22	− 37	−20
− 48	− 47	−19
+ 65	− 53	+18
− 16	−107	+13
− 34	−102	+ 2
− 49	−114	+ 1
− 27	−123	+12
− 11	−118	− 5
− 43	−138	+36
− 39	−173	−42

N.B.—*Land-areas are considered positive, sea negative.*

We then find the following corrected values of the integrals :—

	1st hypothesis.	2nd hypothesis.
𝔄	+0·02237	+0·02247
𝔅	+0·00230	+0·00231
ℭ	−0·01952	−0·01961
𝔇	+0·02665	+0·02676
𝔈	−0·01775	−0·02810
Q	0·279	0·274

and finally the following values of the latitudes and longitudes of evanescent tides:—

Nature of tide.		1st hypothesis.	2nd hypothesis.
Long period	lat. λ_0	34° 33′ N.	34° 7′ N.
Diurnal .... {	lat. λ_1 long. l_1	0° 57′ S. 53 47 E.	0° 57′ S. 53 46 E.
Semi-diurnal .... {	lat. λ_2 long. l_2	81° 23′ N. 2 56 W.	81° 21′ N. 2 56 W.

The estimation of corrections due to these supplementary portions has been checked in two cases by a detailed extension of the method of square blocks of land used previously for evaluation of the whole integrals; that is to say, two of these portions were separately divided into square degrees (instead of squares whose sides were each ten degrees), and the integral evaluated in a similar manner to that previously described. The agreement of the values so calculated with those obtained by the above method of estimation was sufficiently exact to justify a certain confidence in the close agreement of the finally corrected values of the integrals with their theoretically perfect values.

H. H. T.

II. "Description of Fossil Remains of two Species of a Megalanian Genus (*Meiolania*, Ow.), from Lord Howe's Island." By Sir RICHARD OWEN, K.C.B., F.R.S. Received March 15, 1886.

(Abstract.)

In a scientific survey by the Department of Mines, New South Wales, of Lord Howe's Island, fossil remains were obtained which were transmitted to the British Museum of Natural History, and were confided to the author for determination and description.

These fossils, referable to the extinct family of horned Saurians described in former volumes of the "Philosophical Transactions"* under the generic name *Megalania*, form the subject of the present paper. They represent species smaller in size than *Megalania prisca*, Ow., and with other differential characters on which an allied genus *Meiolania* is founded. Characters of an almost entire skull with part of the lower jaw-bone, of some vertebræ and parts of the scapula and pelvic arches, are assigned to the species *Meiolania*

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* Vol. 149, 1858, p. 43; *ib.*, 1880, p. 1037; *ib.*, 1881, p. 1037.

platyceps. Portions of a cranium and mandible are referred to a *Meiolania minor.* Both species, as in *Megalania*, are edentulous with modifications of the mouth indicative of a horny beak, as in the Chelonian order. The cranial and vertebral characters are, however, sauroid. Horn-cores in three pairs are present but shorter relatively, especially the first and third pairs, than in *Megalania prisca.* The indication of a seventh more advanced and medial horn is feeble, and the author remarks that in the small existing lizard (*Moloch*) this horn has not an osseous support. The tail of *Meiolania* is long and stiff; the vertebræ being encased by an osseous sheath, developing, as in *Megalania*, tuberous processes in two pairs, corresponding with the vertebræ within: such defensive parts are less developed, relatively, than in *Megalania prisca.*

The locality of these singular remains is an insular tract not exceeding 6 miles by 1 mile in extent; situated mid-way between Sydney and Norfolk Island, in lat. 31° 31′ S., long. 159° 9′ E. The island is formed of three raised basaltic masses connected by low-lying grounds of blown coral-sand formation, consisting of rounded grains and fragments of corals and shells. In the parts of this formation converted into rock were found the petrified remains which are the subject of the present paper. It is accompanied by drawings of the most instructive fossils: these form the subjects of five plates illustrative of the text.

III. "On the Luni-Solar Variations of Magnetic Declination and Horizontal Force at Bombay, and of Declination at Trevandrum." By CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory, Bombay. Received March 24, 1886.

(Abstract.)

The materials described in this paper are twenty-five years of declination observations, and twenty-six and a half years of horizontal force observations, taken at the Colaba Observatory, Bombay, and some results of ten years declination observations taken at the Trevandrum Observatory. A consideration of the lunar diurnal variations derived from these observations for different seasons and phases of the moon, leads the author to form the hypothesis that these variations are, properly speaking, combinations of solar diurnal variations that run through a cycle of change in a lunation. The characteristics of the variations that give rise to the hypothesis are (1) that generally the great movements occur in them, as in the mean solar diurnal variations for full lunations, in the solar *day hours*, whilst the night hours are relatively quiescent; and (2) that they

have generally the same character and range at intervals of half a lunation, and opposite characters at intervals of a quarter of a lunation. An expression for the variation at any age of the moon that would satisfy these characteristics would take the form

$$f_{c\cdot 2}(h)\cos 2\left(\frac{2\pi}{\mathrm{P}}t\right)+f_{s\cdot 2}(h)\sin 2\left(\frac{2\pi}{\mathrm{P}}t\right),$$

where h is the hour of the solar day, P the mean period of a lunation, and t the age of the moon, and $f_{c\cdot 2}(h)$, $f_{s\cdot 2}(h)$ are solar diurnal variations that are constant for the same season of the year. It was found that although such a formula embraced the bulk of the phenomena, there remained minor characteristics of a systematic kind that found expression only in the extra terms of the formula when extended as follows:—

$$f_{c\cdot 1}(h)\cos\left(\frac{2\pi}{\mathrm{P}}t\right)+f_{s\cdot 1}(h)\sin\left(\frac{2\pi}{\mathrm{P}}t\right)+f_{c\cdot 2}(h)\cos 2\left(\frac{2\pi}{\mathrm{P}}t\right)+f_{s\cdot 2}(h)\sin 2\left(\frac{2\pi}{\mathrm{P}}t\right)$$

Not only does the hypothesis hold good in the different seasons of the year and with respect both to the declination and horizontal force at Bombay, but the variations of the two elements are related to each other in a definite manner; in the winter season the variations of declination at one age of the moon are similar to those of the horizontal force at an age of the moon one-eighth of a lunation greater; and conversely, in the summer and autumn the variations of horizontal force take precedence of those of the declination by one-eighth of a lunation. So far as the means of testing it are available, the hypothesis holds also in respect of magnetic variations at Trevandrum. Each term of the formula is symbolical of a definite physical conception, viz., that an otherwise constant variation swells and contracts with a wave-like motion, as the age of the moon increases, between the limits $-f(h)$ and $+f(h)$. The existence of luni-solar variations of the kind described is, so far as the author is aware, brought to light for the first time, by the long series of observations taken at Bombay, and their capability of expression in a compact form which has a definite physical significance cannot, the author thinks, fail to be helpful towards the discovery of the physical conditions that lie behind them.

IV. "On a New Form of Stereoscope." By A. STROH. Communicated by Lord RAYLEIGH, D.C.L., Sec. R.S. Received March 22, 1886.

Although the late Sir Charles Wheatstone's beautiful invention, the stereoscope, gives the appearance of full relief or perfect solidity to photographs of objects seen by its aid, the photographs for the same must naturally be of limited dimensions; and though viewed through magnifying lenses, the images of the objects are presented to the eye on a scale far below the size of their originals.

It has therefore occurred to me, that if the magnified image of a photograph projected on a screen by the optical lantern could be made stereoscopic, a still greater resemblance to the original might be obtained.

With a view of producing such an effect, I have constructed the apparatus I will now describe, which is, however, not intended to enable a large number of persons to see the projected pictures at the same time, as in the case of dissolving views, but is at present limited to the use of two persons simultaneously. It could, however, be easily constructed so as to be available for a greater number.

The principle of the arrangement depends on the well-known effects of the persistence of vision; revolving disks are employed for alternately obscuring two pictures, projected on a screen in the same place, and at the same time interfering with the view of the observer in such a manner that only one picture is seen by the observers' right eyes, and the other by the left eyes.

Two optical lanterns are placed side by side, as for dissolving views. Two transparencies, photographed in the same manner, as if intended for an ordinary stereoscope, are placed one in each lantern, and projected on a screen in such a position that they overlap each other as nearly as possible. The picture which is intended to be seen by the right eye may be placed in the right hand lantern, and the other in the left.

Supported by suitable framework, and in the front of the two lenses of the lanterns, is a revolving disk, portions of which are cut away, so that during its revolutions it obscures the light of each lantern alternately, or in other words, so that only one picture at a time is thrown on the screen. A continuous change from one picture to the other is thus obtained.

In the same framework, and in convenient positions for the observers, two pairs of eye-holes are provided, one pair on either side of the apparatus. Behind each pair is also a rotating disk, and these disks are connected by suitable wheel-work or driving bands with the one previously mentioned, in such a way that the three disks rotate together, and at the same rate. The two last-named disks are also so cut that they will obstruct the view through the right and the left eye-holes alternately.

Finally the connexion between the three disks has to be so arranged that the time of obscuring the view of the observers' right eyes or left eyes shall coincide with the time when the light is shut off from the right or left lens of the lanterns respectively.

It is obvious that by this arrangement the left eyes can only see the picture projected from the left hand lantern, and the right eyes can only see that from the right hand lantern.

The rotation of the disks must be of such a rate, that the alternate flashes of the right and left pictures on the corresponding eyes follow in such rapid succession that the impression made by one flash does not diminish sensibly before the next flash on the same eye is received. The number of flashes for each eye which is required to produce an apparently continuous view, without any flickering effect, is from thirty to forty per second. As the disks are so cut as to produce two flashes for the right eyes, and two for the left in one revolution, they must consequently be kept rotating at a rate of from fifteen to twenty revolutions per second.

The rotation of the disks is effected by a driving-wheel and band, worked by a crank handle at the back of the apparatus.

The perspective effect obtained by the above arrangement is very perfect, the image of each object standing out in solid relief.

Considering that by this arrangement the two eyes never see at the same time, and that each eye views its picture after the other, it is interesting to find that the persistence of vision so completely bridges over the alternate interruptions to which it is subjected as to produce the effect of a continuous view.

An unavoidable effect resulting from this arrangement is, that by the rotation of the disks one half of the light produced by each lantern is always cut off; the higher, therefore, the illuminating power used the better is the result.

This defect is, however, I consider, counterbalanced by several advantages which this form of stereoscope possesses. Firstly, the pictures can be enlarged to such an extent as to appear equal, or even larger than the original objects from which they were taken; and secondly, the eyes, in looking at the pictures, are not in any way subjected to strain by lenses, prisms, or reflectors, or by the difficulty which some persons experience in getting the two pictures to superpose. For each eye views its corresponding picture in exactly the same position it would see it in if it were looking at the original, since the two pictures are practically in the same place, which is not the case in any other form of stereoscope.

Although with the apparatus as here described only two persons can see the pictures at the same time, it would not be very difficult to construct it so as to be available for a greater number. The side disks above described only serve to control one pair of eye-holes each, but by making them a little larger they would serve for two pairs each, thus accommodating four observers. By increasing the number of disks, the number of observers might be increased proportionately.

April 8, 1886.

Professor STOKES, D.C.L., President in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Croonian Lecture was delivered:—

I. CROONIAN LECTURE.—"On the Coagulation of the Blood." By L. C. WOOLDRIDGE, M.B., D.Sc., Demonstrator of Physiology in Guy's Hospital and Research Scholar to the Grocers' Company. Communicated by Professor M. FOSTER, Sec. R.S. Received April 6, 1886.

(Abstract.)

1. As to the relation of the corpuscular elements of the blood to coagulation. The plasma itself contains all the elements necessary for coagulation.

The white blood corpuscles probably aid coagulation to a certain extent, but their influence is entirely secondary.

The really important factor in initiating coagulation is a substance dissolved in the plasma, discovered by the author, and called by him A-fibrinogen. Lymph cells differ from white blood corpuscles; they are very active in inducing coagulation.

2. As to the chemical processes of coagulation, the author considers there are three coagulable bodies present in the plasma. These he names A-, B-, and C-fibrinogen.* They are closely allied to one another, and are not separated by a sharp line from one another.

C-fibrinogen is identical with the body which has hitherto been known as fibrinogen, but it is only present in minimal quantities in blood plasma; it is coagulable with fibrin ferment. The bulk of the coagulable matter of the plasma is B-fibrinogen; it clots on the addition of lecithin; it does not clot with fibrin ferment; it clots with leucocytes from lymph glands.

A-fibrinogen is separable from the plasma by cooling; it separates as minute, regular, rounded granules; it is not coagulable by fibrin ferment.

A- and B-fibrinogen are compounds of proteid and lecithin. The

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* These names are provisional.

essential point in the coagulation of the blood is a loss of lecithin on the part of A-fibrinogen, and a gain of lecithin on the part of B-fibrinogen. A-fibrinogen loses some of its lecithin to B-fibrinogen, and the result is that in the place of the two fibrinogens we have fibrin. Previous authors have all regarded coagulation as essentially a fermentative process.

The author regards the fibrin ferment as purely subsidiary, and considers that coagulation is nearly allied to crystallisation.

3. In the fluid of lymph glands from which all the cellular elements have been removed, another form of fibrinogen exists closely allied to and probably the precursor of the A-fibrinogen of the blood. It differs from the latter in causing intravascular clotting,* whereas A-fibrinogen only causes under normal conditions clotting in shed blood.

It is a proteid-lecithin compound, and its action can be shown to depend on the lecithin it contains. It has a wide distribution apart from lymph glands.

In the fluid of serous cavities of certain animals, the only coagulable body present is C-fibrinogen, and since the blood of these animals contains both A- and B-fibrinogen, the vascular wall either only allows C-fibrinogen to pass, or changes A- and B-fibrinogen into C-fibrinogen in their passage through.

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* *Vide* "Proc. Roy. Soc.," vol. 40, p. 134.

April 15, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Notes on certain Zoological Observations made at Talisse Island, North Celebes." By SYDNEY J. HICKSON, D.Sc., B.A. Communicated by Professor H. N. MOSELEY, F.R.S.* Received March 25, 1886.

Notes upon an Alcyonarian (Clavularia viridis).

In one of my earliest walks upon the coral reefs of Talisse, I came across a spot where Tubipores and Cornularias were more abundant than elsewhere. Quantities of the little crowds of brownish-green or pure brown polypes of these Alcyonarians, with occasionally a crowd of the emerald-green polypes of a small species of Tubipora, were to be seen on every side. As I was wading along through the water on this spot, my stick accidentally struck against a mass of what I thought was Tubipora; but when the polypes had retracted I saw to my surprise that, instead of the usual bright red skeleton, there was a skeleton of a dirty green colour, the tubes of which were joined, not by platforms, but by tubes. Taking with me a large speci-

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* [*Note by Professor H. N. Moseley.*—The Alcyonarian described here by Mr. Hickson is apparently identical with a specimen in the British Museum, collected by Mr. A. R. Wallace in the Aru Islands, and labelled *Clavularia viridis.* The existence of transverse communicating canals in Clavularia, extending between the vertical tubes at successive heights above the stolon tubes, as in Syringopora, is apparently a new fact, and one of great interest. The genus Clavularia has received considerable attention from modern naturalists. G. v. Koch has described the anatomy of *Clavularia prolifera*, and A. Kowalevsky and A. F. Marion the larval phases of *Clavularia petricola;* but these forms, together with most others included in the genus, appear to have the vertical tubes united only at the level of the stolon, as is the case, according to Mr. Hickson, in the young state of the form he describes. Possibly his form will require to be placed in a separate genus. The existence of rudimentary ampullæ in the cœnosteum of Millepora has been described by Mr. Quelch, of the British Museum, but the actual gonads of the Milleporidæ have hitherto remained undiscovered. The notes have been written by Mr. Hickson where he is of course unable to refer to scientific literature.]

men of it in sea-water, I examined it carefully at my house, and the next morning I procured some more, and treated it in various ways for microscopic examination.

There are one or two features in the anatomy of this Alcyonarian which throw a good deal of light not only upon the zoological position of Tubipora but also that of the extinct Syringopora.

At present I have only found this form on the inside of the reef growing upon old and dead coral masses; in its neighbourhood are numerous specimens of Tubipora, some of them with unusually large tubes, two or three species of Cornularia, a few Madreporas, and one or two Astræids. It clings to the rocks by a stolon of tubes, which run in various directions and follow all the unevennesses of the supporting rock. It is very easy, however, to pull it away bodily, without injuring the stolon.

The polype tubes spring perpendicularly from the stolon, and rise to a height of 2 or 3 inches. I have not found any tubes longer than that at present, in fact the average is rather below that. It may be, however, as is the case with Tubipora, that the masses grow much larger and the tubes much longer in more favourable localities. The tubes are united together, not by platforms, as in Tubipora, but by simple tubes, as in Syringopora (fig. 1), and from these connecting tubes new polype tubes spring. Each polype tube is marked by eight grooves, corresponding with the eight mesenteries, and these grooves, instead of running straight from the stolon to the mouth, turn to the left, and run up the tubes spirally, plainly showing that in the course of the growth of the polype from the stolon or connecting tube it is twisted from left to right. Examining a portion of the dried skeleton, I found that it is not purely calcareous, as is the skeleton of Tubipora, but consists of a few long spicules imbedded in a coriaceous substance, which is unaffected by strong hydrochloric acid. I should not like to say for certain of what chemical nature this substance is, but from its microscopic appearance I should expect elastin. The tubes are not perforated as in Tubipora, and I cannot at present discover any organic connexion between the mesoderm outside the tube and the mesoderm inside the tube.

The polypes very closely resemble the polypes of Tubipora. They are of a rich brown colour, and contract but slowly when irritated. The tentacles have the usual Alcyonarian character, and are richly armed with nematocysts.

At this season of the year this Alcyonarian does not seem to breed at all, as after examining a great many polypes I have found none sexually mature. The young colonies, which are to be found in abundance on the reefs, closely resemble a species of Cornularia, which is found here in abundance, consisting simply of branched stolons, from which the young polypes spring.

Fig. 1.

Small portion of the skeleton (× 2) as it appears when dry, showing the longitudinal grooves which correspond with the mesenteries (*gg*), the connecting tubes (*tt*), and a young polype springing from a connecting tube (*p*).

Histologically it does not seem to differ in any important particular from Tubipora, but I hope in a later and fuller paper to be able to give the results of a further and better investigation.

The importance and interest of this genus is two-fold. In the first place the structure of the stolon, the mode of connexion of the polype tubes, and the fact that its skeleton is imperforate, show that it is closely allied to the extinct genus Syringopora, which it resembles in all these particulars. Notwithstanding the mass of evidence which has been brought by Moseley, Zittel, and others, to prove that this latter genus is Alcyonarian, there are still some authors who maintain that it is Zoantharian. The peculiar structure of the present form goes far to prove that the former opinion is right, and the latter wrong.

In the second place the resemblance of the young colonies of this form to the genus Cornularia, and the resemblance of the adult colonies and polypes to those of Tubipora, justify the conclusion I arrived at in a former paper, that Tubipora should be united with the Cornularidæ into a group, the Stolonifera; this genus is, in fact, the connecting link between these genera which was formerly missing, unless we assumed that Syringopora was undoubtedly Alcyonarian.

I hope in a future paper to be able to give some further particulars of the anatomy of this form, perhaps also some account of the early stages of its development, and some account of my researches upon the other Stoloniferous Alcyonaria, which are present here in abundance.

Note on Tubipora and on Millepora.

I have got the early stages of the development of Tubipora. It is regularly holoblastic, and I think the gastrula is formed by invagination. Finding, however, that it is very difficult to keep the embryos alive in this hot and dusty weather, I must wait until it becomes a little cooler in December before I can get any very satisfactory results on this latter point.

The generative products of Millepora are formed in little capsules in the walls of the canals, and I have found both male and female capsules in the same canals. The embryos, *I believe from the evidence of one preparation only*, reach a certain stage of development in chitinous capsules in the canals, and they are then discharged into the water by the mouths of the gastrozooids.

Fig. 2.

Generative capsules of Millepora.

II. "Dynamo-Electric Machines.—Preliminary Notice." By JOHN HOPKINSON, D.Sc., F.R.S., and EDWARD HOPKINSON, D.Sc. Received April 3, 1886.

Omitting the inductive effects of the current in the armature itself, all the properties of a dynamo machine are most conveniently deduced from a statement of the relation between the magnetic field and the magnetising force required to produce that field. This relation given, it is easy to deduce what the result will be in all employments of the machine, also the result of varying the winding of the machine in armature or magnets. The magnetic field may be expressed algebraically as a function of the magnetising force, or more conveniently by a curve ("Proceedings of the Institution of Mechanical Engineers," April, 1879, p. 246). Amongst the empirical formulæ which have been proposed to express the electromotive force of dynamo machines in terms of the currents around the magnets, we may mention that known as Fröhlich's, where $E = \frac{ac}{1+bc}$, E being the electromotive force of the machine at a given speed, c the exciting current, and a and b constants. For some machines this hyperbola is said to express observed results fairly accurately. In our experience it does not sufficiently approximate to a straight line in the part of the curve near the origin, and gives too high results for large values of c.

One purpose of the present investigation is to give an approximately complete construction of the characteristic curve of a dynamo of given form from the ordinary laws of electromagnetism and the known properties of iron. Let n be the number of convolutions on the magnets, c the current round the magnets, l_1 the mean length of the lines of force in the iron of the armature, A_1 the area of section of iron in the armature, l_2 the distance from iron of armature to iron of pole pieces, A_2 the area of the magnetic field in which the wires move corrected for its extension round the edge of the pole pieces, l_3 the total length of the magnet cores, A_3 the area of the magnet cores, l_4 the mean length of lines of force in the yoke connecting the magnet limbs in machines of the type on which we have principally experimented, A_4 the area of section of the yoke, l_5 the mean length of the lines of force in each pole piece, A_5 the mean area of section of pole piece, I the total induction through the armature, when no current passes in the armature, and νI the total induction in the magnet cores; and finally let the relation between the magnetic force (α) and induction (a) (*vide* Thomson, "Electrostatics and Magnetism,"

p. 397, and Maxwell, "Treatise on Electricity and Magnetism," vol. ii, p. 24) be represented by the equation $\alpha = f(a)$, then the characteristic curve is—

$$4\pi nc = l_1 f\left(\frac{\mathrm{I}}{\mathrm{A}_1}\right) + 2l_2\frac{\mathrm{I}}{\mathrm{A}_2} + l_3 f\left(\frac{\nu\mathrm{I}}{\mathrm{A}_3}\right) + l_4 f\left(\frac{\nu\mathrm{I}}{\mathrm{A}_4}\right) + 2l_5 f\left(\frac{\mathrm{I}_5}{\mathrm{A}_5}\right).$$

If the relation between α and a be given in the form of a curve, this formula indicates at once a perfectly simple graphical construction for the characteristic. Taking the curve of magnetisation determined by one of us for wrought iron, and constructing a characteristic in this way, we have obtained a theoretical curve which agrees over a long range with the actual results of observation on a dynamo machine more closely than any empirical formula with which we are acquainted.

To determine ν, a wire was taken once round the middle of one magnet and connected to a ballistic galvanometer, a known current was then either suddenly passed round the magnets or short circuited, the elongation of the galvanometer being noted. A similar observation was made with the same current, the galvanometer being connected to a single convolution of the armature in the plane of commutation. The ratio of the two elongations is the value of ν.

The distribution of the waste field $(\nu-1)\,\mathrm{I}$ was roughly ascertained in a similar manner.

The currents in the fixed coils round the magnets are not the only magnetising forces applied in a dynamo machine. The currents in the moving coils of the armature have also their effect upon the resultant field. In well-constructed machines the effect of the latter is reduced to a minimum, but it can be by no means neglected. This introduces a second independent variable, viz., C, the current in the armature. The effect of the current in the armature depends upon the lead given to the brushes. Denote this by λ, which we may also regard as an independent variable, as it is subject to arbitrary adjustment.

If $\mathrm{I} = \mathrm{F}(4\pi nc)$ be the characteristic curve when no current passes through the armature, then

$$\mathrm{I} + \frac{\nu-1}{\nu}\,4\lambda m\mathrm{C}\,\frac{\mathrm{A}_2}{l_2} = \mathrm{F}\left(4\pi nc - \frac{4\lambda m\mathrm{C}}{\nu}\right),$$

where m is the number of convolutions in the armature. Here we omit the comparatively unimportant portion of the magnetic force in the core of the armature and the pole pieces. From this formula it is not difficult to deduce a geometrical construction for the characteristic surface (*vide* "Practical Applications of Electricity," Lectures delivered at the Institute of Civil Engineers, 1882–83, p. 98). The equation may be thus expressed in words, if λ be such that the coils at commutation embrace the whole or nearly the whole induction. The effect of the current in the armature upon the difference of potential between the brushes of any machine, is the same as that of an addition to the resistance of the armature proportional to the lead of the brushes, and to the ratio of the waste field to the total field, combined with that of taking the main current $\frac{m\lambda}{\nu\pi}$ times round the magnets in a direction opposite to the current c. Many consequences can be deduced, of which we may notice the following:—In a series wound dynamo C is equal to c, and if c be increased beyond a certain point, I must attain a maximum and then diminish; this has been frequently observed. We now see that it depends upon the existence of a waste field. Secondly, let the coils of the magnets be entirely disconnected, and let λ be the negative: if the armature be short circuited through a small resistance and be run at a sufficient speed, a large current may be produced in the armature. This latter deduction we have verified by direct experiment.

The efficiency of the type of dynamo machine upon which the experiments before indicated have been made, has been accurately determined by the device of coupling two similar machines, both mechanically and electrically, so that one should act as a generator of electricity, driving the other electrically, whilst the latter acted as a motor driving the former mechanically; the loss of power required to keep the whole combination in movement being determined by direct dynamometric measurement, and the power passing electrically from the one machine to the other being measured by ordinary electrical appliances.

The whole of the experiments were carried out at the works of Messrs. Mather and Platt, to whom we are indebted for the exceptional opportunities we have enjoyed of putting theoretical conclusions to the test of experiment on an engineering scale.

The Society then adjourned over the Easter Recess to Thursday, May 6th.

May 6, 1886.

Lieut.-General STRACHEY, R.E., Vice-President, in the Chair.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Bidwell, Shelford, M.A.
Colenso, William, F.L.S.
Dixon, Harold B., F.C.S.
Festing, Edward Robert, Major-General R.E.,
Forsyth, Andrew Russell, M.A.
Green, Professor A. H., M.A.
Horsley, Professor Victor, F.R.C.S.
Lewis, T. R., M.B.
Meldola, Raphael, F.R.A.S.
Pye-Smith, Philip H., M.D.
Russell, Henry Chamberlaine, B.A.
Unwin, Professor W. Cawthorne, B.Sc.
Warington, Robert, F.C.S.
Wharton, William James Lloyd, Captain R.N.
Wilde, Henry.

The following Papers were read:—

I. “On an Effect Produced by the Passage of an Electric Discharge through Pure Nitrogen.”

By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College, Cavendish Professor of Experimental Physics, Cambridge, and R. THRELFALL, B.A., Caius College, Cambridge, Professor of Experimental Physics in the University of Sydney.

Received April 13, 1886.

In the course of some experiments which we have been engaged with for some time past, on the temporary increase in the volume of a rarefied gas which takes place when an electric discharge passes through it (De la Rue and Müller, “Phil. Trans.,” 1880), we found that the passage of the spark always produced permanent as well as temporary effects when the gas was nitrogen and when the pressure was less than that due to 20 mm. of mercury. The experiments described below were undertaken to clear up this point, and from them we have drawn the following conclusions:—

1. That when a succession of electric sparks of the proper kind is sent through a sealed discharge-tube containing nitrogen at a low pressure (less than 20 mm. of mercury), a permanent diminution in

the volume of the nitrogen takes place, which reaches a maximum, after which the passage of sparks of the same kind produces no permanent effect upon the volume.

2. That for nitrogen at a pressure of 8 mm. of mercury, which is the pressure at which we have usually worked, the permanent diminution in volume is from 8 to 12 per cent. of the original volume, while at a pressure of 16 mm. of mercury the diminution is not more than from 2 to 3 per cent.; thus, though there are twice as many molecules in the tube the effect is not so big.

3. The diminution in volume takes a considerable time to reach its maximum value; in our experiments, where the discharge-tubes are 1 cm. in diameter and 25 cm. long, and the sparks were produced by an induction coil giving a spark about 4 inches long in air, it took about eight hours' sparking to produce the maximum diminution.

4. That this diminution takes place equally well whether platinum or aluminium electrodes are used.

5. That the ratio of the maximum diminution to the original volume is independent of the volume of the discharge-tube and of the extent of its surface.

6. That if the tube be maintained at a temperature of over 100° C. for several hours, the gas regains its original volume.

We attribute this diminution in the volume of the gas to the formation of an allotropic modification of nitrogen.*

We now proceed to give a detailed description of the experiments and of the various parts of the apparatus.

Discharge-tube and Gauge.

We had a good deal of trouble in getting this part of the apparatus satisfactory; we found that discharge-tubes of the ordinary kind were very liable to leak round the electrodes after a series of sparks had been passed through them. The form of tube we finally adopted is represented in fig. 1. AB is a glass tube about 25 cm. in length and 1 cm. in diameter, into which the U-piece E, F, G, fused up at G, is fused, sulphuric acid or mercury is placed at the bend of this U-tube, and serves as a gauge to measure alterations in the pressure of the gas in AB. The end B dips into a vessel containing mercury, the level of which is higher than that of the part of the tube through which the electrode passes; a piece of glass tubing is placed over the top of the discharge-tube, and the interval between the tubes caulked with glass wool; the cup thus formed is filled with mercury, which reaches above the entrance of the electrode into the tube. The electrodes are

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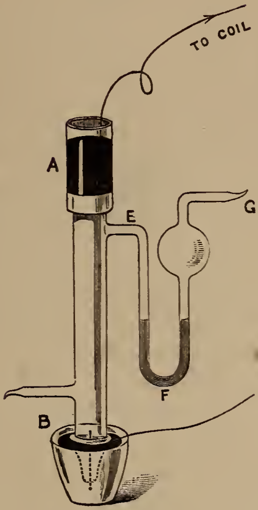
* Since this paper was sent to the Royal Society we have seen a book by Mr. Stillingfleet Johnson, entitled "Elementary Nitrogen," in which the same conclusion is come to from purely chemical reasons.

FIG. 1.

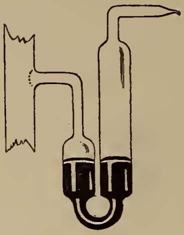


then covered with mercury, which prevents any leakage between the electrode and the tube, which we found frequently happened if this precaution was not taken. The tubes were cleaned before being used by filling them (1) with aqua regia, which was boiled in the tube, (2) with caustic potash, (3) distilled water, (4) very pure alcohol. After this they were carefully heated and dried. This was the treatment adopted for the greater part of the tubes, some of them, however, were treated with boiling sulphuric acid in addition.

The sulphuric acid in the gauge was boiled with sulphate of ammonia before being used; when the sulphate was first added to the acid, the acid became dark, but it was boiled for about half a day until it was quite colourless, and its volume about one-fourth of its original value.

The levels of the liquid in the legs of the gauge were read by a cathetometer; when the liquid was sulphuric acid the readings could be made accurately enough by placing a sheet of white paper behind the gauge and illuminating it by a gas flame. When, however, the liquid in the gauge was mercury a different course was adopted. In the first place, the gauge-tube had to be much larger to prevent mistakes arising from the sticking of the mercury to the sides of the tube. The gauge used for mercury was of the shape shown in fig. 2; the diameter of the tube where the free surface of the mercury came into contact with it was about 2 cm. A different method of reading the levels of the mercury in the legs of the gauge had also to be adopted, because it was found that when the gas flame was in front

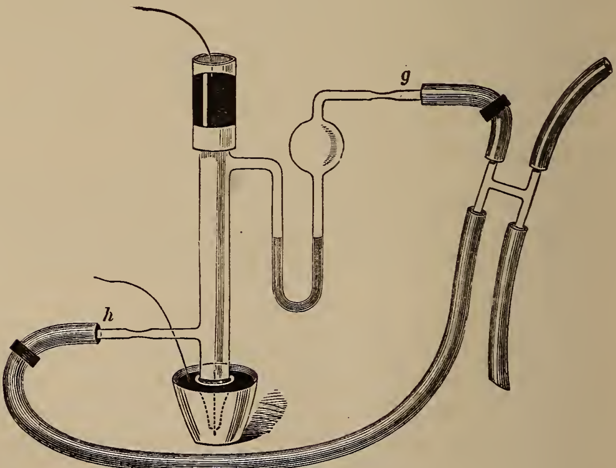
FIG. 2.



of the mercury different readings could be obtained by moving it about, the surface of the mercury was therefore illuminated from behind by a parallel and horizontal beam of light which passed through an alum cell to avoid any heating effect.

The electrodes were either platinum or aluminium, generally platinum; in some of the tubes these were fused into small pieces of glass tubing, so that only the tips of the electrodes were exposed to the nitrogen. Before being sealed the tube was connected with the pump and the gas supply in the way shown in fig. 3. After being

FIG. 3.



cleaned and dried, and the gauge filled either with sulphuric acid or mercury, the tube was pumped out and filled with nitrogen, and this process was repeated several times; when the pressure was very low the tube was heated to as high a temperature as it would stand without softening, in order to drive off any gas that might be on the surface. Then a series of sparks from six very large Leyden jars charged with a Holtz machine were sent through the tube; at first when the light produced by the sparks was examined by the spectroscope the hydrogen lines were seen to be very bright, the hydrogen presumably coming out of the electrodes; as the sparking and pumping continued the hydrogen lines diminished in brightness, and we went on sparking alternately with the Holtz and the induction coil until they had disappeared. We may mention in passing that the relative brightness of the hydrogen and nitrogen lines in a mixture of these gases is to a very large extent a question of pressure; we found that after we had gone on sparking until there were no hydrogen lines visible at a pressure of 8 mm. of mercury, if we pumped out the gas until the pressure was reduced to 2 mm., the hydrogen lines immediately reappeared, and it required a great deal more sparking to get rid of them at this pressure. The lower the pressure the more prominent were the hydrogen lines. We went on sparking until there were no hydrogen lines visible at a pressure of 8 mm., when the sparks were produced either by the Holtz or the induction coil, and until there were no hydrogen lines visible at a pressure of 2 mm., when the sparks were produced by the induction coil. We never, however, were able to satisfy ourselves that the hydrogen lines were absent when the large sparks from the Holtz were sent through the tube at this pressure, though if they were there they were only very faint. When we had reached this stage the hydrogen lines showed no tendency to reappear when fresh nitrogen was introduced into the tube, showing that the hydrogen came from the electrodes and not from damp in the nitrogen. We found more difficulty in getting the hydrogen out of aluminium terminals than out of platinum ones. When the tube had reached this stage fresh nitrogen was let in and pumped out until the pressure in the tube was the required value, generally 8 mm. of mercury; the tubes *g* and *h*, fig. 3, were then fused off, the gauges being watched all the time to see that there was no influx of air during this operation. When the tube had cooled the difference of level of the fluid in the U-tube was read by the cathetometer. It was then generally left to stand over night, and another reading taken the next day; except in the few cases when the tube had cracked, the readings were always found to be the same as those taken on the previous evening. The tube was then ready to be experimented on.

Preparation of the Nitrogen.

The nitrogen was prepared by passing air over red-hot copper. A porcelain tube about 70 cm. long was placed on a gas furnace, it was filled with copper turnings and copper gauze; during one-half of the experiments the gauze was placed at the ends and the turnings in the middle, in the other half, half the tube was filled with copper turnings and the other half with gauze. The air was sucked through a tube containing pieces of pumice moistened with potash, and through a bottle half filled with the same substance, the other end of the tube was stopped by an indiarubber cork coated with paraffin, through which a glass tube passed which conducted the nitrogen to a series of bottles and tubes. These bottles and the porcelain tube were made quite air-tight; this was tested by putting the tube through which the air passed on its way to the copper in connexion with an air-pump, and exhausting down to a pressure of about 20 mm. of mercury, even with this exhaustion there was no appreciable leak through the whole arrangement of porcelain tube, drying-tubes and bottles, discharge-tubes and connexions. The porcelain tube was connected by a piece of thick-walled indiarubber tubing with a series of bottles and tubes containing purifying reagents. After leaving the copper the nitrogen passed through a potash solution in a bottle, then through two large tubes filled with carefully prepared pumice moistened with potash, it then bubbled through sulphuric acid which had been boiled down with sulphate of ammonia to about one-fourth of its original volume, it then passed through two large U-tubes filled with phosphorous pentoxide divided up into a number of layers by asbestos plugs, it then went into a large bottle about one-fourth filled with phosphorous pentoxide. All the phosphorous pentoxide used was tested and found to be free from free phosphorus. The gas after leaving the phosphorous pentoxide bottle passed through thick-walled indiarubber tubing into the discharge-tube. The volume of the tubes and bottles was very large compared with that of the discharge-tube, and as our consumption of nitrogen was slow the gas we used had stood over the phosphorous pentoxide for several days at least and often much longer. On the other hand, the gas had only been in contact with indiarubber for a short time, for the indiarubber bungs in the bottles were all coated on the inside with paraffin, and the only long piece of tubing was that leading from the last drying bottle to the discharge-tube; gas that had stood in this for more than a few minutes was always sucked out, and was never used for filling the discharge-tube.

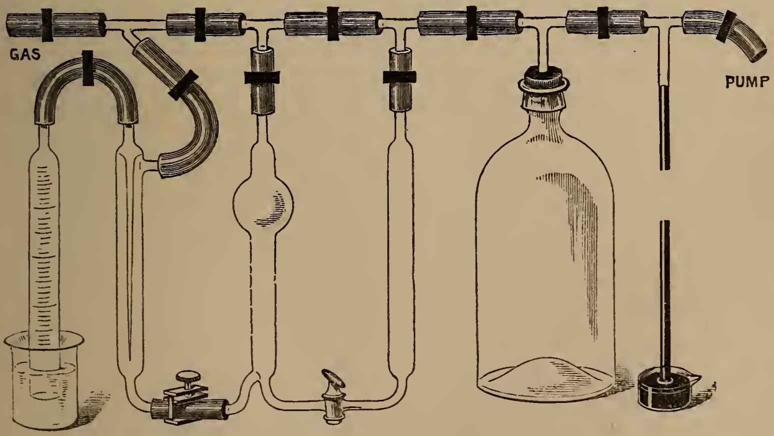
The oxide of copper formed in the tube was reduced from time to time, in most cases by passing hydrogen through the tube; the hydrogen was generally prepared by pouring sulphuric acid on zinc, but as we suspected that a trace of sulphur dioxide which we detected had its origin in this source, we endeavoured to reduce the copper oxide by electrolytically prepared hydrogen; we could not, however, produce the hydrogen fast enough in this way, and so we finally reduced the copper oxide by carbon monoxide prepared from pure formate of potassium; in this case the nitrogen showed no trace of sulphur dioxide, there was, however, no alteration in its behaviour in the discharge-tube.

We detected the trace of SO_2 by the change produced by the gas in the colour of paper soaked in a mixture of ferric chloride and ferric cyanide, the amount of it, however, must have been very small, as the gas produced no colouration in a paper soaked in a solution of iodide of potassium and starch, which is a very delicate test for sulphur dioxide. The diminution in the volume of the nitrogen which we observed could not have been due to the trace of SO_2, as it occurred when the copper oxide had been reduced by CO, and no trace of SO_2 was to be detected even by the ferric chloride and ferricyanide solution.

Determination of the Quantity of Oxygen in the Gas.

We were unable to detect any change of colour in a small quantity of a solution of pyrogallol and caustic potash when 50 c.c. of our gas was passed through into a eudiometer. It was thought desirable, however, to have a more perfect testing arrangement, and the following is a description of the form ultimately adopted.

FIG. 4.



The apparatus consists essentially of four tubes and a bottle whose volume is known; by means of connexions of indiarubber tubing, taps, and clamps, each tube can be put in communication with the nitrogen supply and with the bottle separately. The tap between C and D being closed, and the tubing temporarily removed, C is filled to about half way up the bulb with carefully boiled solution of caustic potash, and D with a solution of pyrogallol, also well boiled. The tube connexions are then replaced, and the bottle E, whose volume is large compared with that of the other parts of the apparatus, is then exhausted as far as possible by means of a water-pump. A stream of nitrogen is allowed to flow into the tubes, which are exhausted by connecting them with the bottle. After exhausting and refilling several times the tube C is left exhausted, and D in connexion with the nitrogen supply. The tap between C and D is then cautiously turned, as soon as this is down the contents of D flow over into C. As the potash solution is denser than the pyrogallol, a very perfect mixing of the fluids takes place automatically in C. As we never succeeded in getting the mixed solution colourless, it is necessary to preserve some of it as a standard for comparison. This is done by connecting B and C with the nitrogen supply together, and opening the clamp between them; the liquid then flows into B till it stands at the same level there as in C. The capacities of the tubes are arranged so that there are sensibly the same quantities of liquid in B and C, this can be done by raising or lowering B. The clamp between B and C is now closed, C put in communication with the exhausted bottle, and D in communication with the gas supply.* The pressure of the gas in the exhausted bottle is observed: suppose it is p. The tap between C and D is then opened, so as to allow a slow stream of the gas to pass from D up through C into the bottle; as soon as a sufficient deepening of the colour has taken place in the liquid in C, the tap is turned off and the new pressure p' in the bottle noted. Knowing p and p' and the capacity of the bottle, we can calculate the quantity of gas which has flowed through the liquid in C. The stream of gas passes very slowly, and it is assumed that all the oxygen it contains has been absorbed by the liquid in C. We now require to know how much oxygen is required to produce the same change in colour, this is done by comparing the colour with that of the liquid in B. A is a pipette divided into cubic centimetres, and dipping below the surface of water contained in a beaker, the top of the pipette is connected with the delivery-tube sealed into and running down B. This tube is very fine inside B, and ends in a very fine point. In order to make the comparison of colour, the upper part of the tube is exhausted, and the clamp connecting it with the pipette slowly opened, a stream of air will pass up from the pipette into B. This process is stopped when the operator judges the colour of the liquid to be the same in B and C. The level of the water around the pipette is brought to the same level as that of the water inside, and the quantity of air taken is read off. From this we can calculate how much oxygen is required to produce the same change of colour as that

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* In the diagram the bottle is at the wrong end.

produced by the gas we are testing. We found in this way that our gas *certainly* did not contain one part of oxygen in 500, and probably not one part in 1000.

The Experiments.

A tube carefully prepared and sealed off in the way we have already described was taken, and after the cathetometer readings had shown that the pressure had remained constant for several hours, it was sparked through, generally with an induction coil. In order to get the effect we are describing, it is necessary to introduce a large resistance into the circuit, for this purpose we used a piece of wetted string, this makes the discharge through the tube much less intense and the heat produced comparatively small; we were not able to get the effect when the discharge passed straight through the tube without any resistance beyond that of the tube and the connecting wires. The fact that heat restores the gas to its original condition is sufficient to account for this, for when there is only a small resistance in the circuit, the heat developed in the tube is much greater and the tube becomes very hot. We noticed a similar thing when we used a Holtz machine instead of a coil: if we charged up five large Leyden jars with the Holtz and then discharged the jars through the tube, no permanent alteration in the pressure was observed; if, however, we never allowed the jars to get fully charged, but sent a succession of small sparks through the tube, then a permanent diminution in the volume of the gas took place.

When the discharge from the coil with a piece of wet string in the circuit went through the tube, a slow diminution in the volume of the gas took place; the rate of diminution diminished as the sparking went on and ultimately the permanent volume of the gas remained unaffected by the passage of the sparks. It took, however, a considerable time to reach this state; as a rule each tube was sparked through for between three and four hours on each of three consecutive days, the diminution in the volume at the end of the first day was about two-thirds of the maximum diminution, there was a diminution of about one-half of this on the following day, and no appreciable diminution on the third day.

The gauges of the tubes used at first were filled with sulphuric acid, and after the discharge had passed through until the pressure had become steady, it was always found that the sulphuric acid in the limb of the gauge next the tube had risen towards the tube, showing that the pressure exerted by the gas in the tube had diminished. The difference of level between the legs of the gauge increased in the case of five different tubes by from 4·5 to 7 mm.; now the pressure in the tube was originally 8 mm. of mercury, or about 58 mm. of H_2SO_4, so that the diminution in the pressure exerted by the gas is from about 8 to 12 per cent. We thought at first that this diminution might be due to the combination of the nitrogen with the sulphuric acid vapour. In order to test this we had a tube made with a mercury gauge of the kind already described, the difference of level between the mercury in the two legs of the gauge was increased by 0·9 mm. of mercury by the sparking: 0·9 mm. of mercury are equivalent to about 6·5 of sulphuric acid, so that the magnitude of the effect is practically the same whether the guage be filled with mercury or sulphuric acid, and the effect therefore cannot be due to any combination of nitrogen with the vapour of sulphuric acid.

We then thought it might possibly be due to condensation of gas on the sides of the tube, though it seemed very improbable that this cause could produce such a large effect upon the pressure. To test this, however, we had a discharge-tube made whose diameter was about 3½ times that of the tubes we had previously been using, so that in this case the glass was much further away from the line joining the electrodes, and in fact was so little affected by the glass that the heating was scarcely perceptible. In this case the final result was the same as for the smaller tubes, though it took longer to arrive at a state of equilibrium; the difference between the levels of the sulphuric acid in the limbs of the gauge was increased by 4·8 mm. Another reason why the diminution in pressure can scarcely be due to this cause is that it depends very much upon the pressure of the gas. We sealed off a tube at a pressure of 16 mm. of mercury, and found that when the discharge had been sent through it until the pressure had reached a steady state, the pressure had diminished only by that due to 2·5 mm. of sulphuric acid; so that the diminution is only about half the absolute value, and therefore only one-quarter of the relative value of that which takes place at a pressure of 8 mm. of mercury. It would seem to be difficult to explain this result by the hypothesis that it is due to an adhesion of gas to the surface of the glass. The experiment with the large tube shows that it cannot be due to an absorption of gas by the electrodes, for in this case the diminution in pressure would depend upon the ratio of the volume of the electrodes to the volume of the tube, so that if we increased the volume of the tube, keeping that of the electrodes the same, the effect ought to be diminished; the experiment with the wide tube showed that it is not. We also found that the effect was not diminished by using a very long tube, about three times as long as the ordinary ones.

We next tried whether the diminution in the pressure depended on the nature of the electrodes by having a tube made with aluminium electrodes, we got, however, with this tube, the same diminution as we had previously obtained with those furnished with platinum electrodes; the tube, however, was more troublesome to prepare, as the aluminium electrodes seemed to contain more hydrogen than the platinum ones.

This result shows that the decrease in pressure is not due to the formation of a compound of nitrogen and platinum, a conclusion which is confirmed by the fact that the decrease is independent of the ratio of the volume of the electrode to that of the tube. The diminution in the pressure is too large to be explained by supposing that it is due to the formation of ammonia, which we know takes place when an electric spark passes through a mixture of nitrogen and hydrogen, for it would require at least 15 per cent. of hydrogen to be present to produce a diminution in the pressure of the gas of from 8 to 12 per cent., and we feel certain from the spectroscopic tests we have applied to the gas that the quantity of hydrogen or hydrocarbon present is extremely small, neither the hydrogen nor the hydrocarbon lines can be detected at the pressure at which we work. Again, the gas is restored to its original pressure by keeping it for some time at a temperature just above 100° C., while ammonia is not decomposed except at a much higher temperature.

The combination of nitrogen and oxygen which takes place when a spark passes through a mixture of the two gases is attended by a diminution in volume, but we have calculated a superior limit to the quantity of oxygen present, and find that it is very much too small to explain the effects which we have observed in our tubes.

It seems to us that the effect is too big to be explained as the result of an impurity in the gas, and that the only hypothesis which agrees with the facts is that we have an allotropic modification of nitrogen produced by the passage of the sparks. The formation of this is quite analogous to that of ozone from oxygen, and we have found that just as ozone is destroyed by continuous heating, so the diminution in pressure which we have observed in nitrogen is permanently destroyed if the tube be kept for some time at a temperature of 100° C.; we have not observed any tendency for the diminution in pressure to disappear as long as the tube is kept at the temperature of the room, about 15° C. The diminution we have observed seems to depend even more than the formation of ozone on the kind of spark which passes through the gas, and we are disposed to attribute partly at any rate the great differences which we have observed at different pressures to this fact, at some pressures it seems impossible to get quite the right kind of spark.

We have noticed that when the electrical discharge goes through nitrogen whose pressure has been diminished by previous sparking, it has a much greater tendency to produce a beautiful golden colour than when it passes through a new tube. Exactly (as far as we can judge) the same discharge which when it goes through a new tube will produce a bluish-pink colour, will, when it goes through an old one in which the pressure has diminished, produce a peculiar yellow colour between that of chamois leather and gold.

We have made no attempts to ascertain the chemical properties of this modified gas, and there are other points which we should have liked to develop before publishing an account of our experiments, but as one of us is leaving Cambridge for Australia it seemed advisable to publish an account of the experiments we have been able to make together.

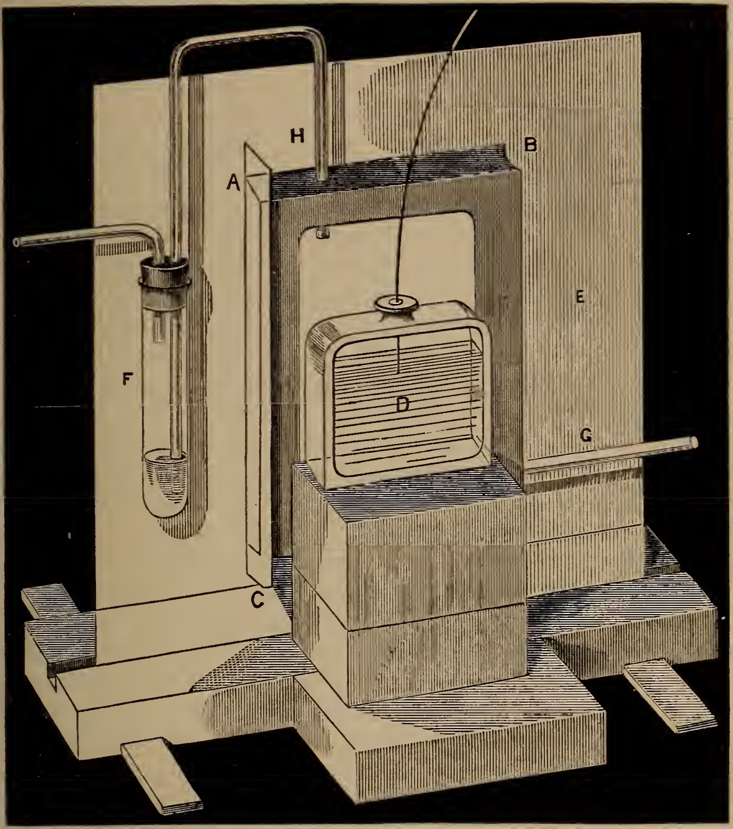
We are indebted to Mr. Robinson for advice on some chemical points, and we cannot conclude without acknowledging how much we owe to the zeal and ability of Mr. Sinclair, the Assistant at the Cavendish Laboratory, who has done much the greater part of the large quantity of glass-blowing required for this investigation.

II. "Some Experiments on the Production of Ozone." By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College, and Cavendish Professor of Experimental Physics in the University of Cambridge, and R. THRELFALL, Caius College, Cambridge, and Professor of Experimental Physics in the University of Sydney. Received May 1, 1886.

The first experiment was made in order to see whether ozone could be formed by placing oxygen in a very strong electric field, the field, however, being just not strong enough to cause sparks to pass through the gas.

This experiment finally took the following form:—ABC is a box made of flat pieces of glass about $\frac{1}{16}$th of an inch thick, fastened together with paraffin; into the box two glass tubes, G and H, are inserted, the air entering the box through G, and leaving it through H. Against one side of the box a glass bottle, D, with flat sides, is placed and filled with water containing a little sulphuric acid, this serves as one electrode; the other electrode is a blackened tin plate, E, placed against the opposite side of the box, the distance between the electrodes being an inch and a half. The two electrodes are connected with the terminals of a Holtz machine. By altering the distance between the terminals any difference of potential can be produced between the plates. When the terminals are close together all the sparks pass between them, but when they are pulled far apart the sparks flash across the box, the discharge taking the form of a great number of separate sparks from the inside of one plate to the inside of the opposite one; the appearance of the box when the discharge passes is very pretty, it looks as if several hundred bright silver nails with broad heads were connecting the insides of the box.

The air entered the box through the tube G, having previously passed through a series of tubes and bottles filled respectively with



phosphorous pentoxide, pumice moistened with sulphuric acid, and caustic potash; it was also freed from dust by passing through a tube containing a plug of cotton-wool. After passing through the box it bubbled through a test-tube, F, containing an iodide of potassium and starch solution, pieces of filter-paper moistened with this solution were also fastened to the sides of the box. We determined the most sensitive solution of potassium iodide and starch by adding a constant quantity of chlorine-water to various proportions of potassium iodide and starch; when the most sensitive solution had been determined it was always made up of this strength. We found that the papers were quite as delicate a test of ozone as the test-tube full of the solution.

When the observations were being made the whole arrangement was placed inside a large wooden box, the sides of which were blackened, the observer put his head through a hole in one of the sides of the box, and a black velvet cloth was then put over the box so that all stray light was excluded, and any spark traversing the box could easily be detected. The air was sucked through the box at the rate of about a litre in ten minutes. Before trying the experiment air was sucked through for about half an hour when the electrodes were at the same potential; but not the slightest colouration of the potassium iodide solution in the test-tube or on the pieces of paper in the box could be detected. We then adjusted the distance between the terminals of the Holtz machine so that the sparks just did not pass across the vessel, in this case the terminals of the Holtz were about 4 inches apart, so that the field was as intense as it could be without producing a discharge. Air was then sucked through for more than an hour, but not the slightest colouration could be detected in either the test-tube or the pieces of paper, though the passage of a single flash was sufficient to produce a most distinct colouration. This experiment was repeated over and over again, but always with the same result; we never found any ozone unless we had previously seen a flash across the vessel, hence we conclude that ozone is only produced when sparks pass through the oxygen.

A special experiment was made in order to estimate the delicacy of the test for ozone: to the same quantity of the solution of iodide of potassium as that through which the air bubbled on its way out from the vessel, chlorine-water was added until we could detect a discolouration. The amount of chlorine in the quantity of chlorine-water added was then determined by finding the quantity of iodine set free by 10 c.c. of it. This was done by means of some very carefully prepared solution of sodium hyposulphite, kindly made up and standardised for us by Mr. M. M. Pattison Muir. From the minimum quantity of chlorine required to produce a discolouration of the solution, we found that the smallest quantity of ozone we could detect with certainty was 0·0384 mgram. But 6 litres of air, that is 1·5 litres of oxygen, had passed slowly through the apparatus and, since no discolouration was produced, the amount of ozone formed must have been less than 0·0384 mgram., or less than 0·00016 of the whole quantity of oxygen which had passed through the apparatus. In the second experiment we took an ozonizer made of two concentric tubes, and sealed up in it air free from ozone and a large quantity of phosphorous pentoxide, this was left for three months, so that at the end of the time the air was presumably dry; on causing the electric discharge to pass through it, however, ozone was produced in large quantities, so that ozone is produced when an electric spark passes through very carefully dried oxygen.

III. "The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity (*continued*). The Effect of Change of Temperature on the Internal Friction and Torsional Elasticity of Metals." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received April 13, 1886.

(Abstract.)

The author has recently had the honour of presenting to the Society a memoir relating to the internal friction of metals when vibrating torsionally at temperatures ranging from 0° C. to 25° C. He now brings forward results which have been obtained in experiments on the effect of change of temperature on the torsional elasticity and internal friction of metals. The apparatus used and the mode of experimenting are fully described in the paper, so that it will be sufficient, perhaps, to state here that the vibration-period and the logarithmic decrement were very carefully determined at four different temperatures between 0° C. and 100° C., and that the formulæ given below were worked out by the method of least squares; these formulæ are to be found in Tables I and II.

A full account of the method adopted for eliminating the effect of the resistance of the air has been given in the previous memoir above alluded to.

Table I.

Metal.	Formula for the torsional elasticity between 0° C. and 100° C. r_t and r_0 represent the torsional elasticity at the temperatures of t° C. and 0° C. respectively.	Percentage decrease of torsional elasticity when the temperature is raised from 0° C. to 100° C.
Silver	$r_t=r_0(1-0\cdot0003769t-0\cdot0000001690t^2)$	3·938
Platinum......	$r_t=r_0(1-0\cdot00004456t-0\cdot0000002987t^2)$	0·744
Platinum-silver	$r_t=r_0(1-0\cdot0003555t+0\cdot0000005467t^2)$	3·008
Aluminium....	$r_t=r_0(1-0\cdot0005713t-0\cdot0000000109t^2)$	5·724
Zinc..........	$r_t=r_0(1-0\cdot0010800t-0\cdot0000049470t^2)$	15·747
Nickel........	$r_t=r_0(1-0\cdot0002267t-0\cdot0000003474t^2)$	2·614
Iron..........	$r_t=r_0(1-0\cdot0002442t-0\cdot0000002510t^2)$	2·693
Copper	$r_t=r_0(1-0\cdot0002472t-0\cdot0000004488t^2)$	2·921

Before the experiments, of which the results are recorded in Tables I and II, were made, the previously well annealed wires were subjected to a preliminary treatment extending over periods ranging from six

Table II.

Metal.	Formula for the logarithmic decrement due to internal friction between 0° C. and 100° C. λ_t and λ_0 represent the logarithmic decrements at t° C. and 0° C. respectively.	Percentage decrease or increase of the logarithmic decrement when the temperature is raised from 0° C. to 100° C. — signifies decrease + ,, increase.
Silver	$\lambda_t=\lambda_0(1-0\cdot01244t+0\cdot0003016t^2)$	+177·2
Platinum	$\lambda_t=\lambda_0(1-0\cdot01235t+0\cdot0001040t^2)$	− 19·5
Platinum-silver	$\lambda_t=\lambda_0(1+0\cdot01410t+0\cdot0001005t^2)$	+141·6
Aluminium....	$\lambda_t=\lambda_0(1-0\cdot00806t+0\cdot0006644t^2)$	+583·8
Zinc	$\lambda_t=\lambda_0(1+0\cdot01413t+0\cdot0007122t^2)$	+853·5
Nickel	$\lambda_t=\lambda_0(1+0\cdot00057t-0\cdot0000205t^2)$	− 14·8
Iron	$\lambda_t=\lambda_0(1-0\cdot01599t+0\cdot0000814t^2)$	− 78·4
Copper	$\lambda_t=\lambda_0(1-0\cdot01801t+0\cdot0006345t^2)$	+454·4

days to two months. This treatment consisted in repeatedly heating the wire to 100° C., and then cooling it again until the torsional elasticity and the internal friction both became constant at all the temperatures at which the wires were tested, and produced the following permanent effects :—

(*a*.) Very appreciable increase of the torsional elasticity in the case of some metals and appreciable increase of the torsional elasticity in all cases.

(*b*.) Large diminution of the internal molecular friction, the effect on the friction being considerably greater than the effect on the elasticity.

(*c*.) Very appreciable increase of the limiting amplitude beyond which the logarithmic decrement ceases to be independent of the amplitude.

From a consideration of Tables I and II it may be gathered that :—

(*d*.) The torsional elasticity of all metals is temporarily decreased by rise of temperature between the limits of 0° C. and 100° C., the amount of decrease per degree rise of temperature increasing with the temperature. To this may be added that the percentage decrease of torsional elasticity produced by a given rise of temperature is for most metals about twenty times the corresponding percentage increase of length.

(*e*.) If we start with a sufficiently low temperature the internal friction of all annealed metals is first temporarily decreased by rise of temperature and afterwards increased. The temperature of minimum internal friction is for most annealed metals between 0° C. and

100° C.; for most hard drawn wire, however, the temperature of minimum internal friction is below 0° C.

(*f.*) The temporary change, whether of the nature of increase or decrease, wrought by alteration of temperature in the internal friction of metals, is in most cases enormously greater than the corresponding change in the torsional elasticity.

IV. "On a New Means of Converting Heat Energy into Electrical Energy." By WILLIARD E. CASE, of Auburn, New York, U.S.A. Communicated by W. H. PREECE, F.R.S. Received April 14, 1886.

It was shown by M. Henri Loewel (see "The Chemist," Part VIII, p. 476) that the addition of a solution of chromous chloride to stannous chloride caused a precipitate of metallic tin, the reaction forming chromic chloride.

On heating the solution to the boiling point, 212° F., it was found the precipitated metal was in a great measure redissolved, forming the original solution, chromous chloride and stannous chloride, without the liberation of hydrogen.

On cooling this solution the tin was again precipitated, the action continuing as often as the solution was heated and cooled.

As chromous chloride has a great affinity for oxygen, it is necessary the air should be excluded from the solution, otherwise the chromous chloride would be reduced to oxychloride of chrome, as Loewel states, and the reactions would cease to take place after a time, the stannous chloride formed during each heating remaining in solution.

I constructed, in the form of a simple galvanic cell, a small element with this solution, chromic chloride,* as the electrolyte, using tin as the positive, and platinum as the negative metal.

At 60° F. this element gives no electromotive force, although in this case, when the cell was first set up, it gave 0·0048 volt, owing probably to the presence of some foreign substance.

On the elevation of its temperature by the application of heat, the electromotive force rose and fell, as indicated in the diagram; the curves A, B, C, D, E, represent its increase during the rising temperature, and the curves F, G, H, I, J, its fall while cooling.

The irregularity of the curves A, B, C, D, E, was probably due to unequal heating.

At the termination of the experiment, when the cell had cooled down to 60° F., no electromotive force was observed, as indicated on

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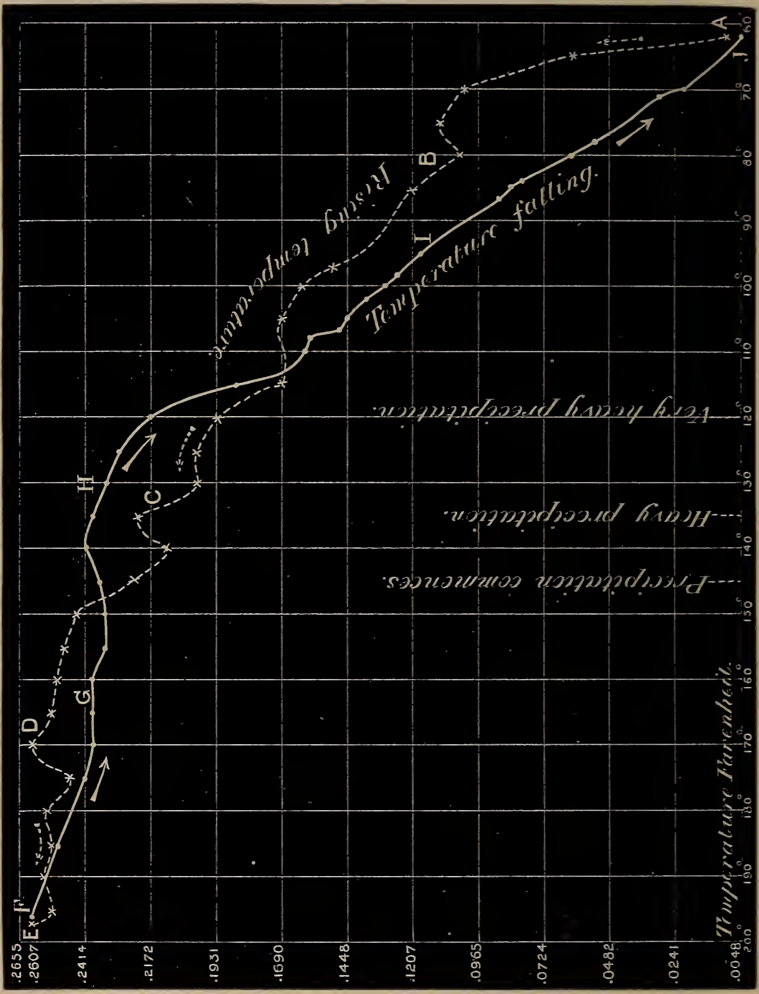
* The solution used was made by combining chromium trioxide with hydrochloric acid, and heating.



the diagram, of which the abscissæ are proportional to the electromotive forces, the ordinates to the temperatures Fahrenheit.

The highest electromotive force was 0·2607 volt at 197° F., the highest degree to which the temperature was raised.

If the platinum be replaced by a negative electrode of carbon, the electromotive force will be higher. It may be of interest to mention that the action of this element during heating is entirely different from that of the galvanic battery during a similar elevation of its temperature.

W. H. Preece, Esq., F.R.S. (see "The Effects of Temperature on the Electromotive Force and Resistance of Batteries," "Proc. Roy. Soc.," vol. 36, p. 48), states "that changes of temperature do not practically affect electromotive forces, but that they materially affect the internal resistance of cells."

When the temperature of this element was lowered to about 145° F., the reactions before mentioned took place.

The tin taken up by the solution during heating commenced to precipitate, increasing as the temperature lowered, and the metal fell to the bottom of the cell in a form to be again utilised in the generation of the current.

The amount of local action or chemical corrosion which took place above 150° F. was excessive, but the metal taken up by the solution was very much less when the temperature of the electrolyte was not raised above the point of precipitation, 140° F.

The metal taken up below this point appears to be precipitated under the same conditions as that taken up at higher temperature, and seems to be precipitated whether the circuit be open or closed.

It will be seen on the curves F, G, H, I, J, with falling temperature that the electromotive force increased between 150° F. and 140° F., this might have been due to the reactions which took place during the precipitation of the metal.

Further investigations to determine the efficiency of this element would be of interest.

V. "Further Discussion of the Sun-spot Spectra Observations made at Kensington." By J. Norman Lockyer, F.R.S. Communicated to the Royal Society by the Solar Physics Committee. Received May 5, 1886.

I have recently discussed, in a preliminary manner, the lines of several of the chemical elements most widened in the 700 spots observed at Kensington.

The period of observation commences November, 1879, and extends to August, 1885. It includes, therefore, the sun-spot curve from a minimum to a maximum and some distance beyond.

It is perhaps desirable that I should here state the way in which the observations have been made. The work, which has been chiefly done by Messrs. Lawrance and Greening, simply consists of a survey of the two regions F—*b* and *b*—D.

The most widened line in each region—not the widest line, but the *most widened*, is first noted; its wave-length being given in the observation books from Ångström's map. Next, the lines which most nearly approach the first one in widening are recorded, and so on till the positions of six lines have been noted, the wave-lengths being given from Ångström's map, for each region.

It is to be observed that these observations are made without any reference whatever to the origin of the lines; that is to say, it is no part of the observer's work to see whether there are metallic coincidences or not; this point has only been enquired into in the present reductions, that is, seven months after the last observations now discussed were made. In this way perfect absence of all bias is secured.

It may further be remarked that the number of lines widened throughout a sun-spot period is about the same, so that the conditions of observation vary very little from month to month, and from year to year.

It may be further remarked that the absolute uniformity of the results obtained in the case of each of the chemical elements investigated indicates, I think, that the observations have been thoroughly well made; and, as a matter of fact, they are not difficult.

I first give tables (A, B, C) showing that for each of the chemical elements taken—iron, nickel, and titanium—the number of lines seen in the aggregate in each hundred observations is reduced from minimum to maximum, and that this result holds good for both regions of the spectrum.

I next give another table (D) showing that during the observations the lines recorded as most widened near the maximum have not been recorded amongst metallic lines by either Ångström or Thalén, and that many of them are not among the mapped Fraunhofer lines, though some of them may exist as faint lines in the solar spectrum when the observing conditions are best.

Table A.—Iron.

Iron Lines observed in Sun-spot Spectra at Kensington among the most Widened Lines.

	4863·2	4870·2	4871·3	4875·5	4877·4	4884·2	4886·5	4888·0	4890·0	4890·6	4907·0	4909·5	4918·0	4919·8	4956·7	4981·8	4982·5	4983·5	4984·8	5004·9	5005·2	5006·5	5011·5	5014·2	5019·2	5026·2	5027·2
1st Hundred. 12th Nov., 1879, to 29th Sept., 1880.		—	—		—			—	—	—	—		—	—	—	—	—	—	—	—	—	—	—				
2nd Hundred. 29th Sept., 1880, to 15th Oct., 1881.	—	—	—	—		—	—	—	—	—			—	—	—	—	—	—	—				—	—	—	—	—
3rd Hundred. 18th Oct., 1881, to 27th June, 1882.																											
4th Hundred. 1st July, 1882, to 28th August, 1883.		—	—						—	—		—															
5th Hundred. 30th August, 1883, to 23rd June, 1884.		—	—						—				—	—													
6th Hundred. 24th June, 1884, to 12th Feb., 1885.																											
7th Hundred. 18th Feb., 1885, to 24th August, 1885.											—	—															

	5038·2	5040·1	5041·2	5047·8	5049·4	5051·0	5064·4	5068·1	5071·9	5074·0	5075·8	5077·9	5078·8	5082·2	5090·2	5096·3	5098·2	5107·0	5109·8	5121·0	5123·2	5126·5	5133·0	5136·8	5138·6	5141·8	5145·7	5150·0	5151·2	5158·5	5161·5	5165·8
1st Hundred. 12th Nov., 1879, to 29th Sept., 1880.		—	—	—	—	—	—	—	—	—	—	—	—			—	—	—		—				—	—	—	—					—
2nd Hundred. 29th Sept., 1880, to 15th Oct., 1881.	—	—	—	—	—	—		—	—	—	—	—	—	—	—		—	—	—		—			—	—	—	—	—	—	—	—	
3rd Hundred. 18th Oct., 1881, to 27th June, 1882.	—											—	—	—						—	—	—	—	—			—	—	—			—
4th Hundred. 1st July, 1882, to 28th August, 1883.		—	—									—	—	—				—									—	—		—	—	
5th Hundred. 30th August, 1883, to 23rd June, 1884.	—																						—									
6th Hundred. 24th June, 1884, to 12th Feb., 1885.												—	—										—									
7th Hundred. 18th Feb., 1885, to 24th August, 1885.																							—									

TABLE B.—NICKEL.

List of most Widened Lines observed at Kensington.

	4865·3	4872·5	4903·9	4917·6	4935·1	4979·6	4983·5	5016·8	5034·6	5079·8	5080·6	5098·5	5099·2	5114·9	5136·8	5141·8	5145·7	5155·1	5168·3	5175·6
1st hundred lines							│		│						│	│	│			
2nd hundred lines							│	│	│	│	│	│			│	│	│			
3rd hundred lines										│	│				│		│	│		
4th hundred lines										│	│						│	│		
5th hundred lines																		│		
6th hundred lines											│									
7th hundred lines																		│		

TABLE C.—TITANIUM.

List of most Widened Lines observed at Kensington.

	4869·5	4884·2	4913·2	4964·5	4981·0	5006·6	5013·3	5035·2	5035·8	5037·8	5038·0	5038·7	5052·3	5061·3	5064·4	5071·8	5086·5	5119·9	5126·6	5144·5	5147·0	5151·2
1st hundred lines					│	│		│	│						│	│						
2nd hundred lines	│	│	│	│	│		│	│	│	│		│	│	│		│	│					│
3rd hundred lines								│	│	│		│		│	│			│	│		│	│
4th hundred lines									│		│			│						│	│	
5th hundred lines										│	│									│	│	
6th hundred lines																				│		
7th hundred lines	No lines.																					

Table D.—Unknown Widened Lines observed at Kensington.

	1st Hundred.	2nd Hundred.	3rd Hundred.	4th Hundred.	5th Hundred.	6th Hundred.	7th Hundred.
4865	..	..	..	..	..	1	..
4885	..	..	..	..	..	..	1
4888·3	1	..	..	..	..	..	..
4891·8	..	..	..	..	1	..	..
4910	..	..	..	2	..	..	..
4944	..	..	1	..	..	..	..
5017·2	..	1	..	..	..	..	..
5028·9	..	1	..	..	..	..	..
5030	..	1	..	..	..	..	..
5034·8	11	..	3	..	..	..	..
5037	..	..	..	..	..	1	..
5038·9	..	..	1	1	..	..	..
5042	..	..	3	..	..	..	..
5042·3	..	..	4	..	..	..	..
5043	..	1	..	..	..	..	..
5044·6	..	3	..	..	..	..	..
5061	..	..	2	..	..	..	3
5061·5	..	..	..	..	..	..	2
5062	..	..	..	..	..	..	5
5062·4	..	..	..	..	..	2	..
5062·8	..	..	..	3	..	2	..
5065	..	..	8	..	..	..	..
5067	..	1	..	..	..	..	..
5069·5	..	1	..	..	..	..	..
5070·8	..	1	..	..	..	..	..
5077	..	..	..	1	..	..	..
5079·5	..	..	..	..	..	2	..
5080	..	..	..	1	..	..	..
5081·5	..	..	..	..	..	..	3
5082	..	..	..	..	..	2	..
5083	..	..	..	..	..	2	..
5083·3	..	1	2	3	..	..	..
5084	..	1	..	..	..	..	3
5084·5	..	..	..	..	..	..	2
5086	..	..	17	..	1	..	..
5086·8	..	1	..	..	..	..	..
5087·7	..	1	..	..	..	..	..
5088·1	..	1	..	..	..	..	..
5088·6	..	1	..	..	..	..	..
5089·0	..	..	..	1	..	..	..
5101	..	..	..	..	..	..	1
5103·5	..	..	..	..	1	..	..
5112·1	..	6	22	4	2	1	..
5115·5	..	..	..	..	..	..	9
5116	3	6	24	3	..	..	..
5116·2	..	..	7	..	..	..	..
5118	4	..	14	..	..	..	..
5127	..	..	..	1	..	..	..
5127·5	..	..	1	..	..	..	..
5128·8	..	..	..	..	..	..	1
5129·6	..	17	19	4	..	..	..
5130	..	..	1	..	..	..	..
5132	..	14	21	6	..	..	..
5132·5	..	...	1	..	..	..	

	1st Hundred.	2nd Hundred.	3rd Hundred.	4th Hundred.	5th Hundred.	6th Hundred.	7th Hundred.
5132·8	..	..	..	3	..	..	..
5133·5	..	..	1	..	1	3	17
5133·8	..	30	47	43	62	3	27
5134	..	..	..	..	12	41	10
5134·4	..	..	..	..	..	19	..
5135	..	..	..	..	16	36	11
5135·5	..	33	15	..	53	36	20
5135·8	..	..	37	52	13	2	..
5136	..	..	4	..	9	22	27
5136·5	..	..	3	1	..	..	..
5137	..	..	2	2	1	..	..
5137·5	..	..	4	..	72	79	22
5137·8	..	12	35	64	13	10	3
5138	..	..	..	..	1	..	3
5139	..	..	..	..	1	..	1
5139·4	..	1	2	3	..	..	..
5140·4	..	2	..	..	..	..	..
5142·2	13	4	..	1	..	..	..
5142·8	..	21	7	19	2	..	..
5143	..	..	..	..	..	..	20
5143·2	..	..	2	..	..	..	..
5144·2	1	3	..	2	..	..	..
5144·5	..	..	..	..	1	..	..
5145·5	..	..	..	..	..	1	..
5146	..	..	36	12	..	..	..
5146·5	..	..	..	2	..	..	..
5148	..	..	..	..	..	..	1
5148·8	..	..	1	2	..	..	..
5149	2	32	31	36	4	..	35
5149·2	..	..	..	1	..	..	..
5149·5	..	..	..	4	..	..	29
5149·8	..	8	2	8	..	8	..
5150	..	..	..	1	..	..	..
5151·8	..	..	..	..	1	..	..
5153·8	..	..	..	..	1	..	..
5154	..	..	..	..	..	..	1
5155·4	..	..	..	..	1	..	..
5156	1	12	37	74	82	91	95
5156·5	..	..	..	..	8	..	..
5157·2	..	..	..	4	..	..	..
5159	..	..	1	8	13	11	41
5159·5	1	..	31	59	80	86	57
5160	..	..	1	4	..	9	..
5160·4	..	1	..	5	..	..	4
5162	..	..	9	7	61	67	62
5162·2	1	..	23	49	21	30	..
5175	..	..	..	..	..	..	3

The reduction of the latitudes of the spots is not yet completed.

The result of these observations may be thus briefly stated. As we pass from minimum to maximum, the lines of the chemical elements gradually disappear from among those most widened, their places being taken by lines of which at present we have no terrestrial representatives. Or, to put the result another way—at the minimum period of sun-spots when we know the solar atmosphere is quietest and coolest, vapours containing the lines of some of our terrestrial elements are present in sun-spots. The vapours, however, which produce the phenomena of sun-spots at the sun-spot maximum are entirely unfamiliar to us.

The disappearance of the lines of iron, nickel, and titanium, and the appearance of unknown lines as the maximum is reached is shown by curves in fig. 1 given on the next page.

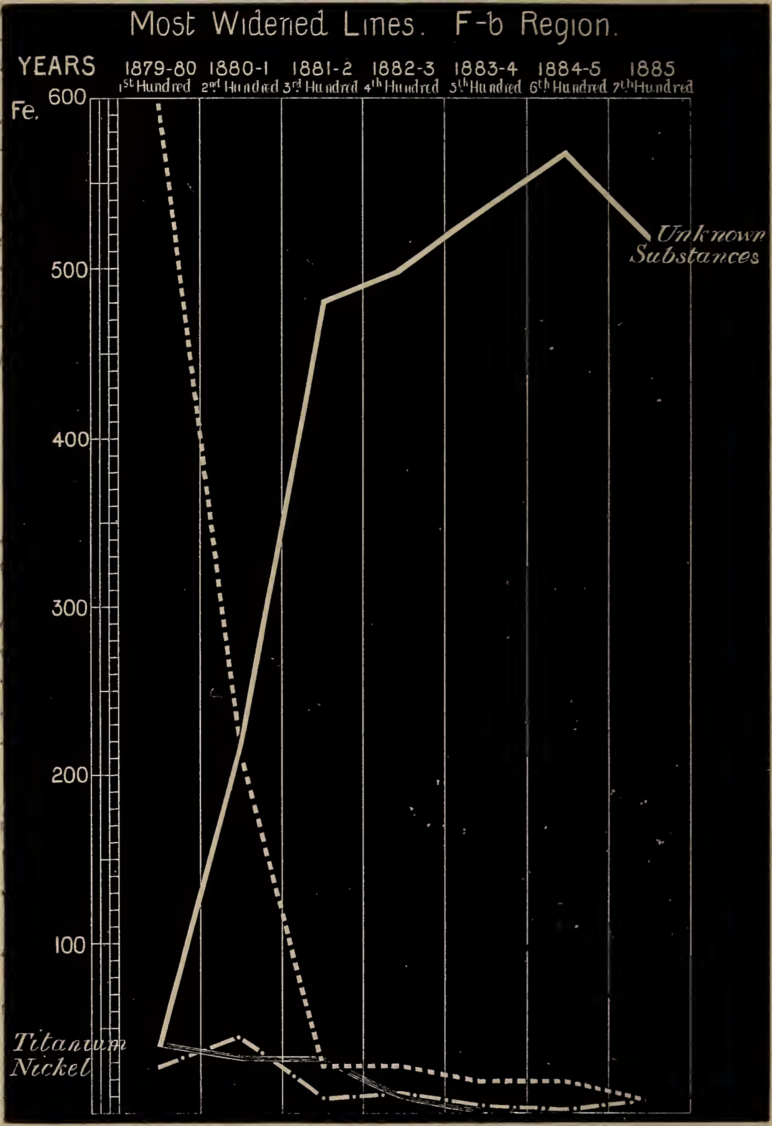
The results, in my opinion, amply justify the working hypothesis as to the construction of the solar atmosphere which I published some years ago. ("Proc. Roy. Soc.," vol. 34, p. 291.) In the region of the spectrum comprised between 4860 and 5160, I find in the case of iron, to take an instance, that sixty lines were distributed unequally among the spots in 1879 and 1880, many iron lines being visible in every spot. In the last observations, about the maximum, only three iron lines in all are seen among the most widened lines. These three lines also have been visible in four spots only out of the last hundred. The same thing happens with titanium and nickel, and with all the substances for which the reductions are finished.

I am quite content, therefore, to believe that iron, titanium, nickel, and the other substances very nearly as complex as we know them here, descend to the surface of the photosphere, in the downrush that forms a spot at the period of minimum, but that at the maximum, on the contrary, only their finest constituent atoms can reach it. It may also be remarked that these particles which survive the dissociating energies of the lower strata are not the same particles among the constituents of the chemical elements named which give the chromospheric lines recorded by Tacchini, Ricco, and myself.

Having thus found the working hypothesis to which I have referred stand the severe test which the sun-spot observations apply to it, I have gone further, and have endeavoured to extend it in two directions.

First. I found that the view to which the hypothesis directly leads, that the metallic prominences are produced by violent explosions due to sudden expansions among the cooler matters brought down to form the spots, when they reach the higher temperature at and below the photosphere level, includes all the facts I know touching spot and prominence formation. Thus, for instance, the close connexion between metallic prominences and spots; the entire absence of metallic prominences with rapid motion from any but the spot-zones; the fact that the faculæ always follow the formation of a spot and never precede it; that the faculous matter lags behind the spot as a rule; the existence of veiled spots and minor prominences in regions outside the spot-zones; the general injection of unknown substances into the

FIG. 1. Number of appearances of known and unknown lines.



lower levels of the chromosphere which I first observed in 1871, and which have been regularly recorded by the Italian observers since that time;—all these phenomena and many others which may be referred to at length on another occasion, are demanded by the hypothesis, and are simply and sufficiently explained by it.

With regard to the extensions of volume to which I have referred, I find that if we assume that metallic iron can exist in any part of the sun's atmosphere, and that it falls to the photosphere to produce a spot, the vapour produced by the fall of one million tons will give us the following volumes:—

Temperature.	Pressure.	Volume in cubic miles.
2,000° C.	380 mm.	0·8
10,000	760 „	1·8
20,000	5 atmos.	0·7
50,000	760 mm.	8·8
50,000	190 „	35·2

If we assume the molecule of iron to be dissociated ten times by successive halving, then the volume occupied will be 1024 times greater, and we shall have—

Temperature.	Pressure.	Volume in cubic miles.
50,000° C.	760 mm.	9,011
50,000	190 „	36,044

In these higher figures we certainly do seem nearer the scale on which we know solar phenomena to take place; the tremendous rending of the photosphere, upward velocities of 250 miles a second, and even higher horizontal velocities according to Peters, are much more in harmony with the figures in the second table than the first.

I may mention in connexion with this part of the subject, that the view of the great mobility of the photosphere which this hypothesis demands, so soon as we regard metallic prominences as direct effects of the fall of spot material, is further justified by the fact that if we assume the solar atmosphere, that is the part of the sun outside the photosphere, to be about half a million miles high, which I regard as a moderate estimate, the real average density of the sun is very nearly equal to one-tenth that of water, instead of being slightly greater than that of water, as stated in the text-books.*

We can then only regard the photosphere as a cloudy stratum existing in a region of not very high pressure. It is spherical because it depends upon equal temperatures.

The second direction in which I have attempted to develop the hypothesis has relation to the circulation in the sun's atmosphere. I have taken the facts of the solar atmosphere as a whole, as they are recorded for us in the various photographs taken during eclipses since

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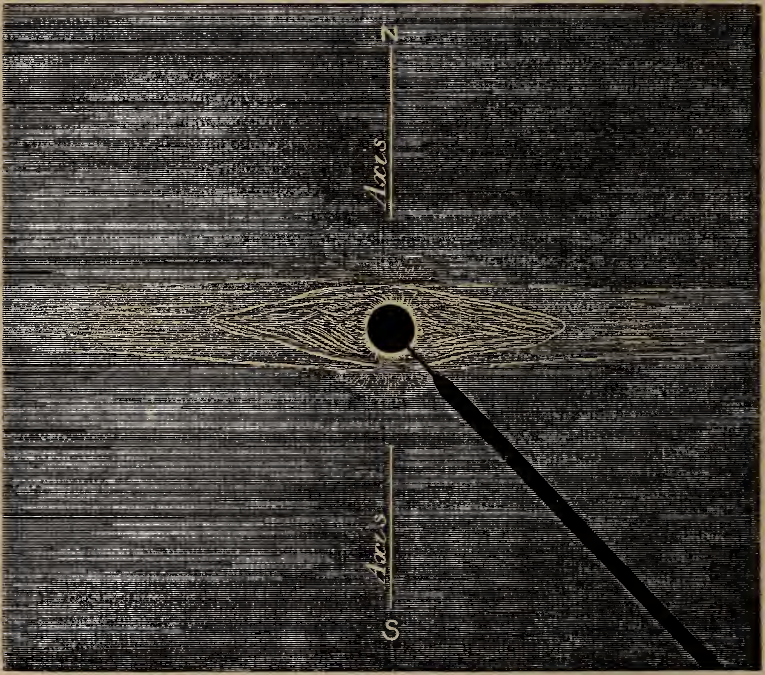
* The density referred to water = 1·444 and to the earth 0·255, according to Newcomb.

FIG. 2. MINIMUM.

Tracing of Newcomb's observation of 1878, the brighter portion of corona being hidden by a screen. Shows the equatorial extension and concentric atmospheres.

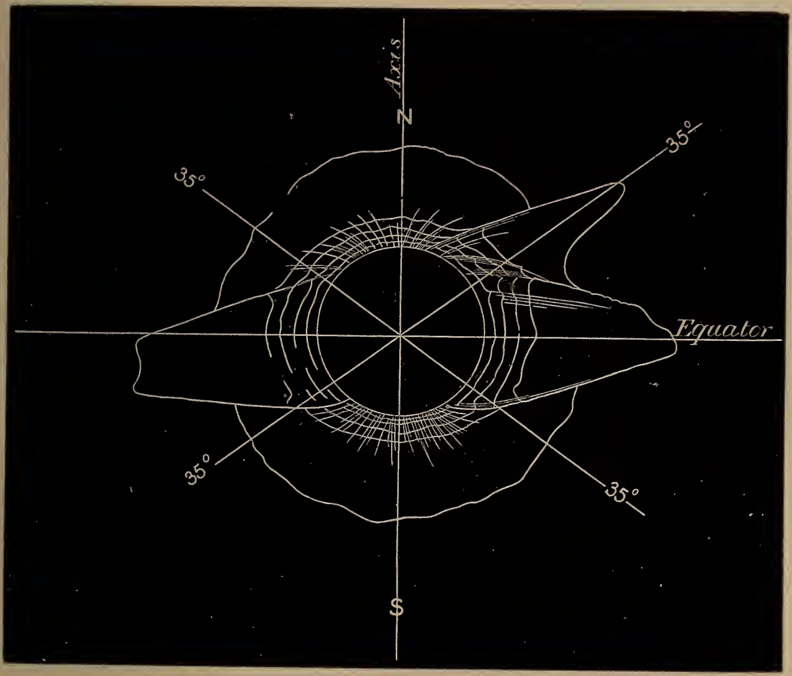


1871, and also in drawings made before that time, the drawings being read in the light afforded by these photographs.

I find that the working hypothesis at once suggests to us that the sun-spot period is a direct effect of the atmospheric circulation, and that the latitudes at which the spots commence to form at the minimum, which they occupy chiefly at the maximum, and at which they die out at the end of one period in one hemisphere, probably at the moment they commence to form a second one in the other (as happened in 1878—9), are a direct result of the local heating produced by the fall of matter from above descending to the photosphere, and perhaps piercing it. The results of this piercing are, the liberation of heat from below, and various explosive effects due to increase of volume, which, acting along the line of least resistance, give, as a return current, incandescent vapours ascending at a rate which may be taken as a maximum at 250 miles a second, a velocity sufficient to carry them to very considerable heights.

FIG. 3. MINIMUM.

Tracing of the results obtained by the cameras in 1878, showing inner portion of equatorial extension, and how the surfaces of it cut the concentric atmosphere in lat. 35 N. and S., or thereabouts.



The view of the solar circulation at which I have arrived may be briefly stated as follows :—

There are upper outflows from the poles towards the equatorial regions. In these outflows a particle constantly travels, so that its latitude decreases and its height increases, so that the true solar atmosphere resembles the flattened globe in Plateau's experiment (see photographs, 1878, and fig. 3).

These currents, as they exist in the higher regions of the atmosphere, carry and gather the condensing and condensed materials till at last they meet over the equator.

There is evidence to show that they probably extend as solar meteoric masses far beyond the limits of the true atmosphere, and form a ring, the section of which widens towards the sun, and the base of which lies well within the boundary of the atmosphere (fig. 2).

If we assume such a ring under absolutely stable conditions, there will be no disturbance, no fall of material, therefore there will be no spots, and therefore again there will be no prominences. Such was the state of things on the southern surface of the ring from December, 1877, to April, 1879, during which period there was not a single spot observed the umbra of which was over 15-millionths of the sun's visible hemisphere.

Assume a disturbance. This may arise from collisions, and these collisions would be most likely to happen among the particles where the surface of the ring meets the current from the poles. These particles will fall towards the sun, thereby disturbing and arresting the motion of other particles nearer the photosphere, and finally they will descend with a crash on to the photosphere, from that point where the surface of the ring enters the atmosphere, some distance further down.

The American photographs in 1878 supply us with ample evidence that this will be somewhere about latitude 30°, and here alone will the first spots be formed for two reasons.

(1.) In the central plane of the ring over the equator, the particles will be more numerous, a rapid descent, therefore, in this central plane will be impossible, for the reason that the condensed matter has to fall perhaps a million of miles through strata of increasing temperature; there will, therefore, be no spots; and practically speaking, as is known, there are no spots at the equator, though there are many small spots without umbræ between latitudes 3° and 6° N. and S.

Above latitude 30°, as a rule, we have no spots, because there is no ring, and further the atmosphere is of lower elevation, so that there is not sufficient height of fall to give the velocities required to bring down the material in the solid form.

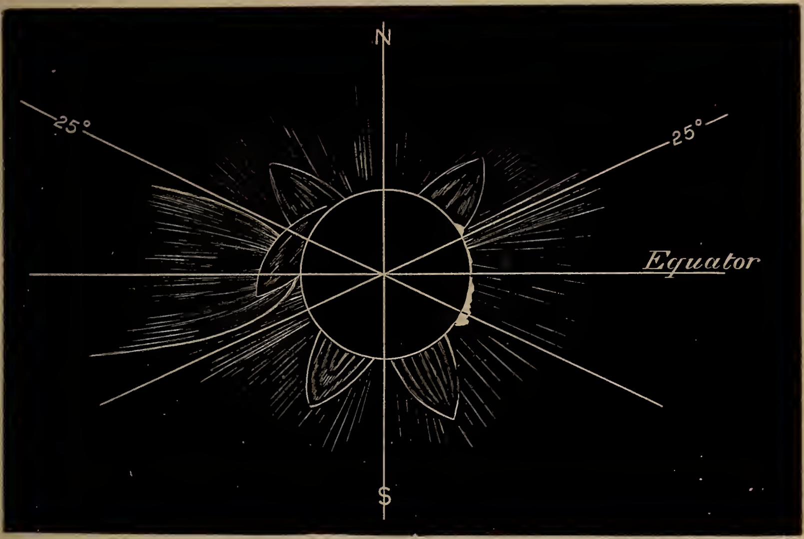
The lower corona where the corona is high, and it is highest over the equator, acts as a shield or buffer, volatilisation and dissociation take place at higher levels. Where this occurs, spots are replaced by a gentle rain of fine particles slowly descending, instead of the fall of mighty masses and large quantities of solid and liquid material.

Volatilisation will take place gradually during the descent, and at the utmost only a veiled spot will be produced.

We know that when the solar forces are weak, such a descent is taking place all over the sun, because at that time the spectrum of the corona, instead of being chiefly that of hydrogen, is one of a most complex nature, so complex that before 1882 it was regarded by everybody as a pure continuous spectrum, such as is given by the limelight.

The moment the fall of spot material begins we get the return current in the shape of active metallic prominences, and the production of cones and horns which probably represent the highest states of incandescence over large areas and extending to great heights; and, besides these, the production of streamers. See fig. 4.

Fig. 4. 1½ years from maximum, 1858. Tracing of drawing by Liais, showing "cones."



Two results follow:—

1. In consequence of the increased temperature of the lower regions, the velocity of the lower currents towards the poles, and therefore of the upper currents from the poles, is enormously increased. The disturbance of the ring will therefore be increased.

2. Violent uprushes of the heated photospheric gases, mounting with an initial velocity of a million miles an hour, can also disturb the ring directly.

In this way the sudden rise to maximum in the sun-spot curve, and the lowering of the latitude of the spots, follow as a matter of course. And the part of the ring nearest the sun, its base, so to speak, is, it would appear, thrown out of all shape, and we get falls over broad belts of latitude N. and S.

Does this hypothesis explain then the slow descent to minimum and the still decreasing latitude? It does more, it demands it. For now the atmosphere over those regions where the spots have hitherto been formed is so highly heated, and its height is so increased, that any disturbed material descending through it will be volatilised before it can reach the photosphere.

The best chance that descending particles have now to form spots, is, if they fall from points in lower latitudes. The final period, therefore, of the sun-spot curve must be restricted to a very large extent to latitudes very near the equator, and this is the fact also, as is well known.

It will be seen that on this view, as the brightness and therefore the temperature of the atmosphere as we know increases very considerably from minimum to maximum, the masses which can survive this temperature must fall from gradually increasing heights.

It may be pointed out, how perfectly this hypothesis explains the chemical facts observed and associates them with those gathered in other fields of enquiry.

At the minimum the ring is nearest the sun, the subjacent atmosphere is low and relatively cool.

Particles falling from the ring therefore, although they fall in smaller quantity because the disturbance is small, have the best chance of reaching the photosphere in the same condition as they leave the ring, hence at this time the widening in many familiar lines of iron, nickel, titanium, &c.

The gradual disappearance of these lines from the period of minimum to that of maximum, is simply and sufficiently explained by the view that the spot-forming materials fall through gradually increasing depths of an atmosphere which at the same time is having its temperature as gradually increased by the result of the action I have before indicated, until finally when the maximum is reached, if we assume dissociation to take place at a higher level at the maximum, dissociation will take place before the vapours reach the photosphere, and the lines which we know in our laboratories will cease to be visible.

This is exactly what takes place, and this result can be connected as I have stated elsewhere, with another of a different kind. This hypothetical increasing height of fall demanded by the chemistry of the spots is accompanied by a known acceleration of spot movement over the sun's disk, as we lower the latitude—which can only be explained so far as I can see by a gradually increasing height of fall as the equator is approached.

There are two other points. (1.) The sunspot curve teaches us that the slowing down of the solar activities at the maximum is very gradual. We should expect therefore the chemical conditions at the maximum to be maintained for some time afterwards. As a matter of fact they have been maintained till March of the present year, and only now is a change taking place which shows us chemically that we are leaving the maximum conditions behind. (2.) The disappearance of the lines of the metallic elements at maximum is so intimately connected with an enormous increase in the indications of the presence of hydrogen, that there is little doubt we are in the presence of cause and effect. The hydrogen, I am now prepared to believe, is a direct consequence of the dissociation of the metallic elements.

It will be convenient to refer here to the facts which have been recorded during those eclipses which have been observed at the sun-spot minimum and maximum.

At the minimum the corona is dim; observations made during the minimum of 1878 showed that it was only $\frac{1}{7}$ as bright as the corona at the preceding maximum. There are no bright lines in its spectrum, and both photographic and eye observations proved it to consist mainly of a ring round the equator, gradually tapering towards its outer edge, which some observations placed at a distance of twelve diameters of the sun from the sun's centre.

The same extension was observed in the previous minimum in 1867, and the polar phenomena were observed to be identical in both eclipses. At the poles there is an exquisite tracery curved in opposite directions, consisting of plumes or *panaches*, which bend gently and symmetrically from the axis, getting more and more inclined to it, so that those in latitudes 80° to 70° start nearly at right angles to the axis, and their upper portions droop gracefully, and curve over into lower latitudes.

Although indications of the existence of this ring have not been recorded during eclipses which have happened at the period of maximum, there was distinct evidence both in the eclipses in 1871 and 1875 of the existence of what I regard as the indications of outward upper polar currents observed at minimum.

The fact that the solar poles were closed at the maximum of 1882, while they were open in 1871, is one of the arguments which may be urged that at times the whole spot-zones are surmounted by streamers, with their bases lying in all longitudes along the zones.

It was probably the considerable extension of these streamers earthwards, in 1882, which hid the finer special details at the poles, while in 1871, the part of the sun turned towards the earth was not rich in streamers of sufficient extension.

Touching these streamers, it is an important fact to be borne in mind, that no spots ever form on the poleward side of them.

It is obvious, therefore, that spots are not produced by the condensation of materials on their upper surfaces, for in that case the spots would be produced indifferently on either side of them, and the width of the spot-zones would be inordinately increased.

Although in the foregoing I have laid stress upon the indications afforded by the observations of 1878 of the existence of a ring, it should be remarked that, so far, the eclipse appearances on which the idea rests have not been observed at maximum. This, however, is not a fatal objection, because precautions for shielding the eye were necessary even in 1878 when the corona was dim; and if it is composed merely of cooled material it would not readily be photographed.

It may be urged by some that the phenomena observed in 1878 may only after all have been equatorial streamers.

It is obvious, therefore, that this point deserves the closest attention during future eclipses, until it is settled one way or the other.

May 13, 1886.

Professor G. G. STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Dr. Walter Lawry Buller (elected 1879) was admitted into the Society.

The following Papers were read:—

I. "On the Structure of Mucous Salivary Glands." By J. N. Langley, M.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. Received April 14, 1886.

The cells of mucous salivary glands I have previously described as consisting of a framework or network, containing in its spaces hyaline substance and granules.* I have also mentioned that during secretion the granules disappear from the outer portions of the cells.† A similar disappearance of granules has been found by Biedermann‡ to occur in the cells of the mucous glands of the tongue of the frog.

The granules of the mucous salivary glands are rendered very distinct by irrigating a mounted specimen of a fresh gland with moderately dilute solutions of neutral or alkaline salts. I have generally used sodium chloride solution 5 per cent., and sodium carbonate solution 3 per cent. In these fluids the granules can scarcely be distinguished from small fat globules; those of the submaxillary gland of the dog have a diameter of 1 to 2 μ, those of the orbital gland of the dog are a little larger.

In the resting gland the granules are fairly closely packed throughout the cell, in a line stretching from basement membrane to lumen

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* "Proc. Camb. Philos. Soc.," vol. v, p. 25, 1883, and "Internat. Jour. Anat. and Histol.," vol. i, p. 69, 1884.

† "Jour. of Physiol.," vol. ii, p. 276, 1879.

‡ "Wien. Sitzungsb.," Bd. 86, Abt. iii, p. 67, 1882.

there are 8 to 12 granules. After a time the outlines of the granules become indistinct; this takes place much sooner in alkaline than in neutral salt solution.

The reactions of the granules are best observed by irrigating pieces of gland which have been teased out in neutral salt solution. On irrigating with dilute alkalis, dilute mineral acids, or with water, the granules disappear as if they were bubbles bursting. It is difficult, however, to be certain that they are completely dissolved; after treatment with dilute mineral acids, and still more frequently after treatment with water, pale, very slightly refractive masses are seen, apparently consisting of swollen and altered granules. On irrigating with osmic acid the granules swell up considerably, and become less refractive. On irrigating with alcohol or with acetic acid they remain, but are somewhat shrunken.

The hyaline substance of the cells swells up and in part dissolves in 3 per cent. sodium carbonate. The solution forms a viscid fluid; on irrigating with acetic acid or with alcohol a membranous precipitate of mucin takes place. Since the granules are not, for a time at any rate, dissolved by sodium carbonate, it follows that the hyaline substance gives rise to mucin. The granules also give rise to mucin; in most of their reactions they resemble mucin; on solution they form a viscid fluid; further, when a gland is hardened in alcohol, and a section mounted in Canada balsam or in glycerine, the granules in the hyaline substance are usually indistinguishable, both together form the clear mucigen portion of the cells.

During secretion both the hyaline substance and the granules are turned out of the cells; after prolonged secretion the cells consist of an outer zone, chiefly of freshly formed substance, and of an inner zone of network, hyaline substance, and granules, as in the resting state. When the saliva has a high percentage of solids, both the hyaline substance and the granules can be seen in it; such saliva is obtained from the submaxillary gland of the dog by stimulating the sympathetic, and often by strong stimulation of the chorda tympani. The hyaline substance is more soluble than are the granules, and is thus less commonly seen; it is partly dissolved, partly swollen up into a continuous mass; the less swollen parts appear as strings or blebs. The granules in saliva vary greatly in appearance; they may be very slightly swollen, and have fairly sharp outlines; or they may be more swollen and run together, forming pale masses of various size; occasionally in more dilute saliva they are just visible as pale spheres; these are probably the spheres mentioned by Heidenhain,* as seen by him in the submaxillary saliva of the dog after combined stimulation of the chorda tympani and sympathetic nerves.

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* "Studien des Physiol. Institute zu Breslau," p. 46, 1868.

Hence then, when a small amount of fluid only is secreted, the hyaline substance and the granules are turned out of the cells without being completely dissolved, when a certain amount more of fluid is secreted the hyaline substance is completely dissolved, and with still more fluid, the granules also are completely dissolved. Some small fat globules are usually turned out of the cells during secretion.

According to Heidenhain,* nerve stimulation causes some constituents of the cell to be converted into a more soluble form, this is usually expressed by saying that mucigen is converted into mucin. Apart from the reasons given by Heidenhain, this is probable, since both hyaline substance and granules appear to be more soluble in osmic acid and in chromic acid when they are in saliva than when they are in the gland cells; but the proof does not seem to me to be conclusive.

Klein† has described the mucous cells as being open towards the lumen, in this I am inclined to agree with him; it is not easy to see in all cells, but in many it is perfectly distinct.

Although I think that the mucous cells are able to turn out bodily their products, I am unable to agree with the view of Heidenhain‡ and of Lavdowsky,§ that the cells disintegrate during secretion. As the decrease in the interfibrillar substance takes place, there is a fresh formation of substance in the outer part of the cells, *i.e.*, as the cell secretes it also grows: the changes which take place are closely comparable to the changes which take place in the pancreas and in other glands, in which there is no question of the disintegration of cells. Moreover, in saliva I can find no evidence of broken down cells; when the cells of a fresh gland are treated with osmic acid the cell membrane becomes very distinct, when sympathetic saliva is treated with osmic acid no signs of cell membrane are found; nor are nuclei present except those in "salivary corpuscles," which, as stated by Pflüger,‖ are leucocytes.

Further, there is not, I think, any satisfactory proof that the demilune cells multiply during secretion, and give rise to mucous cells. I have examined glands at various times after stimulation of the chorda and of the sympathetic, and have not, except extremely rarely, found nuclei undergoing indirect division. As I have previously said,¶ I hold the demilunes to be secreting cells of a different nature from that of the mucous cells, for in different glands all variations are found between glands wholly "albuminous" and

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* *Loc. cit.*, p. 108.

† "Quart. Journ. Micr. Science," vol. xix, p. 151, 1879.

‡ *Loc. cit.*

§ Max Schultze's "Archiv," Bd. xiii, p. 281, 1877.

‖ Stricker's "Histology" (translated by Power), vol. i, p. 454.

¶ "Trans. Internat. Med. Congress," 1880.

glands wholly mucous. Glands with demilunes are simply glands in which the "albuminous" element is reduced to a minimum. The apparent increase in size of the demilunes, described by Lavdowsky* as taking place in the first stage of secretory activity, I take to be due to the decrease in the size of the alveoli, so that the ordinarily flat demilunes become more spherical. Moreover, the demilune cells show signs of secretory activity; in the submaxillary gland of the dog after prolonged secretion the demilune cells, in section of the gland hardened in alcohol, are smaller, they stain more readily with carmine, and their nuclei and nucleoli are more conspicuous. The "young" cells described by Heidenhain and by Lavdowsky are, I think, chiefly altered mucous cells.

The network of the cell consists of two parts, one in the cell-membrane, the other stretching from this throughout the cell. The peripheral network is best seen in the isolated cells of the orbital gland of the dog after treatment with chloral hydrate, 2 per cent., for a week to a fortnight. It consists of very delicate fibres; at some of the nodal points there are small spherical swellings. From lumen to basement membrane there are twelve to fifteen meshes. In many cases this network is perfectly distinct, every fibre in it can be followed without the slightest difficulty. In such specimens, on the other hand, it is often difficult or impossible to make out any cell-membrane. That a membrane exists I conclude chiefly from observing cells isolated in sodium chloride, 5 per cent., and then treated with osmic acid. In such specimens the outline of the cells although beaded appears to be continuous. The beading of cell-membrane has been noticed by Schiefferdecker;† it is obvious with most methods of treatment, it is caused by the fibres of the network, seen in optical section.

The internal network is connected with the peripheral network, but it appears to me to have much larger meshes. From basement membrane to lumen there are in the submaxillary gland of the dog four to six meshes, in the orbital gland of the dog five to seven meshes, *i.e.*, the number of meshes in a given direction in the cell is about half that of the number of granules. This network is seen on treating with dilute mineral acids fresh cells which have been teased out in sodium chloride, 5 per cent.; it is seen more or less distinctly in cells treated with the ordinary dissociating agents, and is seen after hardening in various reagents. The reagent which I have found to give most constantly satisfactory results, is a mixture containing 0·3 per cent. of chromic acid and 0·1 per cent. of osmic acid.

The network which I have described above as the limiting network

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* *Loc. cit.*

† Max. Schultze's "Archiv," Bd. xxiii, p. 382, 1884.

very closely resembles that described by Klein,* as shown by mucous cells after treatment with spirit or with a mixture of chromic acid and spirit. I cannot, however, find a network with such close meshes beneath the limiting membrane. The passage from the close-meshed limiting network to the wide-meshed internal network can often be traced with a good lens, such as Powell and Lealand's $\frac{1}{12}$ oil-immersion, with angular aperture 1·45.

With certain modes of treatment the cell network is not seen, thus when a piece of gland is hardened in osmic acid and subsequently with alcohol, the cell usually appears to consist of faint granules imbedded in the cell-substance. In such cases the hyaline substance and the network are indistinguishable, and the two together may stain and leave the granules unstained. At any one focus the stained substance will then appear as a close network, and the unstained granules as the meshes of the network. On careful focussing, however, it can be seen that the stained substance is simply the mass of the cell in which the granules are imbedded. This is, I think, the explanation of the close network described by Schiefferdecker† and by Paulsen‡ in certain mucous cells. And that the "network" described by Schiefferdecker is in part the hyaline interfibrillar substance of the cell is indicated by his account of it; according to him it consists of mucigen.

The sublingual gland differs in various respects from other mucous glands; a considerable portion of it consists of "albuminous" cells. According to Klein§ no demilunes are present and the gland tubes have only one layer of cells. This is certainly true of the larger part of the gland; the appearance of two layers of cells in a tube is occasionally caused by the section passing obliquely through a spot where a side tube is given off or where the lumen suddenly alters its calibre. But whilst none of the tubes have a complete double layer of cells, it is I think an open question whether demilunes are absent from the gland. The sublingual gland has been taken as an especially favourable one in which to observe the disintegration of the mucous cells. I do not find that there is any more evidence of disintegration here than there is in ordinary mucous glands. The mucous cells undergo the same changes as do these in the submaxillary of the dog, they discharge hyaline substance and granules, and they form fresh cell-substance. Secretion does not cause any division of nuclei. The "albuminous" cells probably secrete on nerve-stimulation as do the mucous cells; in speaking of these cells as "albuminous" cells I only follow the ordinary usage according to which a secreting cell which is

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* "Quart. Journ. Micr. Science," vol. xix, p. 125, 1879; vol. xxi, p. 154, 1882.

† *Loc. cit.*

‡ Max Schultze's "Archiv," Bd. xxvi, p. 307, 1885.

§ "Quart. Journ. Micr. Science," vol. xxi, p. 175, 1882.

granular after a certain mode of treatment is said to be albuminous. It is perfectly possible that such a cell should secrete a substance which is more allied to mucin than to albumin. We do not yet know enough about the chemical characters of the bodies intermediate between proteid and mucin to make any dogmatic statement on this head.

A fuller account of the points dealt with in this paper will shortly be published in the "Journal of Physiology."

II. "On the Computation of the Harmonic Components, &c." By Lieut.-General STRACHEY, R.E., C.S.I., F.R.S. Received April 15, 1886.

(Abstract.)

The object of this paper is to propose a method of computing the harmonic components of formulæ to represent the daily and yearly variations of atmospheric temperature and pressure, or other recurring phenomena, which is less laborious than the ordinary method, though practically not involving sensibly larger probable errors.

According to the usual method the most probable values of the harmonic coefficients are found by solving the equations of condition supplied from the hourly or other periodical observations, by the method of least squares. The number of these equations is, however, much larger than the number of unknown quantities, when these are limited, as is usual, to the coefficients of the first four orders, and the numerical values of the coefficients of those quantities which depend on a series of sines of multiple arcs, afford peculiar facilities for the eliminating process, so that values of the harmonic coefficients may be obtained by applying certain multipliers to combinations of the original observations obtained by a series of additions and subtractions, the results giving probable errors virtually the same as those got by the method of least squares. These multipliers for the two first orders of coefficients are so nearly equal to $\frac{2}{30}$, and for the third order so nearly 0·07, that the values may readily be found without tables, though such tables have been calculated to facilitate computations.

Approximate methods of determining the coefficients and of the components for each interval of the series, are also given, from which last a graphical representation of the components may easily be obtained.

The system of computation is applicable to all cases in which the angular intervals between the observations are such as to make the circle a whole series, exactly divisible by 6 and 8, and it has been extended, by aid of an interpolation, to the case of the 73 five-day means of a yearly period, in which the calculation by the ordinary

method would be so laborious as to be impracticable in most circumstances.

Tabular forms have been prepared by help of which the computations of the coefficients in the form $p\cos\theta$, $q\sin\theta$, may conveniently be carried out, with a minimum of arithmetical labour; also for obtaining the coefficients P and the angle C in the form $\mathrm{P}\sin(\theta+\mathrm{C})$; and the method of correcting the coefficients, as computed from the observed quantities, for any non-periodic variation between the commencement and end of the series is likewise indicated.

III. "On the Sympathetic Vibrations of Jets." By CHICHESTER A. BELL, M.B. Communicated by Prof. A. W. WILLIAMSON, F.R.S. Received April 28, 1886.

(Abstract.)

After a brief historical notice of the observations of Savart, Masson, Sondhauss, Kundt, Laconte, Barret and Tyndall, Decharme, and Neyreneuf, on the sympathetic vibrations of jets and flames, the author describes his own experiments. Attention was directed to the subject by the accidental observation that a pulsating air-jet directed against a flame caused the latter to emit a musical sound. The pitch of this sound depended solely on the rapidity of the jet pulsations, but its intensity was found to increase in a remarkable way with the distance of the flame from the orifice. In order to study the phenomenon, air was allowed to escape against the flame from a small orifice in the diaphragm of an ordinary telephone, the chamber behind the diaphragm being placed in communication with a reservoir of air under gentle pressure. Vibratory motions being then excited in the diaphragm, by means of a battery and a microphone or rheotome in a distant apartment, the discovery was made that speech as well as musical and other sounds could be quite loudly reproduced from the flame. Certain observations led the author to suspect that motion of the orifice rather than compression of the air in the chamber was the chief agent in the phenomenon; and, in fact, precisely similar results were obtained when a light glass jet-tube was cemented to a soft iron armature, mounted on a spring in front of the telephone magnet.

Experiment also showed that an air-jet at suitable pressure directed against a flame repeats all sounds or words uttered in the neighbourhood. Except, however, where the impressed vibrations do not differ widely in pitch from the normal vibrations of the jet (discovered by Sondhauss and Masson), these effects are likely to escape notice, owing to the inability of the ear to distinguish between the disturbing sounds and their echo-like reproduction from the flame.

In these experiments the primary action of the impressed vibrations was undoubtedly exerted on the air-jet; but a singular and perplexing fact was that no sound, or at best very faint sounds, could be heard from the latter when the flame was removed, and the ear or the end of a wide tube connected with the ear, was substituted for it. Suspecting, finally, that the changes in the jet, effective in producing sound from the flame, must be relative changes of different parts of it, the author was led to try a *very small* hearing orifice, about as large as the jet orifice. The results were most striking. By introducing this little hearing orifice into the path of a vibrating air-jet, the vibrations can be heard over a very wide area. Close to the jet orifice they are so faint as to be scarcely audible; but they increase in intensity in a remarkable way as the hearing orifice is moved away along the axis of the jet, and reach their maximum at a certain distance. Experiments with smoked air showed that this point of maximum sound is that at which the jet loses its rod-like character, and expands rapidly; it has been named the "breaking point," because just beyond it the sounds heard from the jet acquire a broken or rattling character, and at a greater distance are completely lost. The distance of the breaking point from the orifice diminishes as the intensity of the disturbing vibrations is increased, and also depends to some extent on their pitch, and on the velocity of the jet. With orifices of 1 to $1\frac{1}{2}$ mm. in diameter it usually varies from 1 to 6 cm. The vibrations of an air-jet may also be heard at points not situated on the axis; but they are always most intense along the axis, and become rapidly fainter as the distance from it increases.

With glass jet and hearing tubes, and a light gas bag to serve as reservoir, these experiments are easily repeated; but simple apparatus for more careful experiments is described. The author's general conclusions from his experiments and those of others are as follows:—

A jet of air at moderate pressure (below 10 mm. of water) from an orifice 1 to $1\frac{1}{2}$ mm. in diameter, forms a continuous column for a certain distance, beyond which it expands and becomes confused.

Any impulse, such as a tap on the jet support, or a short and sharp sound, causes a minute disturbance to start from the orifice. This disturbance increases in area as it progresses, and finally causes the jet to break. By directing the jet against a flame or a hearing orifice it is readily perceived that such disturbances travel along the jet path with a velocity which is not that of sound in air. In fact, the sound heard in the ear-piece resembles an echo of the disturbing sound.

The disturbances produced by sounds of different pitch travel along the jet path with the same velocity. This is evident; since otherwise accurate reproduction of the complex vibrations of speech at a distance from the orifice would be impossible. This velocity is much less than that of sound in air, and is probably the mean velocity of the stream.

A vibrating air-jet playing into free air gives rise to very feeble sounds, but these sounds are much intensified when the jet impinges on any obstacle which serves to divide it into two parts. Of such arrangements the best is a perforated surface, the orifice being placed in the axis of the jet.

A jet of air at low pressure responds to and reproduces only sounds of low pitch. Sounds above a certain pitch, which depends on the pressure, either do not affect it or are only faintly reproduced.

At pressures between 10 and 12 mm. of water an air-jet reproduces all the tones of the speaking voice, and those usually employed in music, with the exception of very shrill or hissing noises. When the pressure in the reservoir equals about 13 mm. of water, hissing sounds are well reproduced, while sounds of low pitch become fainter. At higher pressures, up to about 25 mm. of water, shrill or hissing noises produce very violent disturbance, while ordinary speech tones have little effect. But at these pressures sounds of high pitch frequently cause the jet to emit lower sounds of which they are harmonics.

In general a pressure of about 12 mm. of water will be found most suitable for reproducing speech or music. Under this condition the jet is very sensitive to disturbances of all kinds, and will reproduce speech, music, and the irregular sounds classified as "noises."

It must be understood that the pressures here given are only suitable for jets of not too small diameter. When the diameter of the orifice is only a small fraction of a millimetre the above limits may be much exceeded; since the velocity of efflux no longer depends solely on the pressure.

A jet of air escaping from a perfectly circular orifice does not vibrate spontaneously so as to emit a musical sound. But musical vibrations may be excited in it by the passage of the air on its way to the orifice through a resonant cavity, or through any irregular constriction.

An air-jet impinging on any obstacle, such as a flame, frequently vibrates spontaneously, if the obstacle is at sufficient distance and of such a nature as to diffuse the disturbances produced by impact or throw them back on the orifice. This constitutes one of the chief objections to the use of a flame as a means of rendering audible the vibrations of a jet. The disturbances excited in the surrounding air by the impact of the stream upon it are so intense as easily to react on the orifice. When, therefore, the jet is thrown into any state of vibration it tends to continue in the same state, even after the exciting sound has ceased.

A jet of air usually responds most energetically to some particular tone or set of related tones (Sondhauss). Such a particular tone may be called the jet fundamental. The practical inconvenience arising from this may be diminished by raising the air-pressure until the jet fundamental is higher than any of the tones to be reproduced.

When a flame and an air-jet meet at right angles vibrations impressed upon the flame orifice also yield sound. The conditions of pressure, &c., are somewhat different; but the changes produced at the orifice grow in the same way as those in an air-jet. The best results are obtained when a gentle current of air is directed from a *wide* tube just below the apex of the blue zone.

It is difficult at first sight to account for the fact that a vibrating jet gives rise to sound only when it strikes upon some object which divides it into two parts. The following experiments, however, in some sense explain this. The relative normal velocity at different points in the stream may be measured by introducing into its path the open end of a capillary tube which is connected with a water manometer. This velocity diminishes continuously along the axis from the orifice to the breaking point; and also diminishes continuously from any point of the axis outwards towards the circumference. Now a sudden disturbance communicated to the air at the orifice will be found to produce a *fall* in velocity along the axis of the jet, but a *rise* in velocity along its extreme outer portions. It thus appears that the changes along the axis and along the circumference, produced by a disturbance, are of opposite character. When the jet plays into free air these opposing changes neutralise each other in the main; but this interference is prevented when the jet strikes upon any object which serves to divide it.

When a vibrating air-jet plays against a small flame, the best sounds are heard when the stream strikes the flame just below the apex of the blue zone. At the plane of contact an intensely blue flame ring appears, and this ring vibrates visibly when the jet is disturbed. The production of sound from it doubtless depends on changes in the rate of combustion of the gas. This may be proved by inserting into the ring a fine slip of platinum, connected in circuit with a battery and a telephone. When the jet is thrown into vibration the consequent variations in the temperature of the platinum affect its conductivity, and hence a feeble reproduction of the jet vibration may be heard in the telephone.

To Savart we are mainly indebted for our knowledge of the sympathetic vibrations of liquid jets. This physicist showed that a liquid jet always tends to separate into drops at a distance from the orifice in a regular manner; and that this tendency is so well marked, that when the jet strikes upon any object, such as a stretched membrane, so arranged that the disturbances caused by impact may be conducted back to the orifice, a definite musical sound is produced. The pitch of the sound, or the number of drops separated in a given time, varies directly as the square root of the height of liquid in the reservoir, and inversely as the diameter of the orifice. Savart further showed that external vibrations impressed upon the orifice may act like the impact disturbances, and cause the jet to divide into drops. Impact on a stretched membrane may then cause the reproduction as sound of the impressed vibrations. The tones capable of producing this effect were considered to lie within the limits of an octave below and a fifth above the jet normal.

The author has found, however, that jets of every mobile liquid are capable of responding to and reproducing all sounds whose pitch is below that of the jet normal, as well as some above; and that the timbre or quality of the impressed vibrations is also preserved, provided that the jet is at such pressure as to be capable of responding to all the overtones which confer this quality. Other essential conditions for perfect reproduction are, that the receiving membrane should be placed at such distance from the orifice that the jet never breaks into drops above its surface; and that it should be insulated as carefully as possible from the orifice.

In order to assist the action of aërial sound-waves on the fluid, it is advisable to attach the jet-tube rigidly to a pine sound-board about $\frac{3}{8}$ths of an inch thick. The surfaces of the board should be free, otherwise it may be supported in any way. The receiving membrane is formed by a piece of thin sheet rubber, tied over the end of a brass tube about $\frac{3}{8}$ths of an inch in internal diameter. A wide flexible hearing tube furnished with an ear-piece is attached to the brass tube. The jet-tube is connected with an elevated reservoir by an india-rubber pipe.

With an apparatus of this kind, and a tolerably wide jet-tube having an orifice about 0·7 mm. in diameter, a pressure of about 15 decimetres of water is required to bring the jet into condition to respond to all the tones and overtones of the speaking voice (except hissing sounds) and those employed in music. At a somewhat higher pressure it will reproduce hissing sounds. It is not easy for an untrained ear to distinguish between the disturbing sounds and their reproduction by the jet, when both are within range of hearing. Vibrations may however be conveyed to a jet from a distance in a fairly satisfactory way, by attaching one end of a thin cord to the jet-support, and the other to the centre of a parchment drum. The cord being stretched, an assistant may speak, sing, or whistle, to the distant drum. Other devices for conveying vibrations from a distance are described.

Now when the jet is disturbed in any way, and the receiving membrane is introduced into its path close to the orifice, scarcely any sound can be heard in the ear-piece. But if the membrane be moved away from the orifice along the path of the jet the sounds become gradually louder, until at a certain distance (which varies both with the character of the orifice and the intensity of the impressed vibrations) a position of maximum purity and loudness is reached. At greater distances the reproduction by the jet becomes at first rattling and harsh, and finally unintelligible. In the latter case the jet will be seen to break above the membrane.

From this experiment we may draw the conclusions previously arrived at for air-jets; viz., that all changes produced by sound at the orifice grow in accordance with the same law; and that all changes travel with the same velocity, which is probably the mean velocity of the stream.

The mode in which the jet acts upon the membrane becomes apparent when instantaneous shadow photographs of vibrating jets are examined. When the jet is steady, and the orifice strictly circular and well insulated, the outline in the upper part of the stream is that of a slightly conical rod, the base of the cone being at the orifice. When, however, vibrations are impressed upon the support, swellings and constrictions appear on the surface of the rod, which become more pronounced as the fluid travels downwards. At the breaking point the constrictions give way, those due to the more energetic sound impulses being the first to break. When the impressed vibrations are complex, the outline of the jet may be very complicated. When the membrane is interposed, we have then a constantly changing mass of liquid hurled against it, and vibratory movements are therefore excited in it, proportional to the varying cross section of the jet at its surface.

It would appear at first sight that the mode of growth of the vibratory changes in a liquid jet must be different from that which characterises the vibrations of an air-jet. It is possible, however, by special arrangements, to receive the impact of only a small section of a vibrating liquid jet, and thus to get a reproduction of its vibrations as sound. We are thus led to conclude that the sound effects of a vibrating liquid jet may not be simply due to its varying cross section, since actual changes occur in the translation- or rotation-velocity of its particles. Experiment shows that these changes are greatest along the axis of the jet.

One of the most interesting and beautiful methods of studying the vibrations of a jet consists in placing some portion of it in circuit with a battery and telephone, whereby its vibrations become audible in the telephone. A number of forms of apparatus for this purpose have been constructed, but one will serve as a type. Savart in the course of his experiments showed that the vibrations of the jet are preserved in the "nappe" or thin sheet of fluid formed when the jet strikes normally on a small surface. So far then as vibratory changes are concerned, the nappe has all the properties of the main stream. Although the diameter of this excessively thin film is about the same whatever be the distance of the surface from the orifice, the intensity of the vibratory changes propagated to it varies with this distance, as for the jet itself. It is simply necessary then to insert into the nappe two platinum electrodes in circuit with a telephone and a battery having an electromotive force of from 12 to 30 volts, to get an accurate and faithful reproduction of the jet vibrations. Loud sounds can thus be obtained from a jet which is finer than the finest needle, and the arrangement constitutes a highly sensitive "transmitter."

A jet transmitter, in its simplest form, consists essentially of a glass jet-tube which is rigidly attached to a sound-board, and supplied from an elevated reservoir containing some conducting liquid (distilled water acidified with $\frac{1}{300}$th of its volume of *pure* sulphuric acid is the best); and a couple of platinum electrodes, embedded in an insulator, such as ebonite, against which the jet strikes. The jet may issue from a circular orifice, about $\frac{1}{4}$th of a millimetre in diameter, in the blunt and thin-sided end of a small glass tube. Much smaller jets may be used: but for one of the given size the pressure required for distinct transmission of all kinds of sounds will not exceed 30 inches. The receiving surface is the rounded end of an ebonite rod, through the centre of which passes a platinum wire. The upper end of the rod should be about 1 millimetre in diameter, and should be surrounded by a little tube of platinum; and the end of the central wire and the upper margin of the tube should form a continuous slightly convex surface with the ebonite, free from irregularities. The inner and outer platinum electrodes are joined respectively to the terminals of the circuit. The jet is allowed to strike on the end of the central wire, and thence radiating in the form of a nappe, comes into contact with the tube, thus completing the circuit. The dimensions of the apparatus may be varied to suit jets of different sizes; it is highly desirable, however, that the jet nappe should well overlap the inner margin of the ring-shaped electrode.

With small jets the impact disturbances are so feeble that slight precautions are necessary to insulate the receiving surface from the orifice, unless the former is placed low down in the path. The strength of battery may be increased until the escape of electrolytic gas-bubbles causes a faint hissing noise in the telephone. The liquid on its way to the jet should pass downwards through a wide tube lightly packed with coarse clean cotton, by which minute air-bubbles which violently disturb the jet, and small particles of dust which might obstruct the orifice, are stopped. This tube should never be allowed to empty itself.

Experiments are given to show that in this instrument the jet may act upon the electric current in two ways: firstly, by interposing a constantly changing liquid resistance between the electrodes; and, secondly, by causing changes in the so-called "polarisation" of the electrodes. In one form of instrument, namely, that in which both jet and electrodes are entirely immersed in a mass of liquid of the same kind as the jet liquid, the action must be entirely at the surface of the electrodes.

In the latter case a liquid jet becomes similar in structure and properties to a jet of air in air, and the velocity at different points when it is steady and when it is disturbed varies in precisely the manner already described.

The author briefly passes in review the leading facts to be accounted for, and lays stress upon the parallelism of the properties of gaseous and liquid jets. Some shadow photographs of vibrating smoke jets have shown that these also present drop-like swellings and contractions which grow along the jet-path. The most satisfactory explanation of the phenomena will then be one which refers the vibratory changes in jets of both kinds to the same origin.

The beautiful and well-known experiments of Plateau have supplied a satisfactory explanation of the normal vibrations of a liquid jet in air. He has shown that a stationary liquid cylinder, whose length exceeds a certain multiple of its diameter, must break up under the influence of the "forces of figure" into shorter cylinders of definite length, which when liberated tend to contract into drops. Now, the jet being regarded as such a stationary cylinder, we have a satisfactory explanation of the musical tone resulting when its discontinuous part strikes upon a stretched membrane, and when the impact disturbances may be in any way conducted back to the orifice. These disturbances then accelerate the division of the jet after it leaves the orifice. Plateau endeavoured to show that division of the jet might take place at other than the normal points, thus explaining Savart's experimental conclusion that a jet can vibrate in sympathy with a limited range of tones. Lord Rayleigh, moreover, has recently shown that the inferior limit of this range is not so sharply defined theoretically as Savart's experiments would prove it to be.

Both Savart and Magnus, however, describe experiments in which a water-jet, carefully protected from impact and other disturbances, does not exhibit the peculiar appearances characteristic of rhythmical division; and the author's experiments conclusively prove that this rhythmical division does not take place in a well insulated jet. While the tendency so to divide may therefore be admitted, and the normal rate of vibration of the jet and its greater sensitiveness to particular tones may thereby be explained, Plateau's theory cannot be held to account for the uniform growth along the jet-path of all changes, however complex their form. For this growth takes place independently of the "forces of figure," and under conditions in which they are entirely absent, as when a gaseous or liquid jet plays within a mass of fluid of its own kind.

The author is inclined rather to refer the properties of jets of all kinds to conditions of motion on which hitherto little stress has been laid, viz., the unequal velocities at different points in the stream after it has left the orifice. From the axis towards the circumference of a jet near the orifice the velocity diminishes continuously, and the motions of the stream may be regarded as resultants of the motions of an infinite series of parallel and coaxial vortex-rings. In many respects, in fact, the appearance of a jet resembles the appearance of a vortex-ring projected from the same orifice. Thus a jet from a circular orifice, like a vortex-ring from a round aperture, remains always circular. In a frictionless fluid a vortex-ring, uninfluenced by other vortices, would remain of constant diameter; a condition to which a horizontal liquid jet approximates. When, however, the ring moves through a viscous fluid it experiences retardation and expansion, which are precisely the changes which a jet playing in a fluid of its own kind undergoes. The vibrating smoke-ring projected from an elliptical aperture changes its form in exactly the same manner as a jet, at sufficiently low pressure, from an elliptical orifice. These analogies might be considerably extended.

In a liquid jet in air, or in a vacuum, internal friction must gradually equalise the velocities. At a distance from the orifice, therefore, depending on the viscosity of the liquid, such a jet must approach the condition of a cylinder at rest, and must tend to divide in accordance with Plateau's law. The *rapidity* with which drops are formed depends mainly on the superficial tension of the liquid. The length of the continuous column should therefore bear some inverse ratio to the viscosity and superficial tension of the liquid; a view which is in harmony with the results of Savart's experiments, and some of the author's, in this direction.

Where the jet plays into a fluid of its own kind the retardation and expansion which it experiences are mainly due to its parting with its energy to the surrounding medium. When, as a result of vibration, growing swellings and contractions are formed in it, this loss must be more rapid, and the jet therefore shows a diminution of mean velocity along the axis, which increases with the distance from the orifice.

Such being the conditions, it is evident that any impulse communicated to the fluid, either behind or external to the orifice, or to the orifice itself, must alter the vorticity of the stream. That vortex-rings are generated by impulses of the first kind is well known; the action when the orifice is moved is intelligible, if we consider that a forward motion of it will produce acceleration, a backward motion retardation, of the outer layers of the jet. As the result of a rapid to-and-fro motion we may then imagine two vortex-rings to be developed—the foremost layer of greater energy and moving more slowly than the hindmost. These two rings in their onward course will then act on each other in a known manner; the first will grow in size and energy at the expense of the second, at the same time diminishing in velocity; the second will contract while its velocity increases. The inequalities in cross-section, initiated at the orifice, thus tend to grow along the jet-path, and will be attended also by growing inequalities of the normal and rotational velocities of the particles. Since the stream lines of a vortex-ring are crowded together at its centre, the disturbances produced by impact of the jet-rings will be greatest along the axis, and least along the circumference.

Indeed the sound disturbances produced by impact of a common vortex-ring are quite analogous to those of a vibrating jet. Let an air-ring be projected into a trumpet-shaped tube connected with the ear, and little more than a rushing noise will result. But let it be projected against a *small* orifice in the hearing-tube, and a sharp click will be heard at the moment of impact. This click is loud when the centre of the ring strikes the tube, but faint, although still of the same character, when produced from the circumference.

The foregoing considerations may be extended to cases in which the motions of the orifice are complex vibrations. Expansions and contractions are then initiated in the fluid proportional at every point to the velocity of the orifice. The inequalities must tend to further diverge in the manner described.

Similar considerations apply to cases in which the motions of the orifice are the result of lateral impulses. In these cases the rings formed in the jet will not be perpendicular to its direction, and in their onward course may possibly vibrate about a mean position.

The author further points out how the viscosity and surface-tension of the fluid may influence its sensitiveness. When the surface-tension is very high, as in mercury, it produces a tendency in the jet to break easily under the influence of moderate impulses.

The foregoing is little more than the outlines of a new theory of jet-vibrations. The author hopes to supply in the future further experimental evidence in support of it.

IV. "Intensity of Radiation through Turbid Media." By Captain ABNEY, R.E., F.R.S., and Major-General FESTING, R.E. Received May 3, 1886.

In the Bakerian Lecture for this year, which was delivered before the Royal Society on March 4th, on Colour Photometry, we gave incidentally the results of some measurements we had made of the intensity of visible radiation which penetrated through a transparent medium as compared with that which penetrated through the same medium rendered turbid. We showed that the formula deduced by Lord Rayleigh from the scattering of light by small particles* was confirmed by our experiments. We thought, however, that the theory might be more fully tested if a larger range of spectrum than that to which we had confined ourselves were used, and at the same time it would be more satisfactory if an instrument possessing no personal equation could be utilised. Our thoughts naturally turned to the thermopile, and more particularly to that form which we described in a previous communication to the Royal Society ("Proc. Roy. Soc.," vol. 37, p. 157, 1884), since its delicacy was extremely great. In the Bakerian Lecture the results we gave were obtained from water rendered turbid, and it appeared to us that the same medium would again answer our purpose—more especially if certain precautions were taken. It will be in the recollection of the Society that we have shown that water possesses very definite absorption-bands in the infra-red of the spectrum ("Proc. Roy. Soc.," vol. 35, p. 328, 1883), as also does alcohol. Now, as the turbidity of the water we desired to produce was made by adding a dilute solution of mastic dissolved in alcohol to the water, it was evident that in addition to the water-spectrum we should also have superposed a faint alcohol spectrum, if the addition of the latter was made in any quantity. Had the spectrum of a definite thickness of pure and transparent water been compared with one of the same thickness of water rendered turbid by the mastic, it is evident that a discrepancy might have arisen owing to alcohol being present in the one case and not in the other. To avoid this difficulty, when great turbidity was to be produced, the amount of alcohol added with the mastic was measured, and the same quantity of pure alcohol added to the transparent water. By this plan the mixture of alcohol and water was the same in the two cases, the only difference in the two being that one had extremely fine particles of mastic suspended in it. A reference to our paper ("Proc. Roy. Soc.," vol. 35, p. 328, 1883) will show that a small thickness of water cuts off nearly all the spectrum

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* "Phil. Mag.," vol. xli, p. 447, 1871.

below λ14,000, and that $1\frac{3}{4}$ inches cuts off nearly everything below λ10,000. It was therefore determined to use a thickness of $\frac{1}{2}$ inch, as up to λ14,000 in that case the deflections of the galvanometer would be sufficiently large in both cases to allow the proportion of rays transmitted through the transparent and turbid media to be calculated without fear of any grave error due to inaccuracy of reading.

The spectroscope we have described was used for this purpose, and the source of light used was the crater of the positive pole of the electric light. The opening of the slit was about $\frac{1}{200}$ inch, and that of the linear pile about $\frac{1}{50}$ inch. In the first experiment we described the colour of the crater of the positive pole as seen after passing through the turbid medium was a lemon-yellow, and in the second a deep orange, nearly approaching red. It will be seen that theory and experiment agree within the limits that might be expected.

λ.	$\frac{1}{\lambda^4}$.	Thermopile readings.		Observed.	Calculated.*
		Turbid.	Clear.	$\frac{\text{Clear.}}{\text{Turbid.}}$	$\frac{\text{Clear.}}{\text{Turbid.}}$
		First Experiment.			
524	14·03	3·25	14	4·31	4·20
558	10·60	6·5	20·5	3·15	3·15
609	7·72	12	29·5	2·47	2·42
652	5·29	20	38	1·90	1·91
684	4·58	27	48	1·77	1·77
720	3·72	38·5	63	1·63	1·63
762	2·97	45	68	1·51	1·50
813	2·29	54·5	77	1·41	1·41
877	1·68	64	85	1·33	1·33
960	1·18	58	73	1·26	1·26
1070	0·76	72	85	1·19	1·21
1170	0·53	39·5	45	1·14	1·12
		Second Experiment.			
591	8·24	2	35	17·5	17·75
636	6·10	5	46·5	9·30	9·18
663	4·28	12·5	66	5·28	5·25
774	2·82	27·5	89	3·24	3·35
877	1·67	46	108	2·35	2·35
960	1·18	47·5	99	2·03	2·02
1040	0·855	55	101·5	1·84	1·83
1130	0·613	53	89	1·68	1·68
1230	0·437	28	48	1·60	1·62
1320	0·329	13	20	1·54	1·56

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* These were calculated by the formula $I' = I\epsilon^{-hx\lambda^{-4}}$, using the method of least squares.

Other measurements have been made, and which give results also in accordance with the theory.

It may be well to remark that in order to get a proper fineness of particle in suspension in the water, a very convenient plan is to add the varnish very gently and by very small quantities in a glass jar in which the water is automatically or otherwise stirred. The water should be of large volume to get the best results. To make the fineness still more uniform we have prepared the turbid medium as above, and then rotated the glass flask in which it was placed at the rate of about 2500 revolutions per minute. By this means any particles, except the very finest, are deposited on the sides of the flask, and the filtered liquid—if we may so call it—can be poured off ready for any experiments.

May 20, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Relation of 'Transfer-resistance' to the Molecular Weight and Chemical Composition of Electrolytes." By G. GORE, LL.D., F.R.S. Received May 5, 1886.

(Abstract.)

In the full paper the author first describes the method he employed for measuring the "resistance," and then gives the numerical results of the measurements in the form of a series of tables.

He took a number of groups of chemically related acids and salts of considerable degrees of purity, all of them in the proportions of their equivalent weights, and dissolved in equal and sufficient quantities of water to form quite dilute solutions. The number of solutions was about seventy, and included those of hydriodic, hydrobromic, hydrochloric, hydrofluoric, nitric, and sulphuric acids; the iodides, bromides, chlorides, fluorides, hydrates, carbonates, nitrates, and sulphates of ammonium, cæsium, rubidium, potassium, sodium, and lithium; the chlorides, hydrates, and nitrates of barium, strontium, and calcium; and a series of stronger solutions, of equivalent strength to each other, of the chlorides of hydrogen, ammonium,

rubidium, potassium, sodium, lithium, barium, strontium, and calcium. A series of similar liquids to those of one of the groups of acids, of equal (not of equivalent) strength to each other, was also included.

As electrodes he employed pairs of plates of zinc, cadmium, lead, tin, iron, nickel, copper, silver, gold, palladium, and platinum; and separate ones formed of small bars of iridium.

He took each group of solutions, and measured in each liquid separately at atmospheric temperature, the "total resistance" at the two electrodes, and the separate "resistances" at the anode and cathode respectively with each metal, and thus obtained about seventy different tables, each containing about thirty-six measurements, including the amounts of "total," "anode," and "cathode resistance" of each metal, and the averages of these for all the metals.

By comparing the numbers thus obtained, and by general logical analysis of the whole of the results, he has arrived at various conclusions, of which the following are the most important:—The phenomenon of "transfer-resistance" appears to be a new physical relation of the atomic weights, attended by inseparable electrolytic and other concomitants (one of which is liberation of heat, "Phil. Mag.," 1886, vol. xxi, p. 130). *In the chemical groups of substances examined, it varied in magnitude inversely as the atomic weights of the constituents, both electropositive and electronegative, of the electrolyte, independently of all other circumstances;* and in consequence of being largely diminished by corrosion of the electrodes it appears to be intimately related to "surface-tension." He suggests that corrosion may be a consequence and not the cause of small "transfer-resistance." The strongest evidence of the existence of the above general law was obtained with liquids and electrodes with which there was the least corrosion and the least formation of undissolved films; those liquids were dilute alkali chlorides, with electrodes of platinum.

The research is an extension of a former one on "Transfer-resistance in Electrolytic and Voltaic Cells," communicated to the Royal Society, March 2, 1885. Further evidence on the same subject has been published by the author in the "Phil. Mag.," 1886, vol. xxi, pp. 130, 145, 249.

II. "A Study of the Thermal Properties of Ethyl Oxide." By WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, D.Sc. Received May 5, 1886.

(Abstract.)

A year ago, a paper was communicated to the Society on the behaviour of ethyl alcohol when heated. A similar study of the properties of ether has been made, in which numerical values have been obtained exhibiting the expansion of the liquid, the pressure of the vapour, and the compressibility of the substance in the gaseous and liquid conditions; and from these results, the densities of the saturated vapour and the heats of vaporisation have been deduced. The temperature range of these observations is from −18° to 223° C.

It is the authors' intention to consider in full the relations of the properties of alcohol and ether; in the meantime it may be stated that the saturated vapour of ether, like that of alcohol, possesses an abnormal density, increasing with rise of temperature and corresponding rise of pressure; that at 0° the vapour-density is still abnormal, but appears to be approaching a normal state; and that the apparent critical temperature of ether is 194·0°C.; the critical pressure very nearly 27,060 mm. = 35·61 atmospheres; and the volume of 1 gram of the substance at 184° between 3·60 and 4 c.c.

III. "On the Working of the Harmonic Analyser at the Meteorological Office." By Robert H. Scott, F.R.S., and Richard H. Curtis, F.R. Met. Soc. Received May 6, 1886.

On the 9th of May, 1878, Sir W. Thomson exhibited to the Society a model of an integrating machine, which consisted of a series of five of the disk, globe, and cylinder integrators, which had been devised two years earlier by his brother Prof. James Thomson, and a description of which will be found in the "Proceedings of the Royal Society," vol. xxiv, p. 262. Sir W. Thomson's paper describing this model will be found in vol. xxvii of the "Proceedings," p. 371; and reference should be made to both these papers for an explanation of the principle of the machine. In the communication last named it is stated that the machine was about to be "handed over to the Meteorological Office, to be brought immediately into practical work."

The model was received at the Office in the course of the month, and was at once set in action; the results of the preliminary trials, when obtained, being referred to a Committee consisting of the late Prof. H. J. S. Smith and Prof. Stokes, who, on the 5th of July following, submitted to the Meteorological Council a favourable report on the performance of the model.

The Council at once resolved to have a machine constructed, which should be specially adapted to the requirements of the work for which it was intended, viz., the analysis of photographic thermograms and barograms.

In preparing a working design for actual execution, it was found necessary to make several modifications in the details of the mechanical arrangements of Sir W. Thomson's original model, and these were mainly worked out by Prof. Stokes and Mr. de la Rue. Plans were obtained from two firms of mechanical engineers, and those of Mr. Munro being ultimately adopted, the construction of the instrument was entrusted to him. It was considered sufficient to limit the action of the machine so as to extend only to the determination of the mean, and the coefficients as far as those of the third order, in the expression

$$E = a + a_1 \cos\theta + b_1 \sin\theta + a_2 \cos 2\theta + b_2 \sin 2\theta + a_3 \cos 3\theta + b_3 \sin 3\theta + \&c.,$$

and to obtain these it was necessary to have seven sets of spheres, disks, and cylinders.

A description of the machine, as actually constructed, was published in "Engineering" for December 17th, 1880, and we are indebted to the proprietors of that journal for permission to reproduce the engravings which illustrate that description, as well as a portion of the text, which we now proceed to quote :—

"The machine is shown in the accompanying engravings, figs. 1 to 3; figs. 2 and 3 showing details of the arrangements of the ball, disk, and cylinder. In principle it is, of course, precisely similar to its predecessor—differing from it only in constructive details intended to secure stability, and accuracy in its movements. Instead of being largely made of wood, as was the case with the model, it is entirely of metal; the cast-iron frames carrying the disks being secured to a firm iron bed supported by two substantial uprights; the disks themselves are of gun-metal, and the spheres of steel carefully turned, and nickel-plated to prevent rusting; the horizontal bar carrying the forks for moving the spheres is also of steel and plated, and is carried above the disks upon five iron uprights or guides. The forks are provided with adjusting screws allowing of very accurate centering of the spheres upon the disks, and adjusting screws are likewise provided for the frames carrying the recording cylinders, by which their parallelism to the faces of the disks can be rigidly secured. The spheres are not touched by the forks themselves, but by the flat faces of two screws passing through their lower extremities, and in this way a ready means of preventing looseness or tightness of the spheres in the forks is provided.

"Each recording cylinder has attached to it a counter for registering the number of its complete revolutions, and to secure a maximum of freedom in their movements the spindles of the cylinders, as well as the slides carrying the racks for giving motion to the disks, and the horizontal steel bar, are all made to run upon friction rollers; the slides have in addition counterpoises attached to them to prevent error from backlash.

"The motion of the shaft at the rear of the machine is communi-

FIG. 1.

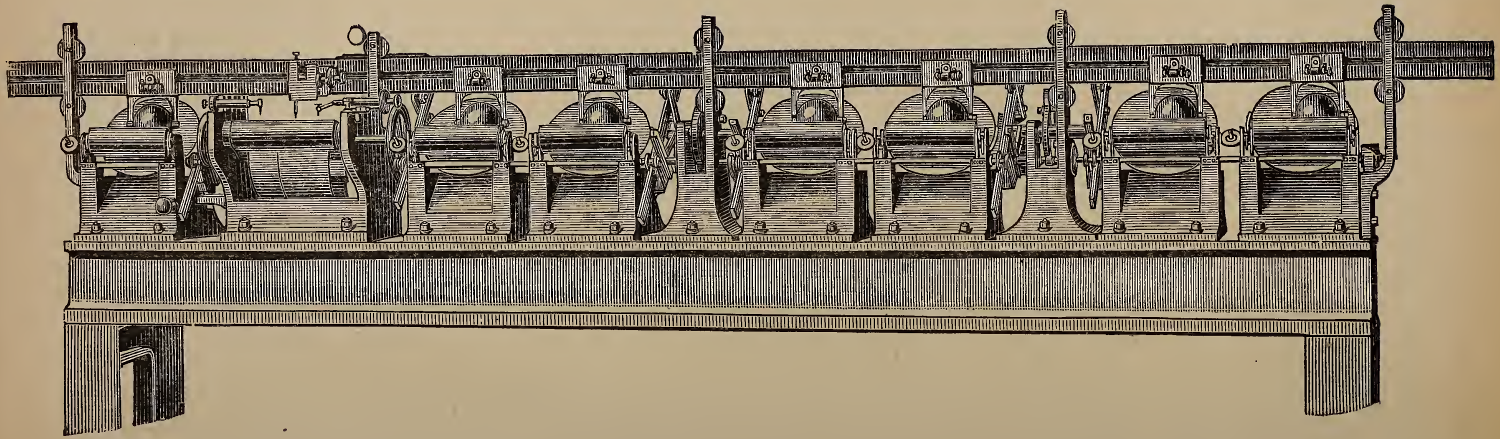


FIG. 2.

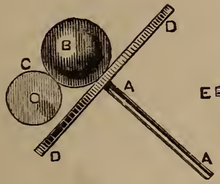
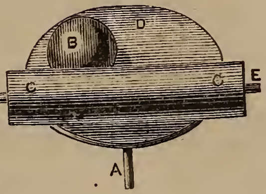


FIG. 3.



cated to the second and third pairs of cranks through sets of toothed wheels, so arranged that they may, if desired, be changed for others of different ratios to the cylinder carrying the curves, and in this way the terms of other orders of the expansion may be obtained, should they be required, with the same instrument, merely going over the curves afresh, and using wheels of the proper ratios in place of those used for the first, second, and third pairs of terms.

"The cylinder over which the curves have to pass is provided with an ingenious arrangement by which it can be expanded or contracted to alter its circumferential measurement by about four-tenths of an inch, so that within certain limits variations in the length of the time ordinates of the curves can be very readily allowed for. To effect this the cylinder (see figs. 4, 5, and 6) is made in three sections, each provided with an eccentric movement; of course, except when these are at their normal positions the "cylinder" is not truly cylindrical, but still, even when moved to their extreme limits, the deviation is not so large as to cause any inconvenience in its use. Adjustments are also supplied to the pointers, of which two are used at once, the one to follow the outline of the curve dealt with, and the other the zero line from which the ordinates are measured; and throughout the machine all racks and toothed wheels are skew cut to further lessen the risk of error from backlash.

"The height of the machine is 3 feet 8 inches, and the length of the steel bar, which is rather longer than the bed of the machine, 9 feet.

"The machine has now for some time been in regular use at the Meteorological Office, and notwithstanding the weight and solidity of some of its parts, the whole is so nicely balanced and fitted that it works with the utmost ease and smoothness."

The machine was delivered at the Office in December, 1879, and a lengthened series of trials was at once commenced, to determine its constants, and thoroughly test the accuracy of its working, for which purpose systems of straight lines and curves, of which the values were known, were first used. A few small unforeseen difficulties were early met with, necessitating slight modifications in some portions of the instrument.

The chief of these faults was a slight turning of the cylinders upon their axes, when the balls were moved to and fro along the disks,

Fig. 4.

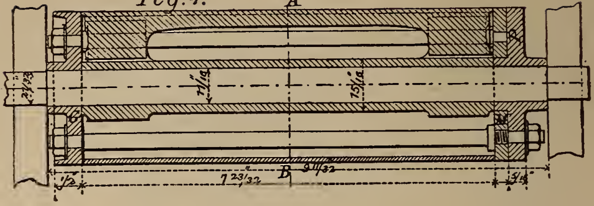


Fig. 5.

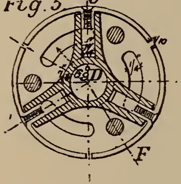
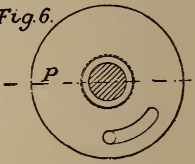


Fig. 6.



parallel to the axes of the cylinders. The movement was always in the same direction, namely, towards the disks, whether the ball was moved to the right or left. After the trial of many expedients the defect was finally, in great measure, overcome by attaching weights to the spindles of the cylinders. It however still exists in the machine to a slight extent, and its effect is to *decrease* the readings on the cylinders by a very small amount.

It was decided to employ the analyser, in the first instance, in the determination of temperature constants, and careful comparisons have been made of the results obtained by its means, with those got by actual measurement of the photographs and numerical calculations, as will presently be mentioned, and the accordance is so very close as to prove that the machine may safely be trusted to effect reductions which could only otherwise be accomplished by the far more laborious process of measurement and calculation.

It will facilitate an apprehension of the method of using the machine to give a somewhat detailed account of the operations involved in the treatment of the curves, with an example of the manner in which the readings of the machine are recorded and dealt with.

The machine is furnished with three pairs of recording cylinders and disks, numbered consecutively 1 to 6, which give the coefficients for the first three pairs of terms of the expansion, and in addition a seventh cylinder and disk from which the mean is obtained. In the thermograms which supply continuous photographic records of the march of temperature, the trace for twenty-four hours covers a length of 8·75 inches, while a vertical height of about 0·7 inch* corresponds to a range of ten degrees in temperature; each thermograph sheet contains the record for forty-eight hours.

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* This value varies slightly for each observatory.

Conveniently placed in the machine is a cylinder or drum, the circumference of which is equal to the length of twenty-four hours upon the thermograms. Round this cylinder the thermograms are rolled, the fluctuations of temperature indicated by the curves being followed, as the cylinder revolves, by a combination of the movement of the cylinder with that of a pointer moving in a line parallel to its axis.

The handle by which the cylinder is turned gives motion at the same time to the seven disks of the machine, and the operator thus controls by his left hand both the speed with which the curves are paid through the machine and the consequent velocity of the angular motion of the disks, while by a suitable contrivance, the movements of the pointer, governed by his right hand and following the curve, produce on the face of the disks corresponding movements to the right or left of the balls by which the motion of the disks is conveyed to the recording cylinders.

At the commencement of an operation all the cylinders are set to zero; the twelve months curves are then passed consecutively through the instrument; the first pair of cylinders, which gives the coefficients of the first order, and also the mean cylinder, 7, being read for each day, while cylinders 3 and 4, and 5 and 6, which give the coefficients of the second and third orders respectively, are only read for each five days and at the end of each calendar month. The numbers on the cylinders are, however, progressive, so that the increments upon them for any given period could very easily be obtained. The form in which the readings are recorded is as follows:—

Readings of the Recording Cylinders of the Harmonic Analyser.
Dry-bulb Thermometer Curves; from July 30 to August 3, 1882.
Kew Observatory.

Month and day.	First order.			
	Cylinder 1.		Cylinder 2.	
	Reading at midnight.	Difference from last reading.	Reading at midnight.	Difference from last reading.
June 30	+10·480	—	−12·333	—
July 30	+12·540	+0·103	−14·926	−0·118
„ 31	+12·678	+0·138	−15·004	−0·078
August 1	+12·773	+0·095	−15·069	−0·065
„ 2	+12·814	+0·041	−15·207	−0·138
„ 3	+12·897	+0·083	−15·287	−0·080

Month and day.	Second order.			
	Cylinder 3.		Cylinder 4.	
	Reading at midnight.	Difference from last reading.	Reading at midnight.	Difference from last reading.
June 30	−3·953	—	−3·256	—
July 30	—	—	—	—
" 31	−4·054	±0·000	−3·454	−0·080
August 1	—	—	—	—
" 2	—	—	—	—
" 3	−4·084	−0·030	−3·466	−0·012

Month and day.	Third order.			
	Cylinder 5.		Cylinder 6.	
	Reading at midnight.	Difference from last reading.	Reading at midnight.	Difference from last reading.
June 30	−1·877	—	−1·827	—
July 30	—	—	—	—
" 31	−2·674	−0·031	−2·391	−0·032
August 1	—	—	—	—
" 2	—	—	—	—
" 3	−2·786	−0·112	−2·456	−0·065

Month and day.	Mean.		Midnight reading of curve.	Difference from last reading.
	Cylinder 7.			
	Reading at midnight.	Difference from last reading.		
June 30	−2·285	—	—	—
July 30	+52·637	+2·302	—	—
" 31	+54·554	+1·917	—	—
August 1	+57·177	+2·623	—	—
" 2	+59·753	+2·576	—	—
" 3	+61·299	+1·546	—	—

At present only the monthly increments of the readings have been dealt with, so as to obtain the coefficients of the mean daily variation for each month of the year. The process followed is, therefore, simply to divide the monthly increment by the number of days in the month, and then to multiply the quotient by a factor which is determined by the scale-value of the thermograms, and which will therefore be different for each observatory.

The ratios of the factors for cylinders 1 to 6 to that of No. 7 were very carefully determined from a series of experimental curves, of which the values were known. The numerical factor is obtained for each observatory by obtaining on cylinder No. 7 the scale reading corresponding to a vertical movement of the pointer of 10° on the thermogram, which in the case of Kew is 0·75 inch. The factor for cylinders Nos. 1 and 2 is eight times that for cylinder No. 7; the factor for Nos. 3 and 4 is four times that quantity, and for Nos. 5 and 6 is eight-thirds of that quantity. The signs of the factors depend on the direction in which the disks and cylinders are caused to revolve. The constant quantity added to the reduced reading of cylinder No. 7 corresponds to the temperature which is assumed as the zero at the commencement of the operation.

As an illustration, the case of Kew for July, 1882, may be taken, the final readings of the cylinders for which month are above given. The increments for the month shown by these figures are as follows:—

Cylinder	1.	2.	3.	4.	5.	6.	7.
Observed increment	+2·198	−2·671	−0·101	−0·198	−0·797	−0·564	+56·839
Divided by 31 (the number of days)	+0·071	−0·086	−0·003	−0·006	−0·026	−0·018	+ 1·834
Factor	−53·52	+53·52	−26·76	−26·76	−17·84	−17·84	+ 6·69
Coefficient deduced	− 3·80	− 4·60	+ 0·08	+ 0·16	+ 0·46	+ 0·32	+12·27

Add constant................ 48·17

Mean temperature .... 60·44

After some trials with the curves for the year 1871, the year 1876 was taken up, inasmuch as for that year the records had been discussed by Mr. H. S. Eaton, M.A., F.R. Met. Soc., who had calculated the hourly means of the various meteorological elements for each month separately, and who kindly placed his results at the disposal of the Council.

The working of the machine was thus subjected to an exact test by comparing the results obtained by it with the coefficients in the harmonic series which were calculated from Mr. Eaton's means; and their trustworthy character, and the adequacy of these calculations to serve as a standard with which the coefficients obtained by means of the machine might be compared, was established by calculating them from the *odd* and *even* hours, quite independently, for all the seven observatories.

The outcome of this experiment was thoroughly satisfactory, and the entire series of results obtained both by calculation and from the machine was published as Appendix IV to the Quarterly Weather Report for 1876, together with a Report prepared by Prof. Stokes, the concluding paragraphs of which may be quoted here, since they sum up in a few words the conclusions arrived at.

"Disregarding now the systematic character of some of the errors, and treating them as purely casual, we get as the average difference between the constants as got by the machine and by calculation from the twenty-four hourly means 0·065°. It may be noticed, however, that the numbers are unusually large (and at the same time very decidedly systematic) in the case of the second cylinder of the first order (b_1), for which the average is as much as 0·150°, the seventh of a degree.

"If b_1 be omitted, the average for the remaining cylinders of the machine is reduced to 0·047°.

"We see, therefore, that with the exception perhaps of b_1, the constants got by the machine for the mean of the days constituting the month are as accurate as those got by calculation, which requires considerably more time, inasmuch as the hourly lines have to be drawn on the photograms, then measured, then meaned, and the constants deduced from the means by a numerical process by no means very short."

The curves for the twelve years 1871 to 1882 inclusive have now been passed through the machine, and the results obtained have been carefully checked so far as the arithmetical work involved is concerned, upon a plan approved by the Council. No *direct* check, short of passing the curves a second time through the machine can however at present be put on any portion of the results except as regards the means, which have been compared with the means calculated from the hourly readings obtained by measurement from the curves. The results of this work will be published as an appendix to the "Hourly Readings from the Self-Recording Instruments," for 1883, but the general results may here be stated.

As a rule, the monthly means yielded by the harmonic analyser agree well within a tenth of a degree with those obtained by calculation from the hourly measurements of the curves; and although in some exceptional cases larger differences have been found, amounting in rare instances to as much as half a degree, it is probable that generally these are less due to defects in the working of the instrument than to other causes. In some cases large breaks in the curves, due to failure of photography, &c., were interpolated when the curves were passed through the machine, but not when the means were worked out from the hourly measurements. Some differences rather larger than usual, and confined chiefly to the earliest years dealt with, have been ascertained to have arisen from the circumstance that when the curves were first measured, to obtain hourly values, the method of making the measurements was not the same as that found by subsequent experience to be the preferable; and also that in some cases the scale-values first used were less accurately determined than has since been found possible.

In both these respects the two methods were on a par in the later years dealt with, and therefore the fairest comparison is to be had with their means.

For 1880, the average difference of the monthly mean for all the seven observatories is 0·09°; for 1881 it is 0·05°; and for 1882 0·06°; and in these three years a difference of 0·3° between the analyser and calculated means occurred but once, and of 0·2° but five times.

What has been said is sufficient to show that the instrument is completely applicable to the analysis of thermograms.

It has also been employed on the discussion of barograms, and the curves for the years 1871, 1872, and 1876 have been passed through the machine.

The year 1876 was selected owing to the existing facilities for comparing the resulting figures with those obtained by calculation from Mr. Eaton's means, and the result in this case was equally satisfactory with that for temperature already mentioned.

In conclusion, the Fellows may perhaps be reminded that on June 18th, 1874, one of us (Mr. Scott) read a paper "On the use of an Amsler's Planimeter for obtaining mean values from Photographic Curves," "Proc. Roy. Soc.," vol. 22, p. 435. This paper contains a table exhibiting the differences between the means so obtained and those yielded by the hourly values.

We reproduce this table, appending to it the values obtained from the analyser for the same period.

It will, of course, be remembered that the mean is the only result which can be got from the planimeter, while it is but a small part of what is yielded by the harmonic analyser; but a comparison of the figures obtained by the two instruments from the same photographic curves may be interesting, as being got in the case of the planimeter from an instrument in which there is a combined "rolling and slipping" movement, while the movement in the analyser is one of "pure rolling contact."

Comparison of Temperature Means obtained from Kew Photographic Thermograms, by Amsler's Planimeter, and Sir W. Thomson's Harmonic Analyser respectively, with those obtained by Numerical Calculation from Hourly Measurements of the Curves.

Groups of five days.	Means.			Differences.	
	Tabulations.	Planimeter.	Harmonic analyser.	T − P.	T − A.
1872. Apr. 1— 5 ..	44·8	45·1	44·8	−0·3	0
6—10 ..	48·1	48·4	48·1	−0·3	0
11—15 ..	52·8	52·7	52·7	+0·1	+0·1
16—20 ..	43·9	44·2	44·1	−0·3	−0·2
21—25 ..	48·0	48·1	48·0	−0·1	0
26—30 ..	53·3	53·5	53·4	−0·2	−0·1
May 1— 5 ..	53·5	53·6	53·5	−0·1	0
6—10 ..	48·1	48·5	48·2	−0·4	−0·1
11—15 ..	46·9	46·9	46·7	0	+0·2
16—20 ..	47·3	47·4	47·2	−0·1	+0·1
21—25 ..	50·8	50·9	50·6	−0·1	+0·2
26—30 ..	59·1	59·0	59·2	+0·1	−0·1
31— 4 ..	53·4	53·5	53·4	−0·1	0
June 5— 9 ..	53·8	54·0	53·9	−0·2	−0·1
10—14 ..	58·0	58·1	58·0	−0·1	0
15—19 ..	68·7	68·7	68·5	0	+0·2
20—24 ..	62·6	62·7	62·3	−0·1	+0·3
25—29 ..	59·5	59·7	59·5	−0·2	0
30— 4 ..	62·6	62·7	62·6	−0·1	0
July 5— 9 ..	66·0	66·3	66·1	−0·3	−0·1
10—14 ..	62·6	62·7	62·5	−0·1	+0·1
15—19 ..	61·0	61·0	61·1	0	−0·1
20—24 ..	69·7	69·8	69·7	−0·1	0
25—29 ..	70·1	70·2	70·1	−0·1	0
30— 3 ..	59·3	59·4	59·4	−0·1	−0·1
Aug. 4— 8 ..	59·9	60·0	60·0	−0·1	−0·1
9—13 ..	59·7	59·8	59·7	−0·1	0
14—18 ..	62·4	62·5	62·4	−0·1	0
19—23 ..	65·3	65·1	65·2	+0·2	+0·1
24—28 ..	60·6	60·6	60·5	0	+0·1
29— 2 ..	59·7	59·6	59·7	+0·1	0
Sept. 3— 7 ..	65·7	65·7	65·7	0	0
8—12 ..	62·6	62·7	62·7	−0·1	−0·1
13—17 ..	62·2	62·1	62·2	+0·1	0
18—22 ..	48·8	49·0	49·0	−0·2	−0·2
1873. Jan. 31— 4 ..	32·9	33·2	33·0	−0·3	−0·1
Feb. 5— 9 ..	34·7	34·9	34·8	−0·2	−0·1
10—14 ..	37·2	37·3	37·1	−0·1	+0·1
15—19 ..	36·4	36·7	36·5	−0·3	−0·1
20—24 ..	32·9	33·2	32·9	−0·3	0
25— 1 ..	39·7	39·8	39·7	−0·1	0
Mar. 2— 6 ..	44·3	44·4	44·4	−0·1	−0·1
7—11 ..	42·3	42·5	42·4	−0·2	−0·1
12—16 ..	37·4	37·5	37·4	−0·1	0
17—21 ..	39·5	39·7	39·6	−0·2	−0·1
22—26 ..	43·6	43·8	43·7	−0·2	−0·1
27—31 ..	47·3	47·4	47·3	−0·1	0
	Mean difference			−0·12	−0·01
	Mean difference from mean			0·07	0·07

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May 27, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Family Likeness in Eye-colour." By FRANCIS GALTON, F.R.S. Received May 10, 1886.

My inquiry into Family Likeness in Stature (*ante*, p. 42) enabled me to define, in respect to that particular quality, the relation in which each man's peculiarity stands to those of each of his ancestors. The object of the present memoir is to verify that relation with respect to another quality, namely, eye-colour.

Speaking of heritage, independently of individual variation, and supposing female characteristics to be transmuted to their male equivalents, I showed (1) that the possession of each unit of *peculiarity* in a man [that is of difference from the average of his race] when the man's ancestry is unknown, implies the existence on an average of just one-third of a unit of that peculiarity in his "mid-parent," and, consequently, in each of his parents; also just one-third of a unit in each of his children; (2) that each unit of peculiarity in each ancestor taken singly, is reduced in transmission according to the following average scale :—from a parent, to $\frac{1}{4}$; from a grandparent, to $\frac{1}{16}$; from a great-grandparent, to $\frac{1}{64}$, and so on.

Stature and eye-colour are not only different as qualities, but they are more contrasted in hereditary behaviour than perhaps any other simple qualities. Speaking broadly, parents of different statures transmit a blended heritage to their children, but parents of different eye-colours transmit an alternative heritage. If one parent is as much taller than the average of his or her sex as the other parent is shorter, the statures of their children will be distributed in much the same way as those of parents who were both of medium height. But if one parent has a light eye-colour and the other a dark eye-colour, the children will be partly light and partly dark, and not medium eye-coloured like the children of medium eye-coloured parents. The blending in stature is due to its being the aggregate of the quasi-independent inheritances of many separate parts, while eye-colour appears to be much less various in its origin. If then it can be shown, as I shall be able to do, that notwithstanding this two-fold difference between the qualities of stature and eye-colour, the shares of hereditary contribution from the various ancestors are in each case alike, we may with some confidence expect that the law by which those hereditary contributions are governed will be widely, and perhaps even universally, applicable.

Data.—My data for hereditary eye-colour are drawn from the same collection of "Records of Family Faculties" ("R.F.F.") as those upon which the above-mentioned inquiries into hereditary stature were principally based. I then analysed the general value of these data in respect to stature, and showed that they were fairly trustworthy. I think they are somewhat more accurate in respect to eye-colour, for which family portraits have often furnished direct information, while indirect information has been in other cases obtained from locks of hair that were preserved in the family as mementos. I have also been able to collate some of my results with those lately published by M. Alphonse de Candolle,* who instituted an inquiry that has in many particulars, though not in the main object of the present memoir, covered the same ground as my own, and which was of course founded on an entirely different collection of data. My conclusions in respect to those particulars, of which only a few find place here, are generally corroborated by his.

Persistence of Eye-colour in the Population.—The first subject of our inquiry must be into the existence of any slow change in the statistics of eye-colour in the population that might have to be taken into account before drawing hereditary conclusions. For this purpose I sorted the data, not according to the year of birth, but according to generations, as that method of procedure best accorded with the particular form in which all my R.F.F. data are compiled. Those persons who ranked in the Family Records as the "children" of the pedigree, were counted as generation I; their parents, uncles and aunts, as generation II; their grandparents, great uncles, and great aunts, as generation III; their great grandparents, and so forth, as generation IV. No account was taken of the year of birth of the "children," except to learn their age; consequently there is much overlapping of dates in successive generations. We may, however, safely say, that the persons in generation I are quite different from those in generation III, and the persons in II from those in IV. I had intended to exclude all children under the age of eight years, but in this particular branch of the inquiry, I fear that some cases of young children have been accidentally included. I would willingly

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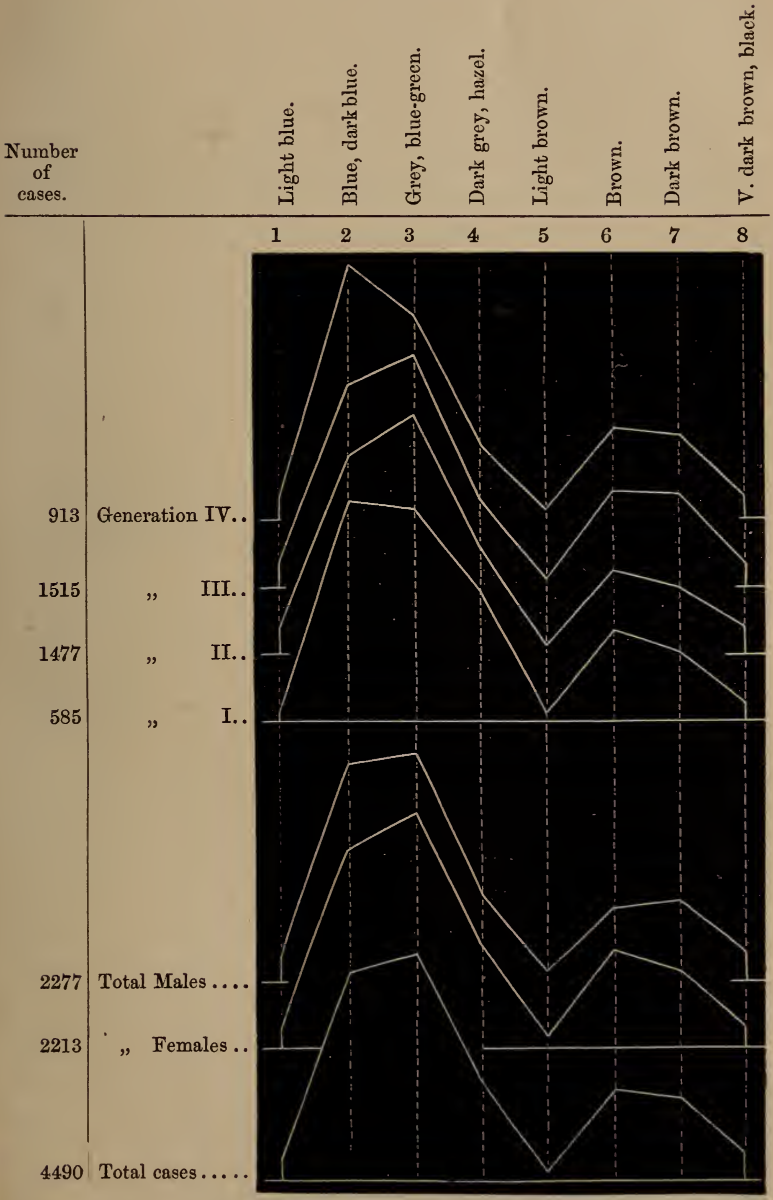
* Hérédité de la couleur des yeux dans l'espèce humaine," par M. Alphonse de Candolle. "Arch. Sc. Phys. et Nat. Genève," Aug. 1884, 3rd period, vol. xii, p. 97.

Table I.—Frequency of Different Eye-colours in Four Successive Generations.

Sex and the No. of the generation.		No. of cases of eye-colour observed.								Percentages.								
	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark grey. Hazel.	5. Light brown.	6. Brown.	7. Dark brown.	8. Very dark brown. Black.	Totals.	1. Light blue.	2. Blue. Dark blue.	3. Grey. Blue-green.	4. Dark grey. Hazel.	5. Light brown.	6. Brown.	7. Dark brown.	8. Very dark brown. Black.	Totals.
Males IV.....	13	177	136	40	2	39	44	12	463	2·8	38·2	29·4	8·6	0·4	8·4	9·5	2·6	99·9
III.....	19	234	233	84	3	79	97	24	773	2·4	30·3	30·1	10·9	0·4	10·1	12·6	3·1	99·9
II......	30	167	236	108	8	83	74	36	742	4·0	22·5	31·8	14·6	1·1	11·2	10·0	4·8	100·0
I.......	3	89	82	47	1	37	31	9	299	1·0	28·9	27·4	15·7	0·3	12·4	10·4	3·0	100·0
General	65	667	687	279	14	238	246	81	2277	2·9	29·3	30·2	12·3	0·6	10·4	10·8	3·6	100·0
Females IV...	7	132	114	48	2	70	58	19	450	1·5	29·3	25·3	10·7	0·4	15·6	12·9	4·2	99·9
III...	22	173	241	89	7	100	93	17	742	2·9	23·3	32·5	12·1	0·9	13·5	12·5	2·3	100·0
II ..	21	210	241	98	3	78	60	24	735	2·9	28·6	32·8	13·3	0·4	10·6	8·2	3·3	100·1
I.....	6	78	82	55	5	33	22	5	286	2·1	27·3	28·7	19·2	1·7	11·5	7·7	1·7	99·9
General	56	593	678	290	17	281	233	65	2213	2·5	26·8	30·6	13·1	0·8	12·7	10·5	2·9	99·9
Males and Females IV...	20	309	240	88	4	109	102	31	913	2	34	27	10	1	12	11	3	100
III...	41	407	474	173	10	179	190	41	1515	3	27	31	11	1	12	12	3	100
II....	51	377	477	206	11	161	134	60	1477	3	26	32	14	1	11	9	4	100
I.....	9	167	164	102	6	70	53	14	585	1	29	28	18	1	12	9	2	100
General	121	1260	1365	569	31	519	479	146	4490	2·7	28·1	30·4	12·7	0·7	11·6	10·7	3·3	100·2

Percentages of the Various Eye-colours in Four Successive Generations.



have taken a later limit than eight years, but could not spare the data that would in that case have been lost to me.

A great variety of terms are used by the various compilers of the "Family Records" to express eye-colours. I began by classifying them under the following eight heads:—1, light blue; 2, blue, dark blue; 3, grey, blue-green; 4, dark grey, hazel; 5, light brown; 6, brown; 7, dark brown; 8, black. Then I constructed Table I.

The accompanying diagram will best convey the significance of the figures in Table I. Considering that the headings for different eye-colours are eight in number, the observations are far from being sufficiently numerous to justify us in expecting clean results; nevertheless the curves come out surprisingly well, and in accordance with one another. There can be little doubt that the change, if any, during four successive generations is very small, and much smaller than mere memory is competent to take note of. I therefore disregard a current popular belief in the existence of a gradual darkening of the population, and shall treat the eye-colours of those classes of the English race who have contributed the records, as statistically persistent during the period under discussion.

The concurrence of the four curves for the four several generations affords some internal evidence of the trustworthiness of the data. For supposing we had curves that exactly represented the true eye-colours for the four generations, they would either be concurrent or they would not. If concurrent, the errors in the R.F.F. curves must have been so curiously distributed as to preserve the concurrence. If not, the errors must have been so curiously distributed as to neutralise the non-concurrence. Both of these suppositions are improbable, and we must conclude that the curves really agree, and that the R.F.F. errors are not large enough to spoil the agreement. The much closer concurrence of the two curves, derived respectively from the whole of the male and the whole of the female data, and the still more perfect form of the curve derived from the aggregate of all the cases, are additional evidences in favour of the goodness of the data on the whole.

Fundamental Eye-colours.—It is agreed among most writers on the subject (*cf.* A. de Candolle) that the one important division of eye-colours is into the light and the dark. The medium tints are not numerous, and they may have four distinct origins. They may be hereditary with no notable variation, they may be varieties of light parentage, they may be varieties of dark parentage, or they may be blends. These medium tints are classed in my list under the heading "4. Dark grey, hazel," and they form only 12·7 per cent. of all the observed cases. It is common in them to find the outer portion of the iris to be of a dark grey colour, and the inner of a hazel. The proportion between the grey and the hazel varies in different cases, and the eye-colour is then described as dark grey or as hazel, according to the colour that happens most to arrest the attention of the observer. For brevity, I will henceforth call all intermediate tints by the one name of hazel.

I will now investigate the history of those hazel eyes that are variations from light or from dark respectively, or that are blends between them. It is reasonable to suppose that the residue which were inherited from hazel-eyed parents arose originally either as variations or as blends, and therefore the result of the investigation will enable us to assort the small but troublesome group of hazel eyes in an equitable proportion between light and dark, and thus to simplify our inquiry.

The family records include 168 families of brothers and sisters, counting only those who were above eight years of age, in whom one member at least had hazel eyes. The total number of these brothers and sisters is 948, of whom 302 or about one-third have hazel eyes. For distinction I will describe these as "hazel-eyed families"; not meaning thereby that all the children have that peculiarity, but only some of them. The eye-colours of all the 336 parents are given in the records, but only those of 449 of the grandparents, whose number would be 672, were it not for a few cases of cousin marriages. Thus I have information concerning about only two-thirds of the grandparents, but this will suffice for our purpose. The results are given in Table II.

Table II.—The Descent of Hazel-eyed Families.

	Total cases.	Observed.			Percentages.		
		Light.	Hazel.	Dark.	Light.	Hazel.	Dark.
General population..	4490	2746	569	1175	61·2	12·7	26·1
III, Grandparents ..	449	267	61	121	60	13	27
II, Parents........	336	165	85	86	49	25	26
I, Children.......	948	430	302	216	45	32	23

It will be observed that the distribution of eye-colour among the grandparents of the hazel-eyed families is nearly identical with that among the population at large. But among the parents there is a notable difference; they have a decidedly smaller percentage of light eye-colour and a slightly smaller proportion of dark, while the hazel element is nearly doubled. A similar change is superadded in the next generation. The total result in passing from generation III to I, is that the percentage of the light eyes is diminished from 60 or 61 to 45, therefore by one quarter of its original amount, and that the percentage of the dark eyes is diminished from 26 or 27 to 23, that is to about one-eighth of its original amount, the hazel element in either case absorbing the difference. It follows that the chance of a light-eyed parent having hazel offspring, is about twice as great as that of a dark-eyed parent. Consequently, since hazel is twice as likely to be met with in any given light-eyed family as in a given dark-eyed one, we may look upon two-thirds of the hazel eyes as being fundamentally light, and one-third of them as fundamentally dark. I shall allot them rateably in that proportion between light and dark, as nearly as may be without using fractions, and so get rid of them. M. Alphonse de Candolle has also shown from his data, that *yeux gris* (which I take to be the equivalent of my *hazel*) are referable to a light ancestry rather than to a dark one, but his data are numerically insufficient to warrant a precise estimate of the relative frequency of their derivation from each of these two sources.

Heredity of Light and Dark Eye-colour.—In the following discussion I shall deal only with those family groups of children in which the eye-colours are known of the two parents and of the four grandparents. There are altogether 211 of such groups, containing an aggregate of 1023 children. They do not, however, belong to 211 different family stocks, because each stock which is complete up to the great grandparents inclusive (and I have fourteen of these) is capable of yielding three such groups. Thus, group 1 contains *a*, the "children;" *b*, the parents; *c*, the grandparents. Group 2 contains *a*, the father of the "children," his brothers and his sisters; *b*, the parents of the father; *c*, the grandparents of the father. Group 3 contains the corresponding selections on the mother's side. Other family stocks furnish two groups. Out of these and other data, Tables III and IV have been made. In Table III I have classified the families together whose two parents and four grandparents present the same combination of eye-colour, no class, however, being accepted that contains less than twenty children. These data will enable us to test the *average* correctness of the law I desire to verify, because many persons and many families appear in the same class, and individual peculiarities tend to disappear. In Table IV I have separately classified on the same system all the families, 78 in number, that consist of six or more children. These data will enable us to test the trustworthiness of the law as applied to individual families. It will be seen from my way of discussing them, that smaller families than these could not be advantageously dealt with.

Table III.—Sixteen Groups of Families, those being grouped together in whom the distribution of Light, Hazel, and Dark Eye-colour among their Parents and Grandparents is alike. Each Group contains at least Twenty Brothers or Sisters.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	..	..	4	..	..	183	174	161	163	172
2	..	..	3	1	..	53	46	47	44	48
2	..	..	3	..	1	92	88	81	67	79
2	..	..	2	1	1	27	26	24	18	22
..	..	2	2	..	2	22	11	6	12	6
1	1	..	3	1	..	62	52	48	51	51
1	1	..	3	..	1	42	30	33	31	32
1	1	..	2	2	..	31	28	24	24	20
1	1	..	2	..	2	49	35	38	28	34
1	1	..	2	1	1	31	25	24	21	23
1	..	1	3	..	1	76	45	44	55	46
1	..	1	2	..	2	66	30	38	38	35
1	..	1	2	..	1	27	15	16	18	16
1	..	1	1	..	3	20	9	12	8	9
1	..	1	1	1	2	22	8	13	11	11
..	1	1	1	1	2	24	9	14	12	10
							629	623	601	614

Table IV.—78 Separate Families, each of not less than 6 Brothers or Sisters.

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Calculated.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.			I.	II.	III.
2	..	..	4	..	..	6	6	5·3	5·3	5·6
2	..	..	4	..	..	6	6	5·3	5·3	5·6
2	..	..	4	..	..	6	6	5·3	5·3	5·6
2	..	..	4	..	..	6	5	5·3	5·3	5·6
2	..	..	4	..	..	7	7	6·2	6·2	6·6
2	..	..	4	..	..	7	7	6·2	6·2	6·6
2	..	..	4	..	..	7	7	6·2	6·2	6·6
2	..	..	4	..	..	7	7	6·2	6·2	6·6
2	..	..	4	..	..	7	7	6·2	6·2	6·6
2	..	..	4	..	..	8	8	7·0	7·1	7·5
2	..	..	4	..	..	8	8	7·0	7·1	7·5
2	..	..	4	..	..	8	8	7·0	7·1	7·5
2	..	..	4	..	..	8	8	7·0	7·1	7·5
2	..	..	4	..	..	8	7	7·0	7·1	7·5
2	..	..	4	..	..	8	7	7·0	7·1	7·5
2	..	..	4	..	..	12	12	10·6	10·7	11·3
2	..	..	3	1	..	7	7	6·2	5·8	6·4
2	..	..	3	1	..	10	4	8·8	8·3	9·1
2	..	..	3	1	..	12	12	10·6	10·0	10·9
2	..	..	3	..	1	7	6	6·2	5·1	6·0
2	..	..	3	..	1	8	8	7·0	5·8	6·9
2	..	..	3	..	1	9	9	7·9	6·6	7·7
2	..	..	3	..	1	9	9	7·9	6·6	7·7
2	..	..	3	..	1	9	7	7·9	6·6	7·7
2	..	..	3	..	1	10	10	8·8	7·3	8·6
2	..	..	2	2	..	7	7	6·2	5·4	6·2
2	..	..	2	2	..	10	9	8·8	7·7	8·8
2	..	..	2	1	1	6	6	5·3	4·0	5·0
2	..	..	2	1	1	10	10	8·8	6·7	8·3
..	2	..	2	1	1	7	4	6·2	4·7	4·6
..	2	..	2	..	2	8	5	5·4	4·6	4·8
..	..	2	3	..	1	6	2	1·7	4·4	2·2
..	..	2	2	..	2	9	1	2·5	5·1	2·5
..	..	2	1	..	3	6	1	2·7	2·5	1·2
..	..	2	1	..	3	11	3	3·1	4·5	2·2
..	..	2	1	1	2	6	..	1·7	3·0	1·5
..	..	2	1	1	2	7	4	2·0	3·6	1·8
1	1	..	3	1	..	6	6	4·7	5·0	4·9
1	1	..	3	1	..	7	6	5·5	5·7	5·7
1	1	..	3	1	..	8	6	6·2	6·6	6·6
1	1	..	3	1	..	9	7	7·0	7·5	7·4
1	1	..	3	1	..	11	10	8·6	9·1	9·2
1	1	..	3	..	1	9	6	7·0	6·6	6·9
1	1	..	3	..	1	11	7	8·6	8·0	8·5
1	1	..	2	2	..	7	6	5·5	5·4	4·4

Table IV—*continued.*

Eye-colours of the						Total children.	Number of the light eye-coloured children.			
Parents.			Grandparents.				Observed.	Children.		
Light.	Hazel.	Dark.	Light.	Hazel.	Dark.				II.	III.
1	1	..	2	2	..	9	9	7·0	6·9	5·7
1	1	..	2	2	..	11	11	8·6	8·5	6·9
1	1	..	2	..	2	6	6	4·7	3·4	4·1
1	1	..	2	..	2	6	4	4·7	3·4	4·1
1	1	..	2	..	2	8	5	6·2	4·6	5·5
1	1	..	2	..	2	9	7	7·0	5·1	6·2
1	1	..	2	1	1	6	6	4·7	4·0	4·4
1	1	..	2	1	1	10	9	7·8	6·7	7·4
1	1	..	1	3	..	9	4	7·0	5·5	6·8
1	1	..	1	1	2	8	5	6·2	4·1	5·3
1	..	1	4	..	..	7	3	4·1	6·2	4·8
1	..	1	3	..	1	6	4	3·5	4·4	3·7
1	..	1	3	..	1	7	3	4·1	5·1	4·3
1	..	1	3	..	1	8	6	4·6	5·8	4·9
1	..	1	3	..	1	8	5	4·6	5·8	4·9
1	..	1	3	..	1	8	4	4·6	5·8	4·9
1	..	1	3	..	1	9	6	5·2	6·6	5·5
1	..	1	3	..	1	9	5	5·2	6·6	5·5
1	..	1	2	..	2	6	5	3·5	3·4	3·2
1	..	1	2	..	2	6	3	3·5	3·4	3·2
1	..	1	2	..	2	8	4	4·6	4·6	4·2
1	..	1	2	..	2	10	2	5·8	5·7	5·3
1	..	1	2	..	2	14	9	8·1	8·0	7·4
1	..	1	2	1	1	7	5	4·1	4·7	4·1
1	..	1	1	2	1	7	3	4·1	4·3	3·9
1	..	1	1	1	2	7	4	4·1	3·6	3·5
1	..	1	1	..	3	8	4	4·6	3·3	3·6
1	..	1	1	..	3	8	3	4·6	3·3	3·6
1	..	1	..	1	3	6	3	3·5	2·1	2·6
..	1	1	2	..	2	6	3	4·8	3·4	2·6
..	1	1	2	1	1	9	4	7·0	6·0	4·4
..	1	1	1	..	3	13	8	10·1	5·3	4·7
..	1	1	..	4	..	7	2	5·5	4·6	3·4

It will be noticed that I have not printed the number of dark-eyed children in either of these tables. They are implicitly given, and instantly to be found by subtracting the number of light-eyed children from the total number of children. Nothing would have been gained by their insertion, while compactness would have been sacrificed.

The entries in the tables are classified, as I said, according to the various combinations of light, hazel, and dark eye-colours in the parents and grandparents. There are 6 different possible combinations among the two parents, and 15 among the four grandparents; making 90 possible classes altogether. The number of observations are of course by no means evenly distributed among the classes. I have no returns at all under more than half of them, while the entries of two light-eyed parents and four light-eyed grandparents are proportionately very numerous. (I shall not here discuss the question of marriage selection in respect to eye-colour, which is a less simple statistical question than it may appear to be at first sight.)

Calculation.—I have now to show how the expectation of eye-colour among the children of a given family is to be calculated on the basis of the law laid down for stature, so that those calculations of the probable distribution of eye-colours may be made, which fill the three last columns of Tables III and IV, which are headed I, II, and III, and which are placed in juxtaposition with the observed facts as entered in the column headed "Observed." These three columns contain calculations based on data limited in three different ways, in order the more thoroughly to test the applicability of the law that it is desired to verify. Column I contains calculations based on a knowledge of the parents only; II contains those based on a knowledge of the grandparents only; III contains those based on a knowledge both of the parents and of the grandparents, and of them only.

I. Eye-colours given of the two parents—

Let the letter M be used as a symbol to signify the person for whom the expected heritage is to be calculated. Let P stand for the words "a parent of M;" G_1 for "a grandparent of M;" G_2 for "a great-grandparent of M," and so on.

Now suppose that the amount of the peculiarity of stature possessed by P is equal to r, and that nothing whatever is known with certainty of any of the ancestors of M except P. We have seen* that though nothing may be actually known, yet that something definite is implied about the ancestors of P, namely, that each of his two parents (who stand in the order of relationship of G_1 to M) will on the average possess $\frac{1}{3}r$. Similarly that each of the four grandparents of P (who stand in the order of G_2 to M) will on the average possess $\frac{1}{9}r$, and so on. Again we have seen that P, on the average, transmits to M only $\frac{1}{4}$ of his peculiarity; that G_1 transmits only $\frac{1}{16}$; G_2 only $\frac{1}{64}$, and so on. Hence the aggregate of the heritages that may be expected to converge through P upon M, is contained in the following series:—

$$r\left\{\frac{1}{4}+2\left(\frac{1}{3}\times\frac{1}{2^4}\right)+4\left(\frac{1}{9}+\frac{1}{2^6}\right)+\ \&c.\right\}$$

$$=r\left\{\frac{1}{2^2}+\frac{1}{2^3\,.\,3}+\frac{1}{2^4\,.\,3^2}+\ \&c.\right\}=r\times0{\cdot}30.$$

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* *Ante*, p. 42 (No. 242).

That is to say, each parent must in this case be considered as contributing 0·30 to the heritage of the child, or the two parents together as contributing 0·60, leaving an indeterminate residue of 0·40 due to the influence of ancestry about whom nothing is either known or implied, except that they may be taken as members of the same race as M.

In applying this problem to eye-colour, we must bear in mind that a given fractional chance that each member of a family will inherit either a light or a dark eye-colour, must be taken to mean that that same fraction of the total number of children in the family will probably possess it. Also, as a consequence of this view of the meaning of a fractional chance, it follows that the residue of 0·40 must be rateably assigned between light and dark eye-colour, in the proportion in which those eye-colours are found in the race generally, and this was seen to be (see Table II) as 61·2 : 26·1; so I allot 0·28 out of the above residue of 0·40 to the heritage of light, and 0·12 to the heritage of dark. When the parent is hazel-eyed I allot $\frac{2}{3}$ of his total contribution of 0·30, *i.e.*, 0·20 to light, and $\frac{1}{3}$, *i.e.*, 0·10 to dark. These chances are entered in the first pair of columns headed I, in Table V.

Table V.

Contribution to the heritage from each.	Data limited to the eye-colours of the					
	2 parents.		4 grandparents.		2 parents and 4 grandparents.	
	I.		II.		III.	
	Light.	Dark.	Light.	Dark.	Light.	Dark.
Light-eyed parent	0·30	..	..	..	0·25	..
Hazel-eyed parent	0·20	0·10	..	..	0·16	0·09
Dark-eyed parent	..	0·30	..	..	..	0·25
Light-eyed grandparent.	..	..	0·16	..	0·08	..
Hazel-eyed grandparent	..	..	0·10	0·06	0·05	0·03
Dark-eyed grandparent.	..	..	..	0·16	..	0·08
Residue, rateably assigned	0·28	0·12	0·25	0·11	0·12	0·06

Table VI.—Example of one Calculation in each of the 3 Cases.

Ancestry and their eye-colours.	I.			II.			III.		
	No. about whom data exist.	Contribute to		No. about whom data exist.	Contribute to		No. about whom data exist.	Contribute to	
		Light.	Dark.		Light.	Dark.		Light.	Dark.
Light-eyed parents ..	2	0·60	..	..	..	..	..	..	..
Hazel-eyed parents ..	..	..	..	..	..	..	1	0·16	0·09
Dark-eyed parents ...	..	..	..	..	..	..	1	..	0·25
Light-eyed grandparents	..	..	..	1	0·16	..	1	0·08	..
Hazel-eyed grandparents	..	..	..	2	0·20	0·12	2	0·10	0·06
Dark-eyed grandparents	..	..	..	1	..	0·16	1	..	0·08
Residue, rateably assigned	..	0·28	0·12		0·25	0·11		0·12	0·06
Total contributions ..	..	0·88	0·12		0·61	0·39		0·46	0·54
		1·00			1·00			1·00	

The pair of columns headed I in Table VI shows the way of summing the chances that are given in the columns with a similar heading in Table V. On the method there shown I calculated all the entries that appear in the columns with the heading I in Tables III and IV.

II. Eye-colours given of the four grandparents—

Suppose r to be possessed by G_1 and that nothing whatever is known with certainty of any other ancestor of M. Then it has been shown that the child of G_1 (that is P) will possess $\frac{1}{3}r$; that each of the two parents of G_1 (who stand in the relation of G_2 to M) will also possess $\frac{1}{3}r$; that each of the four grandparents of G_1 (who stand in the relation of G_3 to M) will possess $\frac{1}{9}r$, and so on. Also it has been shown that the shares of their several peculiarities that will on the average be transmitted by P, G_1, G_2, &c., are $\frac{1}{4}$, $\frac{1}{16}$, $\frac{1}{64}$, &c., respectively. Hence the aggregate of the probable heritages from G_1 are expressed by the following series:—

$$r\left\{\frac{1}{3}\times\frac{1}{2^2}+1\times\frac{1}{2^4}+\frac{1}{3}\times 2\times\frac{1}{2^6}+\frac{1}{9}\times 4 \times\frac{1}{2^8}+ \text{ \&c.}\right\}$$

$$=r\left\{\frac{1}{12}+\left(\frac{1}{2^4}+\frac{1}{3\times 2^5}+\frac{1}{3^2\times 2^6}+ \text{ \&c.}\right)\right\}=\frac{1}{12}+\frac{3}{40}=0{\cdot}16.$$

So that each grandparent contributes on the average 0·16 (more exactly 0·1583) to the heritage of M, and the four grandparents contribute between them 0·64, leaving 36 indeterminate, which when rateably assigned gives 0·25 to light and 0·11 to dark. A hazel-eyed grandparent contributes, according to the ratio described in the last paragraph, 0·10 to light and 0·06 to dark. All this is clearly expressed and employed in the columns II of Tables V and VI.

III. Eye-colours given of the two parents and four grandparents—

Suppose P to possess *r*, then P taken alone, and not in connexion with what his possession of *r* might imply concerning the contributions of the previous ancestry, will contribute an average of 0·25 to the heritage of M. Suppose G_1 also to possess *r*, then his contribution together with what his possession of *r* may imply concerning the previous ancestry, was calculated in the last paragraph as $\frac{3}{40}$=0·075. For the convenience of using round numbers I take this as 0·08. So the two parents contribute between them 0·50, the four grandparents together with what they imply of the previous ancestry contribute 0·32, being an aggregate of 0·82, leaving a residue of 0·18 to be rateably assigned as 0·12 to light, and 0·6 to dark. A hazel-eyed parent is here reckoned as contributing 0·16 to light and 0·9 to dark; a hazel-eyed grandparent as contributing 0·5 to light and 0·3 to dark. All this is tabulated in Table V, and its working explained by an example in the columns headed III of Table VI.

Results.—A mere glance at Tables III and IV will show how surprisingly accurate the predictions are, and therefore how true the basis of the calculations must be. Their *average* correctness is shown best by the totals in Table III, which give an aggregate of calculated numbers of light-eyed children under Groups I, II, and III as 623, 601, and 614 respectively, when the observed numbers were 629; that is to say, they are correct in the ratios of 99, 96, and 98 to 100.

Table VII.

Number of Errors of various Amounts in the 3 Calculations of the Numbers of Light Eye-coloured Children in the 78 Families.

Data employed referring to	Amount of Errors.					Total Cases.
	0·0 to 0·5.	0·6 to 1·1.	1·2 to 1·7.	1·8 to 2·3.	2·4 and above.	
I. The 2 parents only	19	30	18	5	6	78
II. The 4 grandparents only	16	28	10	10	14	78
III. The two parents and 4 grandparents	41	17	8	4	8	78

Their trustworthiness when applied to individual families is shown as strongly in Table IV, whose results are conveniently summarised in Table VI. I have there classified the amounts of error in the several calculations: thus if the estimate in any one family was 3 light-eyed children and the observed number was 4, I should count the error as 1·0. I have worked to one place of decimals in this table, in order to bring out the different shades of trustworthiness in the three sets of calculations, which thus become very apparent. It will be seen that the calculations in Class III are by far the most precise. In more than one-half of those calculations the error does not exceed 0·5, whereas in I and II more than three-quarters of them are wrong to at least that amount. Only one-quarter of Class III are more than 1·1 in error, but somewhere about the half of Classes I and II are wrong to that amount. In comparing I with II, we find I to be slightly, but I think distinctly, the superior estimate. The relative accuracy of III as compared with I and II, is what we should have expected, supposing the basis of the calculations to be true, because the additional knowledge utilised in III, over what is turned to account in I and II, must be an advantage.

Conclusion.—The general trustworthiness of these calculations of the probable proportion of light-eyed and dark-eyed children in individual families, whose ancestral eye-colour is more or less known, is comparable with the chance of drawing a white or a black ball out of a bag in which the relative numbers of white and black balls are the same as those given by the calculation. The larger the proportion of data derived from a certain knowledge of ancestral eye-colours, and not from inferences about them, the more true does the comparison become. My returns are insufficiently numerous and too subject to uncertainty of observation to make it worth while to submit them to a more rigorous analysis, but the broad conclusion to which the present results irresistibly lead, is that the same peculiar hereditary relation that was shown to subsist between a man and each of his ancestors in respect to the quality of stature, also subsists in respect to that of eye-colour.

II. "A General Theorem in Electrostatic Induction, with Application of it to the Origin of Electrification by Friction." By JOHN BUCHANAN, B.Sc., Demonstrator of Physics, University College, London. Communicated by Professor G. CAREY FOSTER, B.A., F.R.S. Received May 13, 1886.

PART I.

This paper contains the results of an investigation into the question: If a dielectric be brought into a field of electric force, and there its specific inductive capacity is changed, what will be the electrical condition of the dielectric? The subject has occupied me both in its theoretical and experimental aspects for a considerable time, and I believe that the answer to the question throws light upon some fundamental electrical phenomena.

This investigation has led me to a general theorem in electrostatic induction which may be stated as follows:—

When a dielectric is brought into a field of electric force and the specific inductive capacity is there altered, in general the dielectric becomes electrified.

To give definiteness to our notions, let us imagine a field of electric force to be due to an electrified conductor, which we will call the "primary;" inclosing this primary is a conducting shell which is connected to earth.

For simplicity we will assume, for the present, that the charge on the primary remains unchanged in magnitude during this series of operations:—

(1.) The dielectric is brought into the field of force;
(2.) The specific inductive capacity is *increased;*
(3.) The dielectric is carried out of the field.

The state of the field is exactly the same as it was before the operations were performed. We can therefore fix our attention on the dielectric.

Let us compare the work done *by* electrical forces with the work done *against* them in the operations (1), (2), (3). We have in (1) work done *by* electrical forces in assisting to bring the dielectric into the field; work is also done *by* (say) the forces in (2). In operation (3) work is done *against* electrical forces. The question to be answered is this, does the following equation hold in every case?

Work done by electrical forces in bringing the dielectric into the field
+ work done by the forces during the change of specific inductive capacity
= work done against the electrical forces in carrying the dielectric out of the field.

If this equation be true under all circumstances, there is no excess of work done by or against electrical forces. We would have then no reason to expect to find an electric distribution on or in the dielectric, whose energy would be the equivalent of the excess of work done. Now that the above equation should always hold seems to me at variance with sound conceptions regarding the effect of an arbitrary change in the physical state of a body.

Take, for instance, a case such as that of a piece of hot glass left to cool, and meanwhile to undergo electrolysis by the action of the electric forces of the field; and when cold carried out of the field.

The important part here played by the element *time*, renders it quite impossible to maintain *à priori* that the above hypothetical equation should hold under all circumstances: the proof would need to be experimental.

The investigation given below is designed to express in definite terms the effect of the somewhat general conditions therein specified.

Let us denote the potential of the primary by V, its charge by q; the specific inductive capacity of the dielectric placed in the field of force by K; and the electrostatic capacity of the whole system by C. Then the theorem is that the magnitude and sign of the "apparent electrification" of the dielectric are given by an equation of the form—

$$h = -\left(\frac{d\pi}{dV} + V\frac{dC}{dK}\right),$$

where h denotes the rate of change of the apparent electrification of the dielectric with regard to the specific inductive capacity K as independent variable; and π denotes the rate of change of the work done against electrical forces with regard to the same independent variable.

By translating the theorem into the language of magnetism, we obtain a theorem relating to magnetic induction in matter placed in a magnetic field of force.

Proof.

The dielectric being supposed in the field of force, let the specific inductive capacity be changed. The influence of this change of specific inductive capacity of the dielectric on the electrical state of the primary can be expressed by taking as independent variables the potential V of the primary, and the specific inductive capacity K of the dielectric. Due to an arbitrary change of potential δV, and an arbitrary change of specific inductive capacity δK, there will be an augmentation δq of the charge of the primary—by connecting it to proper sources of electricity—given by an equation of the form—

$$\delta q = C \,.\, \delta V + V \,.\, \frac{dC}{dK} \,.\, \delta K + h \,.\, \delta K \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

The first term of the right hand member expresses the well-known relation between the charge, the potential, and the capacity of an electrical system; the second term expresses the effect of the change of capacity caused by the alteration of specific inductive capacity; and the third term expresses the effect of the electrification of the dielectric due to the same cause. What I propose to show is, that the quantity h need not be zero, unless under very special circumstances.

As it appears in (1), $h \, . \, \delta K$ is clearly the quantity of electricity that must be given to the primary in order to maintain the potential constant whilst the specific inductive capacity is altered by δK, and this in addition to the influence of the mere change of capacity of the system.

We may assume as a well-known result that for a closed cycle of operations

$$\int \delta q = 0,$$

and δq is a perfect differential.*

Expressed in words, this is equivalent to stating that, when after undergoing a series of changes, the potential is brought back to any given value V, and the molecular condition of the dielectric in the field of force is brought back to its initial state, then the charge of the primary is the same as at first.

The analytical statement of the condition that δq in (1) is a perfect differential gives us—

$$\frac{dC}{dK} = \frac{d}{dV}\left(V \frac{dC}{dK} + h\right),$$

or

$$\frac{dh}{dV} + V . \frac{d}{dV}\left(\frac{dC}{dK}\right) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

In order to obtain another relation between the quantities, let us denote by $\delta\epsilon$ the increment of electrical energy of the system during the series of operations described above as leading to (1). This is expressed by an equation of the form—

$$\delta\epsilon = V \delta q + \pi \delta K. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The meaning of the first term of the right hand number of (3) is obvious; the other term, $\pi \delta K$, denotes the work done *against* electrical forces when the specific inductive capacity of the dielectric is *increased* by δK.

By the principle of the conservation of energy, for a closed cycle of operations

$$\int \delta\epsilon = 0,$$

and $\delta\epsilon$ is a perfect differential. Hence, if we express the analytical condition of this, after putting for δq its value from (1), we get—

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* I would here acknowledge my great indebtedness to a paper on the "Conservation of Electricity," by M. G. Lippmann, "Annales de Chimie et de Physique," 5^{me} Sér., T. 24 (1881) p. 145.

$$\frac{d}{dV}\left(V^2 \cdot \frac{dC}{dK}+hV+\pi\right)=\frac{d}{dK}(VC).$$

Performing the differentiations and making use of (2) we have—

$$\frac{d\pi}{dV}+h+V\frac{dC}{dK}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

If (4) be differentiated with respect to V and (2) be again applied, we find—

$$\frac{d^2\pi}{dV^2}+\frac{dC}{dK}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

Hence, finally, the theorem can be expressed in either of the forms—

$$h=-\left(\frac{d\pi}{dV}+V \cdot \frac{dC}{dK}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

$$h=-\left(\frac{d\pi}{dV}-V \cdot \frac{d^2\pi}{dV^2}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6'.)$$

Since as a rule π will probably increase with V, $\frac{d\pi}{dV}$ will usually have the same sign as π.

The form (6′), amongst other uses, enables us to get at once an important result, viz., the circumstances under which h is zero. We have $h = 0$ when—

$$\frac{d\pi}{dV}-V \cdot \frac{d^2\pi}{dV^2}=0.$$

Integrating twice we get successively—

$$\frac{d\pi}{dV}=aV,$$

where a is an arbitrary constant; and

$$\pi=\tfrac{1}{2}aV^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

The constant of the second integration will in general be zero.

Equation (7), taken in connexion with (5), gives by differentiation,

$$\frac{dC}{dK}=\text{const.}=-a.$$

It appears therefore that in order to have no electrification of the dielectric when the specific inductive capacity is altered, the change of capacity of the system must be proportional to the change of specific inductive capacity.

Remark also that since $\frac{dC}{dK}$ must in general be positive, the quantity a, and therefore also π, must be negative, by (7).

It is not difficult to prove that the condition $\frac{dC}{dK}=$ const. leads to the conclusion that the whole electric field must be occupied by an electrically homogeneous dielectric.

The following proof seems to be convenient. Let us imagine the assumed heterogeneous medium to consist of shells of dielectric material whose boundaries are equipotential surfaces; each shell is supposed to be itself homogeneous. If the bounding equipotential surfaces consisted of excessively thin conducting shells, the distribution of electric force in the field would be unaltered. Each consecutive pair of conducting equipotential surfaces with the (homogeneous) shell of dielectric between, would then form a condenser. And since the same quantity of electric induction crosses all the equipotential surfaces in the field, the capacity of the whole system would be simply that of a series of condensers in "cascade."

When air is the dielectric, denote the capacity of the condenser which consists of the primary, the first conducting equipotential surface, and the space between, by C_1; the capacity of the condenser formed by the first and second equipotentials by C_2, and so on. If, instead of air, the spaces be respectively filled up by dielectrics of specific inductive capacities K_1, K_2, and so on, we have for the capacity of the whole system C, the relation—

$$\frac{1}{C}=\frac{1}{K_1C_1}+\frac{1}{K_2C_2}+\ldots$$

Replacing the shells of dielectric by others of different specific inductive capacity, and denoting the changed quantities by dashes, we have—

$$\frac{1}{C'}=\frac{1}{K_1'C_1}+\frac{1}{K_2'C_2}+\ldots$$

Let now $\quad K_1'-K_1=K_2'-K_2=\ldots=\delta K,$

so that the alteration of specific inductive capacity is the same for all the dielectrics.

It is evident that unless—

$$K_1=K_2=K_3=\ldots$$

and $\quad K_1'=K_2'=\ldots$

it is impossible that—

$$\left(\frac{C'-C}{\delta K}\right)$$

should be independent of δK.

The conclusion given above follows at once.

To sum up the discussion, the result is that the equations (5), (6), and (6′) express the effect of heterogeneity in the constitution of the dielectric medium.

Note.—It may be well to notice here an objection that might be raised against the validity of the above theorem. It could be urged, that since Dr. Hopkinson has found by experiment* that no change of specific inductive capacity could be detected when glass was subjected to electric stress varied through a very wide range of magnitudes, the quantity π in the theorem has no existence. The experiments just referred to, however, only prove that the quantity π is very small. It is shown in Part II of this paper that for most substances π has a value different from zero, being positive in some cases, negative in others.

PART II.

Application to the Theory of the Origin of Electrification by Friction.

The rubbing together of two bodies is the most ancient means known of obtaining electricity. The absence of any accepted explanation of this historical mode of rendering a body electrified does not need to be enlarged upon.

I have ventured to entertain the hope that the general theorem proved above, together with the experimental results obtained by Dr. Kerr in his memorable researches in the region of "electro-optics," may be found to prove adequate to the explanation of the fundamental and important subject of electrification by friction.

As is well known, Dr. Kerr has proved that transparent dielectrics become as a rule doubly refracting when subjected to electric force. Under the action of electric stress, a dielectric becomes strained. With the electric stress different at different parts of the field of force, the strain varies from point to point. This space-variation of strain manifests itself optically by the material assuming the property of converting plane polarised into elliptically polarised light, when the incident light is passed transversely across the direction of the electric induction in the dielectric, and the plane of polarisation is inclined at an angle to this direction.

Moreover, as has been pointed out and proved experimentally by Prof. Quincke,† the electrically-induced strain—the effect of which Dr. Kerr observed as double refraction—produces a change in the index of refraction. When the strain is uniform, Quincke has shown that no double refraction ensues. Doubly refracting properties are

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* "Phil. Trans.," vol. 172, p. 355 (1881).

† Quincke, "On Electrical Expansion," "Phil. Mag.," Ser. 5, vol. x, p. 30.

assumed only in a field of force which is not uniform from point to point.

Dr. Kerr has made the remarkable discovery that some dielectrics become optically "positive," others "negative," when subjected to electric stress. I think it may be inferred from Prof. Quincke's experiments just referred to, that those bodies which Dr. Kerr found to be "positive" have their index of refraction *decreased* by electric stress; "negative" bodies on the contrary have their index of refraction *increased.* I am not aware that this point has been decided, but I hope shortly to investigate it in the laboratory of University College, London.

The sign of the change of index of refraction is not essential to the present discussion. We will assume, however, simply for convenience of statement, that a "positive" dielectric experiences a *decrease*, and a "negative" dielectric experiences an *increase* of index of refraction when placed in a field of electric force.

Now, whatever opinion may be held concerning the electromagnetic theory of light, there can be no doubt that along with change of index of refraction of a dielectric, there goes always change of specific inductive capacity. With the supposition we have made above regarding the sign of the change of index of refraction produced in the dielectrics examined by Dr. Kerr, his results when expressed in electrical terms translate into the statements that: a "positive" dielectric has its specific inductive capacity *decreased* by electric force; a "negative" dielectric has its specific inductive capacity *increased* by electric force. In view of the theorem proved in Part I of this communication, this form of statement is very important.

It means that if the specific inductive capacity of a "positive" body be decreased in presence of a field of force, then the electric forces assist this change—work is done *by* these forces. On the other hand, if the specific inductive capacity be increased, work is done against the forces of the field.

We get corresponding statements for "negative" bodies by changing signs.

Let us return now to equation (6). It is—

$$h=-\left(\frac{d\pi}{dV}+V\cdot\frac{dC}{dK}\right).$$

Let us suppose that the dielectric is placed in a field of zero force. Then, with the disposition of apparatus that we assumed at the beginning of Part 1, $V=0$, and the second term of the right hand number is zero. But the first term $\frac{d\pi}{dV}$ need not necessarily vanish with V. Let us denote by $\left(\frac{d\pi}{dV}\right)_0$ the value of $\frac{d\pi}{dV}$ when $V=0$; then according to the view adopted in this paper, $\left(\frac{d\pi}{dV}\right)_0$ is a quantity which has a value characteristic of each material, and may be regarded as a property of each material in the same sense as, for instance, the index of refraction.

Let now the specific inductive capacity of the dielectric be increased by δK. Thus the *tendency* is for the dielectric to become electrified with a quantity of electricity—

$$h\delta K = -\delta K\left(\frac{d\pi}{dV}\right)_0.$$

This tendency being equal in all directions there is no resultant electrification. If, however, another dielectric is put into close contact with the first, dissymmetry is introduced. Denoting by the suffixes $(_1)$ and $(_2)$ the quantities relating to the two dielectrics, and by ΔE the electrification, we have initially,

$$\left.\begin{aligned}\Delta E_1 &= -\delta K_1\left(\frac{d\pi_1}{dV}\right)_0 + \delta K_2\left(\frac{d\pi_2}{dV}\right)_0 \\ \Delta E_2 &= +\delta K_1\left(\frac{d\pi_1}{dV}\right)_0 - \delta K_2\left(\frac{d\pi_2}{dV}\right)_0\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (8.)$$

These two equations express my view of the mode in which electrification begins when two dielectrics are put into contact and their specific inductive capacities are altered. The change of specific inductive capacity may take place either by pressure or by friction—with liquids it is probable that only the heating effect of friction can influence the results.

According to what law the electrification goes on increasing when once started is a point still to be cleared up, the value for any material of the quantity π having still to be worked out experimentally.

Before discussing (8) it is convenient here to notice that when two bodies are in very close contact, the capacity of the system that consists of the two opposed surfaces and the extremely small distance between them, must be very great indeed. If then at any moment Q be the charge on either of these opposed surfaces and C denote the capacity of the system, then (6) becomes—

$$h = -\left(\frac{d\pi}{dV} + \frac{Q}{C}\cdot\frac{dC}{dK}\right)$$

$$= -\frac{d\pi}{dV}, \text{ nearly,}$$

when $\frac{dC}{dK}$ and Q are finite and C is extremely large. Hence, in considering what is happening when two bodies are rubbed together, we need only take account of the value of $-\frac{d\pi}{dV}$ for each.

To simplify discussion of (8) we will take the second body as "neutral"; *i.e.*, $\left(\frac{d\pi}{dV}\right)_0=0$. We shall see that boxwood appears nearly to fulfil this condition. Also for brevity and convenience, we will put $\frac{d\pi}{dV}=\alpha$.

Two cases arise for consideration.

Case I.

$\frac{d\pi}{dV}(\alpha)$ positive, *i.e.*, work is done against electric forces by increasing the specific inductive capacity.

(*a*). "Positive" liquids. If a liquid dielectric be warmed, the index of refraction, and therefore the specific inductive capacity, is decreased. Hence, by friction there is a change of specific inductive capacity $-\delta K$ to be expected. Using ΔE in the same sense as in (8)—

$$\Delta E=+\alpha_0\delta K.$$

It is shown by the experimental results quoted below, that ΔE positive indicates that by friction this class of liquids tends to become positively electrified. And since in this particular case the sign of ΔE is the same as that of the electrification, it ought to hold in general. It will be seen that this is true.

(*b*). "Positive" solids. Here friction, by raising the temperature of the surface, tends to change the specific inductive capacity by an amount $+\delta K$,

$$\therefore \Delta E=-\alpha_0\delta K.$$

Such bodies tend to become negatively electrified by friction.

Case II.

$\frac{d\pi}{dV}(\alpha)$ negative. Here work is done by electrical forces when the specific inductive capacity is increased.

(*c*). "Negative" liquids. Friction tends to decrease the specific inductive capacity by an amount $-\delta K$,

$$\therefore \Delta E=-\alpha_0 . \delta K.$$

Hence "negative" liquids tend to become negatively electrified by friction.

(*d*). "Negative" solids. Friction tends to increase the specific inductive capacity,

$$\therefore \Delta E = +\alpha_0 \, . \, \delta K.$$

"Negative" solids therefore tend to become positively electrified by friction.

The conclusions under (*a*), (*b*), (*c*), (*d*) are all found to be verified by experiment.

Professor Foster has suggested to me that, in connexion with the ideas expressed by equation (8), it is interesting to find the statement by Beccaria* that the cause of the electrical difference set up between two pieces of similar silk ribbon when rubbed together lies in the unequal warming of the opposed surfaces. The oft-quoted experiments of Faraday with a feather and piece of canvas fall obviously under the same head.

I may perhaps be allowed to cite in addition some very old experiments with glass made by Bergman.† On rubbing two similar strips of glass together, the portion of the surface of either strip which received the greatest amount of friction per unit area became positive. This agrees perfectly with what may be deduced from (8). For the two strips, $\frac{d\pi}{dV} = -\alpha$ was the same; hence by (8)—

$$\therefore \Delta E_1 = \alpha_0(\delta K_1 - \delta K_2).$$

If $\delta K_1 > \delta K_2$ we get ΔE_1 positive, as Bergman found.

Experiments on the Electrification of Steam by Friction.

The experimental results given below are far from complete; but I venture to publish them as affording in some measure an experimental verification of the ideas put forward in Part I.

The method of experimenting and the arrangement of apparatus employed were essentially the same as were used by Faraday in his classical experiments on this same subject.‡

It is needless for me to say how very much I am indebted to Faraday's observations during the whole course of this experimental enquiry. Like all that the great experimenter undertook, the record of his observations in the "Researches" is a treasure-house for later experimenters to draw supplies from.

To generate steam, a small vertical copper boiler was used, which was heated by gas led to the burner by india-rubber tubing. By placing the boiler on small blocks of shellac the insulation was found at all times to be excellent. The weather was very favourable.

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* Riess, "Reibungs-Electricität," § 914.

† *Ibid.*, § 913, fig. 175.

‡ "Experimental Researches," § 2075, *et seq.*

The electrical condition of the boiler was the thing tested in all the experiments: a gold leaf electroscope connected to it served as indicator.

The steam was led from the boiler through a straight brass tube about 1·2 cm. diameter and 120 cm. long, to a steam-globe of copper 10 cm. in diameter. This steam-globe was always kept well supplied with distilled water, as Faraday points out how essential it is to have the steam wet. A "feeder-tube" was used to contain the substance to be experimented upon: it was of glass 1·5 cm. internal diameter, and about 15 cm. long. A short length of narrow tubing, furnished with a glass stopcock, was fused to the main tube at the centre and at right angles to the axis. It was used to renew the supply of material in the wider tube below along which the current of steam was passed to sweep the material away. This arrangement was found convenient in working; and it possessed the great merit of allowing the pieces to be easily cleaned.

For the friction-piece that was rubbed by the current of fluid, I worked principally with a boxwood tube of as nearly as was convenient the dimensions of the tube described by Faraday as an "excellent exciter."*

By a fortunate chance, this tube was found to be very nearly at the neutral line where Dr. Kerr's "positive" and "negative" substances meet.

In order to find out if possible what was the meaning of some anomalous results that appeared in the experiments, a number of observations were made with, amongst others, tubes of pine, hawthorn, birch, sulphur, plaster-of-paris, and a tube formed out of a piece of carbon rod 1·2 cm. diameter, such as is used in electric lighting. The results obtained with the sulphur and plaster-of-paris tubes were interesting, but in no way decisive, as the tubes were found to have become very much disintegrated by the action of the current of steam; they are, therefore, not recorded in what follows. The results with the hawthorn tube will illustrate the effect of friction-tubes of materials whose place is on the negative side of the neutral line; carbon stands on the positive side.

Between the two lies the boxwood tube. This is well shown by the results obtained with methylic alcohol and amylic alcohol as shown in the list given below.

The wooden tubes used were always kept well soaked with distilled water.

After each day's work with the apparatus, it was taken to pieces, and the copper steam-globe and the feeder-tube were left to steep in a strong solution of carbonate of soda; then they were well rinsed out with distilled water before being used again. As occasion required,

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* "Experimental Researches," footnote, § 2102.

methylated spirit was used to give the apparatus a thorough cleansing from all traces of oil, &c.

As a valuable test of the proper working of the apparatus, oil of turpentine was constantly in use. If everything was going well, the effect of adding a small quantity of the oil to the distilled water in the feeder-tube was to make the boiler positive, and the steam negative. On continuing to blow out, the boiler quickly passed on to negative.

This was repeated as a rule between the testing of each pair of substances.

It will be observed in the list given below that there are three persistent apparent exceptions to the rule that holds for all the other substances tried; these are turpentine, sperm oil, and chloroform.

The extremely uncertain composition of the first and second of these three bodies did not allow their exceptional behaviour to assume much importance in my eyes. But that chloroform should remain an exception to the rule appears to indicate that either the influence of the water masks that of the chloroform (*vide* remarks by Faraday on Alcohol, "Exper. Res." §2115, 2116), or that there has been a change of sign in the electro-optical position of the body due to rise of temperature. These points, together with some others that have been raised in my mind in connexion with the present application of the theorem of Part I, I hope to be able to clear up by experiments in a different direction from those recorded here. It may be desirable to state also, that I began experiments in which dry compressed air was used instead of steam, but was not able to continue them.

In conclusion I desire to record my thanks to Prof. Foster, for the valuable criticisms with which he has favoured me during the preparation of this paper.

Note.—In the experimental results which follow, the sign of the electrification is that assumed by the body to whose name it stands opposite when rubbed on the material whose name is at the top of the column.

Experimental Results (November, 1884).

Name of dielectric.	Sign of electro-optical effect.	Hawthorn tube. Sign of electrification.	Boxwood tube. Sign of electrification.		Carbon tube. Sign of electrification.
			First set of experiments.	Last set of experiments.	
Distilled water	+	+	+	+	+
Carbon disulphide..	+	..	+	..	+
Paraffin wax	+	..	+	..	..
Naphthaline.......	+	..	+	..	..
Lubricating oil (petroleum ?) ..	?	..	+	..	..
Methyl iodide	+	..	+	..	..
Oil of turpentine. ..	+	−	−	−	−
Sperm oil	+	−	−	..	−
Benzole	+	..	+	..	+
Caoutchoucine	+	..	+	..	−
Spermaceti........	+	..	+	..	..
Methylic alcohol ...	+	+	+	+	−
Amylic alcohol	−	+	−	+	−
Chloroform	−	+	+	+	+
Glycerine	−	..	−	..	..
Colza oil..........	−	..	−	..	..
Castor oil	−	..	−	..	..
Olive oil	−	..	−	..	..
Cod-liver oil......	−	..	−	..	..

To the above may be added a list of solids whose electro-optical positions have been determined by Dr. Kerr. The electrical position of the first two is well known. The others I examined by rubbing them with the same boxwood tube as was employed in the experiments with fluids.

Name of dielectric.	Sign of the electro-optical effect.	Sign of electrification when rubbed by boxwood.
Glass	−	+
Quartz....................	−	+
Resin	+	−
Sulphur	+	−
Solid paraffin wax	+	−
Spermaceti	+	−
Naphthaline...............	+	−

References to Dr. Kerr's papers on electro-optics :—

"Philosophical Magazine," Ser. 4, vol. 50, 1875; pp. 337 and 446.
,, ,, Ser. 5, vol. 8, 1879; pp. 85 and 229.
,, ,, ,, 13, 1882; pp. 153 and 248.

Faraday gives a list of bodies with the sign of the electrification they acquired by friction, which is here reproduced for comparison with my results.

Name of dielectric.	Sign of the electro-optical effect.	Sign of electrification by friction.
Distilled water	+	+
Carbon disulphide	+	+
Naphthaline	+	+
Naphtha..................	+	+
Caoutchoucine.............	+	+
Spermaceti................	+	—
Oil of turpentine	+	—
Resin (dissolved in alcohol)..	+	—
Alcohol (ethyl)	—	—
Lard	—	—
Beeswax..................	—	—
Castor oil.................	—	—
Olive oil	—	—

III. "Notes on Alteration induced by Heat in certain Vitreous Rocks; based on the Experiments of Douglas Herman, F.I.C., F.C.S., and G. F. Rodwell, late Science Master in Marlborough College." By Frank Rutley, F.G.S., Lecturer on Mineralogy in the Royal School of Mines. Communicated by Professor T. G. Bonney, D.Sc., F.R.S., &c. Received May 18, 1886.

[Plates 3—5.]

In this paper an endeavour is made to show the nature of the changes which have resulted from the action of heat upon certain vitreous rocks. The changes which take place in such rocks through natural processes may sometimes be effected by heat alone, at others by heat in presence of moisture. Of these actions the latter is probably the more frequent, but, at the outset, it seems important to ascertain the action simply of dry heat before studying the more complicated conditions engendered by the presence of water and the pressure of superincumbent rock masses.

The following examples which have been operated upon are few, but typical, and the alterations which they have undergone will be found to have a certain petrological significance.

The first subject taken for experiment was a small fragment of the well-known pitchstone of Corriegills in the Isle of Arran. This was kept at a temperature ranging from 500° up to about 1100° C., during a period of 216 hours. The change visible at the end of the nine days' heating is not so strongly marked as might have been expected, the fragment still exhibiting a resinous lustre, but the colour, originally a deep green, has been altered to a deep reddish-brown or chocolate colour. The rock in its normal condition has already been described by Mr. S. Allport* and others, and a section cut from the specimen before heating presents exactly the same characters shown in Allport's drawings, published in the "Geological Magazine," in Vogelsang's "Krystalliten," Plate 13, fig. 2, and in Zirkel's "Mikroskopische Beschaffenheit der Mineralien und Gesteine," fig. 43.

A section made from the specimen now described, but prepared prior to heating, has furnished the figs. 1, 3, and 5, on Plate 3. The three right-hand figures on the same plate have been drawn from a section made after nine days' heating at a temperature ranging from 500° up to about 1100° C. For the sake of comparison the figures on opposite sides of the plate have been drawn under the same amplification.

When the artificially altered rock is examined under a power of 25 linear it presents the general appearance shown in fig. 2, Plate 3. On comparing this with fig. 1 on the same plate we see that a marked change has taken place. The clear spaces surrounding the crystallites or belonites of hornblende have increased, while the dusty-looking matter no longer shades off into the clear glass but lies within more or less sharply defined boundaries. It also presents a coarser appearance than in the section taken from the unaltered specimen. With increased amplification the character of the change becomes more clearly perceptible.

In fig. 4, Plate 3, we find, on comparing it with fig. 2 on the same plate, that the hornblende belonites themselves have undergone very considerable alteration. They have to a great extent lost their frond-like appearance. Their delicate lateral growths seem to have shrivelled up, and their green or greenish-brown colour has changed to a deep rusty brown. Their stems or central rods have become opaque, and the lateral fringes frequently share this opacity. They seem, in fact, mere withered representatives of the greenish fern-like crystallites which occur in the natural state of the rock, and the change appears

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* "On the Microscopical Structure of the Pitchstone of Arran," "Geol. Mag.," 1872.

to consist in the peroxidation of the protoxide of iron in the hornblende. The general aspect of these crystallites is much darker than that of their unaltered representatives, and they stand out in bold contrast to the clear glass around them.

On looking at the left-hand portion of fig. 4, Plate 3, we see a number of coarse spiculæ which under a lower power, as in fig. 2, Plate 3, appear merely as stippling. This stippling in fig. 2 is the altered condition of those parts of fig. 1 which are softly shaded, or seem to be so under an amplification of 25 linear. When magnified 250 diameters this portion of the normal rock still appears finely stippled, but contains numbers of very minute spiculæ, as shown in fig. 3, Plate 3. When we compare this stippling in fig. 3 with the coarse spiculæ on the left of fig. 4, Plate 3, the extent of the alteration produced by the nine days' heating becomes striking. It is probable that these spiculæ are hornblende, and that they are evidently a further development of the much smaller ones so plentiful in the glassy ground-mass of the unaltered rock. In the dusty looking parts of the unaltered glass we find, under an amplification of 1150 diameters, some indication of the source from which this wealth of crystalline spiculæ has been derived, fainter and smaller spiculæ being visible, together with sparsely distributed dark specks, a few blunt-ended colourless microliths, and a profusion of colourless globulites, as in fig. 5, Plate 3.

The spiculæ shown on the left of fig. 4 are again represented in fig. 6, Plate 3, under a power of 1150 diameters. They are grouped in a stellate manner and constitute a large proportion of the rock, while the fine dusty matter previously visible has almost entirely disappeared. Although, through the agency of dry heat, we have here an instance of further crystalline development, yet no approach to a felsitic structure is discernible in the glassy matter of this rock; the new crystallites which have been formed being certainly neither felspar nor quartz, but possibly actinolite.

The next specimen to be considered is a piece of black obsidian from Ascension, about as typical an obsidian as it would be possible to find, and showing a faintly banded structure. The general microscopic character of this rock is shown in figs. 1 and 3, Plate 4, fig. 1 being magnified 25, and fig. 3 570 diameters. In fig. 1 the banded structure is well marked, and streams of colourless microliths lie with their longest axes in the general direction of the banding. A fragment of this rock was kept for the same period at the same range of temperature adopted in the case of the Arran pitchstone.

The rock in its normal condition is a deep black glass with a well-marked conchoidal fracture. In section, when not very thin, it appears by transmitted light of a brown or coffee colour, and contains, as already stated, numerous microliths, mostly bacillar or spicular, sometimes in rectangular forms, and often shaped like a butcher's meat-tray. In the artificially heated specimen a remarkably vesicular structure has been developed, the rock, in fact, has become filled with vesicles, mostly spherical, or approximately so. The sand in which the specimen was heated has adhered firmly to its surface. Vitreous lustre is visible on fractures. Under the microscope between crossed Nicols the rock shows no sign of devitrification from its protracted heating, but the section is full of the large vesicles which have been developed (fig. 2, Plate 4). The glass still contains great numbers of microliths, a few small stellate or cruciform groups being here and there visible, but it seems probable that they are fresh developments, and that the old ones have been dissolved, for there is no longer any banded structure, or only very faint indications of it, the microliths lying in all directions and not in streams. This view is favoured by the almost necessary assumption that the rock must have been reduced to a condition bordering on fusion to have permitted the development of such an extremely vesicular structure, while further evidence of this is seen in the firm adhesion to the surface of the specimen of the sand-grains in which it was embedded during the process of heating. In spite, however, of the great molecular change of position implied by this development of vesicles, there is no sign of devitrification, unless indeed the microliths be fresh ones formed during the heating of the specimen, or during two days in which it cooled from 800° to about 100° C. They do not probably equal those present in the normal condition of the rock.

Fig. 4, Plate 4, represents part of a section of a piece of the same obsidian, which was kept for 701 hours at a temperature ranging from about 850° to 1100° C. The specimen has been nearly fused, and is pitted on the surface by the impressions of the sand-grains in which it was embedded, a few of the grains still adhering. There is a resinous or subvitreous lustre on some parts of the specimen, but one face is dull. Internally it is full of vesicles, but a thin compact crust exists in which there are none, and this crust is continuous with the spongy vesicular portion. One or two of the cavities are nearly half an inch in diameter, *i.e.*, they occupy nearly the whole thickness of the specimen, while others are of very small dimensions. They are irregular in form, and appear as a rule to communicate with one another.

Under the microscope, with an amplification of twenty-five diameters, the section shows large irregular vesicles; their walls (*i.e.*, what remains of the rock) appearing to consist of greenish-brown matter traversed by opaque and approximately parallel bands. The translucent portions of the section seem, under this low power, to consist of micro-crystalline matter, the general aspect being that of fine dust mixed with a felted microlithic substance, while between crossed Nicols numerous doubly refracting granules and needles are visible.

Where the actual margin or outer crust of the specimen is included in the section the substance is quite transparent and colourless by ordinary transmitted light, and is seen to contain numerous green microliths.

By reflected light the whole section appears of a greyish-white, except the parallel bands, which are of a rather darker grey, and the more vitreous portion of the outer crust, which appears dark. The extreme outer crust is seen by substage illumination to be almost or quite opaque.

Under a power of 250 diameters the outer crust may be distinguished as consisting of three layers, the outermost of extreme thinness, transparent, and coffee-coloured; the next quite opaque or feebly transmitting a brown or brownish-green light. It is of much greater thickness than the outermost glassy layer, and consists of greenish-brown microliths matted parallel to one another, and directed with their longest axes at an angle to the outer surface of the specimen, the angle being sometimes nearly a right angle, and seldom less than about 20° or 30°. This shades off or fringes off into a clear colourless glass layer, also containing numerous greenish-brown spicular microliths, evidently identical in character with those which, by their massing together, form the nearly opaque band last described. It is not easy to say what these microliths are, but they appear to be some form of amphibole or pyroxene; they have, as a rule, a somewhat frayed and ragged or fibrous aspect, and it seems occasionally that they either belong to the rhombic system, or extinguish at a very small angle with their longest axes. With the exception of the glassy band in the thin outer crust, the remainder of the rock has been completely devitrified, fig. 4, Plate 4. Owing to the porous nature of the specimen it was scarcely possible to prepare a very thin section, but, judging from what can be seen, it consists of doubly refracting microliths with an admixture of minute crystalline granules.

The devitrification does not in this case appear to be precisely of the same character as that met with in naturally devitrified obsidians, but at all events we have here a proof that the action of dry heat during 701 hours has been capable of devitrifying this glassy rock.

The next specimen, a black obsidian from the Yellowstone District, Montana, U.S., was in the first instance kept at a temperature ranging from 500° to about 1100° C. for 216 hours. Some of the sand in which the specimen was heated adheres firmly to its surface. On fractures the rock is still vitreous in lustre, but it appears of a much paler colour than in its natural condition. This is probably due to the development of great numbers of small vesicles, the colour being now grey, whereas in the unaltered specimen it was black.

Microscopic examination of a section of this obsidian in its normal condition shows the presence of numerous trichites, resembling small eyelashes, and often occurring in radial or stellate groups; globulites are also plentiful, but, with the exception of these and some minute gas-pores, the obsidian is remarkably free from enclosures, although here and there a few porphyritic felspar crystals and a spherule or two occur. This was, therefore, regarded as a very favourable specimen to experiment upon.

The appearance of a section of this rock in its normal condition is represented in fig. 5, Plate 4, as seen under a magnifying power of 570 diameters. In this drawing rather faint indications of banding are shown. Part of the same section is also represented in fig. 7, magnified 570 linear, in which some of the trichites are visible. The specimen which was heated for 216 hours has developed an exceedingly vesicular structure, but apart from this it appears to have undergone but little change. The vesicles are large, fig. 6, Plate 4. Their sections are nearly all circular or approximately so. The trichites seem to have disappeared entirely, but some small opaque granules are now visible, and in some instances they have distinctly triangular sections. These by reflected light appear black and are no doubt magnetite.

In order to ascertain what change would take place by further heating, another fragment, taken from the same specimen as the preceding, was heated for a period of 701 hours at a temperature of from 850° to about 1100° C.

The rock is still vitreous, but a marked change has occurred. The specimen had no sand adhering to its surface, and it perfectly preserved its original external form, the conchoidal fractures and sharp cutting edges remaining quite distinct, but the outer surface has merely a dull sub-resinous or flint-like lustre, although on freshly fractured surfaces the lustre is quite vitreous. Here and there upon the surface *very slight* elevations occur, and these are mostly perforated by a diminutive hole, as if made with a common sewing-needle. When a point was inserted in one of these apertures and the crust was prized off, a remarkably cavernous interior was exposed, the cavities appearing to have been formed by the coalescence of more or less spherical vesicles, averaging from a quarter to half an inch in diameter.

In these cavities white crystalline pellets were found, for the most part about a third the size of the cavities in which they respectively occurred.

Three of these pellets are represented in the middle line of figures on Plate 4. Some of them are approximately spherical, and they are usually crystalline crusts, either empty or enveloping a smaller pellet of the same kind. The walls of the cavities in which they occur are also at times lined with crystals apparently of the same mineral. The pellets themselves are too friable to admit of any sections of them being cut, while no satisfactory conclusion has yet been arrived at by crushing them and examining the fragments under the microscope. Small glistening faces sometimes showing a certain parallelism of growth may be detected with the help of a lens, and, so far as general appearances go, the mineral bears a somewhat close resemblance to rhyacolite. They are, at all events, probably anhydrous silicates allied to the felspar or nepheline groups. In some cases the pellets adhere slightly to the walls of the vesicles, yet in one or two instances they appeared to be loose, but may possibly have been detached by the shock in breaking open the specimen. On examining one of these pellets by reflected light under a half-inch objective, the white crystalline surface was seen to be studded with minute black or deep blackish-red crystals, having a brilliant metallic lustre. One of them exhibited a six-sided face as shown in the bottom figure of the middle line in Plate 4. This was turned sufficiently well into position to enable all parts of the face to be brought at once into focus, when it was found that measurement of the angle formed by adjacent edges was 60°. There is, therefore, little doubt that these small crystals are specular iron, which has separated out during the process of artificial heating, no such crystals being visible in a microscopic section of the rock in its normal condition.

Under a power of 250 linear the section of this artificially heated rock still appears as a clear glass, but trichites similar to those present in the unaltered obsidian are again seen; they are, however, much more numerous. A vesicular structure still exists, and the sections of the vesicles are sometimes circular, at others oval. Two or three porphyritic felspar crystals occur in this section, one of them, apparently twinned on the Carlsbad type, has a very irregular outline, somewhat like that of a comb with broken teeth. Felspar crystals with equally irregular contours occur, however, in the unaltered rock.

In this specimen the devitrification has been confined to the formation of the white crystalline pellets, the rest is glass, containing trichites and globulites, which of course may be regarded as evidences of incipient devitrification. Still they are also present in the unaltered rock, and between the two sections the differences are barely appreciable, even under the microscope. Figs. 7 and 8 (Plate 4) show how close the resemblance is.

Being anxious to ascertain the result of dry heat upon basic as well as highly silicated glassy rocks, a small fragment of the very vesicular basalt-glass from Mokua Weo Weo, Sandwich Islands, was treated in the same manner as the previously described specimens. This became completely disintegrated in the process of section-cutting.

The specimen had, however, quite lost its vitreous lustre, and had changed from black to a pale brown colour. Another fragment of basalt-glass from Kilauea, very vesicular, but less so than the previous sample, was kept at a high temperature, about 750° to 1200° C., for a period of 960 hours.

The effect of this heating has been to completely destroy all glassy lustre. The specimen is still vesicular, but the colour has, like that of the Mokua specimen, changed from black to purplish-grey or light brown.

Fig. 1, Plate 5, shows the general character of a section of the unaltered basalt-glass of Kilauea, cut from the same specimen as that submitted to the furnace. The drawing, made under an amplification of 25 linear, shows portions of three vesicles, several crystals of olivine, some small spherulites, and numerous crystallites, in a clear brown glass. The minute crystallites in this lava are extremely beautiful, and especially worthy of careful study. They frequently assume delicate pectinate forms, which one would think quite as likely to be preserved or to be re-formed after the fiery ordeal as the little trichites in the obsidian previously described. A glance, however, at the section prepared from the altered specimen at once dispels all hope of seeing them again, or indeed of seeing anything which is not translucent, and does not occupy the entire thickness of the thin section which, in spite of its thinness, appears of a deep brownish-black and perfectly opaque. When examined by reflected light under a power giving about 30 diameters it is then seen to be of a deep reddish-brown colour with paler streaks, often cracks, which circle round the porphyritic crystals of olivine, which are still present and seem to have undergone little or no change (fig. 2, Plate 5). These cracks represent perlitic structure, evidently due to unequal tension in the glass surrounding the olivine crystals. The opacity of the altered rock must probably be attributed to the passage of some of the protoxide of iron in the normal rock into the state of peroxide, thus giving rise to the formation of magnetite. The normal rock is scarcely, if at all, magnetic, but the altered specimen causes a strong deflection of the needle.

This passage of a clear basic glass into a perfectly opaque substance is a point of considerable interest, since it seems highly probable that some of the opaque and extremely scoriaceous lapilli in certain volcanic tuffs of palæozoic age are simply fragments of basic glassy lavas, like those of the Sandwich Islands, which have undergone a change similar to that here described. That in course of time the magnetite in some of these ancient lapilli should pass into limonite is also a point worth bearing in mind.

It seems probable that some of the opaque lapilli and dust in the volcanic ejectamenta of Snowdon and Brent Tor, especially those of the latter locality, where the lavas are chiefly of a basic character, may be simply altered fragments of vesicular basalt-glass.

The following specimens were prepared some years since by Mr. G. F. Rodwell, late Science Master in Marlborough College. Fig. 3, Plate 5, represents part of a section made from a black vesicular glass which resulted from the fusion of some of the basalt from the Giants' Causeway, in an ordinary Stourbridge-clay crucible, over a gas furnace. The mass was then rapidly cooled. The glass appears perfectly clear, and shows merely vesicles and irregular cracks. Another sample of the same rock was also fused in the same manner, but was allowed to cool slowly. A section cut from this (fig. 4, Plate 5) is translucent only on certain edges, where a prismatic structure is visible, the marginal portions of the prisms showing a radiating crystalline or fibrous character as indicated in the drawing. The rest of the section is opaque, as in the altered Kilauea lava. It may be added that a somewhat similar prismatic structure is occasionally to be seen in opaque specimens from the Sandwich Island lavas. Among Mr. Rodwell's numerous experiments he placed a fragment of cold basalt from the Giants' Causeway upon the surface of a molten mass of the same rock and allowed it to sink to the bottom of the crucible. Figs. 5 and 6, Plate 5, represent parts of a section taken through the enveloped fragment and the enveloping glass. The latter is quite clear, as shown on the right of fig. 6. On the left of this figure will be seen a belt of opaque matter, which intervenes between the fragment of basalt, seen on the left of fig. 5, and the clear glass on the right of fig. 6; while on the right of fig. 5 we see more of this opaque belt where it comes in contact with the enveloped fragment of basalt. The boundaries of this opaque belt are sharply defined, especially where it adjoins the clear glass. Within this dark belt are numerous radiating groups of colourless, transparent, acicular crystals, the terminations of some of them being shown in the dark half of fig. 5, Plate 5.

Whether the formation of this opaque zone is due to the chilling influence of the cold fragment on the hot magma into which it sank, is a question which may have its bearings upon the formation of tachylyte at the sides of intrusive veins and dykes of basalt where the molten mass has come in contact with the walls of a fissure. It may also be that the fragment of basalt parted with its heat less rapidly than the surrounding magma, and that the latter consequently cooled more slowly where it came in contact with the fragment. This seems at least a plausible explanation, and harmonises with the results of Mr. Rodwell's earlier experiments, shown in figs. 3 and 4, Plate 5.

Additional investigations will be made upon the rocks treated of in this paper, since the results hitherto obtained have been effected under conditions which may not be regarded as the most favourable for producing changes similar to those which take place in nature, and it is hoped that another series of experiments may throw a better light upon the threshold of this subject.

The pitchstone from Arran (figs. 2, 4, and 6, Plate 3), the obsidian from Ascension (fig. 2, Plate 4), the obsidian from the Yellowstone (fig. 6, Plate 4), and the basalt-glass from Kilauea (fig. 2, Plate 5), were all heated at the same time and under the same conditions as the slab of "British plate" glass represented in figs. 1 and 2, Plate 6, in a paper on devitrified glass laid before this Society last year.[*]

This slab was $\frac{3}{4}$ inch thick, and the heat employed was sufficient to completely devitrify it, and also to partially fuse the glass.

Another piece of "rough plate" glass, one inch thick, containing less lime, and consequently less easily devitrified than the slab just mentioned, was also heated in the same kiln as the obsidians described in this paper, and was completely devitrified, with the exception of a spot about $\frac{1}{8}$ inch in diameter near the centre.

The obsidian from Ascension (fig. 4, Plate 4), and that from the Yellowstone (fig. 8 and small central figures on Plate 4) were in the kiln 701 hours. In the same kiln were the following specimens of artificial glass figured in the paper just cited.

Specimen 132, fig. 4, Plate 6.
" 136, " 5, "
" 137, " 3, "
" 143, " 9, Plate 4.

The cause of the vesicular character, and the changes in volume produced by dry heat in the rocks described in this paper, the enormous swelling which is frequently manifested, and, on continuance of heat, the return to about the original bulk, without appreciable loss of weight, and also the question of the presence of water, will supply materials for a future paper, in which it is hoped that some experiments on changes induced in vitreous rocks by heat in presence of water vapour may also be described.

Alteration of Pitchstone.

Plate 3.

1. Pitchstone, Corriegills, Arran. ×25.
2. The same kept at a dry heat ranging up to about 1100° C., 216 hours. ×25.

3[†]. Pitchstone, Corriegills. ×250.

4. The same after heating from 500° to *cir.* 1100° C., for 216 hours. ×250. Showing the alteration of the hornblende belonites to shrivelled (rusty coloured) bodies, and the coarse spiculæ developed in certain parts of the ground-mass.

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[*] "Proc. Roy. Soc.," vol. 39 (1885), pp. 88–107.

[†] Figs. 1, 3, and 5, Plate 3, represent the rock in its normal condition.

5*. Pitchstone, Corriegills. × 1150. Showing portion of the ground-mass containing globulites and microliths.

6. The same after heating from 500° to *cir.* 1100° C. for 216 hours. × 1150. Showing the alteration which the ground-mass, corresponding to that in fig. 5, has undergone.

Alteration of Obsidian.

Plate 4.

1†. Black obsidian, Ascension. × 25.

2. The same after heating for 216 hours from 500° to 1100° C. × 25.

3†. Black obsidian, Ascension. × 570.

4. The same after heating for 701 hours from 850° to 1100 C. × 570.

5†. Black obsidian, Yellowstone, Montana, U.S. × 25.

6. The same after heating for 216 hours from 500° to 1100 C. × 25.

7†. Black obsidian, Yellowstone. × 570.

8. The same after heating for 701 hours from 850° to 1100 C. × 570.

Of the small figures occupying the middle line in the plate the three upper ones represent greyish-white crystalline pellets, frequently hollow, and often bearing minute crystals of specular iron. These pellets were taken from the large vesicles developed in the Yellowstone obsidian by heating from 850° to 1100° C. during 701 hours. × 4.

The lowest figure in the middle line represents one of the minute crystals of specular iron which was attached to the outer surface of one of the above pellets. × 120.

Alteration of Basalt-glass.

Plate 5.

1‡. Vesicular basalt-glass, Kilauea, Sandwich Islands. × 25.

2. The same after exposure to a dry heat for 260 hours at a temperature ranging from about 700° to 1200° C. × 25.

3. Vesicular glass formed by the fusion of basalt from the Giant's Causeway, Antrim, in a Stourbridge-clay crucible; the fused mass having been rapidly cooled. × 55.

4. The same rock similarly fused and slowly cooled. × 55.

5. Fragment of basalt (Giant's Causeway) placed on the surface of a molten mass, similar to No. 3, and allowed to sink into it. On the left is the fragment of basalt, and on the right the

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* Figs. 1, 3, and 5, Plate 3, represent the rock in its normal condition.

† Figs. 1, 3, 5, and 7, Plate 4, represent the rocks in their normal condition.

‡ Fig. 1 and the left half of fig. 5, Plate 5, represent the rocks in their normal conditions. In the latter case, however, there may be slight alteration.

PITCHSTONE

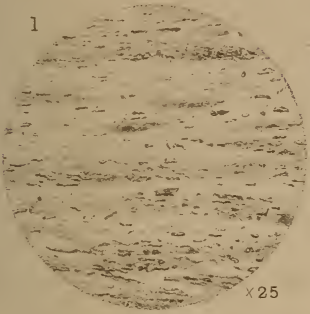
Frank Rutley del. West, Newman & Co lith.

Rutley. *Proc. Roy. Soc. Vol. 40. Pl. 4.*

OBSIDIAN

NORMAL ALTERED

1



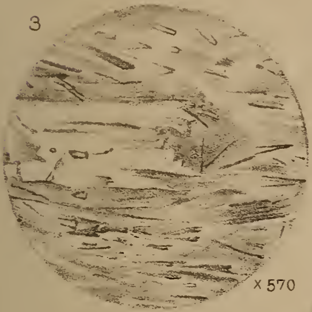
× 25

× 4

2

× 25

3

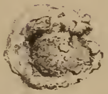


× 570

× 4

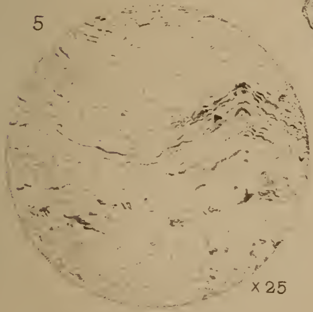
4

× 570



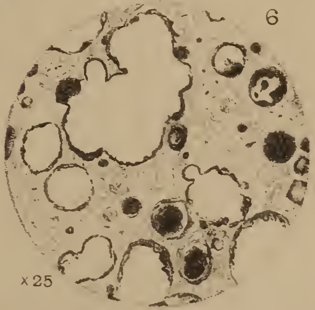
× 4

5



× 25

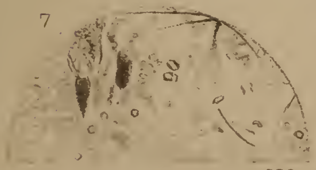
6



× 25

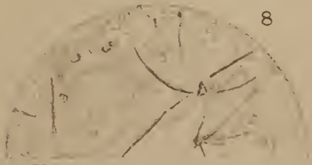
× 120

7



× 570

8



× 570

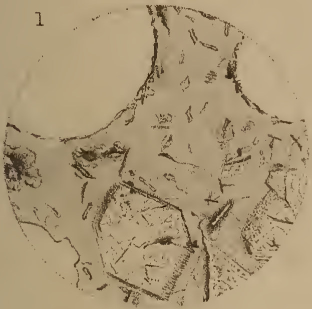
Frank Rutley del. West, Newman & Co. lith.

BASALT-GLASS.

NORMAL

ALTERED.

1



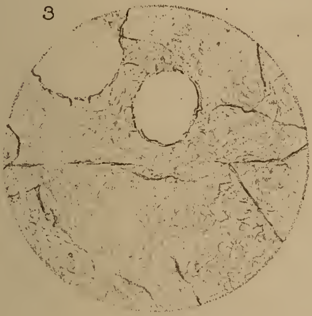
× 25

2

× 25

ALTERED

3



× 55

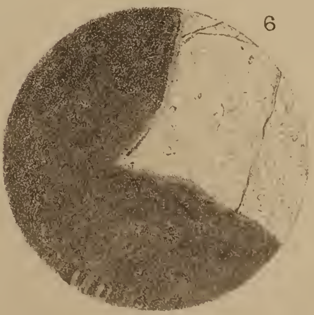
4

× 55

5

× 120

6



× 120

Frank Rutley del.

West, Newman & Co. lith

dark tachylytic belt formed between it and the clear basalt-glass. × 120.

6. Portion of the same preparation, showing the sharp division of the tachylytic belt from the clear basalt-glass. × 120.

IV. "On the Relation between the Thickness and the Surface-tension of Liquid Films." By A. W. REINOLD, M.A., F.R.S., Professor of Physics in the Royal Naval College, Greenwich, and A. W. RÜCKER, M.A., F.R.S. Received May 15, 1886.

(Abstract.)

Plateau, Lüdtge, and van der Mensbrugghe have investigated experimentally the relation between the thickness and surface-tension of thin films. None of these observers, however, have used films thin enough to show the black of the first order of Newton's colours. The authors have therefore made a careful comparison of the surface-tension of black films with that of coloured films, the thickness of which was from 10 to 100 times greater. The principle of their method is the same as that utilised in Lüdtge's experiments. The interiors of the films to be compared are connected, and the relation between their surface-tensions is deduced from measurements by which their curvature is determined. In the authors' experiments a cylindrical film was thus balanced against another, which, though sometimes cylindrical and sometimes spherical, was initially of the same curvature as itself. The necessity for this arrangement arises from the fact that the authors' previous observations have shown that a film thins to the black of the first order more readily if it is cylindrical than if it is of any other form. The fact that small changes in the forms of cylindrical and spherical films, attached to two circular rings, convert them into unduloids or nodoids, renders the mathematical theory somewhat complicated, but other considerations have been made to give way to the necessity of obtaining films which readily yield the black.

The *sensitiveness* of the methods employed by the authors and by previous experimenters is investigated. All these methods depend upon the measurement of a length, such as the change in the diameter of a cylinder, or in the sagitta of a spherical segment (Lüdtge and van der Mensbrugghe), or the displacement of the liquid in a manometer tube (Plateau).

Let an increment $d\mathrm{T}$ in the surface-tension T produce an alteration $d\mathrm{L}$ in this length. The fraction $\mathrm{T}\,d\mathrm{L}/d\mathrm{T}$ is taken as a measure of the *sensitiveness* of the experiment. If $d\mathrm{L}$ and $d\mathrm{T}$ are infinitely small, this

is called the limiting sensitiveness. In any experiment the observed value of dL divided by the sensitiveness gives the fractional change of surface-tension.

The numbers expressing the sensitiveness indicate the relative merits of the different experimental arrangements. Some of the authors' experiments were more sensitive than those of any previous observer, but the possibility of improvement in this direction is limited by the fact that if the arrangement is too sensitive the films do not remain sufficiently steady for accurate measurement.

The apparatus and method of observation are then described. Preliminary experiments were instituted to test the observations of Lüdtge and van der Mensbrugghe as to the difference of surface-tension between two films one of which had been formed more recently than the other. These experiments showed that when one of the films was kept thick by supplying liquid to its upper support (flooding), while the other was allowed to thin, a considerable apparent difference of surface-tension was obtained. Before, however, this could be accepted as a trustworthy determination of an actual difference of surface-tension, several possible sources of error had to be considered. Thus, experiment shows (1) that the fact that the thicker film displays the greater surface-tension cannot be attributed to any peculiarity of the apparatus or mode of thickening adopted; and (2) that it is not due to the weight of the thicker film.

It was also important to determine whether the gradual disappearance of the liquid rings by which the films were attached to their solid supports could produce changes in the forms of the films which would account wholly or partially for the phenomenon observed. Two films of the same surface-tension attached to two rings of different sizes could not both be cylinders, as the curvature and therefore the pressure exerted on the internal air would be different. A difference in the forms of the films might thus be due, not to the difference of surface-tension but to an inequality in the magnitude of their supports. The solid rings were accurately turned to the same diameter, but it was thought possible that the draining away of the small liquid masses by which the thinning film was attached to the solid rings might produce alterations equivalent to a change in the diameters of the latter. To test this the following method was adopted.

Theory shows that provided the form of the film does not differ much from a cylinder, the ratio dT/T can be readily obtained by means of an expression which does not involve the diameters of the supporting rings. To make use of this expression, it is necessary to measure three diameters of the film. The ratio dT/T can also be calculated from measurements of the *principal ordinate* (*i.e.*, the maximum or minimum ordinate which lies halfway between the rings), and from the sensitiveness, in the calculation of which the assumption is made that the generating curve of the film passes through the edges of the upper and lower cups.

The result of comparing the values of dT/T obtained by the two methods was to show that only a small part of the difference of form of two films could be ascribed to a slipping of the film over the liquid attachments to the solid supports.

The method of measuring three ordinates was applied to a great number of films, and gave results from 0·5 to 1 per cent. lower than those obtained from the sensitiveness. In the case, however, of two films which were both flooded, and presumably in the same state, the two methods gave results, the differences between which were both more irregular and less in amount, averaging not more than 0·2 per cent. It is therefore probable that when the bulging or contraction of a film becomes considerable there is a little slipping, though not more than enough to account for a small part of the total change of form observed.

Phenomena similar to those described are explained by van der Mensbrugghe by the consideration that when the surface of a film is being continually renewed, it is cooled, and its surface-tension is in consequence increased. This explanation is shown to be inadequate. A colourless film being certainly 250 times thicker than a black one, the increase of surface-tension due to cooling would be only 0·0016 per cent., whereas in some of the experiments there is evidence of a difference of 9 per cent.

The cause of the phenomenon cannot at present be assigned with certainty. Perhaps many causes are at work. Experiments designed to test the effects of oxygen and carbonic acid on the result, though not conclusive, indicate that the phenomenon under discussion is affected by the nature of the atmosphere in which the films are formed. Reasons are given for the conclusion that it is merely an instance of the difficulty which many observers have found in preserving a liquid surface pure.

On the assumption that the rapid change in the surface-tension of a newly formed film is not due to its thinning, but to a disturbing cause, attempts were then made to eliminate this cause, or reduce it so as to compare films of very different thicknesses.

Two methods of attacking the problem were carried out. In the first the procedure was as follows:—The diameters of two cylindrical films were measured when they were in the same state, an electric current was then passed up one of them in order to thicken it, and after a sufficient length of time had elapsed for the direct effect due to the disturbance produced by the current to pass off, the diameters were again measured. By this means it was possible to compare two films, one of which was nearly all black, while the other displayed a little black and the colours of the first and second orders. Both films were then allowed to thin, and assuming (in accordance with previous observations of the authors) that that which was already black remained in a constant state, any change of diameter which took place as the coloured film became black, could be regarded as due to changes in the thinning film.

An objection to which this method is open requires discussion. Most of the films observed were partly black and partly coloured. If any difference of surface-tension existed between the different parts, the films would not be cylinders or simple unduloids, but the black and coloured parts would have different curvatures. Measurements based on the sensitiveness would not therefore be trustworthy, if, owing to this cause, appreciable changes took place in the forms of the films, or in the pressures which they exerted upon the enclosed air. It was therefore necessary to investigate the form of a film consisting of two parts of different surface-tensions, assuming that it does not much differ from a cylinder. As a result of the mathematical investigation, a table was drawn up giving the ratio of the change of pressure due to a change of surface-tension affecting a part of the surface, to that produced by an extension of the change over the whole film.

Next follows a detailed description of a number of experiments in which two cylinders were balanced against each other, and the electric current was made use of to influence their rate of thinning. The theory applied to the results of these experiments gave the percentage change of surface-tension due to change of thickness.

In a second group of experiments a cylinder was balanced against a sphere. As a spherical film thins more slowly than a cylinder, a comparison between a thick film (sphere) and a black or partially black film (cylinder) could be made without having recourse to an electric current, and greater differences of thickness were obtained than in the earlier observations.

The differences of surface-tension measured in these observations were very small. They never exceeded 1·5 per cent., and the black films were sometimes more and sometimes less curved than the thicker films with which they were compared. There was no evidence of any regular change in the surface-tension as the thickness diminished, and the average difference between the tension of the black and coloured films as deduced from fifteen experiments was only 0·13 per cent.

The general result of the inquiry therefore appears to be that *when the black part of a soap film forms in the normal way, spreading slowly over the surface, no evidence of any change in surface-tension dependent on the thickness of the film is furnished by a direct comparison of the tensions of thin and thick films over a range of thickness extending from* 1350 *to* 12 *millionths of a millimetre.*

This conclusion is based upon a method of experiment by which a change of $\frac{1}{2}$ per cent. in the value of the tension must have been detected, had it existed, and upon fifteen independent comparisons of the tensions of black and coloured films.

The whole of the observations were carried out under the conditions which the previous researches of the authors have shown to be necessary to maintain the composition and the temperature of the films unchanged.

The authors next discuss the bearing of their observations upon the question of the magnitude of the so-called "radius of molecular attraction." They point out that if the mere equality in the surface-tensions of thick and thin films is to be considered conclusive, they have accumulated much stronger evidence for the statement that the radius of molecular attraction is less than half the thickness of a black film, *i.e.*, $< 6 \times 10^{-6}$ mm. than Plateau produced for the assertion that 59×10^{-6} mm. is a superior limit to its magnitude. They are, however, unwilling to draw this conclusion from their experiments until an explanation is forthcoming, in harmony with it, of the apparent discontinuity in the thickness of the film which always (except under very special circumstances) occurs at the edge of the black.

They are themselves inclined to look upon the sharp edge of the black as evidence of a change in surface-tension due to the tenuity of the film, and to regard the result of their experiments as fixing a superior limit (0·5 per cent.) to the difference of the tension of the black and coloured parts.

As no explanation of the discontinuity at the edge of the black has (as far as the authors are aware) ever been put forward, they conclude by a suggestion which, though no doubt of a speculative character, may serve to draw attention to a subject which is they believe of considerable interest.

They show that the main facts to be accounted for, viz., the discontinuity, the uniform thickness of the black, the wide variations in the thickness of the part of the coloured film which is in contact with the black, and the equality in the surface-tensions of the black and coloured films, could be explained if it were supposed that the surface-tension has a critical value when the thickness is somewhat greater than 12×10^{-6} mm.

The possibility of the existence of such a critical value has been pointed out by Maxwell.* It would be explained by the assumption frequently made in discussions on the nature of molecular forces, that as the distance between two molecules diminishes, the mutual force between them is alternately attractive and repulsive.

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* "Encycl. Brit.," art. "Capillarity."

V. "Experiments with Pressure on Excitable Tissues." By GEORGE J. ROMANES, F.R.S. Received May 18, 1886.

The effects of temperature on excitable tissues have been well worked out; but, so far as I have been able to ascertain, no physiologist has tried the effects of pressure. From physical analogies it appeared to me probable that increase of pressure would act on excitable tissues in a manner analogous to decrease of temperature, and conversely; but the results of my experiments have not borne out this anticipation. Nevertheless, the research seems worth publishing.

In a small glass chamber, made for the purpose, I was able to place the freshly excised heart of a frog or a tortoise, and there to submit the rhythmically beating tissue to any increase of atmospheric pressure that I desired, up to a maximum limit of twenty-two atmospheres, beyond which it was not safe to go with the glass chamber that I had. Through one end of this chamber two platinum electrodes were admitted, by means of which I was able to stimulate the nerve of a nerve-muscle preparation, when this, instead of a heart, was placed in the chamber.

The result of a number of experiments was to show that neither the rhythm of the beating heart nor the excitability of the motor nerve was in any way affected by an increase of twenty-two atmospheres; for both the rhythm and the excitability remained the same whether the chamber was exhausted by means of an air-pump or filled with air at a pressure of twenty-two atmospheres. In these experiments the excitability of the nerve was tested from time to time by noting the distance from the primary coil to which the secondary coil of an ordinary du Bois induction apparatus had to be drawn in order to yield a minimal stimulation; and the rhythm of the heart was counted by watching it through the glass. In some of the experiments the pressure was let in gradually, in others suddenly; in some it was let in and again released a number of times in rapid succession; while in others it was allowed to remain at twenty-two atmospheres without alteration for half an hour. During these long exposures the rhythm of the heart and the excitability of the nerve would sometimes slightly and continuously fall; but it did not appear to me that this was due to the pressure, since the diminution of excitability in either case was not any greater than that which is often observable in moist chambers at ordinary pressures.

Wishing to try whether still higher pressures would produce any effects, I discarded the glass chamber and had one made for me of gun-metal. With this I was able to go up to 150 atmospheres, but was not able to see what was going on inside. My method of experimenting here, therefore, was to take the rhythm of the heart, or the excitability of the nerve, immediately before screwing up the apparatus, and again immediately after taking it down. I was thus unable to observe the effects of the pressure during the time that it was being actually applied; but as it only took me a quarter of a minute to unship the chamber and turn its contents out on the table, if the 150 atmospheres had exerted any marked influence on the excitability of the tissues, it would probably have been easily detected by this method. Yet neither the heart nor the nerve showed any change after an exposure of five minutes to this great increase of pressure.

VI. "The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Elasticity—*continued.* The Effect of Magnetisation on the Elasticity and the Internal Friction of Metals." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received May 18, 1886.

(Abstract.)

The principal object of this investigation was to test the soundness of the view advanced by Professor G. Wiedemann respecting the cause of the internal friction of a torsionally oscillating wire.* According to this view the internal friction is mainly due to permanent rotation to-and-fro of the molecules about their axes; it seemed probable, therefore, that experiments on the effects of magnetising a wire either longitudinally with a helix or circularly by passing a current through it would aid in elucidating the matter.

In the experiments on the effects of longitudinal magnetisation arrangements were made so that the heat generated in the magnetising helix should not reach the wire, whilst the effect of the heat generated in the wire when an electric current was passed through it was eliminated in a manner which is fully described in the paper.

Besides the experiments on the effect of magnetisation on the internal friction and on the torsional elasticity of metals, others were made relating to the longitudinal elasticity of metals. The following are the principal results which have been obtained:—

1. When the deformations produced by the oscillations are small, the internal friction of a torsionally vibrating wire of iron or steel is not affected by sustained longitudinal magnetisation of moderate amount. The internal friction is also not affected by the sustained magnetisation even when the latter is carried to the point of satura-

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* "Wiedemann's Annalen," N.F., Bd. vi, p. 485.

tion, provided the magnetising current be, previously to experimenting, reversed a great number of times. When no previous reversals have been made the internal friction is slightly increased by intense magnetisation.

2. When the deformations produced by the oscillations are large the internal friction is very sensibly increased by sustained longitudinal magnetisation of large amount.

3. The torsional elasticity is entirely independent of any sustained longitudinally magnetising stress which may be acting upon an iron or steel wire, provided the deformations produced by the torsional oscillations be small. When the deformations are large, the number of oscillations executed in a given time is very slightly lessened by sustained longitudinal magnetisation of large amount.

4. When the magnetising current is interrupted and, to a greater extent, when it is reversed repeatedly whilst the wire is oscillating, the internal friction is increased, provided the magnetising stress be of moderate amount. The increase of internal friction may become very considerable when the magnetising stress is great.

When the number of interruptions or reversals in a given time of the magnetising current exceeds a certain limit the effect on the internal friction begins to decline.

5. When the deformations produced by the oscillations are small, the torsional elasticity is not affected by either repeatedly interrupted or reversed longitudinal magnetisation even when the magnetising stress is large.

6. There exists a limit of magnetic stress within which no permanent rotation whatever of the molecules is produced. This limit may be widened by previous repeated reversals of a large magnetising stress.

7. The passage of a moderate electric current, whether sustained or interrupted, through a torsionally vibrating wire of iron, steel, or nickel does not affect, except by heating, either the internal friction or the torsional elasticity, provided the deformations produced by the oscillations be small.

8. The effect of longitudinal magnetisation, even when carried to the point of saturation, on the longitudinal oscillation of an iron or steel wire, is *nil*.

9. The passage of an electric current, whether sustained or interrupted, through a longitudinally oscillating wire of iron or steel does not, except by heating, affect the number of oscillations executed in a given time.

10. When the deformations produced by the oscillations do not exceed a certain limit, the internal friction cannot apparently depend upon the *permanent* rotation of the molecules about their axes. When, however, the deformations exceed this limit, the internal

friction becomes very sensibly larger, and does partly, if not mainly, depend upon the permanent rotation to-and-fro of the molecules about their axes. The above-mentioned limit can be widened by allowing the wire to rest after suspension with oscillations at intervals, by annealing, and by repeated heating and cooling.

VII. "Researches in Stellar Photography. 1. In its Relation to the Photometry of the Stars; 2. Its Applicability to Astronomical Measurements of Great Precision." By the Rev. C. Pritchard, D.D., F.R.S., Savilian Professor of Astronomy in Oxford. Received May 20, 1886.

(Abstract.)

I. The objects are, first to enquire, by means of accurate measurement, whether there does not exist a definite relation between the area of the disk of a star image impressed on a photographic film and the "photometric magnitude" of that star as determined by instrumental means.

For this purpose, several plates of portions of the Pleiades were taken by varying exposures in the focus of the De la Rue reflector of 13 inches aperture, in the Oxford University Observatory. The diameters of the star disks on these plates were then carefully measured, both with the macro-micrometer and with a double image micrometer, in the same establishment. The result is that the relation sought for is expressed by—

$$D-D'=\delta\{\log M'-\log M\},$$

where D, D′ are the measured diameters of two star disks *on the same plate*, and M, M′ their corresponding "*magnitudes*," as recorded in the "Uranometria Nova Oxoniensis."

The mean difference between the observed and computed magnitudes as derived from the foregoing formula, applied to 28 stars all impressed on each of the four plates and ranging from magnitude 3 to magnitude 9·5, is 0·16 magnitude. A few stars (3) here stand out, in all the plates, as was to be expected, arising from the peculiar actinic action of their spectra. Similar anomalies, as is well known, exist in the application of the photometer.

II. In the next section of the enquiry the effect of alteration of the time of exposure on the areas of the star images is referred to. The enquiry is not fully completed, but as far as it extends, it indicates that for stars not very faint, the areas of the disks of the same star on the same plate vary as the square root of the time of exposure.

Bond in 1858 considered that these areas varied as the time of exposure. Further investigation is required.

III. In the last section the more important question is answered, as to whether, on these modern photographic plates, where the times of exposure are reckoned by hours and minutes rather than by minutes or seconds, measures may be made of relative stellar coordinates as exact and trustworthy as those derived from the best astronomical instruments applied directly to the heavens. For the purposes of this enquiry, resort is made to the same plates of the Pleiades which furnished the results in Section I of this research. The distances of twenty-five stars from Alcyone were measured with the Oxford macro-micrometer: each measure on each of the four plates was repeated the same number of times as Bessel measured the same distances with his Königsberg heliometer. The result of the comparison is slightly in favour of the photographic measures; the average deviations of the repeated measures from the mean being in the case of photographs 0·24″ and with Bessel's heliometer measures 0·29.″

A remarkable circumstance fortunately occurred in the course of the measures, in the case of one of the four plates; unmistakable evidence appeared that the photographic film had very slightly but measurably shifted in the neighbourhood of three of the stars, but on no other portions of that plate nor on any portion of any of the other plates. This indicates the unadvisability of relying on any single plate uncorroborated by others.

The applicability therefore of this form of photography to the most precise astronomical measurements seems to be established, even for plates exposed for considerable times. I now propose to test the method still further, by applying it to the determination of stellar parallax.

VIII. "Researches upon the Self-induction of an Electric Current." By Professor D. E. HUGHES, F.R.S. Received May 24, 1886.

Numerous researches have been made upon the self-induction of coils of wire, and but few in relation to the influence exerted by the nature and geometrical sectional form of the electrical conductor when employed in straight wires as in those of a telegraph line with the earth as a return, or those of a single wide loop where the distance of the return wire is sufficient to prevent any appreciable effect from the *mutual induction* of separate portions of the wire upon each other; our present theories class all non-magnetic metals together, taking only into account their specific resistance and the diameter of the wires; they admit, however, a certain modification in magnetic metals due to their magnetic permeability, but we have had but little experimental evidence of the effect produced.

The whole subject seemed to me worthy of experimental investigation, and I have lately* given the results of a first series of experiments made last year. I have remarked since writing that paper many new and important effects, and I have made a new series of researches, the results of which I wish particularly to point out in this paper.

In my late researches I made use of a modified "Wheatstone's bridge" together with a portion of my "induction balance," by means of which the induced or extra currents from the wire under observation were balanced by a secondary current induced from an independent circuit; this gave excellent results, but the method has been criticised as having the fault of not being clear in its indications. In order to meet this objection and also verify the results which I had previously obtained, I constructed an entirely new bridge founded upon a most simple and well-known principle, and as the bridge admits of no change in the relative resistance of its sides, its action can be easily understood; it is with this instrument that I have made my new series of experiments.

The instrument consists of an ordinary Wheatstone's bridge, with the exception that a telephone replaces the galvanometer; there are in addition two coils of insulated copper wire, one in each portion of two sides of the bridge, by means of which the mutual self-induction of its convolutions can be increased or decreased as desired.

The electrical contacts are made either by a continuous periodic or tuning fork contact maker, or by a peculiar clockwork rheotome which I have made, in which a contact spring rests lightly on a wheel whose roughened surface is divided into eight equal parts of contact and insulation, by means of which we receive on the telephone eight equal periods of sound and silence each revolution of the wheel. We are by this means better enabled to appreciate feeble sounds than if they were continuous, and as the wheel can be made to revolve at any between two and ten revolutions per second, we have from sixteen to eighty periods of silence between each rubbing contact per second.

The following figure shows the theoretical plan of the electrical communications of the bridge:—

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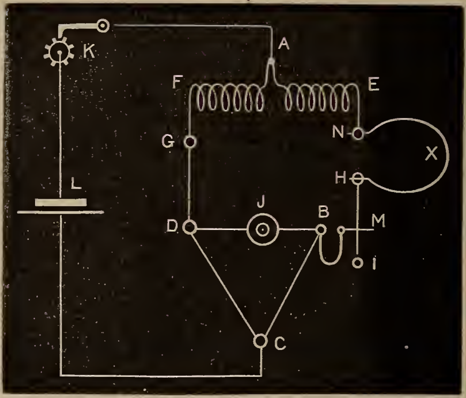
* "Self-induction of an Electric Current in relation to the Nature and Form of its Conductor," "Journal of the Society of Telegraph-Engineers and Electricians," vol. xv (1886), p. 6.

Fig. 1.



A, B, C, D are the four sides of the bridge, the telephone J replacing the ordinary galvanometer; the sides CB and CD are formed of German silver wire, each 50 cm. in length, 0·5 mm. in diameter, and 0·85 ohm resistance; the sides AB and AD have also 0·85 ohm resistance, so that the four sides have equal resistance, and this remains a constant during the whole series of experiments.

AE and AF is a continuous spiral of copper wire, formed of hard copper silk-covered wire of 1 mm. diameter and 4·80 metres in length, wound loosely on a boxwood cylinder of 3·50 cm. diameter and 30 cm. in length, on which it moves freely; the entire helix has 40 turns of 4 cm. diameter, each spiral being separated 5 cm. from each other; the spiral, however, is separated into two equal portions by giving a greater separation at the centre in order to allow it to be attached to a sliding collar of wood, by means of which we can press the spiral closer together on either side as desired; at E and F there are also adjustable collars of wood, and as the boxwood cylinder is graduated, we may (if the central collar is fixed) approach more or less either side of the helix, and read the degree of approximation of the coil, thus adjusting the mutual induction of each side to a perfect balance or zero. In practice I prefer moving the central collar, using the end collars only for the perfect adjustment of its zero, as we then have a double effect, viz., closing the coil say from A to F, increasing its mutual self-induction, and at the same time decreasing the previously balanced induction of the coil AE; this not only gives a wider range of effect, but renders the scale readings more uniform; the end F of the helix is joined to about 10 cm. of German silver wire, completing the circuit from G to D; this supplementary German silver wire is simply for the purpose of making the resistance of AD equal to DC, and its length should be adjusted to this purpose; the end of the helix E joins directly with the terminal N; the wire to be tested X is joined to N and H; from H to I there is a second supplementary German silver wire allowing us by means of the contact side M, which is in direct communication with B, to introduce more or less of the German silver wire in the side AB. The resistance of the wire to be tested should always be less than that of the opposite side of the bridge, and we then make up the total resistance of AB by sliding the contact slide M until the resistance of AB equals AD.

It will be evident that in the whole series of experiments there can be no change in the resistance of either side whenever zero is found, the induction coils or balance having the same proportional current in all the changes of the wires under observation, the battery and telephone circuit present also a continuous absolute relation; we may, however, when desirable, keep the resistance from H to M a constant, and vary the resistance or the length of the German silver wire GD, we then balance the wire X by an equal resistance on the opposite side of the bridge, but then the battery and telephone circuits no longer possess the invariable relations which are so extremely necessary in experiments of the nature of which I have been making.

The battery circuit is joined in the usual manner to AC, the current can be interrupted by the rheotome K, or by means of a commutator (not shown in the diagram) close the battery circuit, and transfer the contact maker to the telephone circuit BD; this allows us to observe the effect of an intermittent current compared with that of a constant or steady flow. M. Gaugain has termed the period of a steady flow of current the *stable period*, and that in which a rise or fall of the current takes place the *variable period;* these terms have since been generally adopted by telegraph electricians, and in order to avoid introducing new terms, they will be used in this paper.

It would require too much space to enter into the details of the construction of the bridge, special care is required in the construction of the balancing induction coils, and in securing perfect electrical contacts in all parts of the bridge; the induction balance requires calibration, and for this purpose I introduce as the wire X successive lengths of 10 cm. 1 mm. diameter copper wire, thus forming a table of values throughout the range of the induction balance, or by equal increments of 10 cm. of copper wire up to 20 metres.

Having no unit of self-induction to which my results could be quickly and practically referred, I have adopted as unit the self-induction of a straight copper wire 1 mm. diameter and 1 metre in length; this gives on my calibrated induction scale 100 degrees, and it is to this standard that all the comparative force of extra currents mentioned in this paper is compared.

The self-induction of a wire is proportional to its length, consequently a source of error might exist in the different lengths of the supplementary resistance wire HI introduced to balance the resistance of GD, but as we are enabled by the high specific resistance of German silver wire to obtain a very great change in resistance by a comparatively small movement of the sliding scale, this error in most cases of comparative experiments is but a fraction of 1 per cent., and when taken into account as it should be the error no longer exists.

The telephone used should be of the most perfect kind, and adjusted expressly for rapid and feeble sounds. I have found it best to employ an extremely soft Swedish iron diaphragm, without varnish or anything that can diminish or deaden the sound; its fundamental note should be higher than those generally in use, or at least 500 double vibrations per second, for we have to deal with extremely rapid effects which on short wires cannot be rendered evident upon a galvanometer, but which with a telephone in perfect adjustment are heard most distinctly with an electromotive force in the battery circuit ranging from 0·001 to 0·250 ampère.

In the sketch of the communications the wire to be tested, X, is shown in the form of a wide loop, but in practice the instrument is constructed on two separate frames of wood articulated together at D, by means of which we can separate the terminals NH, and introduce straight wires, sheets, or tubes of lengths varying from 5 cm. to 1 metre.

The object of my researches being to observe the self-induction which takes place in straight wires, or in those of a single wide loop where the reaction from any return wire is at such a distance that its influence is not appreciable, I shall, therefore, use the term self-induction to indicate the effects due to the electric current in its own portion of the wire, and mutual induction to those where the reactions of different portions of the current and circuit react on each other, as in the case of coils; and although some theoreticians consider the two cases as the same, they are, as my experiments prove, entirely distinct, for we have, as will be shown, for copper wires a low coefficient of self-induction with a high mutual induction, whilst in iron wires the reverse is the case, for we there have a high coefficient of self-induction with an extremely feeble coefficient of mutual induction.

Influence of the Nature of the Conductor upon its Self-induction.

I found, as shown in my late paper, that there was a marked difference in the specific inductive capacity of iron and copper, and this entirely agrees with well-known theories; but I suspected that there might be some difference in the non-magnetic metals independent of their specific resistance. For this purpose I made a series of experiments with wires of the same length and diameter, but of different resistance. These showed a marked difference provided the current was increased in proportion to its diameter or conductivity, but no difference could be found when the current was kept a constant, and the interior differences in resistance of the wires were compensated by an external added resistance; but if we take wires of different metals—all of the same length and resistance, but of different diameters—there is a marked difference due to the mutual reactions of the current in its own wire, being less in wire of large diameter than in small wires. These effects have been fully explained in my late paper, and have since been completely verified by the method used at present. I showed by the use of my late method a critical point in the rise and fall of the induced currents; this is not shown by the present method of compensating by external resistance, but the rapid decrease in the electromotive force of the induced currents, as indicated by the induction balance, is well shown by the present method. The following table shows the observed force of the extra currents for wires of the same length (1 metre), but of different diameters:—

	mm. 0·25	mm. 0·50	mm. 1·00	mm. 2·00	mm. 3·00	mm. 4·00	mm. 5·00	mm. 6·00	mm. 7·00	mm. 8·00	mm. 9·00	mm. 10·00
Iron..	760	621	530	360	269	220	190	171	152	138	128	124
Copper	129	113	100	89	82	78	75	73	72	71·5	71·2	71

The fall in force is now even more rapid than shown by my late method, and it will be seen that iron is peculiarly sensitive to an increase in diameter, having nearly six times the force of copper in wires of 0·25 mm., and not twice the force of copper in wires of 10 mm. section.

Reactions of an Electric Current in its own Portion of Conductor.

The phenomenon of a constant decrease in the electromotive force of self-induction, as measured by the induction balance, with each increase of the sectional area of the conductor, is well shown by the present method, and an experimental investigation of its cause has shown that we should not consider a current in a wire as a single element reacting solely upon exterior wires, but that the current acts precisely as would an infinite number of independent streamlets of current reacting upon each other in the interior of its own wire similarly to their known effects upon exterior wires. My experiments demonstrate this to a degree that leaves no doubt on my mind as to its truth, for according to this view we should be able to reduce the self-induction to a very great extent by employing thin flat sheets where the outlying portions are at a comparatively great distance from the central portions. This I have experimentally proved to be the fact, but this experiment alone does not show if the reduction of self-induction is due to a different arrangement of the current in sheets as compared with solids. If the reduction is due to the greater separation of the streamlets, then we should be able to reduce this induction in a still greater degree by employing a conductor composed of numerous small copper wires through which the current is equally divided, and which could be separated or brought close together as desired. This proves to be also an experimental fact, for the conductor formed of numerous strands has far less self-induction than a thin sheet when its wires are separated, so that they can no longer react on each other, and surpasses the thin sheet and approaches the value of a circular wire when these wires are brought near together, so that their mutual reactions can approximate those of the numerous streamlets in a solid conductor.

Iron shows a still greater reduction in its self-induction when in the form of thin flat sheets or numerous small iron wires separated from each other, with the exception that we cannot restore or approach the value of a solid wire by bringing them in close proximity. The reduction of the induced currents by employing thin sheets instead of a wire is so great in iron that we could not account for it on the mere separation of contiguous portions of the same current, but if we assume that the comparatively high force in iron wires is due to the induced circular magnetism, and that this almost disappears in flat sheets, we account for the fact that a thin flat sheet of iron has less inductive capacity than a similar sheet of copper of the same resistance, but of different widths; and if thin strips of copper, brass, lead, and German silver are compared with similar iron strips, and their resistance rendered equal by the added resistance in the bridge, no difference is found between iron and the non-magnetic metals, for under these conditions their inductive capacities appear equal.

Reactions of an Electric Current between Separate Portions of the same Conductor.

In order to distinguish two distinct effects I have defined self-induction as the effect produced by an electric current on its own portion of the wire, and mutual induction as the effect of the reactions between separate portions of the wire on each other.

The mutual induction in iron and copper wires is very different in degree, as shown by the following table:—

Table I.

Wires, silk-covered, 1 m. in length, 1 mm. in diameter.	Comparative force of the extra currents.	Strips or ribbons, silk-covered, 1 m. in length, 10 cm. wide, 0·1 mm. thick.	Comparative force of the extra currents.
Copper wire, in a single close loop, where each side is in close proximity	18	Copper strip, in a single close loop, where each side is in close proximity	14
Copper wire, in a single wide circular loop ...	100	Copper strip, in a single wide circular loop ...	60
Copper wire, in a coil of 3 cm. diam., having ten layers in close proximity	607	Copper strip, in a coil of 3 cm. diam., having ten layers in close proximity	580
Iron wire (soft), in a single close loop, as above	440	Iron strip (soft), in a single close loop, as above	16
Iron wire, in a single circular wide loop ...	502	Iron strip, in a single circular wide loop ...	60
Iron wire, in a coil as in the case of copper .	570	Iron strip, in a coil as stated for copper	578

There is a remarkable fall in the induction of a straight wire or single wide loop of a copper wire when this wire is doubled upon itself as a return wire in close proximity. The effect of mutual induction is also shown by the equally remarkable rise when the mutual induction of ten layers of a coil react on each other. This is well known, but what I believe has not yet been experimentally observed is the remarkably feeble mutual induction of iron wires, either when in parallel return in close proximity or when in coils of numerous layers. The percentage of increase of induction in a copper wire from a wide single loop to that of the ten layers being 507 per cent., whilst in iron under precisely the same conditions there was an increase of but 13·6 per cent.

That this remarkable difference as shown between the mutual induction of iron and copper wires is due entirely to the circular magnetism in the iron wire, and that the mutual reactions of the streamlets of the current in a thin flat iron conductor prevent this formation I have proved in various ways, for if the circular magnetism is the cause, flat iron sheets which have as low self-induction as copper should be equally sensitive to mutual induction as the non-magnetic metals, for they no longer in any appreciable degree possess the protecting magnetic sheath which enfeebles the mutual reactions of iron wires upon each other; this proves to be the case, a thin ribbon of iron having the same coefficient of mutual induction as copper, and it no longer behaves as a magnetic body.

Table I shows the difference in the mutual induction of iron wires and strips; it will be seen that when the iron is in the form of a wire the circular magnetism produces a marked difference between iron and copper, whilst in the form of flat strips the iron resembles copper (as regards mutual induction) in every respect.

Influence of Circular Magnetism.

I have shown in my late paper that an iron conductor composed of numerous fine stranded wires (as in a rope) behaves like copper, and this I regard as entirely due to the breaking up or prevention of circular magnetism; I also showed a phenomenon which I could not then explain, viz., that when an iron wire was heated to a yellow-red heat it lost its previous high specific inductive capacity and behaved like copper. The effect of heat upon magnetism is well known, consequently we can readily explain the fall in its inductive capacity by the disappearance of its circular magnetism. I found also that in thin flat strips of iron there was no change whatever in its inductive capacity at the yellow-red heat; this can now be readily explained: it behaved like copper when cold, and, having but little circular magnetism to destroy, there was no appreciable change produced, except that due to the extra resistance caused by the increased temperature of the strip, and to which copper and iron are almost equally sensitive, consequently we may consider iron (when in the form of thin flat sheets) to behave like non-magnetic metals throughout all temperatures.

Influence of Self-induction on the Resistance of the Conductor.

A phenomenon of great importance is the effect which I have observed of the resistance of a wire being greater during the rise of the current, as in the variable period, than that measured or known during the constant flow, as in the stable period; by resistance I mean a pure ohmic resistance, a resistance which can only be measured, expressed, or balanced in ohms, and, whatever the cause, the effect is one of pure ohmic resistance.

We can imagine that at the first moment of contact there is no current flowing through the wire, its resistance is then infinite, but the current gradually increases in force until it arrives at its maximum, as in the stable period. We have thus between the moment of contact and period of steady flow a variable period wherein the resistance falls in the form of a curve from infinity to its well-known stable resistance; the telephone is unable to give the exact form of this curve directly, but it gives by the nul method comparative results as to the different duration in time of the curve in different metals.

If we take a straight copper wire 1 metre in length and balance its resistance in the stable period, we shall find that there is only traces of a difference in its resistance in the variable period, and we can balance its self-induction by the induction balance; but if we replace the copper by an iron wire of the same length, or even a much shorter wire, viz., 20 cm., we find that in the variable period we can no longer balance the wire by the induction balance alone, but we must compensate for its increased resistance by the sliding scale, the amount of subtracted German silver wire expressing in fractions of ohms (the value of the wire being already known) the additional resistance of the iron wire in the variable period; we can thus reduce or balance the iron wire to a perfect zero, provided the current from the battery does not exceed 0·10 ampère; but with greater current there will still remain a slight muffled sound, which cannot be reduced to zero either by the induction balance or the resistance slide. It therefore became important to determine if this muffled sound was due to a difference in resistance which could not be balanced, or to the lengthened duration of the extra currents being slower than the balancing current from the induction balance; the latter proved to be the case, for on prolonging the duration of the balancing currents from the induction bridge by introducing a core of iron in its coil, the balance or zero became absolutely perfect. In this case we must choose between cores of different diameter to find one whose reaction on the time effect of the induction coil equals the retardation of the extra currents in the wire tested.

If we observe the method employed in the bridge, we shall see that the induction balance can balance the extra current of the wire X, irrespective of the position of the resistance slide M, or any relation between the sides AB and AD, but we cannot equalise the resistance of these sides except by the necessary adjustment of the resistance slide, consequently when we are forced to adjust the induction balance we are compensating the extra current, and when we are forced to move the resistance slide we are balancing resistance.

The disturbance in the bridge caused by the change of resistance of the wire tested in the variable period, causes a momentary primary current to pass through the telephone in the same direction as the extra current, and if these are not separated by balancing the extra current by an induction balance, the mixed effect would be read as a single effect of the extra current. To show the importance of separating these two effects, it is only necessary to say that in most of the cases cited in this paper the momentary primary current due to the extra resistance greatly exceeds that of the extra current; consequently all measurements taken wherein this separation is not complete, gives the result of a mixed effect.

In the method which I have described, the separation of the extra current from the momentary primary current is so complete that the measures given by the induction balance and resistance slide are invariably the same for a given wire; there is no personal equation, for all observers find precisely the same values both for the resistance and for the extra current.

The method is, however, defective in measuring any small difference of resistance in the variable period in copper wires, as the induction balance itself introduces an additional but opposing resistance by the approaching of its coils, consequently any resistance which we observe is a fraction less than the real amount. To prove this we have only to use a balancing induction current produced from the battery circuit, as shown in my first paper, when we observe this small difference, and better, observe any small difference of resistance in a straight copper wire, and greater when the wire is formed into a coil.

Soft iron wire gives a far higher resistance in the variable period than hard iron, but each wire, according to its molecular structure, has its own value; the effect increases with the diameter, ranging from 25 per cent. increase of resistance for wires of 2 mm. in diameter, to 500 per cent. for those of 6 cm.

Table II.—Resistance of Iron and Copper in the Stable and Variable Periods.

Wires, 1 m. in length, 5 mm. in diameter.	Comparative force of the extra current.	Resistance in ohms, stable period.	Resistance in ohms, variable period.	Percentage of increased resistance in variable period.
Copper	78	0·001284	0·001372	7
Soft Swedish iron	234	0·008346	0·022200	166
American compound wire, copper exterior, steel interior..	83	0·002247	0·002696	20
Ditto, steel exterior, copper interior	213	0·007750	0·0248000	220

Table II shows a few illustrative examples: the resistance is given in the fractions of the ohm, indicated by the resistance slide M, upon the supplementary resistance wire HI.

The table shows that copper and the American compound wire coated with copper have an extremely rapid action or curve from an infinite to its stable resistance, due to their freedom from circular magnetism, whilst iron shows a comparatively slow curve. A remarkable result will be seen where a copper wire has been coated with iron, its variable resistance being 220 per cent. above that of its stable period, and 54 per cent. greater than that of a solid iron wire.

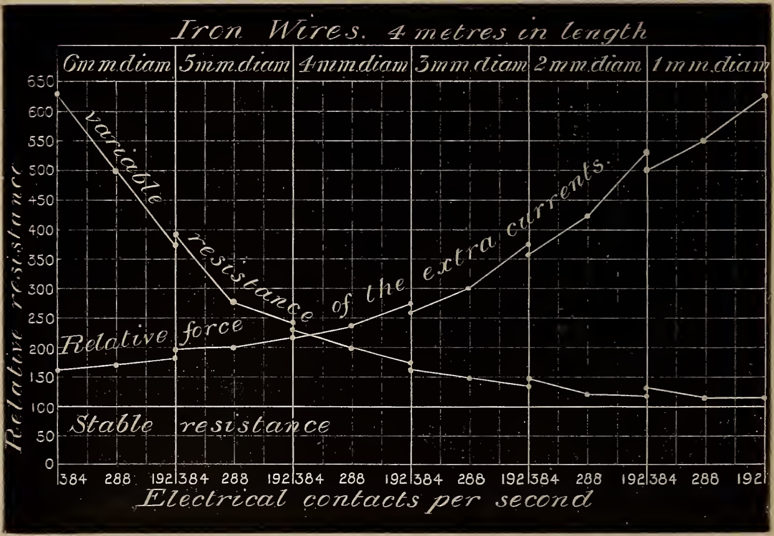
I found in my previous researches that the resistance in the variable period could not be changed by a change in speed of a periodic or tuning-fork contact maker, and that the telephone was more sensitive when the rubbing contacts were used without any regard to their frequency of interval. I also found that the telephone was far more sensitive and alone suitable for these experiments when its diaphragm was entirely free from varnish or coating, and sufficiently thick to give a clear musical dominant tone of about C or 512 double vibrations per second, the electromagnet being also as close as possible without being in actual contact. I noticed that no matter what number of vibrations were sent, that the tones given by the telephone on each side of the zero of the bridge were invariably those of its dominant note. Suspecting that the frequency of vibration of the diaphragm had a direct relation with the resistance found, I altered its note through a range of one octave by employing different thickness of diaphragms, and found that the results were in absolute proportion to the frequency of the vibrations of the diaphragm. The highest tone giving a perfect zero with a high resistance in the variable period and the low tone giving its zero at a marked less resistance. Thus it was evident that the telephone selected from the mixed vibrations sent by the transmitter those which alone corresponded to its period of vibration, and that we could thus observe the effect of more or less rapid periodic contacts.

The power which vibrating bodies have of selecting from a confused number of vibrations those in accordance with their own, is shown in Helmholtz's resonators, and still more as regards electrical waves in the remarkable harmonic telegraph of Elisha Gray, who proved that if the armatures of several electromagnets are made to vibrate, each with a different musical tone or rate of vibration, then we may by the use of a vibrating transmitter of a fixed rate of speed select the electromagnet alone whose period of vibrations is similar, and that we may send several different periods of vibrations at the same time without any confusion in the receiving magnets, as each electromagnet selects from the compound vibrations those in accordance with its own. This is exactly what occurs with a telephone when using a periodic contact maker, but I have found in addition, that a telephone arranged as I have described, will select and respond loudly in its own clear musical dominant tone to a confused mass of rapid contacts such as those produced by a scraping contact, and which in an ordinary telephone gives only the effect of noise, of not one-third the power or loudness of that produced by the tone of the musical telephone. We may thus have ten or more telephones of different musical notes, each responding separately in their own tone or all at the same time, as each diaphragm selects its own period from the confused periods of contacts sent by the scraping contact maker.

In order to verify the results, I have used in these experiments both periodic and mixed sound transmitters, but find that owing to the difficulty of keeping the periodic transmitter and telephone in perfect tune with each other, that the mixed sound transmitter is preferable, as it gives no trouble whatever, and we have more accurate results, as we then have only to keep our telephones in accord with the number of vibrations desired.

The experiments mentioned in this paper were all made with a fixed note of vibration of the diaphragm of 512 double vibrations per second. I will, however, mention a few made with varying rates of vibration, and as I propose in a future paper to give a more extended series of observations through a wider range: I will confine myself to these examples.

Fig. 2.



The diagram shows the results obtained on iron wires of different diameters; their stable and variable resistance were all reduced to comparative values, the stable resistance being taken as 100. Three telephones were employed, the first having the musical note of G or

768 double vibrations, the second C sharp or 576 double vibrations, and the third, one octave below the first or G 384 double vibrations per second.

It will be seen that a great change in the variable resistance takes place in the range of a simple octave, and that the fall from the wires of 6 mm. to that of 1 mm. is so regular as to be almost a continuous curve, and the total curve would be probably that taken by the wire of 6 mm. in its approach to its stable resistance. If this is the case, as future researches may show, then the resistance of this wire with the comparatively slow contact speed of six per second would still have 14 per cent. higher resistance in the variable than in the stable period, or an ordinary Morse telegraph instrument working at a slow speed of only eleven words per minute would experience this additional resistance; consequently this effect should be easy to prove by the ordinary methods with galvanometers, and this can be accomplished as soon as we have found a method of completely separating the extra current from that due to the increased resistance, as I have already succeeded in doing by the method which I have described.

The iron wire of 6 mm. diameter shows for a speed of 384 contacts per second a comparative resistance in the variable period of 638, or more than six times its stable resistance, but with 192 contacts per second its resistance is but 371; the fall of resistance is so rapid here that for a single octave difference in the telephones, the fall is far greater than the whole stable resistance. The extra current, as well known, is proportional to the length of contact for fine wires, but in large wires the curve indicates that the extra currents have a local reaction on the cessation of the primary current.

Influence of a Magnetic Sheath.

The remarkable phenomenon wherein a non-magnetic metal such as copper shows a higher "variable" resistance when under the influence of an external coating of iron than a wire of solid iron (and this notwithstanding that the resistance of the side of the bridge is kept a constant or by balancing its resistance on the opposite side GD as mentioned) seemed worthy of experimental research.

Experiments were made in order to find what form of conductor would give the maximum increased resistance in the variable period, and I found that an external tube of iron insulated from its central core produced this effect, for if the tube was not insulated but joined in electrical contact the results were reduced, due I believe to a transversal neutralisation taking place in its own portion of the wire, as I have mentioned in my reply to the discussion on my first paper.

Table III shows the effect produced upon an electric conductor by surrounding it with an insulated sheath of iron. It will be seen that

Table III.

Influence of an Iron Tube upon an Interior Insulated Wire.

Iron gas tube, 90 cm. in length, 10 cm. diameter, 2 mm. thick. Wires 1 metre in length, 3 mm. diameter.	Comparative force of the extra currents.	Resistance in ohms, stable period.	Resistance in ohms, variable period.	Per centage of increased resistance in variable period.	Absolute increase of resistance in ohms.
Copper wire alone	82	0·00460	0·00482	7	0·00022
Copper wire insulated in the interior of the iron gas tube	410	0·00460	0·03220	600	0·02760
Brass wire, ditto	410	0·01380	0·03974	188	0·02594
Iron wire, ditto	615	0·02944	0·05888	100	0·02944
Lead wire, ditto	410	0·05750	0·08682	51	0·02932
German silver wire, ditto	410	0·07636	0·10384	36	0·02748

the magnetic reaction of the tube upon the primary current passing in the wire produces a marked effect upon the different metals, the force of the extra currents as measured by the induction balance rise in value from 82 to 410, this being a constant for all metals except iron, which alone shows the higher force of 615; the force of the extra currents, however, has no direct relation to the extra resistance shown in the variable period, but the latter must be in direct relation with the duration or length of time required to pass from an infinite to the stable resistance.

The table shows on comparing the resistance of a metal in its stable period with that found in the variable, that copper has the highest percentage of increased resistance, being 600 per cent. increase, or seven times that found for its stable resistance, each metal in the order of their specific resistance having a less percentage of increased resistance until we arrive at the German silver wire, which shows only 36 per cent. against the 600 per cent. of the copper; we have also here the remarkable fact that iron even with a circular section stands simply in its order of specific resistance, showing only one-sixth of the increased resistance experienced by copper; the percentage of increased resistance of a metal when under the influence of an insulated magnetic sheath is directly as its conductivity or inversely as its specific resistance.

If we regard the phenomenon from a different point of view, confining ourselves simply to the additional resistance above that of its specific stable resistance, all metals whether magnetic or non-magnetic have an almost invariable quantity dependent entirely upon the coefficient of the electromagnetic action of the iron tube, and independent of the resistance or nature of the wire.

The experiments with an insulated iron sheath may be considered as a forced condition which does not occur or need not be taken into account, but the reaction of the iron tube is evidently electromagnetic, and as our atmosphere is also magnetic, we may assume that its reaction would be similar though in a far less degree to that which I have shown.

Submarine Cables.

The influence of a magnetic sheath upon an external insulated wire is one of vast practical importance in relation to our electrical submarine cables, as they are all constructed with an insulated wire surrounded by a spiral sheath of exterior iron wires. In order to study this question I made several cables of short lengths (1 metre) and found (most fortunately for our practical applications) that when the exterior iron sheath is divided as in a cable, or when the sheath consists of several large wires in close contact, that its effects are reduced to a mere fraction of what it would be if the external sheath was a continuous tube; this is entirely due (as I have shown in the case of stranded wires as compared with solid) to the imperfect formation of circular magnetism. I will cite a few examples illustrating this: a cable was formed of a similar copper wire and insulation to that shown in Table III, the iron tube being replaced by eight iron wires (of 2 mm. diameter each) wound with a slight spiral of five turns per metre, this showed but 100 per cent. increase in the force of the extra currents against 400 per cent. increase when the same wire was in the iron tube; the increased resistance in the variable period was but 50 per cent. against 600 per cent. for the iron tube, or only one-twelfth of the additional resistance caused by the tube; these wires were replaced by galvanised iron wire which prevented magnetic contact, and the variable resistance became much less, and when these were replaced by numerous fine iron wires the effect was reduced to the minimum found, or only 20 per cent. increased resistance in the variable period, or 30 times less than that of a solid iron tube of much less iron.

It is therefore evident that the circular magnetism plays an important *rôle*, and that most fortunately our cable manufacturers (without knowing the immense reaction which would be produced by a continuous sheath of iron) have constructed our cables with a protection of iron divided into several iron wires instead of a continuous iron sheath; there are, however, many telegraph lines which in passing through tunnels use a continuous iron tube as the protection, and therefore such lines must feel all the deleterious effects that I have shown to be caused by the reaction of circular magnetism.

Influence of Copper and Iron Cores upon the Induction and Resistance of Coils.

It is well known that a coil of wire has a higher self-induction than the same wire in a single loop, and that the coil has a still higher induction when we introduce an iron core. I have made, however, a series of experiments in order to measure the influence of a core upon the resistance in the variable period, and also note the influence exerted by the induced or eddy currents circulating in the core: the following table shows some comparative results.

Table IV.

Helix formed of insulated copper wire, 1·50 metre in length, 2 cm. diameter, and 24 turns.	Comparative force of the extra currents.	Resistance in ohms, stable period.	Resistance in ohms, variable period.	Percentage of increased resistance in variable period.	Absolute increase of resistance in ohms.
Helix alone......	460	0·02632	0·02896	10	0·00264
Same with a core of solid copper..	352	0·02632	0·04013	52	0·01381
Same with a core of insulated copper wire......	460	0·02632	0·02896	10	0·00264
Same with a core of solid soft iron	2338	0·02632	0·09870	275	0·07238
Same with a core of 445 separate fine iron wires, each 0·25 mm. diameter	5360	0·02632	0·04448	69	0·01816
Same with a core of silk-covered fine insulated iron wires......	5820	0·02632	0·04075	55	0·01443

A helix was formed of an insulated silk-covered copper wire of 24 single layers, having an interior diameter of 2 cm., the object being to form a coil having as little mutual induction as possible, but which would be readily acted upon by any core of metal introduced; and in order to measure the high forces obtained an induction balance of great power and range was used, the method, however, being the same as that already described.

The helix alone showed an induction value for its extra currents of

460, and a feeble increased resistance in the variable period of but 10 per cent.; if we now introduce a core of solid copper of 1·75 cm. diameter and 5 cm. long, a great change takes place both in the force of the extra currents and the resistance in the variable period, the extra currents fall in value from 460 to 352, whilst the extra resistance is increased to 52 per cent. above that of the stable period; we have here a double and contrary effect, the reduction of the force of the extra currents and the increase in the resistance cannot be due to the magnetic nature of copper, but must be due to the induced or so-called "Foucault currents" circulating in the core; to prove this the core was replaced by another similar in every respect but cut longitudinally to its centre, the currents now ceased to circulate, and the copper core had not the slightest effect either on the extra currents or the variable resistance; this is shown in the table, where for greater precautions a core of insulated copper wires replaced the solid core. Evidently the induced current in the core was the cause of the extra resistance; work was done by the primary current, and a loss of energy at the expense of the electromotive force of the extra currents, but in doing this work a resistance was produced which was no doubt caused by the currents circulating in the core. These currents required time, passing through the variable stage and thus producing from their electromagnetic inertia an equivalent reaction and electromagnetic inertia in the primary coil itself. That this inertia is due to the electromagnetic character of the current and not to an electric current considered apart, is proved by the fact (which I have verified) that when we coil a wire into a coil of several superposed layers, its electromagnetic reactions introduce a measurable resistance in the variable period precisely similar though feebler than that which would be produced by the reaction on the conductor of a magnetic body such as iron.

The effect of a solid iron core and a bundle of iron wires on the increase of the force of the extra currents is well known, but the table shows an interesting result as to their effects on the variable resistance; the solid iron core shows a very high force of extra currents produced by its magnetic reaction on the wires of the helix, the resistance in the variable period has increased 275 per cent., and its extra currents are extremely high, as we should expect; now if the extra resistance is due to magnetic reaction alone it should increase when we are enabled to increase this reaction, but if it is due in greater part to the electromagnetic inertia of electric currents circulating in the bar, then by preventing these currents from being formed (as we did in the case of copper) we should greatly reduce the extra resistance. This proved to be the case, for on replacing the solid iron core by a bundle of fine iron wires the force of the extra currents rose from 2338 to 5360, or more than double the force of that produced by a solid bar, whilst the resistance in the variable period fell from 275 to but 69 per cent.

It is well known that a bundle of fine wires magnetises quicker than a solid bar, and this may have had its effect, although not more important than the suppression of the eddy currents, as the latter may be the cause of the former. The table shows that where we have suppressed these currents as far as possible by introducing a core of insulated wires containing less iron than in the previous experiment but fully insured against eddy currents, the induction was the highest and the resistance the lowest yet found for a helix containing an iron core; the experiment proves to my mind that the extra resistance found in the three last experiments is due both to the electromagnetic inertia of the eddy currents, and the inertia of the magnetic molecules of iron.

The experiments related in this paper have been most carefully made and verified, and from the ease and certain action as well as the invariable results obtained by the method which I have described, they should be easy to repeat by others.

If we regard the whole of these researches we cannot fail to notice certain important laws which act in the greater portion of them. I have shown: 1st. That the contiguous portions of the same current react upon each other in the interior of its own portion of the conductor similar to their known exterior reactions on separate portions of the same conductor. 2nd. That the coefficient of mutual induction is less in iron than copper wires, but that their coefficient is the same when the conductor is in the form of a ribbon. 3rd. That the inductive capacity of different metals depends on their specific resistance, on their electromagnetic capacity for circular magnetism, and on the geometrical form of their conductors. 4th. That the inductive capacity of a conductor of magnetic metal is dependent upon the formation of circular magnetism and not upon its internal magnetic permeability. 5th. That a magnetic metal can be rendered equally free from circular magnetism as the non-magnetic metals. 6th. That we have experimental evidence of electromagnetic inertia and the deleterious effects of eddy currents in the cores of electromagnets.

In addition to the above effects, we have the discovery of a large increase in the ohmic resistance during the variable period, allowing us to demonstrate and measure the gradual rise of an electric current in its conductor.

In conclusion, I desire to express my warmest thanks to Lord Rayleigh, Mr. F. L. Pope, Professor Forbes, Dr. Hopkinson, Mr. W. H. Preece, Dr. Fleming, Mr. Fitzgerald, Professor Silvanus Thompson, and Professor Ayrton, for the important theoretical contributions they gave on the discussion of my first paper. Mr. W. H. Preece, Electrician of the Post Office, not only gave information of great practical value, but has kindly supplied me with the wires used in these experiments. The discussion proved the necessity of the researches which I have undertaken, and the importance of an experimental determination of the self-induction of an electric current in relation to the nature and form of its conductor.

IX. "Contribution to the Study of Intestinal Rest and Movement." By J. THEODORE CASH, M.D. Communicated by T. LAUDER BRUNTON, M.D., F.R.S. Received May 25, 1886.

(Abstract.)

Experiments were made upon a dog in which a Weller's fistula had been established. The length of the isolated intestine (upper part of the jejunum) was 18·5 cm.

By merely observing the mouths of the fistula, it was ascertained that during a condition of hunger, periods of complete quiescence, varying from two to twelve minutes, occur. At any time, however, contractions having a very regular rhythm, might make themselves manifest. On food being presented to the animal, immediately after swallowing and for some time after ingestion the movements became much more active and persistent. Some hours (four to five) after a full meal they fell to their minimum.

For the closer study of the speed of transit of a solid or semi-solid body through the fistula, travelling sounds connected with a registering apparatus were employed. Compressible but fixed sounds for the study of local contraction were also used. The most important results obtained were the following:—

1. That the act of swallowing, whether empty, *i.e.*, produced by external friction of the larynx, or of liquids, is frequently succeeded by contraction, sometimes by distinct peristalsis of the intestine. Inhalation of sulphuric ether for a few seconds—which causes abundant salivation and deglutition—is almost invariably followed by active intestinal contraction.

2. Mental impressions, tickling the walls of the abdomen or the application of cold to them, are amongst the causes of contraction of the small intestine.

3. An unfailing means of producing well-marked peristaltic contraction is the administration of food to the fasting animal. Not only does this cause the individual contractions to ensue with greater regularity and force than before, but they have less of the pendulum character (contraction succeeded by complete relaxation), and during the pauses the travelling sound is so effectually "gripped" that it

recedes but little, and is therefore forwarded through the fistula with great rapidity. For a varying time after a full meal (two to three hours), the progress of a body is steady and moderately rapid; after that time it usually becomes much slower.

The following figures give an approximate idea of the speed of transmission under various conditions :—

Fasting—
  1 cm. of fistulous intestine traversed in 2 to 4′.
Immediately after a meal—
  Ditto in 30 to 40″.
Several hours after a meal (4 or 5)—
  Ditto in some instances 1 cm. only in 10′.

4. Propulsion of a solid body is in the physiological direction.

5. The introduction of water or bodies in a state of solution into the fistulous intestine is succeeded by a constant and peculiar modification of the peristalsis which may be present at the time.

6. Exercise of slight traction upon the travelling body renders its passage difficult or impossible, although it calls into play powerful and "griping" contractions of the intestine.

7. Exercise is highly favourable to a rapid and effective peristalsis.

8. Mechanical irritation (as the licking of the mouth of the fistula by the rough tongue of a dog) causes powerful co-ordinate movement.

9. The effect of electrical stimulation, and the action of a large number of drugs have been examined, the results obtained will be made the object of a further communication.

The President announced that Professor Dewar, F.R.S., had succeeded in obtaining oxygen in the solid state, and that he, with some others, had that afternoon witnessed the experiment.

The Society adjourned over Ascension Day to Thursday, June 10th.

June 4, 1886.

The Annual Meeting for the Election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, Sir Risdon Bennett and Mr. R. H. Scott, were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Bidwell, Shelford, M.A.
Colenso, William, F.L.S.
Dixon, Harold B., F.C.S.
Festing, Edward Robert, Major-General R.E.
Forsyth, Andrew Russell, M.A.
Green, Professor A. H., M.A.
Horsley, Prof. Victor, F.R.C.S.
Meldola, Raphael, F.R.A.S.
Pye-Smith, Philip H., M.D.
Russell, Henry Chamberlaine, B.A.
Sedgwick, Adam, M.A.
Unwin, Prof. W. Cawthorne, B.Sc.
Warington, Robert, F.C.S.
Wharton, William James Lloyd, Capt. R.N.
Wilde, Henry.

Thanks were given to the Scrutators.

June 10, 1886.

THE PRESIDENT in the Chair.

M. Alfred Cornu, Foreign Member (elected 1884), Mr. Shelford Bidwell, Mr. Harold B. Dixon, Major-General Edward Robert Festing, Mr. Andrew Russell Forsyth, Professor A. H. Green, Professor Victor Horsley, Mr. Raphael Meldola, Dr. Philip H. Pye-Smith, Mr. Adam Sedgwick, Professor W. Cawthorne Unwin, Mr. Robert Warington, Captain William James Lloyd Wharton, and Mr. Henry Wilde were admitted into the Society.

The Right Hon. Archibald Philip Primrose, Earl of Rosebery, and the Right Hon. Thomas John Howell-Thurlow Cumming-Bruce, Lord Thurlow, whose certificate had been suspended as required by the Statutes, were balloted for and elected Fellows of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Blood-vessels of *Mustelus antarcticus*: a Contribution to the Morphology of the Vascular System in the Vertebrata." By T. JEFFERY PARKER, B.Sc., C.M.Z.S., Professor of Biology in the University of Otago, N.Z. Communicated by Professor M. FOSTER, Sec. R.S. Received June 4, 1886.

(Abstract.)

The paper describes in detail the arteries and veins of *Mustelus antarcticus*, comparing them with those of other elasmobranchs, and with the embryonic trunks of the vertebrata generally.

As far as the author's resources would allow, the synonymy of the principal vessels is given.

The paper is illustrated by four quarto plates, and two diagrams in the text; the figures are, as a rule, made by combining the results of several dissections; the topographical relations of the chief vessels are shown in drawings of transverse sections of a frozen specimen.

The account of the vessels in *Mustelus* is supplemented by reference to the chief vascular trunks in the embryo of *Scymuus* of "Stage O" (Balfour); several sections of this embryo are figured.

The blood is taken from the heart to the gills by five pairs of *afferent branchial arteries*, one for the hyoidean demibranch or half-gill, and one for each of the *holobranchs* or complete gills borne by the first four branchial arches.

The blood is returned from the gills by nine pairs of *efferent branchial arteries*, one for each demibranch; the two efferent arteries of each holobranch are united by two transverse commissures at about the middle of their length, and the arteries of the adjacent demibranchs of successive gills anastomose dorsally and ventrally, forming loops which encircle the internal gill-clefts.

The anterior efferent artery of each holobranch is the main re-vehent trunk of the branchiomere, and is directly continued into an *epibranchial artery* lying dorsal to the gill; the posterior efferent artery discharges into the epibranchial artery of the next following gill.

There are thus four pairs of epibranchial arteries belonging to branchial arches 1—4, *i.e.*, formed from the dorsal portions of the aortic arches of those branchiomeres.

The first efferent artery—that of the hyoidean demibranch—discharges into the first epibranchial artery, *i.e.*, into the aortic arch of the next succeeding branchiomere.

The dorsal portion of the hyoidean aortic arch (*hyoidean epibranchial artery*) is represented by the proximal portion of the posterior carotid artery and by a delicate vessel proceeding backwards and inwards from it, which, uniting with its fellow of the opposite side, forms a slender median longitudinal trunk, the rudimentary anterior (interhyoidean) portion of the dorsal aorta.

It is suggested that the original mandibular aortic arch is represented partly by the *pseudobranchial artery* which supplies the pseudobranch (mandibular gill) and partly by the *mandibular artery*, which springs from the ventral end of, and takes a course parallel with, the first efferent branchial artery.

A classified list of the peripheral arteries is given, dividing them into (*a*) those arising from the efferent branchial arteries (aortic arches); and (*b*) those arising from the dorsal aorta.

The anterior carotid artery, formed by the union of the efferent arterioles of the pseudobranch, supplies the brain and eye, and anastomoses with the posterior carotid.

The posterior carotid supplies the orbit, snout, and jaws.

The mandibular artery supplies the angle of the mouth and the submental region, and gives off nutrient vessels to the hyoidean demibranch.

The subclavian artery—the first vessel springing from the dorsal aorta—divides into a brachial artery for the pectoral fin, and a *hypobranchial artery*, which unites with its fellow, anastomoses with the whole series of efferent branchial arteries, and gives off coronary, pericardial, and nutrient branchial arteries.

The splanchnic arteries consist of a cœliaco-mesenteric, a lienogastric, and anterior and posterior spermatico-mesenteric arteries; the two latter also supply the gonad.

There is only a general correspondence between the splanchnic arteries and veins of different elasmobranch genera.

An embryological classification of the veins is given: they are divided into the following groups: (*a*) system of the subintestinal vein, consisting of renal portal, and hepatic portal sections; (*b*) system of the cardinal veins; (*c*) system of the lateral veins; (*d*) system of the coronary veins; and (*e*) system of the cutaneous veins.

The caudal vein, on emerging from the hæmal canal, divides into right and left *renal portal veins*, which send branches into the kidneys and receive the posterior spinal and oviducal veins.

There is a large *intra-intestinal vein* enclosed in the free edge of the spiral valve, and continued directly into the hepatic portal vein.

There are two hepatic veins, opening into a large hepatic sinus, which communicates by two small apertures with the sinus venosus.

The paired precaval sinuses receive the jugular, the inferior jugular, and the lateral veins, and are directly continued one into each end of the sinus venosus.

The jugular veins receive the blood from the orbital and hyoidean sinuses; the anterior cerebral and anterior facial veins discharge into the orbital sinus, the posterior facial and nutrient hyoidean veins into the hyoidean sinus. The posterior cerebral and myelonal veins open by numerous commissures into the jugular, cardinal, renal portal, and caudal veins.

The inferior jugular receives a nutrient branchial vein from each of the four holobranchs, and anastomoses in front with the hyoidean sinus.

The cardinal veins are asymmetrically developed, only the right being complete. The latter arises by the confluence of the efferent renal veins, and, passing forwards, joins with the left cardinal. Each vein dilates anteriorly into a *cardinal sinus*, which communicates with its fellow by a wide aperture, that of the right side receiving a large spermatic vein.

There is a large anastomotic trunk between the spermatic and portal veins.

The lateral vein receives the veins from the pectoral and pelvic fins, and unites with its fellow across the dorsal aspect of the pubic cartilage.

The lateral vein is considered to be homogeneous with the epigastric or anterior abdominal vein of amphibia and reptiles; it is also suggested that it may be derived from the vein of the hypothetical lateral fin of the proto-vertebrata, and possibly also from the primary lateral vessel of the proto-chordata. In this connexion some recent speculations on the phylogeny of the chordata are discussed in their bearing on the vascular system.

There are two coronary veins opening into the sinus venosus.

The cutaneous veins are five in number, a dorsal, an anterior and a posterior ventral, and a pair of lateral vessels. The dorsal cutaneous vein opens into the left renal portal, the anterior ventral into the lateral veins anteriorly and into the iliacs posteriorly, the posterior ventral into the cloacal veins, and the lateral into the subscapular sinus. The dorsal and the posterior ventral vessels form closed loops round the bases of the median (first and second dorsal and ventral-anal) fins. The lateral cutaneous vein anastomoses posteriorly with the caudal and with the dorsal cutaneous vein.

Attention is drawn to the large number of anastomoses between important arteries and veins, and to the sinus-like or lacunar character of many of the venous channels.

II. "A Minute Analysis (experimental) of the various Movements produced by stimulating in the Monkey different Regions of the Cortical Centre for the Upper Limb, as defined by Professor Ferrier." By CHARLES E. BEEVOR, M.D., M.R.C.P., and Professor VICTOR HORSLEY, F.R.S., B.S., F.R.C.S. Received June 4, 1886.

(Abstract.)

The following investigation was undertaken as prefatory to a research into motor localisation of the spinal cord.

Anatomy.—(1.) Attention is drawn to some minute details of the topographical anatomy of the upper limb centres as defined by Professor Ferrier.

(2.) Outlines of the shape and arrangement of the fissure of Rolando, the precentral and intraparietal sulci.

(3.) Proof adduced in support of the authors' opinion that the small horizontal sulcus named X by Professor Schäfer really corresponds to the superior frontal sulcus of man.

Previous Researches.—Ferrier's results are then given in full.

Method of Experimentation is explained in detail, as also the mode of subdivision of the part of the cortex investigated into centres of about 2 mm. square.

From the results of excitation are then deduced the two following axioms:—

Axiom I.—Viewing as a whole the motor area of the cerebral cortex for the upper limb, as defined by Professor Ferrier, we find that the regions for the action of the larger joints are situated at the upper part of the area, *i.e.*, closer to the middle line, while those for the smaller and more differentiated movements lie peripherally at the lower part of the area.

Axiom II.—As a broad result, extension of all the joints is the most characteristic movement of the upper part of Ferrier's arm centre; while flexion is equally characteristic of the movements obtained by stimulating the lower part. Finally, between these two regions there is a small portion where flexion and extension alternately predominate, a condition to which we have given the name of *confusion.* (Here both flexors and extensors are contracting at the same time, and consequently the joint is usually fixed in a neutral position, each group of muscles alternately drawing it in opposite directions.)

In confirmation of the facts supporting these axioms, reference is made to a table, the details of which are expanded.

Priority of Movements is found to take place in the order given in

another table, and follows the "march" first indicated by Dr. Hughlings Jackson as existing in epileptic seizures.

This *march* is in accordance with Axiom I, since the shoulder commences the series of movements in the uppermost part of the area, the thumb at the lowest part, and the wrist in the intermediate part.

Summary.—1. That X is the superior frontal sulcus of man.

2. That the movements of the joints are progressively represented in the cortex from above down.

3. Localisation of sequence of movements.

4. Localisation of quality of movements.

5. That there is no absolute line of demarcation between the different centres.

III. "On the Discrimination of Maxima and Minima Solutions in the Calculus of Variations." By E. P. CULVERWELL. Communicated by Professor B. WILLIAMSON. Received June 5, 1886.

(Abstract.)

In the first part of the paper it is shown that the usual investigation by which the second variation of an integral is reduced, requires that the variation given to y (the undetermined function) is such that its differential coefficients, taken with regard to x (the independent variable) are continuous up to the twice-nth order, $\frac{d^ny}{dx^n}$ being the highest differential coefficient of y appearing in the function to be integrated. But it is not necessary that the variation should be continuous beyond its $(n-1)$th differential coefficient, and a method of reducing the variation to Jacobi's form by a process which is not open to the above objection is then given; and the method has the additional advantage that its simplicity enables it to be easily extended to other cases where there are more than two variables.

But in dealing with multiple integrals especially, any method depending on algebraic transformation is necessarily defective, inasmuch as it is invalid unless solutions, which do not become either zero or infinite within the limits of the integration, can be found for a number of simultaneous partial differential equations containing at least as many unknown quantities as equations. It is pointed out that it is not in general possible to obtain such solutions, and that even when the particular problem is assigned, it would be impracticable to ascertain whether there were such solutions.

The method given in the second part of the paper does not depend on or require any algebraic transformation. The second variation is

taken in its unreduced form, and, by considerations founded on the *degree of continuity* required in the variations of the dependent variables, it is shown without difficulty that, *when the range of integration is small*, the sign of the second variation is the same as that of a certain quadratic function. The limits of integration within which this result is applicable are then determined by considerations depending on the continuity of the integrals.

The following is a brief sketch of the method of obtaining the quadratic function on which the sign of the second variation depends. Let the function to be made a maximum be—

$$U=\iint\ldots\int\left(x_1, x_2, \ldots x_m, y_1, y_2, \ldots y_n, \frac{dy_1}{dx_1}, \frac{dy_1}{dx_2}, \ldots \frac{d^p y_n}{dx_m^p}\right)dx_1 dx_2 \ldots dx_m,$$

(the form of $y_1, y_2, \ldots y_n$ as functions of $x_1, x_2, \ldots x_m$ having already been determined by making the first variation vanish). Taking the second variation by Taylor's theorem, we may write—

$$\delta^2 U=\tfrac{1}{2}\iint\ldots\int\Sigma\frac{d^2 f}{dz dz'}\delta z\delta z' dx_1 dy_2 \ldots dx_m,$$

when z and z' typify any of the quantities $y_1, y_2, \ldots y_n$, or their differential coefficients, and Σ means that all such terms are to be taken. Restricting ourselves to the case in which the limits are fixed, we have $\delta y_1, \delta y_2, \ldots \delta y_n$ zero at the limits, and similarly any differential coefficient of these quantities must be zero at the limits, provided it appears among the limiting terms in the first variation. Now if θ be any function of x which vanishes when $x=x_0$, we have—

$$\theta=\int_{x_0}^{x}\frac{d\theta}{dx}dx.$$

Hence, if $x-x_0$ be a small quantity of order β, $\theta\Big/\frac{d\theta}{dx}$ is also a small quantity of the same *or a smaller order*, and therefore if we require to obtain the sign of a quadratic function of θ and $\frac{d\theta}{dx}$, we may neglect all terms except those involving θ^2. Reasoning of this character is applied to the variations in $\delta^2 U$, and the important terms are thus picked out. In this way a set of inequalities is obtained which enable us to determine the important terms in $\delta^2 U$, and therefore its sign.

IV. "On the Anatomy, Histology, and Physiology of the Intra-ocular Muscles of Mammals." By WALTER H. JESSOP, M.A., M.B. Cantab., F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital, London, &c. Communicated by Sir W. BOWMAN, Bart., F.R.S. Received June 7, 1886.

(Abstract.)

This research was prompted by the variety of opinions on the subject of the histology, anatomy, and physiology of the intra-ocular muscles.

Owing to the wide range of the subject I have been obliged to restrict myself almost entirely to the action of these muscles in mammals.

Most of the experiments have been on rabbits, dogs, or cats, and numerous observations, especially on the histology and action of the ciliary muscle, have been on human eyes. The animals experimented on have always been completely anæsthetised.

The subject has been divided into three chief parts—

I. The anatomy and histology; II. The physiology; III. The action of cocaine, atropine and eserine, both singly and in combination.

I. The anatomy of the intra-ocular muscles has been considered as to (1) the muscular tissue, (2) the nerves, (3) the vessels.

(1.) The muscular tissue.

A. *The Iris* consists of a sphincter muscle.

This muscle in mammals is suspended to the ciliary border by a posterior limiting membrane of elastic tissue. I have found no developed dilator muscle extending from the pupillary to the ciliary border except in rabbits, and in these animals it is very thin. Koganei says in the otter, however, it is well developed.

Any muscular fibres found are in the stroma of the iris in front of the posterior limiting membrane.

B. *The ciliary muscle* consists of unstriped muscle in mammals attached by a tendon to the sclero-corneal junction, and its fibres interlace by bundles in every direction, the most developed being the radial and circular fibres, and the position of the ciliary body determines the preponderance of so-called radial and circular fibres. The author is inclined to think from the great variety in length and development of the longitudinal fibres that they are chiefly attachments of the muscle to the elastic tissue of the choroid, and may thus correspond to the so-called radial fibres of the iris when they exist.

(2.) The nerves proceeding to the intra-ocular muscles are branches of (α) the third, (β) the nasal branch of the ophthalmic, and (γ) the sympathetic.

α. The nerve to the inferior oblique muscle from the third nerve generally gives off the branch to the ophthalmic ganglion, and from this are given off the short ciliary nerves proceeding to a plexus in the ciliary muscle.

β. The nasal nerve gives off generally about four long ciliary branches, which surround the optic nerve, piercing the sclerotic and going to the plexus before mentioned.

The nasal also gives off farther forward the long branch to the ophthalmic ganglion.

γ. The sympathetic or so-called mydriatic nerve of the eye receives from the lower cervical and upper dorsal part of the spinal cord fibres generally by the second dorsal nerve. From the second dorsal they pass to the superior thoracic ganglion of the sympathetic and by the annulus Vieussenii to the inferior cervical ganglion, then along the cervical sympathetic to the superior cervical ganglion, and through the carotid canal to the Gasserian ganglion. From here they pass by the nasal branch of the ophthalmic through the long ciliary nerves to the plexus before described.

This plexus situated in the ciliary muscle, and containing ganglion cells, receives fibres from all three nerves; from the plexus are given off branches to the iris and ciliary muscle.

(3.) A. The arteries of the iris come from the ciliary branches of the ophthalmic, and are arranged at the ciliary border as the circulus arteriosus iridis major, and from this are given off radial branches proceeding towards the sphincter, and forming the circulus arteriosus minor, which gives branches to the sphincter. The veins accompany the arteries.

B. The arteries of the ciliary body come from the long and anterior ciliary and form an anastomosis behind the circle for the iris.

II. The physiological action of the intra-ocular muscles may also be classified under the headings of (1) the muscular tissue, (2) the nerves, and (3) the vessels.

(1.) A. *Pupil.*—Direct stimulation of the sphincter pupillæ gives rise to myosis in the normal eye; this also takes place in the bloodless and the exsected eye, and therefore after section of the various nerves and vessels.

On the iris cut out of the eye faradaic stimulation near the pupillary border gives rise to contraction of the pupil.

(2.) A. The nerves supplying the iris are α, the third; β, the sympathetic or mydriatic; γ, the trigeminus.

α. (1.) Stimulation of third nerve or short ciliary nerves is followed by myosis.

(2.) Section of third nerve or short ciliary nerves gives rise to partial dilatation of the iris, and the pupil is inactive to light or accommodation.

β. Stimulation of the path taken by the mydriatic nerve, as described in the anatomical division, gives rise to mydriasis. Section of the nerve in any part of its course is followed by contraction of the pupil, but never excessive; the myosis is increased a little the higher up and the nearer the eyeball the section is made, and this is due to the tonic action of the ganglia.

The mydriatic fibres accompanying the sympathetic therefore find their way by the long ciliary nerves to the eyeball; this may, as the rest of their course, be proved by division of them all, when irritation of the cervical sympathetic gives rise to no alteration in the pupil.

I found on stimulating a single long ciliary nerve that, if the pupil was dilated to start with, general increase in the mydriasis occurred. If, however, the pupil were partially contracted by pilocarpin only half the pupil responded, and if the pupil were more contracted only one-quarter dilated.

In birds stimulation of the cervical sympathetic has no effect on the pupil.

Dual Nature.—The cervical sympathetic containing vaso-constrictor nerves for the head and neck, it is necessary to show that the dilatation of the pupil is not a purely vascular act.

This is shown by—

1. Bleeding an animal to death, and then on exciting the cervical sympathetic several times total mydriasis still occurs.
2. Mydriasis on stimulation of the cervical sympathetic precedes the contraction of the carotid vessels.
3. The maximum dilatation is reached before the carotid vessels are completely constricted.
4. The carotid vessels are constricted when the pupil begins to contract again.
5. The pupil is dilated a short time, while the carotid vessels are constricted for a longer time.
6. The pupil is again constricted before completion of relaxation of the vessels.

γ. Section of the trigeminus in front or behind the Gasserian ganglion gives rise to myosis which soon passes off.

Stimulation of the trigeminus generally gives rise to myosis, which is probably reflex, as on section of the third nerve, I obtained an increase to the mydriasis by stimulating it, as also Fr. Franck has done.

Balogh and others also attribute a mydriatic function to the trigeminus.

(2.) B. The nerves supplying the ciliary muscle are the (α) third and (β) the long ciliary branches of the nasal.

α. (1.) Stimulation of the third nerve or the short ciliary nerves gives rise to contraction of the ciliary muscle.

(2.) Section of the third nerve gives rise to relaxation of the ciliary muscle.

β. When the ciliary muscle is not completely relaxed stimulation of the long ciliary nerves gives rise to relaxation of the muscle, and in consequence paresis or paralysis of accommodation.

Stimulation of the track followed by the cervical sympathetic in the neck gives rise to no alteration in the ciliary muscle, observed by the images on the anterior surface of the lens seen through the phakoscope, therefore the fibres to the ciliary muscle along the long ciliary nerves must come from another source, and this I have little doubt is the fifth, but here as with the pupil the difficulty is to prevent the reflex along the third nerve.

(3.) On section of the cervical sympathetic the arteries of the iris are dilated.

On irritation of the cervical sympathetic the arteries of the eye are contracted.

On section of the trigeminus there is also dilatation of the blood-vessels of the iris and ciliary body according to Rogow.

The separation of purely mydriatic fibres of the cervical sympathetic from the vaso-constrictor has been already proved.

The fact of tapping the anterior chamber of a normal or atropinised eye being followed by myosis, is probably due to relaxation of tension overfilling the arteries of the iris, as proved by an experiment cited.

In an animal bled to death the pupil contracts on faradaising the third nerve or its branches: the pupil also dilates on stimulating the cervical sympathetic.

Ciliary Muscle.—In the bloodless eye accommodation has been seen to take place by stimulating the third nerve.

III. The drugs selected for the following experiments were cocaine, atropine, and eserine, and the results are appended in the above order.

Cocaine. A. *On the Pupil.*—1. Cocainised pupil is of large size, acting to light and accommodation.

2. Cut third nerve, add cocaine and get increased mydriasis.

3. Cocainised pupil not increased by section of third nerve, but no longer acts to light and accommodation.

4. Stimulation of third nerve induces myosis in the pupil under cocaine.

5. Stimulation of sphincter pupillæ easily overcomes the cocaine mydriasis.

6. On the exsected eye cocaine induces mydriasis.

7. On the eyes of animals bled to death, cocaine increases the mydriasis due to hæmorrhage.

8. On cocaine mydriasis section of the cervical sympathetic had no effect.

9. In cases of ad maximum cocaine mydriasis, stimulation of the cervical sympathetic had no effect.

10. In cases of section for some time of cervical sympathetic cocaine has no effect on the pupil.

11. On tapping the anterior chamber of an eye under cocaine there is very little and often no constriction of the pupil.

From these data we may conclude that cocaine acts on the endings of the cervical sympathetic nerve in the eye.

B. *On the Ciliary Muscle.*—Cocaine on the ciliary muscle produces relaxation and paresis, or in some cases paralysis of accommodation, and it is reasonable to suppose from analogy that it acts here on the endings of the fifth nerve to the muscle.

Atropine. A. *On the Pupil.*—1. Atropine produces, in mammals, mydriasis not acting to light and accommodation.

2. It has no effect on the pupils of birds.

3. It only produces a partial mydriasis in rabbits.

4. It increases the mydriasis produced by section of the third nerve, or the short ciliary nerves.

5. Stimulation of the third nerve has no effect on an atropinised pupil.

6. The myosis due to section of the cervical sympathetic is overcome and mydriasis produced by atropine; this seen even after section for three months in a rabbit.

7. Section of the cervical sympathetic diminishes slightly atropine mydriasis.

8. Stimulation of the cervical sympathetic increases atropine mydriasis.

9. Atropine dilates the pupil after excision of the superior cervical ganglion.

10. Atropine mydriasis increased by stimulating one or more of the long ciliary nerves.

11. Atropine mydriasis increased by faradaising the periphery of the cornea.

12. Atropine mydriasis diminished somewhat by cutting the trigeminus.

13. Section of the trigeminus and atropine subsequently put in the eye produced mydriasis.

14. Section of the trigeminus and the third nerve, and instillation of atropine followed by mydriasis.

15. In an ordinary atropine mydriasis direct electrical stimulation of the sphincter produces myosis, but if the pupil be completely under atropine stimulation of the sphincter has no effect.

16. Czermak cut away the cornea, letting out the contents of the anterior chamber, and on adding atropine produced mydriasis.

17. Tapping the anterior chamber of an eye under atropine produces contraction of the pupil.

18. On exsected eye atropine produces mydriasis.

19. On the eye of an animal bled to death, atropine dilates the pupil.

20. Atropine also dilates the pupil after death.

B. *On the Ciliary Muscle.*—On the ciliary muscle atropine produces relaxation, and if used for a long time complete paralysis.

From the above experiments on atropine, and the fact that elsewhere atropine paralyses the unstriped muscular tissue of the body, it may be assumed that it acts so on the intra-ocular muscles.

The effect of section of the cervical sympathetic and trigeminus, preventing extreme atropine mydriasis, may be explained by their effect on the blood-vessels producing turgescence of the iris, and therefore acting against the elastic recoil of the sphincter.

Eserine. A. *On the Pupil.*—1. Eserine on the pupil induces myosis.

2. It contracts the pupil of an animal after section of the third nerve or short ciliary nerves.

3. In full eserine myosis faradaic excitation of the third nerve does not increase the contraction of the pupil.

4. Section of the fifth nerve in front of the Gasserian ganglion, and on putting eserine into the eye the pupil contracts as usual.

5. Faradaic stimulation of the cervical sympathetic overcomes eserine myosis if partial, but if complete it has no effect.

6. Cut cervical sympathetic in the neck and eserine still induces myosis, even after the section has been made three months.

7. For an animal bled to death I have seen eserine induce myosis.

B. *On the Ciliary Muscle.*—Eserine produces contraction of the ciliary muscle, giving rise to spasm of accommodation.

In cases of palsy of the third nerve eserine induces contraction of the ciliary muscle.

The action of eserine on the intra-ocular muscles can be explained by its stimulating directly the muscular tissue.

The antagonism of atropine, eserine, and cocaine is next discussed, and found to agree with the action attributed to each alkaloid.

Conclusion.—From consideration of the foregoing experiments and observations the writer describes the intra-ocular muscular system as consisting of two circular muscles, the pupillary and ciliary. These are supplied, the first by three nerves, the third, the cervical sympathetic, and the fifth, and the second by the third nerve and the fifth.

The muscles are capable of extreme relaxation and contraction, owing to the elastic supports they have.

Section of a nerve producing contraction does not give rise to complete contraction of the muscle, nor does section of a nerve producing relaxation have as a sequel complete relaxation.

The state of the muscle after section of its nerves is that of "tonus."

Atropine by acting directly on the unstriped muscular tissue, and paralysing it, can produce a greater dilatation than mere section of the third nerve, and so also on section of the third nerve eserine directly stimulating the muscular fibre gives rise to extreme contraction.

The intra-ocular muscles therefore have a similar anatomical, histological, and physiological action, and they also are associated together during the act of accommodation. The pupil contracts on accommodating, and dilates on relaxation of the ciliary muscle; the former action is by means of the third as motor nerve of the pupillary and ciliary muscles, and the latter is through the long ciliary nerves from the nasal branch of the fifth.

The ordinary light reflex of the pupil takes place by the third, as the motor nerve, and the cervical sympathetic as the inhibitory nerve giving rise to dilatation of the pupil. The two chief actions of the pupillary muscle are thus divided off from one another in mammals. In birds possessing voluntary control over their irides this is not so, and the cervical sympathetic has no power over the pupil, the nerve producing dilatation of the pupil being the trigeminus.

V. "On the Place of Origin of Uric Acid in the Animal Body." By ALFRED BARING GARROD, M.D., F.R.S. Received June 8, 1886.

(Abstract.)

The endeavour of the author in this communication has been to show the place of origin of uric acid in the animal body, and to ascertain which of the two hypotheses on the subject is correct, viz., whether uric acid is first present in the blood and then secreted from the blood by the kidneys, or whether it is formed by the kidneys themselves. To enable him to satisfactorily prosecute many of his observations, the author has devised a new method for discovering the presence of uric acid in very minute quantities of blood.

The results of his investigations are embodied in the form of the nine following propositions:—

Prop. I.—Uric acid is secreted by the kidneys as ammonium urate, and in the case of birds and reptiles, whose urine is semi-solid, it is found in a definite physical form, more in the vitreous condition than in the truly crystalline shape.

Prop. II.—Uric acid, when present in the blood, is found under the

form of sodium urate, and, when deposited from the blood during life in any tissue, it is also as sodium urate in its characteristic crystalline form.

Prop. III.—The daily quantity of uric acid in relation to their body-weights secreted by different animals varies extremely. In some, as the carnivorous mammalia, the ratio may be less than 1 to 1,000,000, whereas, in others, as birds, it may be as 1 to 85. In man it may be regarded as about 1 to 120,000.

Prop. IV.—The quantity of uric acid contained in the blood of different animals has little relation to that secreted by the kidneys. In birds, secreting daily so large a quantity, the blood is often found to be as free from uric acid as it is in animals whose daily elimination of uric acid is excessively small.

Prop. V.—When uric acid is absorbed from the alimentary canal the blood becomes strongly impregnated, and, in fact, often almost saturated with it, so that its presence is readily discovered by any ordinary test.

Prop. VI.—One cause of the appearance of an unusual quantity of uric acid in the blood of birds in health is the presence of uric acid in the water they drink, and occasionally in their solid food.

Prop. VII.—When uric acid is taken into the stomach of man, or other animals, the secretion of this principle from the kidneys is not increased, although at the time the blood may be rich in it.

Prop. VIII.—Uric acid is found in varying quantities in the blood obtained from different veins in the same animal. It is found in larger quantity in that from the efferent renal veins of birds than in that from the portal afferent, or from the jugular veins; and the same test which freely exhibits uric acid in the blood from the former, often fails to show it at all in that from the latter two.

Prop. IX.—The quantity of uric acid secreted daily by the kidneys of a bird is in close relation to the quantity of nitrogenised food taken during the time.

Having brought forward proofs to confirm these propositions severally, the author draws the following conclusion, viz., that every argument is in favour of the hypothesis that uric acid is formed by the kidney-cells, in the form of ammonium urate, and that the traces of sodium urate ordinarily found in the blood are the result of a necessary absorption, slight in amount, of the ammonium urate from the kidneys into the blood, and its subsequent conversion by that fluid into sodium urate.

VI. "On the Lifting Power of Electromagnets and the Magnetisation of Iron." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Lord RAYLEIGH, Sec. R.S. Received June 1, 1886.

I believe that no very recent investigations have been made with reference to the maximum lifting power which an electromagnet is capable of exerting, and the experiments conducted by Joule between the years 1839 and 1852 still form the basis of most of our practical knowledge on the subject.*

It is a matter of common experience that if an electromagnet be excited by a gradually increasing current a limit is soon reached, beyond which the ratio of increase of sustaining power to increase of current becomes rapidly smaller; and it has generally been assumed that this ratio continues to diminish indefinitely, so that an infinite current would not impart to a magnet much greater lifting power than that which it possesses when an approach to saturation is first indicated.

Joule, after having shown by experiment that the power of an electromagnet varies as its sectional area, expressed the opinion that "no force of current could give an attraction equal to 200 lbs. per square inch," † and much later Rowland stated,‡ as a probable result of his well-known researches in magnetic permeability, that the greatest weight which could be sustained by an electromagnet with an infinite current was, for good but not pure iron, 177 lbs. per square inch, or 12,420 grams per square centimetre of section.

It has long been known that when an iron rod is magnetised its length is in general slightly increased. Some experiments on this effect of magnetisation, an account of which has been given in two papers recently communicated to the Royal Society,§ show that if the magnetisation is carried beyond the point at which the magnetic elongation of the rod reaches a maximum, the length of the rod, instead of remaining unchanged, steadily diminishes, the curve expressing the relation between the length and the magnetising force descending in a perfectly straight line which within the limit of the experiments shows no tendency to become horizontal. Some further experiments (not yet published) have also been made with rings of iron instead of rods, and effects of precisely the same character were

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* Physical Society's Reprint of Joule's Papers; also "Phil. Mag.," ser. 4, vol. 3 (1852), p. 32.

† The greatest attraction which he succeeded in actually producing was 175 lbs. per square inch.

‡ "Phil. Mag.," ser. 4, vol. 46 (1873), p. 140, and vol. 48 (1874), p. 321.

§ "Proc. Roy. Soc.," vol. 40 (1886), pp. 109 and 257.

obtained. The diameter of a ring was found to be increased by a comparatively small magnetising current, and diminished by a strong one. Now the retraction in question does not begin to occur until after the stage of magnetisation, loosely called the "saturation point," has been passed, when, according to the common belief, the magnetisation of the iron has practically reached a limit, and is not sensibly affected by any further increase of the magnetic force; and hence arises a difficulty in accounting for the phenomenon. The most obvious method of explaining the retraction is to assume that under the influence of increasing currents the magnetic attraction of the particles of the iron towards one another is increased, and thus the rod becomes compressed. But this cannot be the case if the magnetic condition of the iron has become constant and independent of the magnetising current. And a similar objection will apply to any hypothesis which assumes (as I think all must) that some property of the iron dependent upon its magnetic condition varies in a sensible degree with the magnetising force after the "saturation point" has been passed.

I was led by considerations of this nature to the belief that it would be desirable to make some experiments with the view of ascertaining whether the lifting power and general magnetic condition of an iron rod are as nearly uniform under strong magnetising forces as is commonly supposed. Two pieces of apparatus were therefore prepared. The first consisted of a rod of iron hooked at each end and divided transversely in the middle, together with a long solenoid, inside which the divided rod could be placed. The second was an iron ring cut into two equal parts each of which was encircled with a coil of insulated copper wire. In both cases the construction was such that an intense magnetic force could be produced with comparatively small battery power. The divided ring could be used either as a semicircular electromagnet with a semicircular armature, or, if the current were passed through both coils, as two semicircular electromagnets.

Merely to test the hypothesis of Joule and Rowland, two or three determinations were made of the weight which could be sustained when the current was caused to circulate around one only of the semicircles, the other being used as an armature. With a current of 4·3 ampères the weight supported was 13,100 grams per square centimetre of surface; with a current of 6·2 ampères the weight supported was 14,200 grams per square centimetre. In the latter case, therefore, the lifting power exceeded that which both Joule and Rowland considered the greatest that could be imparted to a magnet by an infinite current. Had it been worth while to incur the risk of injury to the insulation of the coil, there is no doubt whatever that by applying stronger currents the lifting power might have been carried still further—for there was no indication that a limit was being approached. But it was of greater interest to study the effects produced when both portions of the ring or of the rod were under the direct influence of the magnetising coil.

The first experiment was made with the divided rod. One portion was supported by means of its hook in a vertical position; a scale-pan was attached to the hooked end of the other portion, and the flat ends of the two were brought into contact and surrounded by the solenoid. Currents of gradually increasing strength were then caused to pass through the solenoid, and note was taken of the greatest weight which could in each case be placed in the scale-pan without tearing asunder the ends of the two rods. The general results are briefly as follow:—When the intensity of the field at the junction had reached about 50 C.G.S units, the weight supported was nearly 7000 grams per square centimetre of the section of the rod. After this value was exceeded it became quite evident that the weight which could be sustained was increasing more slowly than the magnetising current, and the proportionate increase became rapidly smaller as the current was made stronger. This state of things continued until the intensity of the field was about 270 units, when the weight supported amounted to 10,800 grams per square centimetre of section. But from this point onwards *the magnetising current and the weight that could be carried increased in exactly the same proportion.* The rate of increase of the load was, indeed, comparatively small, but it was perfectly constant, and continued so until the field had attained the high intensity of 1074 C.G.S. units. Here the experiment was stopped, the greatest weight supported having been 15,100 grams per square centimetre.

On account of some uncertainty as to the possible influence of the external ends of the divided rod, it was thought desirable to make the experiment with the divided ring, the current being caused to pass in the same direction through the coils surrounding both portions. The general character of the results was the same as before, but the weight supported per unit of area was from first to last somewhat greater. The falling off in the rate of increase of the lifting power was well marked when the intensity of the magnetic force had reached 50 C.G.S. units, at which point the weight sustained was about 10,000 grams per square centimetre. And it continued to diminish until the magnetic force was 250 units and the weight supported 14,000 grams. From this point the increments of lifting power and of magnetic force appeared to be exactly proportional, and continued to be so until the magnetic force had been carried up to 585 units, when the limit of the battery power was reached and the experiment stopped, the maximum weight supported having been 15,905 grams per square centimetre or 226·3 lbs. per square inch.*

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* As mentioned below, a part of the effect is to be attributed to the action between the coil and the iron. If in respect of this we deduct $\mathfrak{H}\mathfrak{I}/g = 912$ grams

I proceed to describe the experiments in greater detail.

For several reasons it appeared desirable that the apparatus should be on as small a scale as conveniently possible. Accordingly, I chose for the rods a piece of iron wire, the diameter of which as measured by a micrometer gauge was 2·64 mm. The length of each rod was 12 cm. when straight, and 10·5 cm. when one end had been bent into the form of a hook. The flat ends were rendered as true as was possible by means of a lathe with a slide rest, but they were not ground. The bobbin consisted of a brass tube 4 mm. in internal diameter and 112 mm. in length fitted with ebonite ends. Around this insulated copper wire, 0·7 mm. in diameter, was wound in nine layers, each layer containing 104 turns. The internal diameter of the coil of wire was 6 mm., its external diamater 21 mm., and its length between the ebonite ends 99 mm. The magnetic field at its centre produced by a current of C ampères was therefore

$$\frac{4\pi \times 9 \times 104}{9{\cdot}9} \cos 7^\circ\, 41' \times 10\ \mathrm{C} = 118\ \mathrm{C} \text{ very nearly.}$$

The coil was fixed in a vertical position beneath a small table. One of the iron rods was passed into the coil through a hole in the table and suspended by its hooked end from a horizontal brass bar, the height of which was so adjusted that the flat end of the iron was exactly in the centre of the coil. A scale-pan for carrying the weights was attached to the hook of the other iron rod. Around each rod near the flat end two or three layers of paper were gummed, of sufficient thickness to serve as a guide and ensure that the two ends should meet concentrically inside the coil, but not so thick as to cause any material friction.

The source of electricity was a battery of 27 Grove cells, and the strength of the current was varied by varying the number of cells in use, or when there was only one cell by inserting resistance. The current was measured by a tangent galvanometer of Helmholtz's pattern (by Elliott), having two rings of thick copper wire. With the stronger currents the heating effect was found to be so great that the circuit could not be kept closed for more than a fraction of a second. This of course did not allow sufficient time to make a galvanometer reading. A spiral of bare German silver wire was therefore provided of the same resistance as the coil when cold, through which the current could be diverted by means of a switch, and the galvanometer reading leisurely made.

The method of proceeding was as follows:—A certain number of battery cells having been arranged so that they could be thrown into

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(see Table II, cols. 1 and 2), it appears that the lifting power *due to the magnetisation of the iron only* was 14,993 grams per square centimetre, or 213·3 lbs. per square inch.—[July 1, 1886.]

circuit with the coil by depressing a key, the rod carrying the scale-pan was introduced into the coil and held in contact with the fixed rod. Weights were then placed in the scale-pan and the key momentarily depressed, the lower rod at the same time being left free to hang or to drop; and this operation was repeated with various weights in multiples of 10 grams until it was found that the further addition of 10 grams was sufficient to detach the two rods from each other. A record having been made of the greatest weight which was supported, the current was switched through the German silver wire and a galvanometer reading was made with the current both direct and reversed. The same process was gone through with currents of different strength until the whole number of 27 battery cells had been used.

The results of the experiment are given in Table I, the first column of which shows the intensity of the magnetic field at the centre of the coil, and the second the weight supported per square centimetre of the section of the rod. In fig. 1 the weight supported has been plotted against the intensity of the field, and it will be seen that the curve from $\mathfrak{H}=270$ upwards, is, within the limits of experimental error, a perfectly straight line, which is represented by the equation $W=5{\cdot}16\,\mathfrak{H}+9563$.

Table I.

Divided rod.		Divided ring.	
Magnetic force.	Weight supported.	Magnetic force.	Weight supported.
C.G.S. units.	Grams.	C.G.S. units.	Grams.
40	6,550	3·9	2,210
67	7,640	5·7	3,460
163	9,460	10·3	5,400
264	10,920	17·7	7,530
404	11,650	22·2	8,440
616	12,920	30·2	9,215
735	13,290	40	9,680
896	14,010	78	11,550
1010	14,740	115	12,170
1074	15,110	145	12,800
		208	13,810
		293	14,350
		362	14,740
		427	15,130
		465	15,275
		503	15,365
		557	15,600
		585	15,905

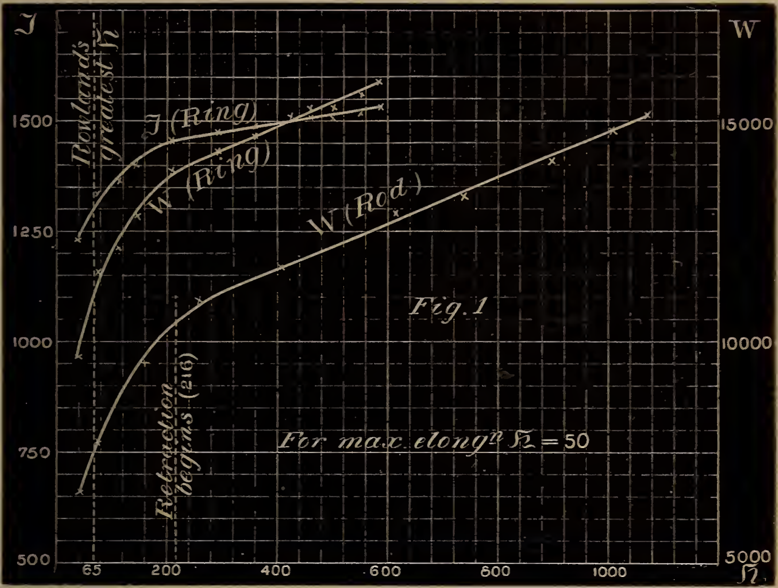

Fig. 1

A point marked on the curve indicates the magnetic force under which in some experiments with a rod of the same iron retraction began. The point corresponding with the maximum magnetic force reached in Rowland's experiments is also marked.

The ring was made of very soft charcoal iron rod 6·4 mm. in thickness, the joint being carefully welded. It was turned in a lathe to a uniform circular section, and when finished its external diameter was 8 cm., and the diameter of its transverse section 4·82 mm. The ring was sawn into two equal portions, and the cut faces ground as flat as possible, first with fine emery, and afterwards with rottenstone. But I was unable to get rid of a very slight convexity which, though it perhaps affected the permeability of the ring, was of some advantage in preventing the possibility of complications arising from non-magnetic adhesion. Upon the ends of one-half of the ring pieces of thin brass tube 5 mm. in length were fitted: these projected 1 mm. beyond the faces, forming shallow sockets into which the ends of the other portion of the ring could be inserted, thus insuring exact coincidence of the cut surfaces. Each half of the ring was then covered with ten layers of insulated wire 0·7 mm. in thickness, the radial gaps in every layer being filled with paraffin wax before a new layer was wound on. The number of turns of wire upon the portion having the brass tubes was 980, and upon the other portion, the ends of which protruded about 1·2 mm. beyond the wire, 949. The total number of turns was, therefore, 1929, and when the two halves were fitted together the coil was practically continuous.

The mean radius of the divided ring being 3·76 cm., its mean circumference is 23·6 cm. Hence the magnetic force produced by a current through the coil of C ampères = 102·7 C.

The experiment was made in the same manner as before, but the length of the wire contained in the ring-coil was about twice the length of that in the straight solenoid, and in consequence of the increased resistance thus introduced and other differences in the conditions of the arrangement, the greatest magnetic force which could be produced by the whole battery of 27 cells did not exceed 585 units. On the other hand, it was found possible to obtain satisfactory results with smaller currents than could be used in the former case. When the magnetic force was less than 40 units the residual or permanent magnetism of the divided rod seriously interfered with the observations; but in the experiment with the divided ring, owing partly perhaps to the superior quality of the iron, and partly to the difference of the arrangement, the magnetic force could be diminished to about 4 units before the residual magnetism began to be troublesome.*

The results of the experiment are given in the third and fourth columns of Table I, and plotted in fig. 1 as a curve which from $\mathfrak{H}=240$ is sensibly a straight line represented by $W=5{\cdot}3\,\mathfrak{H}+12{,}800$.

It occurred to me that if an expression could be found which would satisfactorily connect the magnetic force and the weight lifted with the magnetic intensity of the iron ring, the results above obtained might be applied to the investigation of the changes of magnetisation which correspond to changes of magnetic force.† The common belief that at a comparatively early stage the intensity of magnetisation becomes sensibly constant is, I imagine, founded rather upon inference than upon actual observation. At all events, I am acquainted with no experiments bearing upon the subject which have been made with magnetic forces at all comparable in magnitude with those used by myself.

If from a portion of a magnet in which the direction and intensity of the magnetisation are uniform there be hollowed out a cavity in the form of a thin disk whose plane is normal to the direction of magnetisation, a unit magnetic pole at the middle of the axis experiences a force $4\pi I$ due to the attraction of the superficial magnetism of the disk on the positive side, and the equal repulsion

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* If it did in fact exert any material influence before this point was reached, the effect was at all events uniform, and not apparently capricious.

† The expression used by Rowland in the paper before referred to is $W=Q^2/2465500000$ kilograms per square centimetre, which translated into C.G.S. units and the ordinary notation, becomes $W=\mathfrak{B}^2/8\pi g$ grams per square centimetre. This is clearly erroneous.

of the superficial magnetism of the disk on the negative side.* In calculating the force upon unit of area of one surface of the disk itself, we must omit the half due to the surface in question. Thus the force becomes $2\pi I^2$ per unit of area of surface. To this must be added the force due to the mutual action of the coil and the iron $=\mathfrak{H}\mathfrak{I}$.

If, therefore, W = the grams weight supported per unit of area, we have for the divided ring—

$$Wg=2\pi\mathfrak{I}^2+\mathfrak{H}\mathfrak{I},$$

and by giving to W and $\mathfrak{H}$ the values which have been found by experiment to correspond with each other, we have the means of finding corresponding values of $\mathfrak{H}$ and $\mathfrak{I}$. These are given in the first and second columns of Table II, and are plotted as a curve in fig. 1. Here again it will be seen that when $\mathfrak{H}$ has exceeded the value of about 200, the ratio $\mathfrak{I}$ to $\mathfrak{H}$ no longer continues to diminish, and the curve apparently becomes a straight line, the equation to which is—

Table II.

$\mathfrak{H}$.	$\mathfrak{I}$.	κ.	μ.	$\mathfrak{B}$.
3·9	587	151·0	1899·1	7,390
5·7	735	128·9	1621·3	9,240
10·3	918	89·1	1121·4	11,550
17·7	1083	61·2	770·2	13,630
22·2	1147	51·7	650·9	14,450
30·2	1197	39·7	500·0	15,100
40	1226	30·7	386·4	15,460
78	1337	17·1	216·5	16,880
115	1370	11·9	150·7	17,330
145	1403	9·7	122·6	17,770
208	1452	7·0	88·8	18,470
293	1474	5·0	64·2	18,820
362	1489	4·1	52·7	19,080
427	1504	3·5	45·3	19,330
465	1508	3·2	41·8	19,420
503	1510	3·0	38·7	19,480
557	1517	2·7	35·2	19,630
585	1530	2·6	33·9	19,820

$$\mathfrak{I}=0{\cdot}19\mathfrak{H}+1418.$$

I do not suggest that the portions of both the curves obtained for W and $\mathfrak{H}$, and for $\mathfrak{I}$ and $\mathfrak{H}$, which so far as the experiment goes differ insensibly from straight lines, would, in fact, continue to

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* Maxwell's "Electricity," vol. 2, §§ 396–9.

appear as absolutely straight if they were prolonged indefinitely. Assuming the curve of W to be really straight, the curve of $\mathfrak{I}$ is represented by

$$2\pi\mathfrak{I}^2+\mathfrak{H}\mathfrak{I}=(5{\cdot}3\mathfrak{H}+12800)g,$$

or

$$\frac{2\pi\mathfrak{I}^2-12800g}{\mathfrak{H}}+\mathfrak{I}=5{\cdot}3g.$$

Hence for an infinite magnetic force

$$\mathfrak{I}=5{\cdot}3g=5200 \text{ C.G.S. units.}$$

The curve is a hyperbola, an asymptote of which is parallel to the axis of $\mathfrak{H}$.

Similarly, if we assume the curve of $\mathfrak{I}$ to be a straight line, the curve of W must be a parabola. From the experimental results it is impossible to determine whether either curve is in fact a perfectly straight line. But in neither case do they give any evidence of the existence of a limit, and if there is one it must be very much higher than it is generally believed to be.

The third column of Table II shows the values of the coefficient of magnetisation or the susceptibility, κ (derived from $\mathfrak{I}=\kappa\mathfrak{H}$), which correspond to different values of $\mathfrak{H}$.

If $\mathfrak{B}$ denote the magnetic induction and μ the magnetic permeability—

$$\mathfrak{B}=\mu\mathfrak{H},$$

and

$$\mu=1+4\pi\kappa.$$

We can, therefore, easily find the values of $\mathfrak{B}$ and μ corresponding with different values of $\mathfrak{H}$. These values are given in the fourth and fifth columns of Table II.

In connexion with the well-known experiments and views of Professor Rowland, the figures thus obtained are of the highest interest. In order to exhibit the results of his experiments in the form of a curve which (as he believed) would be of finite dimensions, Rowland adopted the method of plotting the values of μ as ordinates against those of $\mathfrak{B}$ as abscissæ, and thus obtained the curve shown by a dotted line in fig. 2, which is taken from his paper. The curve was carried by actual experiment as far as the point marked X, before reaching which it had apparently become an almost perfectly straight line. Rowland, therefore, assumed that the line would continue to be straight until it met the horizontal axis at a point C, the abscissa of which would indicate the greatest possible value of $\mathfrak{B}$ for an infinite magnetic force. He thus arrived at the conclusion that for ordinary bar iron the maximum of magnetic induction was about

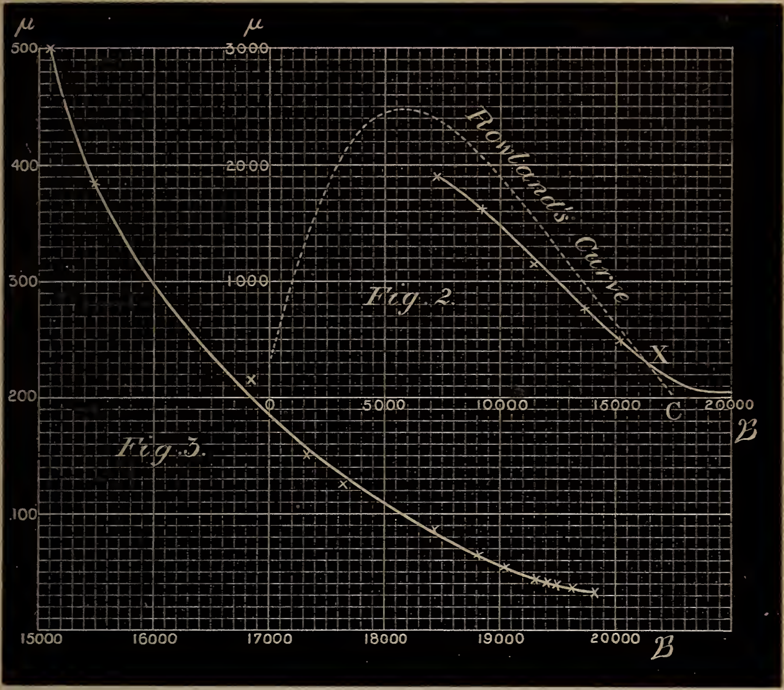

Fig. 2.

Fig. 3.

17,500 units. For pure iron he thought it might reach 18,000, or even go above that.*

Now, the magnetic force used in Rowland's experiments was very small. The highest value—that corresponding to the point X in the curve, fig. 2—being only 65 C.G.S. units. The imaginary part of the curve from X to C therefore corresponds to values of $\mathfrak{H}$, ranging from 65 to infinity. A part of this exceedingly wide gap is filled by my experiments, which include values of $\mathfrak{H}$ up to 585. A curve constructed from the fourth and fifth columns of Table II, showing the relation between μ and $\mathfrak{B}$, is given on a small scale in fig. 2, beside that of Rowland. It corresponds, of course, only with a portion of Rowland's curve, the lowest value of $\mathfrak{B}$ included in it being 7390. But it seems to throw much new light on the subject. Beginning with a rapid descent it turns aside soon after the limit of Rowland's observations has been passed, and ultimately when $\mathfrak{B}=$ 19,800 it has become almost parallel to the horizontal axis. The latter portion of the curve, from $\mathfrak{B}=15{,}100$, is given on a larger scale in fig. 3. We may conclude then that if $\mathfrak{B}$ has any ultimate limit at

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* In his second paper, however, he says that we have at present no data for determining whether $\mathfrak{B}$ attains a maximum or only $\mathfrak{J}$.

all it is, at all events, very much higher than that which was assigned to it by Rowland.*

There may, perhaps, be some doubt whether the expression used above is exactly applicable to the case of my divided ring, and small errors may possibly be introduced by the fact that the contact between the opposite faces was not quite perfect throughout.† But apart from minute accuracy of detail, the general character of the results is entirely free from doubt, and would be quite unaffected by a very large margin of uncertainty in the expression. They show that the generally accepted ideas with regard to several important points need modification.

Thus it is not true that the lifting power of an electromagnet reaches a practical limit under a comparatively small magnetising force, and that even if excited by an infinite current it could not support a weight of 200 lbs. per square inch of surface.

It is not true that the magnetisation of iron becomes sensibly constant when the magnetic force exceeds a certain moderate value.

And it is not true that the maximum of magnetic induction, if it exists at all, is represented by anything like so small a value as 18,000 units.

In conclusion, I have to express my great obligation to Lord Rayleigh for much valuable assistance and advice in the preparation of this paper.

VII. "On a New Scale for Tangent Galvanometers." By W. H. PREECE, F.R.S., and H. R. KEMPE. Received May 6, 1886.

Tangent galvanometers are much used for the exact quantitative measurement of currents of considerable strength, such as are measured in ampères, but they are not so generally used for the measurement of smaller currents in milliampères. This arises from a notion that they are not sensitive enough; although the most sensitive instrument in practice—Sir William Thomson's mirror galvanometer—is really a tangent instrument. The ordinary forms —Joule's, Gaugain's, or Helmholtz's—are not very sensitive. Their

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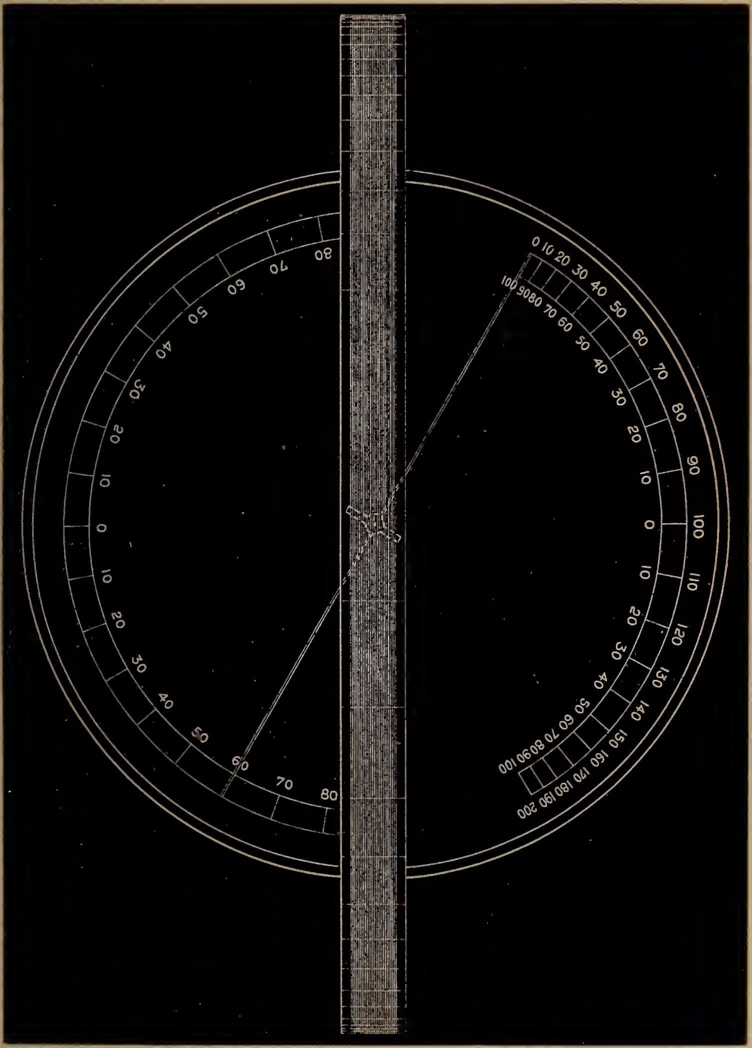
* It is hardly necessary to point out that if there is any limit to $\mathfrak{B}$, the susceptibility, κ, must become *negative* when the magnetic force exceeds a certain value. Maxwell appears to have considered this not impossible. See "Electricity," vol. 2, § 844.

† The close agreement of the values of κ in Table II with those obtained in the experiments of Stoletow (so far as the latter go) affords strong evidence of the accuracy of the method (see "Enc. Brit.," 9th edit., vol. 15 (1883), p. 255).

FIG. 1.



constant is about 3, that is, 3 ampères are required to produce the unit deflection of 45°.

The Post Office has for many years used a more sensitive and portable form, having a constant of 0·00214, for testing wires, batteries, and apparatus. Two scales have hitherto been engraved on the dial of the instrument, one in degrees and the other in tangent divisions; and to avoid parallax the indicator is reflected in a mirror, so that when the image and the indicator are in one line, no error from this cause occurs.

Mr. Eden, one of the assistant electricians of the Post Office, discovered while experimenting that the galvanometer could be made more sensitive to increments of current for high deflections if it were given a false zero. In fact, if the instrument be "slewed" round so that the plane of the coil makes an angle of 60° with the meridian, then the instrument becomes twice as sensitive as it was before; and he suggested a double scale such as is shown in Fig. 1 (p. 497) to utilise this fact.

This plan has been adopted, and all tangent galvanometers in the Post Office service will be gradually altered to the new scale.

It is quite clear that it owes its increased sensitiveness to the fact that when the needle reaches its maximum deflection, its angle to the lines of force of the field is in the most favourable direction for deflection by the current. In fact, the plane of the coil becomes parallel to the plane of the needle which is then in the most uniform portion of the field.

By changing the zero of the instrument the range of movement can be considerably increased. Thus if β° be the angle which the needle normally makes with the coil, and if α° be the angle to which the needle has been deflected on the other side of the coil, then, f being the deflective force, we have—

$$f = f_1 \frac{\sin(\alpha^\circ + \beta^\circ)}{\cos \alpha^\circ},$$

where f_1 is a constant; but if the needle were parallel to the coils, then—

$$f = f_1 \tan \alpha_1,$$

therefore—

$$f_1 \frac{\sin(\alpha^\circ + \beta^\circ)}{\cos \alpha^\circ} = f_1 \tan \alpha_1^\circ.$$

Now if we have $\beta^\circ = 60^\circ$, and if the current were sufficient to turn the needle through $2\beta^\circ$, or 120°, that is, if we have $\alpha = 60^\circ$, then—

$$\frac{\sin 120^\circ}{\cos 60^\circ} = \tan \alpha_1^\circ,$$

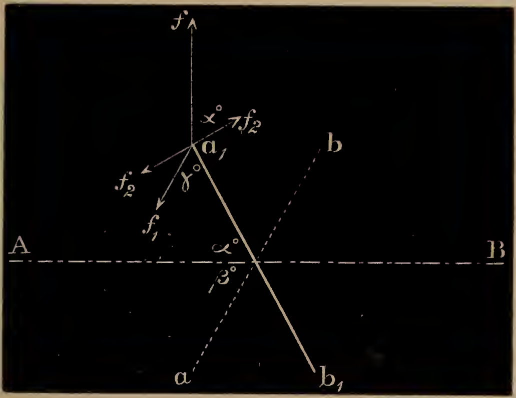
but $\sin 120^\circ = \sin(180^\circ - 60^\circ) = \sin 60^\circ$, therefore—

$$\frac{\sin 60^\circ}{\cos 60^\circ} = \tan \alpha_1^\circ = \frac{\sin \alpha_1^\circ}{\cos \alpha_1^\circ},$$

or

$$\alpha_1 = 60^\circ,$$

FIG. 2.



that is to say, the angle through which the needle would have been turned when the zero was 60° to one side of zero, would be twice what it would be if it had been deflected from the ordinary zero. The ratio of these angles, however, decreases in value, and at a certain point becomes equal to 1; that is to say, the angular deflection from the new zero is the same as it is from the old zero.

Let
$$\frac{\sin(\alpha^\circ+\beta^\circ)}{\cos\alpha^\circ}=\tan\alpha^\circ,$$

therefore
$$\frac{\sin(\alpha^\circ+\beta^\circ)}{\cos\alpha^\circ}=\frac{\sin\alpha^\circ}{\cos\alpha^\circ},$$

or
$$\sin(\alpha^\circ+\beta^\circ)=\sin\alpha^\circ.$$

If the angle α° is negative, that is to say, if the angular deflection from the new zero is less than the angle β°, then we have—

$$\sin(\beta^\circ-\alpha^\circ)=\sin\alpha^\circ,$$

or
$$\beta^\circ-\alpha^\circ=\alpha^\circ;$$

that is,
$$\beta^\circ=2\alpha^\circ,$$

or
$$\alpha^\circ=\frac{\beta^\circ}{2},$$

that is to say, whatever be the angle β° (the angular distance of the new from the old zero), then a current sufficient to move the needle a distance of $\frac{1}{2}\beta^\circ$ from the old zero would move it the same distance from the new zero. If the deflection from the old zero be less than

$\frac{1}{2}\beta^\circ$, then the deflection from the new zero will be less still; so that there is no advantage in the use of the new zero unless the deflections exceed $\frac{1}{2}\beta^\circ$.

If the angle β° be made greater than 60° then the possible angular movement of the needle becomes still further increased; but inasmuch as any increase in the length of the tangent scale brings the divisions at the ends of the scale proportionally closer together, and makes them more difficult to read from, there would be no practical advantage in making the angle larger.

A marked advantage under certain conditions is found when the new zero has such a value that the deflection from a given current causes the needle to move up to the ordinary zero, that is to say, to the position where the needle becomes parallel to the coil; in this case the instrument becomes highly sensitive, and any increase in the strength of the current produces a very considerable change in the deflection.

VIII. "On Fluted Craterless Carbons for Arc Lighting." By Sir James N. Douglass. Communicated by Sir William Thomson, F.R.S. Received June 4, 1886.

[Plate 6.]

On the 8th December, 1858, at the South Foreland High Lighthouse, and with the direct current magneto machines of Holmes, the first important application of the electric arc light, as a rival to oil and gas for coast lighting, was carried out by the Trinity House, under the advice of Faraday. The carbons then used, and for several years afterwards, were sawn from the residuum carbon of gas retorts; they were square in section, $6\frac{1}{4} \times 6\frac{1}{4}$ mm., and the mean intensity of the arc, measured in the horizonal plane, was 670 candle units, being 17 candle units nearly per square millimetre of cross sectional area of the carbon. The crater formed at the point of the upper carbon of the "Holmes" lamp was so small that no appreciable loss of light was found to occur, and the arc proved to be very perfect in affording an exceptionally large vertical angle of radiant light for application with the optical apparatus as shown, one-third full size, in the sketch (Plate 6).

The most reliable and efficient machine that has yet been tried for lighthouse purposes is the large size alternate current magneto machine of De Meritens. The average results with these machines are as follows, viz.:—

	One machine.	Two machines supplying currents to one lamp.
E.M.F.	38 volts	48 volts.
Mean current	206 ampères	372 ampères.
Diameter of carbons (cylindrical)	35 mm.	50 mm.
„ crater in carbon....	13 „	18 „
Mean intensity of arc measured in the horizontal plane (candle units	15,000	30,000
Light per square millimetre of carbon section (candle units)	12	12

It will be observed from this statement that the intensity of the arc in the horizontal plane per square millimetre of sectional area of carbon is about 35 per cent. less than it was with the small square carbons used by Holmes, although it might reasonably be expected that, with the improvements since effected in the manufacture of carbons, the efficiency of the old small carbons would at least be maintained. The relative efficiency of the large carbons used with the powerful currents now available appears to be due, 1st, to the loss of a large portion of the most intense part of the arc which is confined within the crater of each carbon; and, 2nd, to the fluctuations in the intensity of the arc caused by the current passing between various parts of the end of each carbon. For a new electric light installation, about to be made by the Trinity House at St. Catherine's Lighthouse, Isle of Wight, it is intended to utilise the large "De Meritens" machines that were used at the recent South Foreland experiments for determining the relative merits of electricity, gas, and oil as lighthouse illuminants. The electric light at St. Catherine's is intended to be "single flashing" at periods of 30 seconds. Each flash is to have a duration of 5½ seconds, followed by an eclipse of 24½ seconds. It is intended to use one "De Meritens" machine during clear weather, and two whenever the atmosphere is found to be so impaired for the transmission of light, that the flashes are not reaching their advertised range. The defect here arose, which is common to all electric flashing lights where a minimum and a maximum intensity are adopted, viz., that the duration of the flashes of minimum and maximum intensity would vary in the ratio of the difference in the diameters of the carbons employed with one and two machines respectively, which in this case should be 50 mm. and 35 mm., this mean difference amounting to 36½ per cent. nearly.

It is evident that such a variation in the duration of flash would seriously impair the distinctive character of the signal. It occurred to me, however, that, if carbons of a fluted section were employed for the arc of minimum intensity whose extreme diameter corresponded exactly with the diameter of the carbons used for the arc of maximum intensity, and of exactly half the sectional area of the latter, the defect referred to would be entirely obviated, and the flashes of maximum and minimum intensity would have exactly the same duration. As all carbons for electric arc lights are now made in moulds, I saw that such a form as shown in the accompanying full-size sketch (and model) would not involve any more difficulty in manufacture than if cylindrical, while there would be less liability of fracture occurring in the process of drying and baking. Other advantages to be obtained with fluted carbons are, 1st, a larger vertical angle of radiant light from the arc, and with a higher coefficient of intensity in consequence of the unobstructed radiance through the fluting at the points of each carbon; and, 2nd, a steadier light is obtained owing to the localising of the current at the central portion of each carbon.

The results of many experimental trials with fluted carbons 50 mm. diameter, as shown by the sketch and models submitted herewith, have entirely confirmed my expectations. It will be observed that no crater is formed, and the point of each carbon is all that can be desired for utilising fully the maximum light of the arc. My experiments have not been sufficient to determine accurately the additional intensity of light obtained from the arc of a pair of the fluted carbons as compared with that from the arc of a pair of cylindrical carbons, but I am of opinion that the gain with fluted carbons is not less than 10 per cent.

IX. "On some new Elements in Gadolinite and Samarskite, detected spectroscopically." By WILLIAM CROOKES, F.R.S., V.P.C.S. Received June 9, 1886.

The recent discovery by my distinguished friend M. de Boisbaudran* on the existence of a new element which he calls Dysprosium makes it unadvisable on my part, as a fellow investigator in spectroscopic research, to delay any longer the announcement of some of the results I have obtained during the fractionations of the samarskite and gadolinite earths.

I will first take the earths which give absorption-spectra when their solutions are examined by transmitted light. These occur chiefly at the higher end, beginning with didymium and proceeding, through samarium, holmium, &c., to erbium, which is one of the least basic. The earths which give phosphorescent spectra chiefly occur at the lower end, but each group overlaps the other; for instance yttria occurs above erbia.

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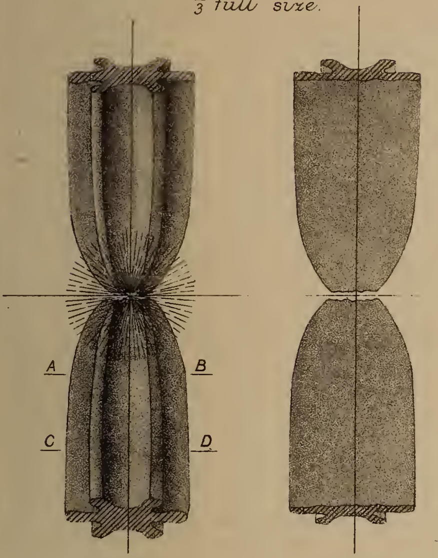
* "Comptes rendus," vol. 102 (1886), p. 1003.

Fluted Carbons.
50 m. m. diam.

$\frac{1}{3}$ full size.



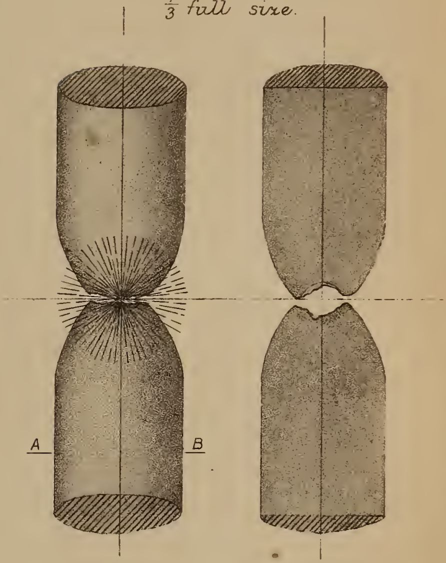
Elevation.   Vert.^{l} Section.

Sectional Plan at A. B.

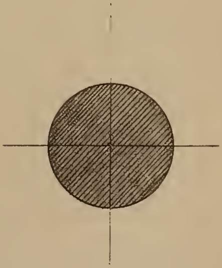
Sectional Plan at C. D.

Cylindrical Carbons.
50 m. m. diam.

$\frac{1}{3}$ full size.



Elevation.   Vert.^{l} Section.



Sectional Plan at A. B.

West, Newman & Co lith

One of the highest of the absorption-spectrum earths is didymia. The spectrum of didymium, as generally met with, is well known, and is given in my paper on "Radiant Matter Spectroscopy: Part 2, Samarium" (par. 135).

It has long been suspected that didymium is not a simple body, and in June, 1885, Dr. Auer v. Welsbach announced that by a series of many hundred fractional crystallisations he had succeeded in splitting up didymium into two new elements, one giving leek-green salts and the other rose-red salts. The green body he called Praseodymium and the rose-red Neodymium. I have not found that my method of fractionation gives a decomposition similar to this; probably didymium will be found to split up in more than one direction, according to the method adopted; but by pushing the fractionations at the didymium end of the series to a considerable extent, a change gradually comes over the spectrum. At the lower end the earth gives an absorption-spectrum such as is usually attributed to didymium, but with no trace of some of the bands in the blue end, the one at λ 443 being especially noticeable by its absence. The intermediate earths give the old didymium spectrum, the relative intensities of some of the bands varying according to the position of the earth in the series, the band 443 becoming visible as the higher end is approached. The highest fractions of all give the band 443 one of the most prominent in the spectrum, being accompanied by other fainter bands which are absent in the lowest didymium spectrum.

In my note-book, under date 3rd March, 1886, after discussing the absorption-spectrum given by one of my earths rather lower down in the series (fraction −3), and comparing coincidences of the lines with those given by holmia, erbia, didymia, thulia, and samaria, I remark "the big blue line (λ 451·5) is still unclaimed." In ignorance that my friend M. de Boisbaudran was on the same track, and running me somewhat close, I deferred further examination of these fractions till a few months longer work had been performed on them, when I hoped to get fuller evidences of a new absorption-spectrum. This big line in the blue, λ 451·5, now proves to be the characteristic line of dysprosium. This line does not occur in didymium. The next strongest line, λ 475, is coincident with a very faint line in the old didymium spectrum, and it also falls within a broad band of samarium. M. de Boisbaudran says that this line is not due either to didymium, erbium, or samarium; as it follows the same variations of intensity as the other lines of dysprosium he considers it due to the same element.

The earth (−3), which I have already mentioned as giving the broad black band (451·5) of dysprosium, shows this band wider (and therefore presumably stronger) than in the spectrum given by M. de Boisbaudran, without the slightest trace of the band 475, which, according to M. de Boisbaudran, should be wider and almost as dense as the band 451·5. It is obvious therefore that the element giving the band 475 cannot be the same as the one causing the band 451·5, and if the body giving the strongest of these is called dysprosium another name must be chosen for the element which gives rise to the absorption-band 475.

And now comes the question: What is the origin of band 475? In remarks made on the band 443 I mentioned that it is accompanied by other fainter lines. One of these occurs at 475, and therefore I was prepared to connect these bands as being due to one and the same element; but M. de Boisbaudran, in his description of the spectrum of dysprosium, has shown that band 475 can be obtained strong in the absence of 443. The bands 443 and 475 therefore are not caused either by didymium, dysprosium, or any hitherto identified element; consequently each must be regarded as characteristic of a new body.

I now come to a branch of the subject which promises to yield results even more fruitful than those given by the examination of absorption-spectra: I refer to the spectra yielded by some of the earths when phosphorescing *in vacuo*. This method has been so fully explained before the Royal Society, in my papers on "Radiant Matter Spectroscopy," that I need not repeat it.

In my Bakerian Lecture on Yttrium* I described the phosphorescent spectrum of this earth, and gave a drawing of it. In the Samarium paper† I gave a similar description and drawing of the samarium spectrum, and also described and illustrated some anomalous results obtained when yttria and samaria were mixed together. Under the conditions described in the paper a sharp and brilliant orange line made its appearance, which at that time seemed as if it belonged to the samarium spectrum, and was only developed in greater intensity by the presence of yttria. This explanation, however, did not satisfy me, and I called the line $\left(\lambda\, 609 = \frac{1}{\lambda_2}\, 2693\right)$ "the anomalous line," intending to return to it at the first opportunity.‡ I have since further investigated the occurrence of this line, with more than usual good fortune in the extent and importance of the new facts thereby disclosed.

Systematic fractionation was carried on with the portions of the general series giving the strongest appearance of line 609, and it soon became apparent that the line closely followed samarium. The

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* "Phil. Trans.," vol. 174 (1883), p. 891.

† *Ibid.*, vol. 176 (1885), p. 691.

‡ In paragraph 146, p. 713, of the Samarium paper already quoted, speaking of this line I said, it was "so unlike the bands usually met with in the spectra of phosphorescent earths as to suggest the explanation that some other spectrum-forming body was present in the mixture."

presence of yttria was not necessary to bring it out, although by deadening the brightness of the other bands it was useful, not seeming to affect the line 609. Several circumstances, however, tended to show that although line 609 accompanied samarium with the utmost pertinacity, it was not so integral a part of its spectrum as the other red, green, and orange lines. For instance, the chemical as well as physical behaviour of these line-forming bodies was different. On closely comparing the spectra of specimens of samaria from different sources, line 609 varied much in intensity, in some cases being strong and in others almost absent. The addition of yttria was found to greatly deaden the red, orange, and green lines of samarium, while yttria had little or no effect on the line 609; again, a little lime entirely suppressed line 609, while it brought out the samarium lines with increased vigour. Finally attempts to separate line 609 from samarium and those portions of the samarskite earths in which it chiefly concentrated resulted in sufficient success to show me that, given time enough and an almost inexhaustible supply of material, a separation would not be difficult.

But what was then practically impossible to me, restricted by limited time and means, Nature has succeeded in effecting in the most perfect manner. I had been working on samarskite, and many observations had led me to think that the proportion of band-forming constituents varied slightly in the same earth from different minerals. Amongst others, gadolinite showed indications of such a differentiation, and therefore I continued the work on this mineral. Very few fractionations were necessary to show that the body giving line 609 was not present in the gadolinite earths, no admixtures of yttria and samaria from this source giving a trace of it. It follows, therefore, that the body whose phosphorescent spectrum gives line 609 occurs in samarskite, but not in gadolinite; thus it cannot be due to samarium, yttrium, or a mixture of these two elements; the only other probable alternative is that the source of this line is a new element.

Chemical fractionation is very similar to the formation of a spectrum with a very wide slit and a succession of shallow prisms. The centre portion remains unchanged for a long time, and the only approach to purity at first will be at the two ends, while a considerable series of operations is needed to produce an appreciable change in the centre.

During the later fractionations of the yttria earths another set of facts, formerly only suspected, have assumed consistent form. The spectrum bands which hitherto I had thought belonged to yttria soon began to vary in intensity among themselves, and continued fractionation increased the differences first observed. It would exceed the limits of a preliminary note were I to enter into details respecting the chemical and physical reasons which lead me to the definite conclusions I now bring before this Society. More than 2000 fractionations have been performed to settle this single point. I will content myself with stating the results. The element hitherto called yttrium appears to be a highly complex molecule, capable of being dissociated into several simpler substances, each of which gives a phosphorescent spectrum of great simplicity, consisting for the most part of only one line.

Taking the constituents in order of approximate basicity (the chemical analogue of refrangibility) the lowest earthy constituent gives a violet band (λ 456), which I have reason to believe belongs to ytterbia. Next comes a deep blue band (λ 482); then the strong citron band (λ 574), which has increased in sharpness till it deserves to be called a line; then come a close pair of greenish-blue lines (λ 549 and λ 541, mean 545); then a red band (λ 619), then a deep red band (λ 647), next a yellow band (λ 597), then another green line (λ 564); this (in samarskite yttria) is followed by the orange line (λ 609) of which I have already spoken; and finally, the three samarium bands remain at the highest part of the series. These for the present I do not touch, having my hands fully occupied with the more easily resolvable earths.

In the "Comptes rendus" for April 19th, 1886, M. de Boisbaudran announces to the Academy that M. de Marignac, the discoverer of Yα, had selected for it the name Gadolinium. In February last I gave a short note on the earth Yα,* in which I described its phosphorescent spectrum (agreeing exactly with that given by Yα of M. de Marignac's preparation). Referring to my paper it will be seen that Yα is composed of the following band-forming bodies:—(541, 549), (564), (597), (609), (619), together with a little samarium. Calling the samarium an impurity, it is thus seen that gadolinium is composed of at least four simpler bodies. The pair of green lines (λ 541 and λ 549, mean 545), being the strongest feature in its spectrum, may be taken as characteristic of gadolinium: the other lines are due to other bodies.

It is by a method of his own, differing from mine, that M. de Boisbaudran has obtained phosphorescent spectra of some of these earths. He takes the induction spark between the surface of a strong and acid solution of the metallic chloride, and a clean platinum wire a few millimetres above it. The platinum wire is kept negative and the solution positive; it is then observed that in many cases a thin layer of fluorescent light is seen at the surface of the liquid. This layer gives a spectrum of nebulous bands. For the sake of brevity I will adopt M. de Boisbaudran's term, and call this process the *method of reversion* (the direction of the spark being reversed). As this method is entirely different to the one I adopt, it is not surprising that the results also are different. Experimenting in this way M. de Bois-

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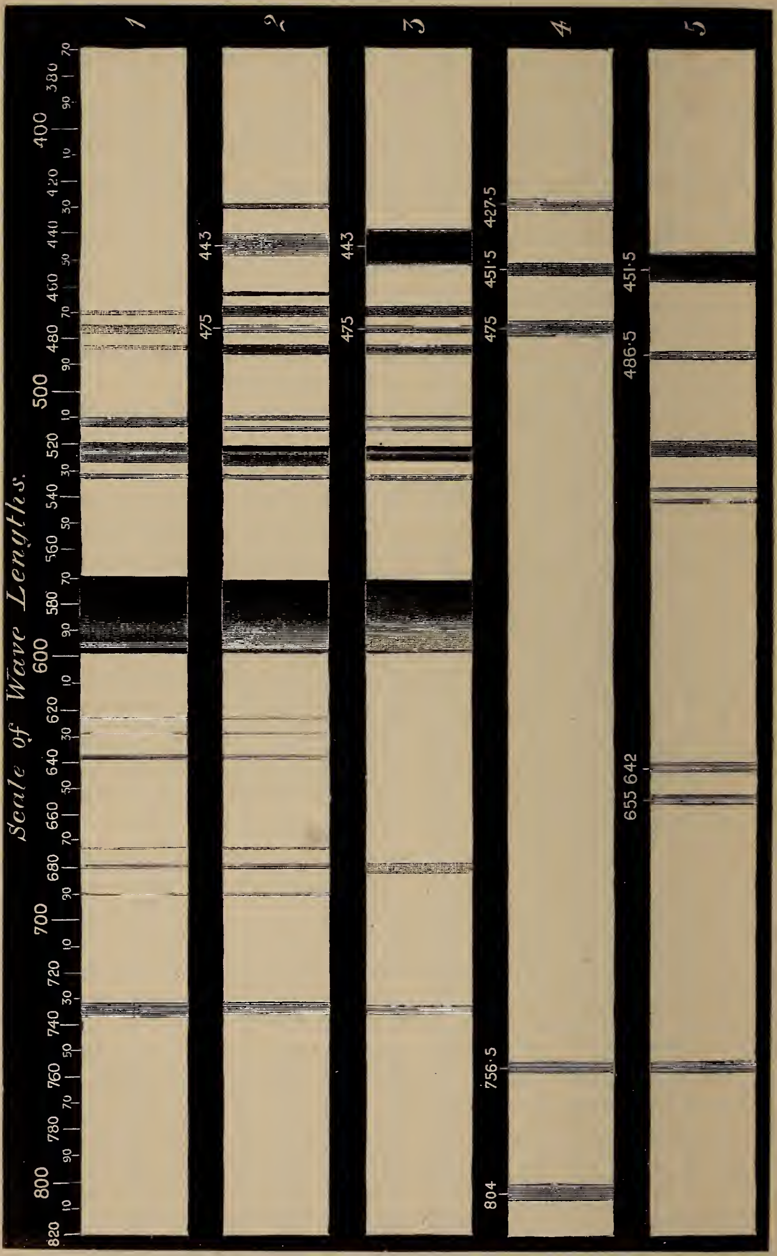
* "Proc. Roy. Soc.," vol. 40 (1886), p. 236.

baudran has obtained, among others, two bands (λ 573 and λ 543·2), which he considers to be caused by two elements, named respectively $Z\alpha$ and $Z\beta$, and which he considers new, at all events if we except terbium, and possibly the elements of what was formerly called holmium. His method fails to show any spectrum in solutions of yttria which by my method give the yttria bands with the greatest brilliancy; while conversely his method shows a fluorescent spectrum in solutions of earths separated as widely as possible from yttria, chemically as well as spectroscopically. My experiments on both these methods tend to the conclusion that our bands are not due to the same cause, although M. de Boisbaudran's experiments have led him to the opposite conclusion. The band of $Z\beta$ (543) falls between the double green band of gadolinium (549 and 541), and the band of $Z\alpha$ (573) would come very near the citron line (574).

A hitherto unrecognised band in the spectrum by absorption or phosphorescence is not of itself definite proof of a new element, but if it is supported by chemical facts such as I have brought forward there is sufficient *prima facie* evidence that a new element is present. Until, however, the new earths are separated in sufficient purity to enable their atomic weights to be approximately determined, and their chemical and physical properties observed, I think it is more prudent to regard them as elements on probation. I should therefore prefer to designate them provisionally by the mean wave-length of the dominant band. In this I am following the plan adopted by astronomers in naming the minor planets, which are known by a number encircled by a line. If, however, for the sake of easier discussion among chemists a definite name is thought more convenient I will follow the plan frequently adopted in such cases, and provisionally name these bodies as shown in the following table :—

Position of lines in the spectrum.	Scale of spectroscope.	Mean wave-length of band or line.	$\frac{1}{\lambda^2}$	Provisional name.	Probability.
Absorption-bands in violet and blue ..	8·270°	443	5096	$D\alpha$	New.
	8·828	475	4432	$S\beta$	New.
Bright lines in—					
Violet............	8·515	456	4809	$S\gamma$	Ytterbium.
Deep blue.........	8·931	482	4304	$G\alpha$	New.
Greenish-blue (mean of a close pair) ..	9·650	545	3367	$G\beta$	Gadolinium, or $Z\beta$.
Green	9·812	564	3144	$G\gamma$	New.
Citron	9·890	574	3035	$G\delta$	New, or $Z\alpha$.
Yellow............	10·050	597	2806	$G\varepsilon$	New.
Orange	10·129	609	2693	$S\delta$	New.
Red..............	10·185	619	2611	$G\zeta$	New.
Deep red..........	10·338	647	2389	$G\eta$	New.


Scale of Wave Lengths.
70
380
90
400
10
420
30
440
50
460
70
480
90
500
10
520
30
540
50
560
70
580
90
600
10
620
30
640
50
660
70
680
90
700
10
720
30
740
50
760
70
780
90
800
10
820
1
2
3
4
5
443
475
443
475
427·5
451·5
475
756·5
804
451·5
486·5
655 642

The initial letters D, S, and G recall the origin of the earths respectively from Didymium, Samarskite, and Gadolinite.

The radiant-matter test applied to these phosphorescing bodies proves itself to be every day more and more valuable, and one of the most far-searching and trustworthy tools ever placed in the hands of the experimental chemist. It is an exquisitely delicate test capable of being applied to bodies which have been approximately separated, but not yet completely isolated, by chemical means ; its delicacy is unsurpassed even in the region of spectrum analysis; its economy is great, inasmuch as the test involves no destruction of the specimen, and its convenience is such that any given specimen is always available for future reference. Likewise the quantity of material is limited solely by the power of the human eye to see the body under examination. Beyond all these excellencies is its trustworthiness. I should be exceeding the legitimate inference from experience were I to claim that this test is infallible; but this I may say—during the five years in which this test has been in daily use in my laboratory, I never once have been led to view its indications with suspicion. Anomalies and apparent contradictions have cropped up in plenty; but a little more experiment has always shown that the anomalies were but finger-posts pointing to fresh paths of discovery, and the contradictions were due to my own erroneous interpretation of the facts before me.

DESCRIPTION OF THE FIGURES.

Fig. 1.—Absorption-spectrum of *Didymium*, showing the absence of the element forming the band λ 443.

Fig. 2.—Absorption-spectrum hitherto ascribed to *Didymium*.

Fig. 3.—Absorption-spectrum of *Didymium* showing the concentration of the element forming the band λ 443.

Fig. 4.—Absorption-spectrum of *Dysprosium* according to M. de Boisbaudran.

Fig. 5.—Absorption-spectrum showing the isolation of the band λ451·5, included by M. de Boisbaudran in the spectrum of *Dysprosium*.

X. "The Distribution of Micro-organisms in Air." By Percy F. Frankland, Ph.D., B.Sc., F.C.S., F.I.C., Assoc. Roy. Sch. Mines. Communicated by E. Frankland, F.R.S. Received June 7, 1886.

The micro-organisms in air have formed the subject of investigations by Pasteur, Tyndall, Miquel, and many others. The researches of these experimenters have shown that although these organisms are most widely distributed throughout the accessible regions of the atmosphere, yet that very marked differences do exist in the numbers which are present in different places, and in the same places at different times.

The majority of these experiments were made before the methods of cultivating micro-organisms on solid nutritive media had been developed and perfected. These methods of cultivation have proved of such inestimable advantage in the study of micro-organisms in all its branches, that it is not surprising that they should have also been applied to a reinvestigation of the micro-organisms of the air. These methods are, moreover, particularly fitted for this purpose, inasmuch as they simultaneously supply information as to the number of micro-organisms present, as well as furnishing these micro-organisms in a state of pure cultivation for purposes of further study.

A method of adapting the solid culture-media to the bacterioscopic examination of air has been devised by Hesse ("Mittheilungen Kaiserl. Gesundheitsamte," Berlin, 1884), who has conducted a number of experiments showing that the results obtained by this method can lay claim to a fair degree of quantitative accuracy, and since the gelatine-peptone of Koch is used for the cultivation of the organisms obtained, the range of organisms which are capable of being discovered in this way is very considerable indeed.

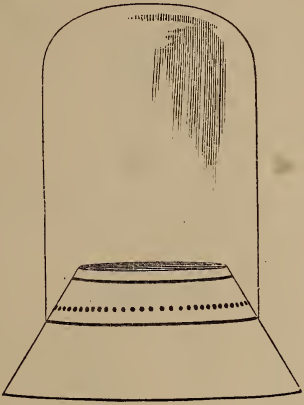
Wishing to conduct some experiments on the relative abundance of micro-organisms in the air of various places and at different altitudes, I selected, after careful consideration and preliminary trial, the method of Hesse for their execution, and have adopted the various precautions which he has recommended so as to render my results as comparable as possible with those which he has obtained in his investigation on the air of Berlin.

Description of Apparatus employed.

This consists essentially of a glass tube about 2 feet 6 inches in length and 1½ to 2 inches in diameter, coated on its internal surface with the nutritive gelatine medium. One extremity of this tube is fitted with a perforated aperture 0·5 inch in diameter, the other extremity being provided with a tightly fitting india-rubber cork, through which passes a short glass tube plugged with cotton-wool.

In preparing the tube for use, the perforated cap mentioned above is covered with a second non-perforated one, which is tightly wired on so as to be watertight. The empty tube with its caps and cork are first sterilised by placing them in the steamer for several hours, the cotton-wool plug employed in the tube which passes through the cork having been previously sterilised by heating in an air-bath until it is browned. The cork is now removed, and about 50 c.c. of the melted peptone-gelatine are poured into the tube. The cork is replaced, and the whole tube with its contents is then steamed for fifteen minutes on three successive days. In this sterilisation the tube is placed, with the capped end downwards, in the ordinary steamer, the lid of which is replaced by a truncated conical shade, through which several of these tubes can protrude, their ends being then covered by a glass shade externally coated with cotton-wool or any other non-conducting material. By contriving this simple arrangement I was able to keep every portion of the tubes in an atmosphere of steam, whilst no condensed water could find its way into the tubes.

FIG. 1.



I have found it very necessary not to overdo the steaming, as the melting point of the gelatine is considerably reduced by its being prolonged, and in these experiments it is of the greatest importance that the gelatine should be capable of resisting the temperatures which are incidental to the experiments, and which are encountered in travelling with the apparatus. If the gelatine be too sensitive to heat many experiments may be entirely lost, and the time and labour spent upon their careful execution wasted. I would therefore recommend that both in the preparation and in the sterilisation of the gelatine-peptone used in these tubes, the steaming should be reduced to a minimum consistent with sterility.

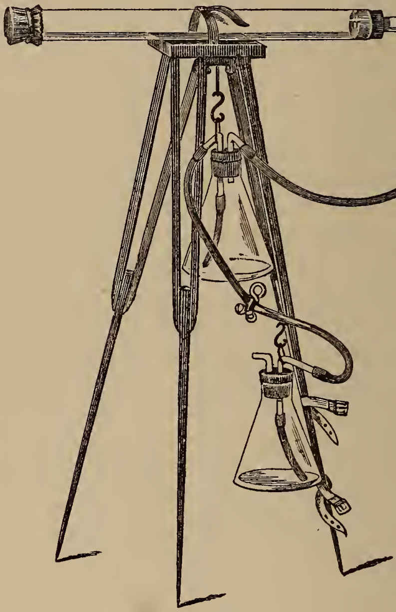
When finally sterilised, and whilst the gelatine is still fluid, the tube is held horizontally under a cold-water tap, being simultaneously rotated and moved backwards and forwards, so that uniform cooling is effected. As the gelatine approaches its solidifying point it becomes more and more viscid, and gradually adheres to the surface of the tube, so that with a little management the whole interior can be uniformly coated. In this operation contact between the gelatine and the cotton-wool plug in the small tube at the end must be carefully avoided, otherwise when cold the tube may become blocked up.

The tube after being preserved for some time to insure sterility, is then fit for use.

Method of Experiment.

Tubes prepared as above are easily transportable in cylindrical cardboard boxes, the exterior of which should be coated white to prevent heating if they have to be exposed to sunshine. The tube is strapped to a small horizontal table supported by an ordinary portable camera stand, and the end of the small tube passing through the cork is connected by flexible tubing with an aspirator, which, so as to render it as portable as possible, consists of two bottles or strong flasks, each of rather more than 1 litre capacity, arranged as in the figure to form a reversible syphon. A measured litre of water is poured into one of these flasks, and by syphoning this into the second, a litre of air is made to pass through the experimental tube. The rate of flow is regulated by a screw clamp, and by alternately connecting the end of the tube with the two syphon flasks any desired volume of air may be drawn through the apparatus.

Fig. 2.



The experiments of Hesse show that the rate of aspiration should not exceed 1 litre in two or three minutes, and that when this precaution is observed the organisms present in the air are almost wholly deposited in the first two-thirds of the tube, the remaining third being either wholly free or practically so. In my experiments I have almost uniformly restricted the rate to the 1 litre in three minutes, and can fully confirm the fact of the remarkably complete deposition of the organisms in the front part of the tube, as well as of their being almost uniformly found on the bottom. In conducting experiments in the open air, I have made, with few exceptions, the invariable practice of directing the aperture of the tube at an angle of about 135° to the direction of the wind, so as to avoid currents of air penetrating into the tube irrespectively of the action of the aspirator.

In commencing the experiment the outer unperforated india-rubber cap is removed and carefully folded up, so that its inner surface is not exposed to the air, and as soon as the experiment is over it is carefully replaced and wired on. The tube is kept in a chamber at 20—25° C. for incubation in the position in which it was used, and in the course of a few days the organisms which have been arrested by the gelatine are readily distinguishable by the colonies, visible to the naked eye or by means of a low power, to which they give rise. Unless the number of organisms which have fallen on the gelatine is very great, each colony will consist of a pure cultivation of a particular organism, and can be further examined as desired.

From the number of colonies found in a given volume of air the number in any standard volume, say 100 litres, can be calculated.

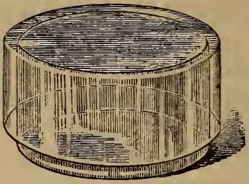
Examination of Air by Gelatine-surface Exposure.

In addition to the tube experiments above described, I have considered it advisable to make simultaneous tests by exposing for a definite period of time a surface of nutrient gelatine of definite area as originally recommended by Koch ("Mittheilungen Kaiserl. Gesundheitsamte," Berlin, 1881), as in this manner an idea is obtained, not of the number of organisms contained in a given *volume* of air, but of what for many purposes is more important, of the number falling on a given surface in a definite time.

For this purpose small circular glass dishes rather less than 1 inch in height and about 3 inches in diameter, and provided with a glass cover fitting loosely and overlapping like the lid of a pill-box, were filled to a depth of about one-third of an inch with nutrient gelatine and sterilised for fifteen minutes on three successive days in the steamer. As long as the covers are on, the gelatine in these dishes remains sterile for practically an indefinite length of time, and can be transported without danger in a tin box.

In using these for experiment, the lid is removed and placed with its mouth downwards on a clean surface, and then after the desired exposure replaced on the dish.

Fig. 3.



General Scope of the Experiments.

A number of experiments have been made on the roof of the Science Schools, South Kensington Museum, with the view of ascertaining the influence of season and atmospheric conditions generally upon the abundance of micro-organisms. This position is well fitted for observations of the kind, the roof being about 50 or 60 feet above the surface of the ground, and thus removed from local and accidental influences.

A series of experiments was carried out with a view of ascertaining the relative abundance of micro-organisms at different altitudes in towns. These comparisons were effected by collecting samples of air at different elevations on the spire of Norwich Cathedral, on the dome of St. Paul's in London, and on Primrose Hill. Comparative experiments have also been made in the country, as well as in buildings such as museums, hospitals, &c.

The results obtained in the tube experiments are calculated to the number of micro-organisms contained in 10 litres of air, whilst the results yielded by the exposure of the gelatine-dishes are stated so as to represent the number of micro-organisms falling on 1 square foot in one minute. The conditions under which the experiments were performed are also recorded.

Table I.—Roof of Science Schools, South Kensington Museum.

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
From N.E. window of room on top floor but one. 21st January, 1886.	Wind N.N.E., moderate. Snow on ground and falling slightly during first part of experiment. Temp. 3·5° C. (noon).	10 in 25 litres	4	—
Exhibition Road, at bottom of Science Schools. 28th January, 1886.	Thick white fog lifting. Wind S.W. by W, very moderate. Road wet, pavement drying. The dishes were placed on balustrade 5 ft. above pavement. Temp. 6° C. 11 A.M.	......	......	30
Ditto	Ditto.	......	......	32
Roof of Science Schools. 28th Jan., 1886.	Conditions identical. Dishes exposed on western (windward) parapet. Temp. 5·5° C. 11 A.M.	......	......	26
Ditto	Ditto.	......	......	33
Roof of Science Schools. 9th March, 1886.	Cold S.E. wind, strong, had continued for several days previously; everything dry, and roads unwatered; sun shining. Temp. (shade) 3·5° to 5° C. Dishes exposed on E (windward) parapet in full sweep of wind. 3 to 4.30 P.M.	......	......	433
Ditto	Ditto.	......	......	414
Ditto	The tube was not pointed quite sufficiently away from the wind; the result is therefore, probably, somewhat too high.	135 in 12 litres.	113	—

Table I.—*continued.*

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Roof of Science Schools, 16th March, 1886.	Wind N.W., moderate, ground dry and frosty. Tube pointed S.W. 11.50 A.M. to 12.30 P.M. Temp. 3° C.	13 in 10 litres	13	—
Ditto	Four dishes were simultaneously exposed on W. (windward) parapet.	......	......	101 109 83 101
Roof of Science Schools, 31st March, 1886.	Wind S.W. by W., intermittent and variable. Sun shining greater part of time; ground wet. Wind increased considerably at 1.45 P.M. Temp. 12·5° to 11° C. } P.M. 1 to 1.10. 1.20 to 1.35. 1.38 to 1.58. 1.40 to 1.50.	......	......	851 215 803 1,302
Ditto	....	42 in 15 litres	28	—
Ditto	Heavy shower since previous experiment. Wind S.W., strong, falling later. Temp. 8° to 9° C. } 3.54 P.M. to 4.45 ,,	......	......	516 429 375 372
Ditto	Ditto.	61 in 16 litres	38	—
Roof of Science Schools, 12th May, 1886.	Continuous rain previous day until morning of experiment; ground and surroundings thoroughly wet. Wind E., fairly strong. Tube pointed N.W. 4.12 P.M. Temp. 12° C.	......	......	66 60 61

Table I.—*continued.*

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Roof of Science Schools, 12th May, 1886.	Continuous rain previous day until morning of experiment ; ground and surroundings thoroughly wet. Wind E., fairly strong. Tube pointed N.W. 4.12 P.M. Temp. 12° C.	29 in 12 litres	24	—
Roof of Science Schools, 13th May, 1886.	Intermittent drizzling rain during morning ; very heavy rain previous night. Wind S.W., not quite so strong as on previous day. 5.25 to 6.30 P.M. Temp. 12° C.	37 in 12 litres	31	—
Ditto	Ditto.	......	......	88
Roof of Science Schools, 22nd May, 1886.	After thunderstorm (3 to 4 A.M.) and abundant rain. Wind fairly strong and irregular in direction, N.E. to S.E. 10.45 A.M. Temp. 16·5° C.	48 in 12 litres	40	—
Ditto	Ditto.	......	......	71
Roof of Science Schools, 25th May, 1886.	Wind S.W., moderate. Pavement dry, but heavy rain all previous day and part of night. 6.10 P.M. Temp. 13° C.	32 in 12 litres	27	—
Ditto	Ditto.	......	......	130

These figures show that in cold weather, especially when the ground is covered with snow, the number of organisms in the air is very much reduced and presents a very striking contrast to the number found in the warmer weather, even immediately after much rain.

The experiments made on the 9th of March show that during cold and dry weather with a strong east wind blowing over London, a large number of micro-organisms may still be present in the air; for although the tube-experiment made on that day cannot be regarded as satisfactory, yet from the number falling on the square foot it is evident that they must have been very abundant, certainly very much more abundant than on the 16th of March, when with much the same temperature the wind was more moderate and blowing from the north-west.

It is particularly noticeable that even after such exceedingly heavy rain as was experienced on the morning of the 22nd of May, within a few hours afterwards the number of micro-organisms in the air should be as abundant as usual.

It will be seen on comparing the number in the above table that, with the exception of the experiment made on January 21st, when snow was on the ground and actually falling at the time, the number of micro-organisms present in the air collected on the roof of the Science Schools, never fell so low as in some of the experiments to be presently mentioned, which were made in the country.

Taking the average of the experiments recorded above, it will be seen that the mean number of organisms found in 10 litres of air amounted to 35, whilst an average of 279 fell on 1 square foot in one minute.

Table II.—Experiments in Country Places.

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Edge of Reigate Hill, about 700 ft. 7th February, 1886.	Wind E., in gusts, but not very strong; ground partially hard with frost and partially moist; occasional sunshine. Noon. Temp. 1° C.	2 in 10 litres	2	15
Garden on Reigate Hill, 200 ft. lower. 7th February, 1886.	Similar, but less wind. Dish exposed on grass lawn. 1.30 P.M. Temp. 5° C.	......	......	30
Lawn in garden on Reigate Hill. 23rd May, 1886.	Wind S.W., very gentle. 6 to 7 P.M. Temp. 16° C.	30 in 12 litres	25	31
Edge of Chalk Down, Reigate, about 700 ft. high. 23rd May, 1886.	Mist; wind S.E., strong to moderate; ground wet (grass); air very moist. Dish exposed 3 ft. from ground. Temp. 13° C. Noon.	16 in 12 litres	13	48
Mousehold Heath, Norwich. 23rd April, 1886.	Wind E., moderate, blowing across country. Heath covered with dry heather. Noon; sunshine. Temp. 13° C. Dish placed on stand 3 ft. 6 in. above ground.	7 in 10 litres	7	18
Garden, near Norwich. 23rd April, 1886.	Wind E., blowing across Norwich, less strong than in morning; sunshine. Temp. 12° C. 4 to 5 P.M. Grass.	......	......	252
Garden, near Norwich. 25th April, 1886.	Wind N.E., slight. Temp. 4° C. 6 P.M.	......	......	100
Mousehold Heath, Norwich. 27th April, 1886.	Wind E.; conditions much as in previous experiment. 11 to 12 A.M.; sunshine.	5 in 10 litres	5	16
Garden, near Norwich. 28th April, 1886.	Wind E., slight; sunshine. Temp. 12° C. Noon.	31 in 10 litres	31	386

In the table (p. 519), the results of the experiments made near Reigate and in the vicinity of Norwich are recorded.

These figures present a very marked contrast to those contained in the previous table; thus the average number of organisms found in the country experiments amounts to only 14 in 10 litres, whilst an average of 79 fell on 1 square foot in one minute.

Particularly noticeable is the great relative freedom from micro-organisms of the air collected on the heath near Norwich during the comparatively warm weather of April last when the ground was dry. In the experiments made both at Norwich and Reigate it will be observed that the air in gardens was richer in micro-organisms than that of the open country, although in the Reigate experiments of May 23rd there were more on the dish exposed on the hill than in the garden, but this is easily accounted for by the fact that the wind on the hill was very much stronger than in the garden, the number found in a given volume being very much less in the case of the air on the hill.

In the table (p. 521) are the results of the examination of the air in Kensington Gardens, Hyde Park, and Primrose Hill.

From these figures it will be seen that on the whole the number of organisms found in the air of these open spaces is less than in that collected on the roof at South Kensington, but greater than in the experiments made in the country, although the number found in the second experiment in Kensington Gardens is scarcely in excess of anything found in the country places.

The average number of micro-organisms found in 10 litres amounts to 24, whilst an average of 85 fell on 1 square foot in one minute.

Table III.—Experiments in Open Places in London.

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Kensington Gardens, near Round Pond. 1st April, 1886.	East side of Round Pond. Wind S.W. by W., fairly strong, blowing across grass; grass and ground damp; streets dry; sunshine. 12 to 2 P.M. Temp. 12° C.	32 in 15 litres	21	88
Ditto	Conditions similar, but wind much less strong. 2.15 to 3.30 P.M. Temp. 11° C.	7 in 15 litres	5	74
Hyde Park. 18th May, 1886.	Grass in hollow. Wind S.W., fairly strong. 5 P.M. Temp. 16° C. to 14·5° C.	57 in 12 litres	48	207
Ditto	Similar. Wind considerably less. 6 P.M. Temp. 14° C.	45 in 12 litres	38	74
Primrose Hill, top. 19th May, 1886.	Ground very wet; grass. Wind S.E., fairly strong. 2 P.M. Temp. 13·5° C.	11 in 12 litres	9	12
Primrose Hill, bottom. 19th May, 1886.	Similar. Wind not so strong as in above. 3 P.M. Temp. 13·5° C.	29 in 12 litres	24	57

Table IV.—Experiments at Different Altitudes.

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Cathedral Spire, Norwich. 26th April, 1886.	Height about 300 ft. Wind E., fairly strong. Tube projecting from S.E. window at top of spire. 5 P.M.	7 in 10 litres.	7	49
Cathedral Tower, Norwich. 26th April, 1886.	Wind E., fairly strong. Tube projecting from northern battlements; sunshine. Temp. 8° C. 4 P.M. Height, 180 ft.	9 in 10 litres.	9	107
Cathedral Close, Norwich. 26th April, 1886.	Wind E., moderate. Gravel space in front of south transept of Cathedral. Ground dry; sunshine. Temp. 9° C. 12 to 1 P.M.	18 in 10 litres.	18	354
Golden Gallery, St. Paul's. 26th May, 1886.	Wind S.W., strong; streets dry. Day fine, with exception of few drops of rain during experiment. 2 P.M. Temp. 12° C.	13 in 12 litres.	11	115
Stone Gallery, St. Paul's. 26th May, 1886.	Wind as above; sunshine. 3.20 P.M. Temp. 13° C.	41 in 12 litres.	34	125
St. Paul's Churchyard, 26th May, 1886.	Pavement in gardens at foot of St. Paul's. 5 P.M. Temp. 12·5° C.	84 in 12 litres.	70	188
Golden Gallery, St. Paul's. 29th May, 1886.	Wind N.W., moderate; hazy. Slight rain during morning. Pavement and road wet at beginning of experiment; slight sunshine. 12.30 P.M. Wind changed completely in direction during experiment to S.W.; blew down aperture of tube. Temp. rose from 11° to 12·5° C.	58 per 10 litres.	58	113
Stone Gallery, St. Paul's. 29th May, 1886.	Wind S.W., strong. 2 P.M. Temp. 13° C.	23 in 10 litres.	23	226
Churchyard, St. Paul's. 29th May, 1886.	Sunshine. 3 P.M. Temp. 13·5° to 14° C.	41 in 10 litres.	41	341

At the suggestion of Mr. Edwin Chadwick, C.B., I was induced to make a comparison between the air at different altitudes in towns. The results obtained are recorded in the above table.

These figures are particularly instructive, as illustrating the fact that with increasing altitude the air becomes poorer in micro-organisms. The tube experiment on the Golden Gallery at St. Paul's on the 29th May must be disregarded, as during the progress of the experiment the direction of the wind changed completely and vitiated the result by blowing down the open extremity of the tube, thereby increasing the number of organisms deposited beyond what was actually due to the volume of air aspirated. The exposed dish, on the other hand, would not be affected in the same way, and it will be seen that, as regards the number of organisms falling on the square foot, the result on the Golden Gallery carries on the diminution noticed on the Stone Gallery.

The differences noticed in the case of the St. Paul's experiments may perhaps be most vividly appreciated by comparing the averages of the two experiments with some of the averages already quoted for other places. Thus at the base of St. Paul's we find an average of 56 organisms in 10 litres, which is considerably greater than the average for the South Kensington experiments, in which only 35 were found in the same volume; on the Stone Gallery there were 29 in 10 litres, whilst in the Golden Gallery in the successful experiment the number only amounted to 11 in 10 litres, thus closely resembling the average number found in the experiments made in country places, which was 14 in that volume.

Table V.—Indoor Experiments.

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Drawing room before children's dance. 6th February, 1886.	Room almost empty. Dishes exposed 4 ft. above the ground.	......	......	44 50
Ditto during dancing....	About 20 children dancing, otherwise conditions as above.	......	......	400 333
Railway carriage. 28th April, 1886.	Four passengers; one window quite open, one closed. Dish exposed by open window 3 ft. above ground.	......	......	395
Ditto	Ten passengers; one window open 4 inches; one window closed. Dish exposed by closed window.	......	......	3,120
Norwich Union Life Office. 24th April, 1886.	Room 45 ft. × 27 ft. × 15 ft. high. No windows open, but ventilated by lateral tube. About 10 clerks walking about. Dish exposed about 4 ft. above ground.	......	......	78
S. Kensington Museum. Central Hall. Morning. 14th May, 1886 (Friday).	Italian Court, centre. Very few visitors. Temp. 17° C. Dish exposed 3 ft. 6 in. above ground.	20 in 11 litres.	18	20
S. Kensington Museum. Morning. 15th May, 1886 (Saturday).	Same place. Considerably more visitors. Noon. Temp. 17° C.	97 in 12 litres.	81	76
S. Kensington Museum. Afternoon. 15th May, 1886 (Saturday).	Ditto, 4 to 5 P.M. Temp. 17·5° C.............	78 in 12 litres.	65	97

Table V—*continued.*

Place.	Conditions of experiment.	No. of colonies found.	Total number found in 10 litres (calculated).	Total number falling per sq. ft. per minute (calculated).
Natural History Museum. 17th May, 1886. (Monday morning.)	Centre of entrance hall. Noon. Temp. 16·5° C. Dish exposed on pillar at foot of main staircase. Considerable number of visitors passing. Entrance door closed.	Tube lost.	......	30
Natural History Museum. 17th May, 1886. Afternoon.	Ditto. 3.40 P.M. Temp. 17° C. More visitors than in morning.	Tube lost.	......	293
Natural History Museum. 21st May, 1886. Morning.	Same place. One entrance door open, causing perceptible draught of air. Number of visitors few. Noon. Temp. 18·5° C.	60 in 12 litres.	50	136
Natural History Museum. 21st May, 1886. Afternoon.	Ditto. Entrance door less open. Number of visitors much greater. 2.25 P.M. Temp. 18·5° C.	84 in 12 litres.	70	255
Hospital for Consumption Brompton. 27th May, 1886.	"Richmond" ward; 8 beds; 8 persons in room. 1 P.M. Temp. 15° C. Windows closed. Lateral ventilation, and door open on to corridor. Dish exposed on chair placed on bed.	43 in 10 litres.	43	11
Ditto	Ditto. 8 persons in room; some moving about. 4.20 P.M. Temp. 16° C. Dish similarly exposed.	130 in 10 litres.	130	130
Ditto	Ditto. 8 persons in bed, 9 P.M. Temp. 16·5° C. Dish exposed on chair on ground.	42 in 10 litres.	42	44
Chemical Laboratory, Science Schools, South Kensington Museum. 13th January, 1886.	Windows shut, door open; 2 persons in room. Noon. Temp. 17° C. Wind and snow outside.	26 in 20 litres.	13	—

These experiments show that in enclosed spaces, when there is little or no aërial commotion, the number of suspended organisms is very moderate, but as soon as any atmospheric disturbance is occasioned either by draughts or by the moving about of people, the number rapidly rises, and may become very large indeed. This is a fact very familiar to all who have had much experience in the cultivation of micro-organisms, and it is of particular importance in connexion with the process of plate cultivation with gelatine.

The experiments made in the railway carriage afford a very striking example of the enormous number of micro-organisms which become suspended in the air when a large number of persons are crowded together.

In my own laboratory, where every care is taken to prevent the circulation of dust, the number of organisms did not amount to more than 13 in 10 litres, and of the comparative purity of this air I have had the most abundant evidence in the almost inappreciable amount of aërial contamination which is exhibited by the plate cultivations prepared there.

In conclusion, I have to express my thanks to the Deans of St. Paul's and Norwich, General Festing, Professor Flower, Dr. Theodore Williams, and Mr. F. E. Colenso, through whose courtesy I have been enabled to carry out many of the experiments recorded in the above paper.

I must also acknowledge the valuable help which I have received from my assistant, Mr. Hart, A.R.S.M., who is now proceeding with these investigations, the results of which we shall hope to have the honour of communicating later.

XI. "On the Multiplication of Micro-organisms." By Percy F. Frankland, Ph.D., B.Sc., F.C.S., F.I.C., Assoc. Roy. Sch. Mines. Communicated by E. Frankland, F.R.S. Received June 8, 1886.

In a previous communication "On the Removal of Micro-organisms from Water" ("Proc. Roy. Soc.," vol. 38 (1885), p. 379), I had occasion to point out the extraordinary rapidity with which micro-organisms may become multiplied even in ordinary distilled water. It was there shown that if a few drops of diluted urine-water be added to ordinary distilled water and kept in a sterilised bottle plugged with sterilised cotton-wool, the number of micro-organisms remaining suspended in the water became multiplied in the following manner:—

Number of hours.	Number of micro-organisms obtained from 1 c.c. of water.
0	1,073
6	6,028
24	7,262
48	48,100

These numbers represent the number of micro-organisms found in the upper layers of the water without agitating the contents of the bottle. More recently this subject has been touched upon by Dr. T. Leone ("Gazzetta Chimica Italiana," xv, 385), who found that the micro-organisms present in the water supplied to the city of Munich became multiplied in the course of five days from 5 per cubic centimetre to upwards of half a million in the same volume of water.

As this subject is one which is of much importance, from many points of view, I have carried out a number of experiments with the object of throwing some light upon these phenomena.

Methods of Experiment.

The number of micro-organisms present in the liquids employed was determined by means of Koch's method of plate cultivation with gelatine peptone. The apparatus used was substantially the same as that described in my previous paper (*loc. cit.*), a few modifications of the process having, however, been since then introduced. The sterile plates are now placed upon a levelled glass slab, resting upon a dish containing iced water, and covered with a glass shade, the latter being momentarily raised whilst the inoculated and fluid gelatine is poured upon the plate. With this arrangement the solidification of the gelatine is almost instantaneous, and the plate can be at once transferred to the moist chamber for incubation, thus enabling the preparation of a very much larger number of plates in a given time than was formerly possible, when the rate of solidification of the gelatine was dependent upon the temperature of the room. Instead of a solution of mercuric chloride in the moist chamber, I now simply employ sterilised distilled water, as I have found that the presence of the mercuric chloride in the moist chamber may exercise a prejudicial effect upon the full development of the colonies on the plate. When these modifications of the process are carried out with care in a room in which the distribution of dust is scrupulously avoided, the aërial contamination is altogether insignificant, but the process should in all cases be checked by pouring blank plates, in order to ascertain whether the precautions employed have been adequate or not. When, however, sufficient experience has been acquired in the management of the process, the preparation of these blank plates becomes rather a matter of form than of any practical importance, nevertheless when mixtures of organisms and not pure cultivations are being dealt with, it is a check which should on no account be neglected.

Scope of the Experiments.

The bacteriological examination by plate cultivation of a large number of natural waters showed that practically all waters contain micro-organisms in greater or less abundance, but that the number found in such natural waters varies within exceedingly wide limits. Thus whilst the number found in the rivers Thames and Lea usually amounts to thousands in the cubic centimetre, that found in certain deep well-waters, which I have had under continual observation, rarely exceeds ten in the same volume. It appeared to me, therefore, of interest to study the further history of the organisms present in these waters of different origin and character.

In the second part of the paper will be found a description of a number of experiments made upon the power of multiplication possessed by certain pathogenic organisms, when placed under similar conditions.

I. *Experiments with Micro-organisms present in Natural Waters.*

The waters which were made the subject of study are those supplied by the eight Metropolitan Companies to London, and which for various reasons are well adapted for purposes of experiment. Firstly, they include waters derived from various sources, viz., from two different rivers—the Thames and Lea—and from deep wells in the chalk; secondly, their chemical composition is well known, and is made the subject of a very complete monthly investigation; and thirdly, their biological character has now been under periodical observation for eighteen months past.

Micro-organisms in Crude River-water.

Samples of unfiltered river-water (temp. 2° C.) collected in sterile stoppered bottles were submitted to plate cultivation the day after collection, whilst duplicate samples were allowed to stand at a temperature of about 20° C. for five days, and then similarly examined. The following results were obtained:—

Description.	Temperature.	Colonies found in 1 c.c.
River Thames at Hampton Ferry, 15th January, 1886	2° C.	—
Examined 16th January, 1886	..	45,392
Examined 20th January, 1886	20°	35,790
River Lea at Chingford, 15th January, 1886	3°	—
Examined 16th January, 1886	..	39,307
Examined 20th January, 1886	20°	63,488

Number of Colonies obtained from 1 c.c.

Description.	Feb. 19/86.	Feb. 20/86.	Feb. 20/86.	Feb. 27/86.	Feb. 27/86.	Feb. 27/86.	Mar. 8/86.	Feb. 27/86.	Mar. 8/86.
	Day of collection.	Standing through a frosty night.	Standing a night in the incubator, 35° C.	Standing 7 days in diffused light at 20° C.	Standing 7 days in darkness at 20° C.	Standing 7 days in flask plugged with cotton wool, in diffused light at 20° C.	Standing 16 days ditto (plugged flask), at 20° C.	Standing 8 days in incubator at 35° C.	Standing 16 days in incubator at 35° C.
Thames at Hampton, collected Feb. 19, 1886	15,800	18,155	665,280	..	..	..	..	{ 4,062 3,170 }	15,460
Lea at Chingford....	20,600	15,646	..	8,017	17,555	14,161	15,897	—	—

The above figures show that the organisms present in the crude river-waters undergo no material alteration in number when preserved in frosty weather for a period of twenty-four hours, but that when kept during the same period of time in the incubator (temp. 35° C.), the multiplication which takes place is altogether enormous. In those samples which were kept at the ordinary temperature of the air there is no instance of multiplication, but, on the contrary, there is on the whole a slight decrease, and in one case a considerable reduction; but by far the largest reduction was found in the samples which had been incubated for eight days, the rapid increase in numbers which at first takes place on exposure to the temperature of the incubator being apparently followed by a correspondingly rapid decline.

Thus in the one case there is a slight diminution in the number, and in the other case increase; but in neither is the alteration very considerable. Similar samples collected on the 19th February, 1886, were examined after exposure to various conditions, as specified on p. 528.

Further experiments were made with a view of testing this behaviour of the micro-organisms present in river-water. The results obtained were as follows:—

Number of Colonies obtained from 1 c.c.

Description.	April 13/86. On day of collection, temp. 8° C.	April 15/86. Standing in dark at 20° C. for 2 days. (Same bottle.)	April 17/86. Standing in dark at 20° C. for 4 days. (Same bottle.)
River Thames at Hampton	12,250	4,386	2,018
River Lea at Chingford...	7,300	2,148	1,286

Thus in both cases there was a marked reduction in the micro-organisms after storing for two and four days respectively.

From the experiments made with these river-waters, it would appear that there is a decided tendency for the micro-organisms which they contain to become reduced in number when the waters are kept at the ordinary temperature of the air (20° C.), whilst the numbers may be temporarily enormously increased by exposing them to an incubating temperature.

A very large number of experiments was also made with the filtered river-waters, as supplied to the metropolis. These waters have substantially the same chemical composition as the rivers from which they are derived, but the number of micro-organisms which they contain at the time of collection rarely exceeds on the average 5 per cent. of that present in the raw river-waters. The results obtained are recorded in the following tables:—

Number of Colonies obtained from 1 c.c.

Description.	Jan. 16/86. Day after collection, temp. about 3° C.	Jan. 20/86. Standing 5 days in dark at 20° C.	Jan. 23/86. Standing 7 days in refrigerator.	Feb. 12/86. Standing 27 days at 20° C. in dark.	Mar. 2/86. Standing 45 days at 20° C. in dark.
Filtered River Waters.					
Chelsea	159	lost	11,437	2,923	—
West Middlesex ..	180	lost	2,063	312	76
Southwark	2,270	5,285	8,311	1,323	—
Grand Junction ..	4,894	15,950	14,965	3,757	—
Lambeth	2,587	6,980	6,774	2,749	—
New River.......	363	lost	750	3,132	43
East London	224	lost	3,394	947	410
Average	1,525	9,405	6,813	2,163	176

Experiments were also made in order to ascertain the extent of multiplication taking place when samples of the filtered river-waters are exposed for twenty-four hours respectively in frosty weather, and in the incubator. The results obtained are comparable with those already recorded for the crude river-waters which were similarly exposed.

Number of Colonies obtained from 1 c.c.

Description.	Feb. 23/86. Day of collection.	Feb. 24/86. Standing through frosty night.	Feb. 24/86. Standing through night in incubator.
Filtered River Waters.			
Chelsea	305	305	62,483
West Middlesex	80	112	3,580
Southwark	284	360	1,954
Grand Junction	208	263	22,842
Lambeth...........	265	314	44,289
New River	74	107	8,892
East London	252	633	7,152
Average...........	210	299	21,599

Thus the micro-organisms in the filtered river-waters, on standing for twenty-four hours, even in cold weather, undergo a slight although distinct multiplication, thus bearing out the observations made with the same waters when kept in the refrigerator for a week. It is further seen that when allowed to stand at a temperature of 20° C., the multiplication which takes place in the course of a few days is generally very considerable indeed, whilst at the temperature of the incubator (35° C.) the multiplication, even in the course of a single night, is enormous. On the other hand, when the storage is continued for a sufficient length of time, the number of micro-organisms diminishes, and may become reduced below that which was present at the outset. It would appear, however, that the micro-organisms in these *filtered* waters become multiplied at 20° C. with far greater rapidity than those in the *unfiltered* waters, which, as already pointed out, have rather a tendency to become diminished, unless raised to the temperature of the incubator.

It now became a matter of interest to compare with the above the power of multiplication which is possessed by the micro-organisms found in deep well-waters. Similar experiments were, therefore, made with the waters obtained from one of the wells in the chalk at Deptford, belonging to the Kent Company.

Number of Colonies obtained from 1 c.c.

Description.	Jan. 27/86. Day after collection.	Jan. 30/86. Standing 3 days in refrigerator, temp. several degrees above 0°.	Jan. 30/86. Standing 3 days, at 20° C. (dark).	Feb. 12/86. Standing 16 days at 20° C. (dark).
Kent well...	96	163	178,379	51,843

On comparing these results with those previously referred to, it will be seen that whilst these organisms in the deep well-water have but little tendency to multiply in the cold, the multiplication at 20° C. is far in excess of anything observed in the case of the river-waters.

Experiments were also made to ascertain the influence of the temperature of the incubator upon the organisms in these well-waters.

Number of Colonies obtained from 1 c.c.

Description.	Feb. 23/86. Day of collection.	Feb. 24/86. Standing through frosty night.	Feb. 24/86. Standing 1 night in incubator, 36—39° C.
Kent well...........	5	18	743

Again, in another experiment, the following results were obtained:—

Number of Colonies obtained from 1 c.c.

Description.	Apr. 14/86. Day of collection.	Apr. 15/86. Standing 1 day, at 20° C.	Apr. 17/86. Standing 3 days, at 20° C.
Kent well...........	7	21	495,000

These tables show the enormous capacity for multiplication which is possessed by the micro-organisms present in this deep well-water. This is the more surprising, at first sight, when it is borne in mind that this water contains the merest trace of organic matter. It must, however, be remembered that this water is at the outset almost wholly free from micro-organisms, and that it has never before been inhabited by such living matters; it is only reasonable to infer, therefore, that those of its ingredients which are capable of nourishing the particular micro-organisms which flourish in it are wholly untouched, whilst in the case of the river-waters, the most available food supply must have been largely explored by the numerous generations of micro-organisms which have inhabited them. It should also be mentioned that the number of different varieties of micro-organisms is far greater in the case of the river-waters than in the deep well-water, as is at once evident to the naked eye on inspection of a plate cultivation, the deep well-water plates having generally the appearance of a pure culture; in the latter case, then, the organisms present will probably have a freer field for multiplication than in the presence of competitors, some of which may not improbably give rise to products which are hostile to others. This would also explain the greater capacity for multiplication which we find, as indicated above, in the filtered as compared with the unfiltered river-waters. By the process of filtration the number of different varieties of micro-organisms is largely reduced, as is at once seen by the inspection of the plate cultivations, and those varieties which remain have apparently a more favourable opportunity for reproduction than in the presence of more numerous varieties.

II. *The Multiplication of Pathogenic Micro-organisms.*

The remarkable phenomenon of multiplication which is exhibited by the micro-organisms found in most natural waters obviously leads us to a consideration of the behaviour of parasitic micro-organisms when accidentally introduced, as they frequently must be, into waters of different composition.

In studying the behaviour of parasitic micro-organisms under these conditions, I have selected three forms which are recognisable with particular facility, owing to the highly characteristic appearances to which they give rise when cultivated in the gelatine-peptone medium.

These forms are—

1. Bacillus pyocyaneus.
2. Finkler-Prior's Comma Spirillum.
3. Koch's Comma Spirillum.

The pure cultivations of these organisms were obtained directly, in the case of the first two mentioned, and indirectly, in the case of the third, from the laboratory of the Hygienic Institute of Berlin.

1. *Appearance of the Bacillus pyocyaneus in Gelatine-tube Cultivations.*

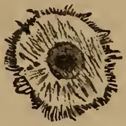
At 20° C., within twenty-four hours of inoculation, the path of the needle is indicated by incipient liquefaction at the surface, which as growth proceeds undergoes funnel-shaped depression, the fringe of which soon exhibits blue-green fluorescence. The diameter of the liquefied portion increases until the whole width of the tube is affected, the downward extension of liquefaction proceeding in the form of an inverted cone, in the apex of which a flesh-coloured precipitate is formed. The blue-green fluorescence is seen only on and near the surface. Ultimately the whole of the gelatine becomes liquid, and of a dirty green colour.

Appearance of the Bacillus pyocyaneus in Gelatine-plate Cultivations.

At 20° C. the colonies are generally distinctly visible on the second day as small opaque light green disks, the whole plate presenting a blue-green fluorescence when viewed by reflected light. The size of the colonies is dependent upon the number present on the plate, being exceedingly minute when the latter is densely crowded, and reaching several millimetres in diameter when only a few are scattered over the surface. The amount of liquefaction also varies inversely as the number of colonies, but even after considerable liquefaction has taken place over the surface of the gelatine, the individual colonies remain perfectly distinct, and do not become confluent, and so lose their identity, as is the case with many liquefying organisms. Not unfrequently, especially when there are comparatively few organisms on the plate, those situated on the surface give rise to a remarkable and very beautiful flocculent expansion of irregular contour and sometimes several millimetres in diameter.

The plates possess a highly characteristic and very nauseous odour, which is at once apparent on opening a dish in which they have been developed. Viewed under the low power of the microscope each colony when young has a very characteristic appearance, resembling a spherical or ovoid sea-urchin covered with spines, and light brown or gray in colour.

Magnifying power nearly 1000.



Magnifying power about 100.

2. *Appearance of the Finkler-Prior's Comma Spirillum in Gelatine-tube Cultivations.*

The growth of this organism very much resembles that of the *Bacillus pyocyaneus*, only that no colouring-matter is produced. The liquefaction, which is at first funnel-shaped, rapidly extends across the whole tube, and the downward extension into the solid gelatine becomes filled with a plug of a yellowish viscid material. In the course of a few weeks the whole gelatine, or at least as far as the needle at the time of inoculation has reached, becomes liquid, the yellowish precipitate resting on the bottom, but the liquefied portion also remains turbid.

Appearance of the Finkler-Prior's Comma Spirillum in Gelatine-plate Cultivations.

The plate cultivations of this organism are very characteristic. The colonies making their appearance already on the first or second day as small milky white disks, which increasing in size cause rapid liquefaction of the gelatine, the depressions being filled with a white fluid resembling thin milk. If the centres are at all closely approximated they very soon become confluent, and lose their identity, the whole plate becoming fluid.

Viewed under the low power of the microscope the colonies appear as brownish very finely granular disks, with a highly refracting edge, which is also granular and not perfectly sharp.

Magnifying power nearly 1000.

Magnifying power about 100.

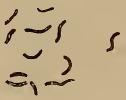
3. *Appearance of Koch's Comma Spirillum in Gelatine-tube Cultivations.*

The growth of this organism in gelatine is slower than that of the other two. At 20° C., within a day or two of inoculation, the path of the needle becomes visible as a fine thread, which sooner or later is followed by a conical depression at the surface; this depression is filled with air at its wider upper extremity, and slowly increases in width; as growth proceeds the track of the needle becomes thicker, and the air-cavity at the surface becomes partially filled with clear liquid, at the bottom of which there is a whitish somewhat flocculent precipitate.

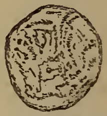
The growth in peptone-broth is very rapid, the liquid becomes turbid, and on the surface there is formed a tough pellicle which increases in thickness, whilst the subjacent liquid is again clarified.

Appearance of Koch's Comma Spirillum in Gelatine-plate Cultivations.

The plate cultivations exhibit in the course of two or three days minute whitish spots, each situated at the bottom of a small depression on the surface. The colonies remain small, but when very numerous the substance of the gelatine becomes largely liquefied.



Magnifying power nearly 1000.



Magnifying power about 100.

Under the low power of the microscope the colonies present the appearance of somewhat irregular disks, with a more or less irregular and jagged edge, the interior of the disk being filled with coarsely granular matter.

MULTIPLICATION EXPERIMENTS WITH SPECIFIC ORGANISMS.

The pure cultivations of the three specific organisms mentioned above were introduced into the experimental waters in the following manner, so as to secure a degree of attenuation which admitted of satisfactory examination by plate cultivation.

One or more needlefuls of the cultivation are first inoculated into about 50 c.c. of sterilised distilled water contained in a sterile stoppered bottle; this first attenuation is then well shaken, so as to distribute the introduced organisms evenly throughout. From this first attenuation a certain number of drops are withdrawn by means of a sterilised pipette, and introduced into the sterilised experimental water contained in another sterile stoppered bottle. The latter is then in turn well shaken, and the number of organisms in a given volume determined by plate cultivation. This second attenuation is then exposed to any desired conditions of temperature, &c., and the number of organisms determined in it at suitable intervals of time.

Experiments with the Bacillus pyocyaneus.

A large number of experiments were made upon the vitality of this organism when introduced into various kinds of water. The results of these experiments show that on introducing this organism into purer water, such as distilled water, deep well-water, or filtered Thames water, in the first instance a greater or less reduction in the number of organisms capable of development often takes place, but sooner or later a considerable increase in their numbers generally follows, and only in very exceptional cases was the complete destruction of their vitality observed. On the other hand, when the same organism is introduced into sewage a rapid and large multiplication was observed in every case. From a number of bottles of distilled water which were inoculated with the *Bacillus pyocyaneus* in the manner described above, the following number of colonies were obtained.

Bacillus pyocyaneus in Distilled Water.

Number of Colonies obtained from 1 c.c.

	Day of preparation.	2nd Day.	3rd Day.	7th Day.	20th Day.	53rd Day.
No. 1 ..	6,100	203	—	—	276	100,000
„ 2 ..	6,800	368 (Incub.)	—	—	13,400 (Incub.)	69,000
„ 3 ..	15,400	1,557 (Incub.)	—	—	69,600 (Incub.)	249,000
„ 4 ..	3,200	—	6,200 (Incub.)	96,000 (Incub.)	—	—
„ 5 ..	3,200	—	0	19	—	—
„ 6 ..	114,000	—	—	—	38,000	—
„ 7 ..	165,000	—	—	0 (Incub.)	—	—
„ 8 ..	165,000	—	—	0	—	—

Note.—" Incub." means that the sample above was kept in the incubator at 35° C.

Bacillus pyocyaneus in Filtered Thames Water.

Number of Colonies obtained from 1 c.c.

	Day of preparation.	3rd Day.
No. 1	3,900	Innumerable.
No. 2	3,900	Innumerable (incub.).

Bacillus pyocyaneus in Deep Well-water.

Number of Colonies found in 1 c.c.

	Day of preparation.	3rd Day.	5th Day.	8th Day.
No. 1	3,900	Innumerable.	—	—
		(Incub.)		
„ 2	3,900	Innumerable.	—	—
„ 3	269,000	—	432,000	395,000
		—	(Incub.)	(Incub.)
„ 4	269,000		422,000	Innumerable.
„ 5	262,000	—	851	87
			(Incub.)	(Incub.)
„ 6	262,000	—	195,000	227,000
„ 7	10,000	0	—	0
„ 8	10,000	0	—	0
„ 9	10,000	0	—	0
		(Incub.)		(Incub.)

Bacillus pyocyaneus in Sewage.

Number of Colonies found in 1 c.c.

	Day of preparation.	3rd Day.	8th Day.	18th Day.
No. 1	29,000	Innumerable.	Innumerable.	547,000
		(Incub.)	(Incub.)	(Incub.)
„ 2	29,000	90,500	Innumerable.	Innumerable.
„ 3	29,000	116,500	Innumerable.	Innumerable.

The great vitality of this organism is well illustrated by experiments No. 1, 2, and 3, with distilled water, in which after fifty-three days there were in each case a great many more colonies obtained than at the outset, although in each case the number was greatly reduced immediately after the inoculation. For experiment No. 5, with distilled water, the number of organisms was in the first instance so largely reduced that on the third day there were no organisms demonstrable even in a whole cubic centimetre, and even on the seventh day the number had only risen to nineteen in that volume. The corresponding sample (No. 4) placed in the incubator multiplied much more rapidly, showing 6200 colonies on the third day and 96,000 on the seventh in 1 c.c.

In the deep well and filtered Thames waters there is abundant evidence of extensive multiplication, the only cases (No. 7, 8, and 9) in which the organisms disappeared being all taken from one and the same bottle. In the sewage again the multiplication of the organism was in each case very great.

Experiments with Koch's Comma Spirillum.

The first experiments were made with comma Spirilla from feeble cultivations in gelatine. In these cultivations the organisms appeared to grow with difficulty, producing little or no liquefaction, but only a deep depression on the surface of the gelatine at the point of inoculation. On subsequently inoculating into broth a powerful growth was obtained, and from this again a strong growth in gelatine was procured. It was with comma Spirilla from this broth cultivation that the subsequent experiments were made. When the comma Spirilla from the weak cultivations were introduced into various waters, including sewage, their presence was, with one exception, no longer demonstrable after the first day, whilst the waters inoculated from the broth cultivation not only yielded colonies of comma Spirilla on plate cultivation after seven days, but during this time slight multiplication was exhibited in the potable water, and very extensive multiplication in the sewage. Thus—

Comma Spirillum (from a weak cultivation in Gelatine) in Distilled Water.

Number of Colonies obtained from 1 c.c.

	Day of preparation.	2nd Day.	3rd Day.	5th Day.	6th Day.	8th Day.	11th Day.	15th Day.	19th Day.
No. 1 .	100	0	—	0	0	0	0	0	—
,, 2 .	10	—	—	0	0	0	0	0	—
,, 3 .	52	0 (Incub.)	—	0 (Incub.)	0 (Incub.)	0 (Incub.)	0 (Incub.)	0 (Incub.)	—
,, 4 .	4,000	—	—	0	—	—	—	—	—
,, 5 .	2,800	—	0	—	—	—	—	—	—
,, 6*.	—	—	—	2,213	286	498	4	25	0

---

* In this case the comma Spirilla were introduced into the experimental bottle *from the gelatine cultivation direct*, so that an appreciable quantity of the gelatine peptone must have been introduced at the same time; it is not, therefore, comparable with the preceding five experiments.

Comma Spirillum (from weak cultivation in Gelatine) in Sewage and Deep Well-water.

Number of Colonies obtained from 1 c.c.

		Day of preparation.	2nd Day.	4th Day.
No. 1	Sewage	525	0 (Incub.)	0 (Incub.)
„ 2		525	0	0
„ 3		525	0	0
„ 4	Deep well water	395	0 (Incub.)	0 (Incub.)
„ 5		395	0	0
„ 6		395	0	0

These experiments show that in every case the comma Spirilla from this *weak cultivation* lost their vitality in twenty-four hours when introduced into these dilute media. The one case (No. 6 in distilled water) in which they were subsequently demonstrable, differed from the others, inasmuch as this was the bottle in which the cultivation was first attenuated, in fact Nos. 1, 2, and 3 were prepared by diluting No. 6 about 300 times. But even in this first attenuation the vitality of the organism is seen to be destroyed within nineteen days, and similarly the first attenuation, from which the six last experimental bottles above were inoculated, was also examined after nine days, and the comma Spirilla no longer found demonstrable.

In the following experiments the comma Spirilla employed were taken from a vigorous cultivation in peptone-broth, the results obtained being as follows:—

Comma Spirillum (from vigorous Broth Cultivation) in Deep Well-water, Sewage, and Filtered Thames Water.

Number of Colonies obtained from 1 c.c.

	Day of preparation.	2nd Day.	5th Day.	6th Day.	9th Day.	11th Day.	17th Day.	29th Day.
No. 1	5,750	0	0	0	—	0	—	—
Deep well		(Incub.)	(Incub.)	(Incub.)		(Incub.)		
„ 2	5,750	0	0	0	—	0	—	—
„ 3	4,750	Innum.	Innum.	Innum.	—	96,000	—	—
Sewage ..		(Incub.)	(Incub.)	(Incub.)		(Incub.)		
„ 4	4,750	60,000	Innum.	Innum.	—	Innum.	—	—
„ 5	456	18	1,225	—	147	—	0	0
Deep well		(Incub.)	(Incub.)		(Incub.)		(Incub.)	(Incub.)
„ 6	456	57	3,834	—	1,232	—	0	0
„ 7	300	Innum.	Innum.	—	Innum.	—	128,000	56,000
Sewage ..		(Incub.)	(Incub.)		(Incub.)		(Incub.)	(Incub.)
„ 8	300	19,000	Innum.	—	Innum.	—	Innum.	Innum.
„ 9	—	188	0	0	0	—	—	—
Filtered Thames		(Incub.)	(Incub.)	(Incub.)	(Incub.)			
„ 10	—	63	313	480	173	—	—	—

Thus the comma Spirilla from the vigorous cultivation in broth was found in every case to flourish in sewage; on the other hand, in deep well-water their vitality was lost in Nos. 1 and 2, whilst in Nos. 5 and 6 they were still demonstrable on the ninth day, having in the interim undergone considerable reduction in the first instance, and then multiplied, although incomparably less than in the sewage experiments.

The first attenuation (in distilled water) from which Nos. 1, 2, 3, and 4 above were inoculated was also examined on the ninth day, and 15,650 colonies obtained from the cubic centimetre. Thus, although still alive, the reduction in number must have been very great, as originally this attenuation must have contained upwards of 1 million per cubic centimetre.

Dr. Koch (Second Cholera Conference, May, 1885, "Brit. Med. Journ.," January 9, 1886) mentions that Nicati and Rietsch have demonstrated the vitality of the comma Spirilla in the harbour water of Marseilles after a period of eighty-one days, and that he himself has proved them to be still alive in different kinds of water over periods of time varying from twenty-four hours to thirty days. From my experiments it appears that the behaviour of the Spirillum in different waters is largely dependent upon the source from which they are obtained, but that under favourable circumstances a large amount of multiplication may take place when they are introduced into sewage of the composition recorded, their behaviour in this medium presenting a very marked contrast to that in deep well and filtered Thames water.

Experiments with Finkler-Prior's Comma Spirillum.

Similar experiments to those described above were also made with this organism. These experiments show that although it possesses such far greater vital activity in gelatine cultures than Koch's comma Spirillum, yet when introduced into different kinds of water, including sewage, its vitality is so rapidly lost that in no single instance have I been able to demonstrate its presence even after twenty-four hours, still less to obtain evidence of any multiplication in these media. In fact so perishable does this organism appear to be when placed in water, that I have very frequently been unable to demonstrate its presence even on the day of inoculation, whilst with the *Bacillus pyocyaneus* I have never failed in this manner, and with the comma Spirillum only rarely. The following results will serve to illustrate how rapidly this organism loses its vitality in waters of various kinds:—

Finkler-Prior's Spirillum in Distilled Water.

Number of Colonies obtained from 1 c.c.

	1st Day.	3rd Day.	5th Day.	6th Day.	20th Day.
No. 1	12,000	0	0	—	—
,, 2	4,500	0	—	0	0

Finkler-Prior's Spirillum in Deep Well-water and Filtered Thames Water.

Number of Colonies obtained from 1 c.c.

		1st Day.	2nd Day.	4th Day.	5th Day.	9th Day.
No. 1	Deep well-water.	300	0	0	—	—
,, 2		300	0	0	—	—
,, 3		300	0	0	—	—
,, 4		17,450	0	—	0	0
,, 5		17,450	0	—	0	0
,, 6	Filtered Thames water	12,000	0	—	0	0
,, 7		12,000	0	—	0	0

Note.—Nos. 3, 5, and 7 were placed in the incubator.

Finkler-Prior's Spirillum in Sewage.

Number of Colonies obtained from 1 c.c.

	1st Day.	2nd Day.	4th Day.	5th Day.	9th Day.
No. 1	390	0	0	—	—
„ 2	390	0	0	—	—
„ 3	390	0	0	—	—
„ 4	10,750	0	—	0	0
„ 5	10,750	0	—	0	0

Note.—Nos. 3 and 5 were placed in the incubator.

From the differences which these three organisms present in their behaviour in water, and in different kinds of water, it is evident how fallacious must be any conclusions as to the vitality of pathogenic micro-organisms in general, more especially when such conclusions are based upon observations made with organisms which are the natural inhabitants of natural waters, as has often hitherto been the case. It is obvious that each individual organism must be made the subject of separate investigation as to its vitality. To render such investigations complete, it is necessary also that the organisms under examination should be taken from different sources, as from the experiments which I have quoted in the case of the comma Spirilla it is evident that an initial weakness of the growth from which the inoculation is made renders the organisms less capable of withstanding the conditions of the experiment. It would also appear probable that this initial difference in vitality may be the cause of some of the many discrepant results which have been obtained by different observers in the study of antiseptic action. That there exists a difference in resisting power according to the virulence or initial vitality of the organisms themselves has already been drawn attention to by Dr. Klein ("Micro-organisms and Disease," 1886).

In the following table the chemical composition of the sewage and other waters employed in the above experiments is recorded :—

Results of Analysis expressed in parts per 100,000.

	Total solid matter.	Organic carbon.	Organic nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Chlorine.	Hardness.
Sewage No. 1	60·20	2·350	1·387	2·200	0	8·5	—
,, ,, 2	84·60	7·550	4·210	3·500	0	9·6	—
Deep well-water No. 1....	43·44	0·027	0·010	0	0·446	2·5	27·2
No. 2....	42·34	0·023	0·007	0	0·442	2·5	27·9
Filtd. Thames water......	26·42	0·111	0·021	0	0·202	1·6	17·4

In conclusion I have to express my indebtedness to my wife for the great assistance which I have received from her in the most laborious task of estimating the colonies on the gelatine plates, amounting to nearly 1000 in number, which this investigation has entailed.

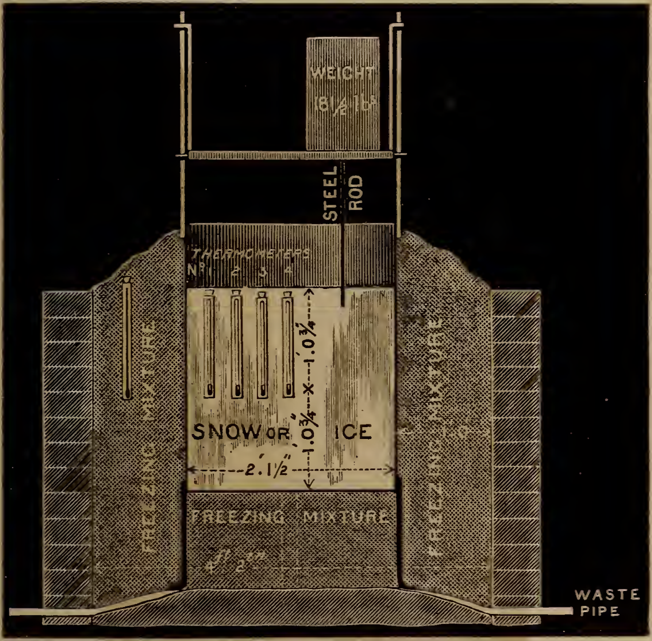
XII. "Observations on Pure Ice and Snow." By THOMAS ANDREWS, F.R.S.E., F.C.S., Wortley Iron Works, near Sheffield. Received June 10, 1886.

The recent very severe winter afforded favourable opportunity and material for investigating some of the properties of ice and snow. The object of the following observations was to obtain information on the relative conductivity of ice and snow, the dilatation of pure ice, and its relative hardness or penetrability at various temperatures.

SET I.—*The Relative Conductivity of Pure Ice and Snow.*

The experiments on the conductivity of the ice were made as follows:—47 gallons of distilled water at a temperature of 48° F. were placed in a large circular iron tank, A, of the internal dimensions given on fig. 1. Around this tank was built a brick enclosure, of 4 feet 2 inches internal diameter; an opening was left near the bottom for easily removing the expended freezing mixtures, &c. The intermediate space between the iron tank and the brickwork was filled with a freezing mixture of snow and salt, maintaining a constant temperature of $-4°$ F. throughout. 18 cwts. of this mixture was required for each charge, and the charge was entirely renewed every 12 hours during the $115\frac{1}{4}$ hours needed to freeze the above volume of water. Thus about 8 tons of freezing mixture was used for this part of the operation, producing for the experiments a cylindrical mass of pure ice 2 feet 1½ inches diameter, having a cubic content of 8·305 feet, and weighing nearly 4 cwts. 22 lbs. A wooden frame had been securely fixed in the tank to hold the necessary tubes containing the thermometers. These were four in number, and were placed at equal distances between the centre of the circular mass of ice and its circumference, the bulbs being also midway between the top and bottom as indicated in fig. 1. The thermometers were each protected by iron pipes closed with corks. The bulb of each thermometer was immersed in mercury placed at the bottom of the pipe, the top being also closed airtight by corks. These thermometers were therefore easily removed, and replaced at the stated times of observation. As the water gradually froze, the arrangement became securely embedded in the ice.

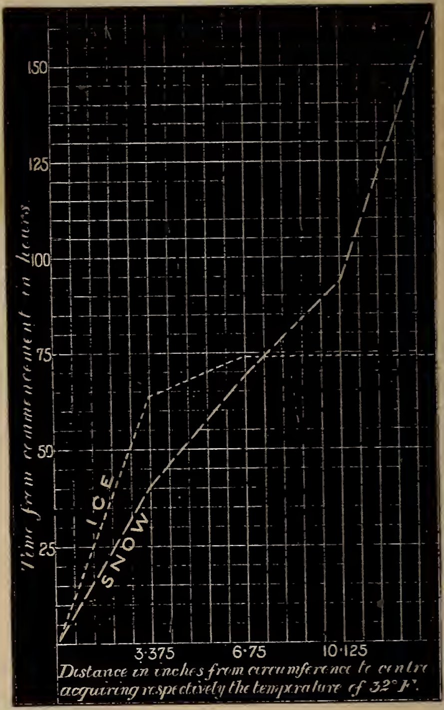
Fig. 1.



Sectional Elevation.

A suitable mass of pure ice having been thus obtained, and the thermometers registering its temperature at 0° F. throughout, the freezing mixture surrounding the tank was withdrawn and its place supplied with about 15 cwt. of snow; the gradual increase in the temperature of the ice cylinder from exterior to centre until it reached an uniform temperature of 32° F. was simultaneously periodically taken by the thermometers Nos. 1, 2, 3, and 4, and the results are graphically recorded in Table No. 1. The surface of the ice was kept covered to a depth of some inches with sawdust. 73½ hours were required for the mass to attain the temperature of 32° F. from zero (0° F.).

Table I.—Relative Conductivity of Ice and Snow compared by the time required under equal conditions in each case for the respective mass of Ice or Snow to acquire an uniform temperature throughout.



Temperature of ice and snow cylinders at commencement = Zero (0°) F.
Temperature surrounding ditto = 32° F.

The relative conductivity of snow was taken in a similar manner. An equal volume of fresh fallen snow was placed in the iron tank, A, and only very lightly pressed to ensure solidity of the snow, but so as to avoid regelation. Its weight was 1 cwt. 2 qrs. 14 lbs. The iron tank was then surrounded by the freezing mixture until the internal cylindrical mass of snow was at the uniform temperature of 0° F. The freezing mixture was then removed and replaced by about 15 cwt. of snow, and the temperature readings regularly taken until the snow mass in the tank arrived at one even temperature of 32° F. throughout. The readings are delineated on Table I. 165½ hours were occupied in arriving at this result.

The relative conductivity of the ice was thus found to be about 122 per cent. greater than the snow under the conditions of experimentation.

SET II.—*Dilatation of Ice between the Temperatures of* −35° *F. and* +32° *F.*

In conducting these experiments, the dilatation of the ice between the temperatures of zero (0° F.) and 32° F. was ascertained by the expansion of the ice measured between two iron bars 1 inch square by 19½ inches long, securely embedded to a depth of 13 inches in the ice, and perpendicular to its surface, at a distance of 16¼ inches apart. Two other square iron bars of the same dimensions were also securely frozen horizontally into the side of the ice cylinder. The measurements between the respective sets of iron bars were accurately taken by a delicate micro-vernier gauge, the limit of error in reading which by the aid of a telescope did not exceed one two-thousandth of an inch. The gauge was securely placed on a suitable wooden frame, and thus removed from the influence of the low temperature, so that all the measurements were taken throughout at exactly the same spot on the bars. The average of 100 measurements in each case was regarded as the correct reading, and the results are recorded on Table II. After the measurements had been taken with the ice at zero (0° F.), the freezing mixture surrounding the ice tank was removed and replaced by about 15 cwts. of snow, until the whole mass of ice again reached the temperature of 32° F. The relative distances between the respective sets of bars at the higher temperature of 32° F. were again taken, and the average of 100 readings regarded as correct. The dilatation of the ice cylinder, both longitudinal and transversely, thus obtained between the temperatures of zero (0° F.) and 32° F. is given on Table II. A difference was observed between the longitudinal and transverse dilatation. This was not owing to error in observation, as the deviation was constantly noticeable during the course of the measurements. Certain crystalline bodies dilate unequally, and the ice also appeared to behave in a similar manner; the difference noticed may, therefore, possibly have been due to the mode of crystallisation of the cylindrical mass of ice.

The observations relating to the expansion of ice between −35° F. and +32° F. were conducted in a similar manner: but to obtain the lower temperature using a freezing mixture of three parts by weight of crystallised calcium chloride and two parts of snow, which yielded a temperature of −39° F.

The vessel containing this mixture was itself further surrounded by another freezing mixture of a constant temperature of −4° F., and this arrangement was found to work admirably in maintaining a prolonged and constant low temperature. An alcohol thermometer was employed for taking the internal temperature of the mass of ice, substituting a little alcohol at the bottom of the protecting tube instead of mercury. The results are contained in Table II.

It will be noticed that the coefficients become less as the temperature is reduced.

Table II.—Dilatation of pure Ice between −35° F. and +32° F.

	Column 1.	Column 2.	Column 3.	
	Transversely measured at 32° F. become	Longitudinally measured at 32° F. become	Linear expansion.	
			Transverse.	Longitudinal.
1000 parts at +16° F...	1000·654	..	1 in 1529	—
,, zero F. ..	1001·103	1000·657	1 ,, 906	1 in 1522
,, −21° F. .	1001·533	..	1 ,, 652	—
,, −30° F. .	1001·712	..	1 ,, 584	—
,, −35° F. .	1001·871	..	1 ,, 534	—

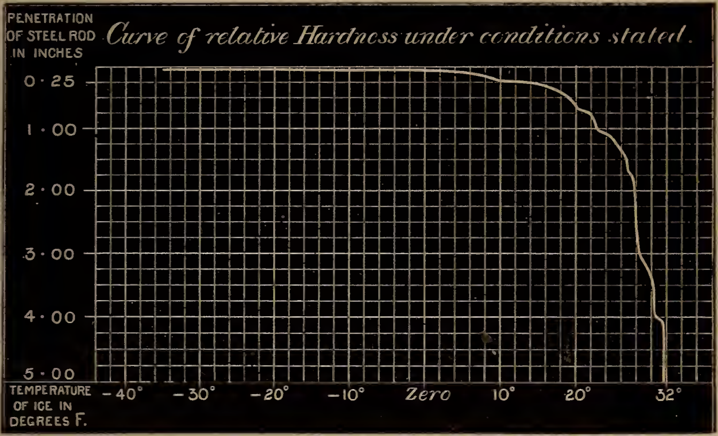
Linear coefficient for 1° between +16° F. and +32° F. = 0·000040876.
,, ,, 0 ,, +16 ,, = 0·000028042.
,, ,, −21 ,, 0 ,, = 0·000020484.
,, ,, −30 ,, −21 ,, = 0·000019744.

Set III.—*Relative Hardness or Penetrability of Ice at Temperatures between −35° F. and +32° F.*

The observations were periodically taken during the gradual increase in the temperature of the ice cylinder from −35° F. to +32° F. A polished steel rod, 16 inches long by 0·292 inch diameter, blunt at the end, was allowed to penetrate the ice under the influence of a weight of 181½ lbs. resting on its summit. A suitable apparatus was arranged to carry out this set of observations, which is roughly delineated on the sketch, fig. 1.

The relative depths to which the steel rod penetrated the ice under the varied conditions of temperature (which were frequently ascertained by thermometers) compared with the penetrability at −35° F. afforded approximate indications of the relative hardness or penetrability of the pure ice under the conditions of experimentation. The

Table III.—Relative Hardness or Penetrability of Ice at various Temperatures.



The steel rod penetrated at −35° F. 0·043 in., and at zero F. 0·094 in.

results are given in Table III. These are the average of very many observations. The ice appeared practically to maintain its almost impenetrable hardness from −35° F. until about +10° to 20° F., after which its power of resistance to the penetration of the steel rod rapidly decreased with the increase of temperature. It will also be noticed that the relative contraction and dilatation of the ice between the extremes of low temperature employed was considerable. The whole of the experiments were many times repeated to ensure accuracy, and it may be observed that above 20 tons of snow and above 7 tons of salts for freezing mixtures, &c., were consumed in conducting the varied experiments of the investigation.

XIII. "On the Gaseous Constituents of Meteorites." By Gerrard Ansdell, F.C.S., and Prof. James Dewar, F.R.S. Received June 10, 1886.

The nature of the occluded gases which are present to a greater or less extent in all meteorites, whether belonging to the iron, stony, or carbonaceous classes, has engaged the attention of but few chemists. It is, nevertheless, an especially interesting and important subject, owing to the uncertainty which still exists as to the origin of these celestial bodies.

Graham ("Proc. Roy. Soc.," vol. 15 (1867), p. 502, was the first who made any experiments in this direction, when he determined the gases occluded in the Lenarto meteoric iron, which yielded 2·85 times its volume of gas, ·86 per cent. of which was hydrogen, and 4·5 per cent. carbonic oxide. He was followed in 1872 by Wöhler ("Pogg. Ann.," vol. 146, p. 297) and Berthelot ("Compt. rend.," vol. 74, pp. 48, 119), who estimated approximately the gases contained in the Greenland Ovifak iron. These gases consisted of about equal parts of carbonic acid and carbonic oxide; the celestial origin of this iron is, however, very doubtful.

In the same year (1872) the American chemist, Mallet ("Proc. Roy. Soc.," vol. 20, p. 365), made a very complete determination of the gases occluded by the Augusta Co., Virginia, meteoric iron, which, however, differed very considerably from Graham's results. He obtained an amount of gas equal to 3·17 times the volume of the iron, made up of 35·83 per cent. of hydrogen, 38·33 per cent. carbonic oxide, 9·75 per cent. of carbonic acid, and 16·09 per cent. of nitrogen.

Wright and Lawrence Smith followed Mallet, and our present knowledge of this interesting subject is principally due to these American chemists. They have taken advantage of the numerous meteoric masses which have fallen from time to time throughout America, and which can easily be obtained in sufficient quantity for complete and accurate observations on their gaseous constituents.

Wright contributed several papers to the "American Journal" in 1875 and 1876, and, according to his analyses, the total volume of gas occluded and the composition of the same differs considerably in the two principal classes of meteorites. He found the total volume of gas extracted was much greater in the case of the stony meteorites than in the iron ones, the principal characteristics of these gases being, that in the former the carbonic acid greatly predominated, accompanied by a comparatively small amount of carbonic oxide and hydrogen, whereas in the latter the carbonic acid never exceeded 20 per cent., the carbonic oxide being, as a rule, considerably more than this, and the hydrogen sometimes reaching as high as 80 per cent.

It is impossible, however, to arrive at anything more than general conclusions as to the total amount of gas given off by any special meteorite, or its composition, for, as shown by Wright and confirmed by ourselves, both the total quantity and composition of the gases vary very considerably according to the temperature at which they are drawn off.

Wright found a notable quantity of marsh-gas in all the stony meteorites which he examined, though not a trace in any of the iron ones; this seemed to be a distinctive difference between the two classes of meteorites, but subsequently Dr. Flight, of the British Museum ("Phil. Trans.," vol. 173 (1882), p. 885), found marsh-gas in a specimen of the Cranbourne siderite, so that it is evident certain of the iron meteorites also contain this gas.

Lawrence Smith ("Amer. Journ.," 1876) confined himself principally to an examination of the graphite nodules which are frequently found imbedded in the iron meteorites, and to the nature of the carbon in the so-called carbonaceous meteorites. He extracted the organic or hydrocarbon-like bodies by means of ether, but did not determine the gases given off on heating. Previous to this, Roscoe ("Pro. Phil. Soc. Man.," 1862) had obtained the same hydrocarbon-like body by exhausting the Alais meteorite with ether, but the quantity he had to work upon was so small that he could not make a very complete examination.

These are some of the principal points that have been made out with regard to the gases occluded by meteorites. The results, however, are so comparatively few, that we thought it worth while to take the opportunity which presented itself, of having several good specimens of meteorites at our disposal, to confirm these results, and, if possible, add something to our present knowledge of the subject.

The investigation may be divided into five parts, having the following objects in view: firstly, the confirmation of previous results by the examination of some well-known meteorite; secondly, the analysis of several whole meteoric stones, whose interior had never been exposed to the effects of the atmosphere, by reason of the characteristic coating of glaze; thirdly, the examination of a celestial graphite nodule, taken from the interior of an iron meteorite; fourthly, the comparison of some meteorite of the carbonaceous class with the above; and, fifthly, the examination of different terrestrial graphites.

The method employed for the abstraction of the gases was exactly the same in every case, so that a short description will suffice for all. The temperature was kept as nearly as possible the same in every experiment, but no doubt differences of many degrees occurred in some of the experiments, which was unavoidable in using an ordinary combustion furnace.

The meteorite or graphite, as the case might be, was broken up into a coarse powder, introduced into a convenient length of combustion tubing, and connected up with a Sprengel pump, a small bulb-tube immersed in a freezing mixture intervening, so as to retain any moisture or condensable volatile products that might come off. The tube was first thoroughly exhausted and then heated in an ordinary gas combustion furnace to a low red heat. The gases, during the heating, were gradually drawn off by the Sprengel, and when the tube had remained for several minutes at a low red heat it was completely exhausted. The total quantity of gas collected was in every case used for the analysis.

The "Dhurmsala" specimen was an ordinary fragment of a much larger original mass, but in the case of the Pultusk and Mocs meteorites, we were fortunate in obtaining complete stones, weighing respectively 57 and 103 grams, having the characteristic black glaze on their surfaces.

Such a large quantity of water was condensed in the bulb tube in heating the Dhurmsala meteorite, it being the first one examined, that it was thought it might be principally due to the great absorptive power of these porous bodies, and that therefore the moisture might have been condensed in the pores of the meteorite from the surrounding air. The Pultusk and Mocs specimens appeared to be especially adapted for ascertaining whether this was the case, as the complete covering of black glaze would probably prevent the moisture from penetrating to the interior of the stones. The fragments of these stones were therefore transferred as quickly as possible to the combustion tube after they had been broken up. Notwithstanding these precautions, fully as much water was condensed from them as from the Dhurmsala specimen, which seems to suggest that the water is really combined in some form in the stone and not obtained directly from the surrounding atmosphere, although it must be admitted that the glaze on both the stones was not of a very glossy character, and did not have the appearance of being absolutely impervious to moisture.

The pumice-stone was examined merely with a view to comparing the gases occluded by a porous body of volcanic origin with those contained in meteorites. The sample taken was a fresh piece of stone, which had not been dried or purified in any way.

It is evident that it differs considerably from the meteoric stones, the total occluded gas being very small, only about half its volume, the carbonic acid at the same time being much less with a proportionate increase in the carbonic oxide.

The general method of analysis was as follows, and the accuracy of the results was confirmed by varying in some cases the method of separating the gases. The carbonic acid was first removed from the mixture by caustic potash, the carbonic oxide being then absorbed by subchloride of copper, and the remainder of the gases exploded with excess of oxygen. The carbonic acid formed was again removed by caustic potash, and the excess of oxygen by alkaline pyrogallate, the residue being taken as nitrogen. The relative quantities of marsh-gas and hydrogen were calculated from the total diminution after explosion, and the amount of carbonic acid formed :—

	Sp. gr.	Occluded gases in vols. of the meteorite.	Percentage composition.				
			CO_2.	CO.	H.	CH_4.	N.
Dhurmsala....	3·175	2·51	63·15	1·31	28·48	3·9	1·31
Pultusk	3·718	3·54	66·12	5·40	18·14	7·65	2·69
Mocs	3·67	1·94	64·50	3·90	22·94	4·41	3·67
Pumice-stone..	2·50	0·55	39·50	18·50	25·4	—	16·60

It will be seen that the above numbers are quite confirmatory of Wright's results, the carbonic acid in the three meteorites examined being by far the largest constituent, while marsh-gas in considerable quantity was found in all. The percentage of this latter gas is somewhat higher than that found by Wright in the stony meteorites he examined, but this is probably due to the fact that a rather higher temperature was employed by us to drive off the gases. This supposition seems to be confirmed on considering the analysis of the Pultusk meteorite, which is the only one examined both by Wright and ourselves; for whereas his abstracted gas only reached 1·75 times the volume of the stone, the total quantity of gas obtained by us was twice as much or equal to 3·54 times its volume.

It is therefore unquestionable that marsh-gas is given off on heating these meteoric stones, but whether it exists as such occluded in the material, or whether it is formed by some chemical decomposition of some organic constituent of the mass is by no means clear.

Wright came to the conclusion "that the marsh-gas really existed as such in the stony meteorites, as the temperature at which it was driven off would be too low for its formation," at the same time he thinks it quite possible that "at very much higher temperatures, in the reaction by which the carbonic acid is broken up by the iron, a portion of the carbon might combine with the hydrogen present to form marsh-gas."

We shall return to this question of the formation of marsh-gas, after we have described the experiments in the various forms of graphite.

Knowing the great absorptive power for gases possessed by porous bodies generally, it was thought advisable to determine directly what this absorptive power was in the case of these stony meteorites, which are of such an eminently porous nature.

For this purpose the powdered Dhurmsala meteorite, from which the gases had been removed, was left in moist air under a bell glass, for different periods of time as tabulated below, the gases being drawn off at a low red heat after each period:—

	Occluded gas in vols. of the meteorite.	CO_2.	CO.	H.	N.
After 24 hours........	0·61	54·0	—	42·4	3·6
After 6 days more	2·47	47·0	5·0	47·0	1·0
After 8 days more	0·63	96·1	2·0	1·5	—

The absorption of water and gases evidently went on tolerably rapidly for the first seven days, but after the second heating of the meteorite, its porosity seems to have been affected in some way, for after a further period of eight days, we find it taking up only about a fourth of the quantity of gas which it had absorbed in the previous six days.

The actual amount of water given off after this exposure to a moist atmosphere was considerably less than what was obtained in the original heating of the meteorite, and from this we infer that the water is chemically combined in the stone. It would be difficult to explain, otherwise than by chemical combination, the power by which the water is retained by these meteorites, as it is not given off until a very high temperature is reached. In any case it is clear that the hydrogen must come from the action of water on the iron nickel alloy, or finely disseminated carbon. Greville Williams has pointed out that the large amount of hydrogen obtained from heating finely divided zinc-dust is not due to free hydrogen, but to the action of the zinc on the hydrated oxide of zinc.

We now pass on to the consideration of the various graphites examined. The celestial graphite was a perfect oblong nodule weighing 30 grams, which had been taken from the interior of a mass of the Toluca meteoric iron. It had a uniform dull-black colour, except at one end where there was a slight incrustation of sulphide of iron. Its fracture showed a uniform dull black, compact mass; it was easily pounded up in a porcelain mortar, and formed a fine granular powder without any lustre.

On extracting the gases from this specimen a considerable quantity of marsh-gas was obtained, so that it appeared to us most important to compare it with some samples of terrestrial graphites, more especially as the occluded gases had, to our knowledge, never before been determined in these bodies.

For this purpose four samples of native graphites were obtained. The Cumberland graphite was a magnificent specimen of the original Borrodale, and had been in a private cabinet for over fifteen years. It had the characteristic dense homogeneous structure, and brilliant external lustre of the graphites coming from this district. The Siberian example was from the Alexandref Mine; its structure was columnar and striated, with little external lustre; it was rather more easily broken up than the Borrodale, but formed the same dull black powder. The specimen from Ceylon was of the type usual to that island; highly lustrous and flaky, breaking up very easily, and forming small shining plates when ground up. The last sample, which was from the same cabinet as the others, but whose origin was unfortunately unknown, had a dull external surface, was exceedingly porous, and much more brittle than any of the previous ones, grinding up very easily into a dull black powder. It had more the appearance of the celestial graphites, which was heightened by having slight incrustations of sulphide of iron in its surface. Its low specific gravity also shows it to be some exceptional variety.

It seemed to us most important in connexion with this subject to examine some matrix with which the graphites are usually found associated. These rocks are very variable, but consist principally of a kind of decomposed trap or gneiss. We succeeded in obtaining a good specimen of semi-decomposed gneiss from Canada with a considerable quantity of graphite disseminated throughout the mass, and also several samples of Ceylon graphite imbedded in its matrix, which in this case consisted of felspar and quartz.

The results, as tabulated below, confirm Wright's analyses of several trap rocks, in which he found principally carbonic acid and hydrogen. The small quantity of marsh-gas no doubt comes from the disseminated graphite, but the presence of the hydrogen is more difficult to explain and requires further investigation.

	Sp. gr.	Occluded gases in vols. of the graphite.	CO_2.	CO.	H.	CH_4.	N.
Celestial graphite	2·26	7·25	91·81	—	2·50	5·40	0·1
Borrodale ,,	2·86	2·60	36·40	7·77	22·2	26·11	6·66
Siberian ,,	2·05	2·55	57·41	6·16	10·25	20·83	4·16
Ceylon ,,	2·25	0·22	66·60	14·80	7·40	3·70	4·50
Unknown ,,	1·64	7·26	50·79	3·16	2·50	39·53	3·49
Gneiss	2 45	5·32	82·38	2·38	13·61	0·47	1·20
Felspar	2·59	1·27	94·72	0·81	2·21	0·61	1·40

On comparing these samples of graphite, it will be seen that the Borrodale and the Siberian give off about the same total volume of gas, that the celestial and the unknown graphites closely approximate each other in this respect, yielding more than double the volume of the others, and that the Ceylon sample stands alone in yielding a very minute quantity. All the terrestrial samples, except that from Ceylon, are alike in giving off a very considerable quantity of marsh-gas, though they differ somewhat in the actual quantity, and it is evident that, although the celestial graphite contains a considerable amount, it is very much less than that yielded by the terrestrial samples.

A few tentative experiments were made to ascertain the absorbing power for gases of this celestial graphite. For this purpose dry carbonic acid, marsh-gas, and hydrogen were respectively drawn through the tube containing the previously exhausted graphite for twelve hours in the cold, the gases being pumped out at a low red heat after each treatment with the dry gas. After the carbonic acid treatment the volume of gas collected was only 1·1 times that of the graphite, containing 98·4 per cent. of carbonic acid; after the marsh-gas the volume of the gas was only 0·9 that of the graphite, containing 94·1 per cent. carbonic acid; and after the hydrogen the volume of the gas collected was only 0·17 times that of the graphite, containing 95·0 per cent. of carbonic acid. It is therefore evident that the large quantity of gas occluded in celestial graphites cannot be explained by any special absorptive power of this variety of carbon. In view of the large and varying percentages of marsh-gas in the gaseous products of all these graphites, it appeared to us of especial interest to ascertain whether the quantity of marsh-gas extracted coincided in any way with the hydrogen obtained by their combustion. We therefore submitted all the samples to ultimate analysis, with the following results:—

	Percentage composition.		
	Hydrogen.	Carbon.	Ash.
Celestial graphite	0·11	76·10	23·50
Borrodale ,, ...	0·11	94·76	4·85
Siberian ,, ...	0·17	79·07	20·00
Ceylon ,, ...	0·017	90·90	9·08
Unknown ,, ...	0·246	78·51	21·26

These analyses do not seem to point to any very definite conclusion as to the origin of the marsh-gas. The unknown graphite, which contains the largest percentage of marsh-gas, certainly comes out far the highest in hydrogen, and the hydrogen in the Ceylon graphite also bears a certain relation to the small quantity of marsh-gas it contains, but the first three samples are very similar to each other in the amount of hydrogen they contain.

In order to get some further insight into the origin of this marsh-gas in the celestial graphite, about 2 grams of the original nodule were very finely ground up and digested for several hours with strong nitric acid. After being thoroughly washed from every trace of nitric acid and dried at 110° C., it was again submitted to analysis, with the result that the amount of hydrogen remained exactly the same as before, proving that it existed in the form of some very stable compound in the graphite.

To clear up this matter still further, about 10 grams of the original nodule were digested with pure ether in the way described by Lawrence Smith for extracting the hydrocarbon-like bodies. It was allowed to stand for twenty-four hours with excess of ether, and then filtered, and washed with more ether. The graphite thus treated was dried at 110° C., and the gases extracted from it.

For the purpose of comparing one of the terrestrial graphites with the above in regard to its behaviour with ether, the specimen of unknown origin was selected, as yielding the largest quantity of marsh-gas. The residue, after digestion with ether, was dried, and the gases pumped out as before.

It will be seen that by this treatment with ether the volume of gas given off by the celestial graphite, and also the marsh-gas, have been reduced to rather more than one-half, while with regard to the unknown graphite, although the total volume of gas remains about the same (probably due to a rather higher temperature being employed), the marsh-gas has also been reduced to rather less than one-third the original amount, and the hydrogen has correspondingly increased.

	Occluded gases in vols. of the graphite.	CO_2.	CO.	H.	CH_4.	N.
Celestial graphite before treatment with ether...........	7·25	91·81	—	2·50	5·40	0·1
Celestial graphite after treatment with ether...........	3·50	81·50	10·63	1·41	2·12	0·74
Unknown graphite before treatment with ether...........	7·26	50·79	3·16	2·50	39·53	3·49
Unknown graphite after treatment with ether...........	7·15	64·86	5·67	14·37	12·96	2·00

These experiments prove that either the ether did not dissolve out all the actual carbonaceous compounds present, or that the marsh-gas was subsequently formed during the heating of the graphite.

As Dr. De La Rue had kindly placed at our disposal a splendid specimen of the Orgueil meteorite, we took the opportunity of comparing the gases occluded by this typical specimen of the carbonaceous class with those obtained from the stony meteorites and the graphites. This meteorite has been so thoroughly examined by Clöez and Pisani ("Compt. rend.," vol. 59 (1864), pp. 37, 132) with regard to its chemical inorganic constituents, that nothing need be said as to its general composition. We therefore confined ourselves to the gases given off on heating which had not previously been determined.

During the heating of the meteorite a large quantity of water, on which floated numerous small pieces of sulphur, collected in the bulb tube immersed in the freezing mixture. This water was strongly acid, and indeed smelt strongly of sulphurous acid. On evaporating it to dryness with a drop of hydrochloric acid, abundance of ammoniacal salts were found in the residue. In the cool anterior part of the combustion-tube a considerable sublimate had collected, which proved to be principally sulphate of ammonium with traces of sulphides and sulphites, and a large quantity of free sulphur. A very large quantity of gas was given off, having the following composition:—

	Sp. gr.	Occluded gases in vols. of the meteorite.	Percentage composition.				
			CO_2.	CO.	CH_4.	N.	SO_2.
Orgueil meteorite	2·567	57·87	12·77	1·96	1·50	0·56	83·00

Sulphurous acid is evidently the main constituent of the gases given off; but if we eliminate this gas, which has been formed from the decomposition of the sulphate of iron, we find the meteorite yields 9·8 times its volume of gas, having very much the same composition as that from some of the stony meteorites, viz.:—

CO_2, 76·05; CO, 11·67; CH_4, 8·93; N, 3·33.

Clöez found the organic matter in this meteorite to be composed of carbon 63·45, hydrogen 5·98, oxygen 30·57, which is nearly in the proportions of a terrestrial humus substance. We know such substances break up by the action of heat into gases of the nature found above, at the same time, however, a quantity of the carbonic acid undoubtedly comes from the presence of the carbonates of magnesium and iron. The operation by which terrestrial carbon has been changed into graphite is by no means clear. As a rule the transition of one kind of carbon into another necessitates the action of a very high temperature. If, therefore, a really high temperature is in all cases necessary, it is difficult to explain how compounds of carbon came to resist decomposition, and should come to be found associated with all natural graphites.

We may assume that the graphite resulted from the action of water, gases and other agents, on the carbides of the metals, and that during the chemical interactions which took place, a portion of the carbon became transformed into organic compounds.

In either case we are led to the conclusion that the method of formation of the meteoric and terrestrial graphites was similar, and it is perfectly possible they may after all have come from a common source.

We purpose continuing this investigation, and in order to acquire further information, it is our intention to examine the gases given off from meteorites at definite temperatures, and especially the gases from such as can be found coated with an impervious glaze, and to examine more particularly into the presence of water in such bodies, and the source of the nitrogen found in the same.

Note.

Since the above analyses of different graphites were made, we have examined a sample of the artificial graphite which results from the action of oxidising agents in the cyanogen compounds present in crude caustic soda. The following analysis shows that this artificial variety of graphite is characterised by giving a very large yield of marsh-gas.

CO_2	45·42
CO	39·88
CH_4	4·43
H	8·31
N	2·00

Occluded gases in volumes of the graphite=53·13.

XIV. "Preliminary Communication on the Structure and Presence in Sphenodon and other Lizards of the Median Eye, described by von Graaf in *Anguis fragilis*." By W. Baldwin Spencer, B.A., Demonstrator of Comparative Anatomy in University of Oxford, Fellow of Lincoln College. Communicated by Prof. H. N. Moseley, F.R.S. Received June 10, 1886.

In 1872 Leydig* described a structure in *Lacerta agilis*, *L. muralis*, *L. vivipara*, and *Anguis fragilis*, to which he gave the name of "frontal organ."

In the embryo, owing to its being deeply pigmented, it forms a prominent feature on the roof of the original forebrain in connexion with the pineal gland; in the adult it lies immediately beneath the skin, and, according to him and subsequent observers, completely separated from the brain.

In *Anguis fragilis* the organ is seen microscopically to consist of long cells like those of a cylindrical epithelium, which are so arranged that together they form a shallow pit with a circular outline. The edge of the pit is directed downwards, and has a thick black girdle of pigment. It corresponds in position to that occupied by the parietal foramen in the adult.

Leydig regarded our knowledge of the organ as insufficient to allow of any statement being made with regard to its function.

Rabl-Rückhard,† in 1882, describing the development of the pineal gland in the trout, pointed out the resemblance between its development as a hollow outgrowth of the brain and that of the optic vesicles.

Granted such secondary developments from the epiblast and mesoblast as combine to produce the eye, and which are absent in the case of the pineal gland, though the distal extremity of the latter lies in a

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* "Die in Deutschland lebenden Arten der Saurier," p. 72, taf. 12.

† "Zur Deutung und Entwickelung des Gehirns der Knochenfische." Arch. für Anat. u. Phys., Jahrg. 1882, p. 111.

favourable position immediately beneath the epiblast, and, he states, there is no difficulty in the way of the idea that an unpaired sense organ similar to the eye would be developed out of the pineal gland.

Ahlborn* also, in 1882, independently arrived at the conclusion that the epiphysis is to be regarded as the remains of an unpaired median eye, founding this conclusion upon general considerations, such as the agreement in origin of the eye vesicles and the epiphysis as hollow outgrowths of the brain, the connexion of the epiphysis with the eye region of the brain (especially the optic thalami), and the peripherally directed position of the structure in Selachian Ganoids and Petromyzon, and the completely peripheral position in Amphibia on the outside of the skull. He even goes so far as to suggest a comparison of this structure with the unpaired eye of Amphioxus and Tunicates.

More lately Henri de Graaf† has published an outline of his results in studying the development of the epiphysis in Amphibia, and its structure in the adult *Lacerta agilis* and *Anguis fragilis.*

He agrees with Strahl and Hoffmann in stating that the "frontal organ" of Leydig is the distal part of the epiphysis completely separated off from the proximal.

He describes in detail the structure of the organ in Anguis, where it develops, he says, into a structure very similar to a highly organised invertebrate eye, as that of Cephalopods, Pteropods, and Heteropods.

The following is a preliminary notice of results obtained recently by studying the structure of the organ in various forms of lizards, at the suggestion of Prof. Moseley, and by means of materials procured for the purpose with great kindness by him from various sources.‡

The forms investigated at present are the following, though only the more important results obtained from a few are given in this abstract:—

Hatteria punctata.
Lacerta ocellata.
Lacerta vivipara.
Iguana (2 sp.).
Calotes ophiomaca.
Calotes versicola.
Leiodera nitida.
Plica umbra.
Anolius (sp ?).
Grammatopleora barbata.
Chameleo vulgaris.
Stellio cordylina.
Varanus bengalensis.
Varanus giganteus.
Cyclodus gigas (?).
Seps chalcidica.

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* "Ueber die Bedeutung der Zirbeldrüse." "Zeit. für Wiss. Zool.," vol. 40 (1884), p. 336.

† "Zur Anat. u. Entwick. der Epiphyse bei Amphibien u. Reptilien." Zool. Anzeig., Jahrg. 9 (1886), p. 191.

‡ I am especially indebted to Professor Günter, through whose kindness I have

(1.) *External Appearance.*

The organ is situated upon the dorsal surface in the median line, and at varying distances posterior to the level of the paired eyes; the presence or absence of an external indication of the organ can be by no means relied upon as indicating the existence or non-existence of the structure in a highly developed state. In many cases, as *Varanus Bengalensis*, the various species of *Calotes* and *Lacerta ocellata*, the organ is marked externally by the presence of a specially modified scale, usually considerably larger than the surrounding ones, and with a circular patch of pigment behind the whole resembling a cornea. Being transparent, and forming the anterior boundary of a capsule containing the organ, the appearance of a dark pupil surrounded by a light circle is produced. On the other hand, as in *Plica umbra* or Cyclodus (sp.), a more or less highly specialised scale may be present, but the organ beneath be not highly developed; or again, as in Hatteria, there may be no special scale, but only a general transparency in the median line immediately above the organ, which may nevertheless be in a highly developed state.

(2.) *Position of the Organ.*

The organ may lie at different levels imbedded more or less deeply in connective tissue beneath the skin, or even within the skull cavity, but is always placed external to the dura mater. It always has a definite relationship to the parietal foramen usually lying within this. In Calotes it is placed immediately beneath the specially modified scale; in most forms, such as Varanus, Seps, Anolius, Leiodera, &c., it lies within the foramen, and separated by specially modified connective tissue from the skin. In Hatteria it lies on the inner side of the foramen, which is filled up by a plug of connective tissue, and in *Lacerta ocellata* the bone around the foramen is modelled to fit closely to the outline of the organ and the connective tissue surrounding this.

(3.) *Structure of the Organ.*

It may be said at once that Leydig's "frontal organ" resembles in essential structure an *invertebrate eye*.

This resemblance has lately been clearly pointed out by Graaf, in the case of *Anguis fragilis*, and is found to hold good for many others. He in common with all previous observers regards the organ as the

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been allowed to examine duplicate specimens of ten species from the British Museum; they are not all described in this communication, but will be dealt with more fully subsequently. By Professor C. Stewart's kindness also I have been able to examine duplicate specimens of Iguana and Varanus from the museum of the Royal College of Surgeons.

distal portion of the epiphysis, which becomes completely separated off from the proximal portion of the same, and lies completely surrounded by connective tissue in the parietal foramen. If in contact with a nerve, as frequently happens according to Graaf in Amphibia, then the nerve in question is a subcutaneous branch of the ramus supramaxillaris of the trigeminal.

It is difficult to imagine why a single medianly placed organ should be supplied in any way by a branch from one only of two paired lateral nerves.

The two most important facts established by the present series of observations are—

(1.) That Leydig's "frontal organ" exists as a structure comparable to an invertebrate eye, widely distributed amongst Lacertilians. It may, in reference to its position and structure, be perhaps best called the *pineal eye*.

(2.) That the eye is connected by a medianly placed nerve with the proximal portion of the epiphysis, and thus with the dorsal surface of the brain in the median line.

There can be further little doubt that this nerve is the remains of the part connecting the distal with the proximal part of the epiphysis, that it is in other words formed as the optic nerve from a hollow outgrowth of the brain, which subsequently becomes solid.

The structure of the eye in two or three typical cases is as follows:—

(*a.*) *Hatteria.*—In this form the organ is well developed, and being through Prof. Moseley's kindness enabled to procure a fresh specimen, it has been possible to determine the elements comprising the optic vesicle.

In all the eyes yet examined a lens is present. Von Graaf figures it in Anguis as separated from the hinder part of the vesicle, but this does not hold good for any of those examined during the course of this work. The lens, on the contrary, appears to be only the modified anterior portion of the optic vesicle with the hinder walls of which it is directly continuous. In Hatteria it is somewhat cone-shaped, with a broad base corresponding to the anterior surface of the vesicle; it is distinctly cellular, the nuclei being well marked, and the cells having a definite arrangement.

The walls of the vesicle posterior to the lens consist of the following elements:—(1.) A layer of rods bordering the vesicle internally, deeply imbedded in dark pigment, arranged as seen when the rods are separated so as to give the latter a clearly marked striated appearance. (2.) External to these is a layer composed of rounded nucleated elements, two, and in fact possibly three, rows deep. (3.) External to this what may be called a molecular layer, consisting of finely punctated material, through which seems to run a supporting structure;

processes from the structures on both sides of the layer seem to run into its substance. (4.) External to the molecular layer an outer part, in which three kinds of elements may be distinguished: (*a*) round nucleated cells, somewhat larger than those of the inner layer; (*b*) rod-like structures, somewhat conically shaped, with their broad ends external; (*c*) small nucleated spindle-shaped elements, placed between the latter at their bases. (5.) Though difficult to trace, a fine layer of nerve fibres appear to spread round the vesicle from the nerve which enters it at the surface nearest the epiphysis. The elements are connected serially, though processes from the rods may be seen passing at times directly into the molecular layer, or the rod elements externally, or even right through to the external surface. Such processes are accompanied by pigment, and may in some cases merely indicate supporting structures.

In Hatteria, as in other forms, a special bundle of rods lying in the optic axis is highly developed, being much lengthened out and running down into the nerve, their outer extremities being in connexion with a particular group of nucleated cells.

Von Graaf describes in Anguis a layer of small rod-like structures, similar apparently to those found in many invertebrate eyes, though he is not certain as to their nature. In Hatteria and other forms examined, the vesicle appears to have been filled during life by a fluid material, and this in coagulating adheres to the wall. The coagulation often apparently sets in from definite points, and these being the ends of rods, gives the appearance, under certain conditions of light, of refracted processes attached to these structures.

The *nerve* enters the vesicle posteriorly, certain fibres appear to enter into connexion with the cells connected with the specialised rods, the remainder spread out around the external surface of the vesicle, and here enter into connexion with the elements, that is, the rods bound the internal surface of the vesicle, and the nerve-fibres the external.

The nerve, whilst differing in appearance from an ordinary one, yet resembles more closely than anything else the developing optic nerve, being formed of long spindle-shaped elements, which recall the stage passed through when the at first round cellular elements of the optic stalk are gradually lengthening out. The nerve in both cases developes in a similar manner.

The whole eye lies in a special capsule of connective tissue into which enters and breaks up a blood-vessel, this vessel being present in connexion with the eye in all Lacertilia examined, even in those in which a nerve could not be distinctly traced.

(*b*.) *Lacerta ocellata.*—In this form the organ lies considerably below the surface, and so shut in by bone that it may be said to lie within the skull. The dura mater which surrounds it is deeply pigmented, and the presence of the branched pigment cells renders the examination of its structure very difficult.

A well developed cellular lens is present, formed from the anterior part of the vesicle apparently. The retinal elements are imbedded in pigment, and, save the rods, are difficult to detect, though by careful examination two rows of round nucleated cells may be detected. The pigment obscures the nerve, which is nothing like so clearly marked as in Hatteria, due largely to the dura mater encasing the eye so closely that no capsule is formed.

The nerve enters posteriorly, and a slight differentiation of the rods at two points may be noticed, the nerve appearing to divide into two just before entering the eye; it passes down, lying in the dura mater to join the proximal part of the epiphysis, which is itself deeply pigmented.

The blood-vessel accompanying the nerve is well developed.

(*c.*) *Iguana.*—The structure agrees in the main with that of Hatteria, though, owing to the eye being not so well preserved as in the latter, the elements cannot be so clearly differentiated. The lens is cellular, and somewhat similarly shaped to that of Hatteria; the rods are as usual deeply pigmented, and external to them may be detected (1) a row of round nucleated cells; (2) a well-marked molecular layer, in which the nucleated cells are often embedded; and (3) an outermost layer of cone-shaped bodies, similar to those of Hatteria.

The rods in the optic axis are again lengthened out and prominent, running down into the nerve.

The eye lies in a capsule of connective tissue within the foramen, and into the same space passes also a hollow process from the epiphysis, into which the nerve enters.

In another Iguana examined the process appears not to be hollow, and the eye, instead of lying in a capsule, is closely invested by connective tissue.

In both cases the vesicle is filled with a coagulation, indicating the presence of a fluid material in life.

(*d.*) *Anolius* (sp.?).—The eye lies close beneath the skin, and almost entirely fills up the parietal foramen, the remainder being occupied by vacuolated tissue, in which large nucleated cells are present at intervals, together with branching pigment cells.

The eye is elongated in the direction of the optic axis, and provided with a nerve running back through the vacuolate tissue and entering the proximal part of the epiphysis.

Pigment is largely developed, ensheathing all the elements of the retina; the rods may be traced into rounded elements; these again externally into cone-shaped elements. The rods in the optic axis are again modified and prominent, and their ends facing into the vesicle appear striated.

The lens is cellular, and has a slight development of pigment in some of its cells in the optic axis.

Special connective tissue fibres pass, as in some other forms, from the capsule to the edge of the lens.

(*e.*) *Leiodera nitida.*—This may be taken as the type of several forms, such as *Calotes ophiomaca* and *versicola* and *Seps chalcidica*, in which the eye is lengthened out in a direction at right angles to the optic axis. The lens is distinctly cellular, and continuous with the hinder walls of the vesicle. The retinal elements consist of (1) rods; (2) a layer of round nucleated cells; (3) a well marked clear space, corresponding in position to the molecular layer; and (4) an external layer of cone-shaped elements.

In these forms a nerve cannot be detected with certainty, though very possibly with freshly killed specimens its presence might be demonstrated.

In Leiodera the scale above the eye is beautifully modified, and a transparent dome-shaped cornea developed, sections showing that the pigment is absent from the scale in this region, though very abundantly developed elsewhere.

(*f.*) *Varanus Bengalensis and giganteus.*—The eye in these forms will be dealt with fully on a subsequent occasion; at present one point only will be mentioned. In two specimens examined (perhaps of different species of Varanus) the connexion with the proximal part of the epiphysis was of an importantly different nature; in one it was in the form *of a hollow process*, in the other of a *solid stalk*, much as in Hatteria.

There can be no doubt that the connecting parts in the two instances are equivalent to each other.

(*g.*) *Cyclodus gigas* (?).—This may be taken as the type of those forms, in which no structure comparable to an eye is at present found. The distal extremity of the epiphysis is swollen out, the cells of its walls, which are thrown into folds, become shaped like those of cylindrical epithelium, and amongst them pigment is deposited, but no true retina is formed, or any structure comparable to a lens.

The distal swollen part of the epiphysis is enveloped in pigment in the dura mater, some distance in front of the proximal part, with which it is connected by a hollow process. The whole structure lies on the inner side of the cranium, closely fitted to the bone, as in *Lacerta ocellata.*

The scale on the surface of the head is imperfectly modified to form a cornea, and has the appearance of degenerating.

XV. "Star Photography; the Effects of Long and Short Exposures on Star Magnitudes." By ISAAC ROBERTS, F.R.A.S. Communicated by the Rev. S. J. PERRY, F.R.S. Received May 21, 1886.

[Publication deferred.]

XVI. "An Instrument for the speedy Volumetric Determination of Carbonic Acid." By W. MARCET, M.D., F.R.S. Received June 9, 1886.

[Publication deferred.]

XVII. "On the Practical Measurements of Temperature; Experiments made at the Cavendish Laboratory, Cambridge." By H. L. CALLENDAR, B.A., Scholar of Trinity College, Cambridge. Communicated by J. J. THOMSON, F.R.S., Professor of Experimental Physics at the Cavendish Laboratory. Received June 9, 1886.

[Publication deferred.]

XVIII. "The Determination of Organic Matter in Air." By Professor T. CARNELLEY and WILLIAM MACKIE. Communicated by Sir H. E. ROSCOE, F.R.S. Received June 10, 1886.

[Publication deferred.]

XIX. "The Carbonic Acid, Organic Matter, and Microorganisms in Air, more especially of Dwellings and Schools." By Professor T. CARNELLEY, J. S. HALDANE, and Dr. A. M. ANDERSON. Communicated by Sir H. E. ROSCOE, F.R.S. Received June 10, 1886.

[Publication deferred.]

XX. "Preliminary Report on the Pathology of Cholera Asiatica (as observed in Spain, 1885)." By C. S. ROY, F.R.S., J. GRAHAM BROWN, M.D., &c., and C. S. SHERRINGTON, M.B. Received June 10, 1886.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 18th.

Presents, May 27, 1886.

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June 10, 1886.

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NOTICES TO FELLOWS OF THE ROYAL SOCIETY.

After October 1st the Library will be open from 11 A.M. to 6 P.M., except on Saturdays, when it will be closed at 1 P.M.

A printed post-card of the papers to be read at each meeting will be sent weekly to any Fellow upon application to Messrs. Harrison and Sons, 46, St. Martin's Lane, W.C.

Applications and Reports to be considered at the November Meeting of the Government Grant Committee must be sent in by September 30th.

HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,
AND ALL BOOKSELLERS.

OBITUARY NOTICES OF FELLOWS DECEASED.

Captain Sir Frederick J. O. Evans, R.N., K.C.B. There have been few men perhaps who, launched into the active and engrossing professional duties of the Naval Service, at a period of life when their compeers in other callings had scarcely entered on their college course, who have achieved so solid a reputation for scientific eminence as the subject of this notice.

The only son of Mr. John Evans, a Master in the Royal Navy, Frederick Evans was born at Southsea, in March, 1815, and at the age of thirteen entered the Navy on board His Majesty's ship "Rose," employed on the North American coast, and subsequently joined the "Winchester," carrying the flag of Sir E. G. Colpoys on the same station.

In 1833 he was transferred to the surveying vessel "Thunder," employed in the West Indies, and under the auspices of her gifted captain, Richard Owen, he found the opportunity of cultivating the science of nautical astronomy, surveying, and other branches of knowledge, and thus laid the foundation of his subsequent long, useful, and distinguished career. In 1836 he joined His Majesty's ship "Caledonia," the flagship in the Mediterranean, and afterwards erved in various other vessels on that and the African stations. He soon gained for himself the reputation of a skilful and accomplished navigator, and was rewarded by early promotion to the rank of master.

In the year 1841 Mr. Evans was selected to accompany Captain Francis Price Blackwood in command of an expedition, consisting of Her Majesty's ships "Fly" and "Bramble," for the survey of the eastern coast of Australia, and was attached to the former vessel as master and senior surveyor. For more than four years he took a very leading part in the important but harassing service of exploring and surveying—in a ship without the aid of steam—the intricate channels leading from the Coral Sea through the Barrier Reefs, the passages through Torres Strait, and the southern shores of New Guinea, regions then almost entirely unknown to the navigator. A narrative of this remarkable voyage was published in 1847 by J. B. Jukes, the naturalist to the Expedition.

After a short period of surveying service on the coasts of England, he was appointed to Her Majesty's ship "Acheron," fitting out under the late Admiral Stokes, for the survey of the coasts of the then young colony of New Zealand. On this service he was arduously engaged for a period of four years, and seamen of all nations are in no small degree indebted to him for the excellence of the charts of these coasts, which have been in constant use to the present time.

In these two important expeditions Mr. Evans gained for himself the reputation of a scientific surveying officer, second to none in his profession. His great forte had always been extreme accuracy of observation and fidelity in execution, two qualities perhaps in no profession of more vital importance than in the calling of the nautical surveyor. Always painstaking and patient, sometimes thought to be over-fastidious, yet he was never outstripped in the race by his more ardent contemporaries, and the accuracy of his work was never questioned; during these two voyages he had paid considerable attention to the science of terrestrial magnetism, and had made frequent observations on the three elements, thus in a measure preparing himself for the important duties which were destined to devolve on him at a later period of his career.

After his return to England, at the end of 1851, he was for a considerable time employed at the Hydrographical Department of the Admiralty, in preparing the charts of New Zealand for publication, and on other nautical duties connected with that survey.

His abilities as a surveying officer had long been noticed by the then chief of the department, Sir Francis Beaufort, who was never slow to recognise and reward real merit; and it was on his nomination that, at the commencement of the Russian war, Mr. Evans was employed on reconnoitring service with the Baltic Fleet; for a year he was vigorously engaged on inshore service on the Russian coasts, and was present at the operations against Bomarsund and among the Aland islands, for which his name was mentioned in Gazetted Despatches; with this service his active career afloat ceased, after twenty-six years of constant employment, both in the regular and surveying branches of the Navy, and surely no man was more entitled than himself to look back with pride and satisfaction on a career of unremitting labour and enduring usefulness for so long a period.

A new era was now about to commence in the construction of the ships of the Royal Navy, and Captain Evans was destined for the next thirty years to take a leading part in the development of a science upon which the safety of their navigation was mainly to depend.

In 1855 he was appointed Chief of the Compass Department of the Admiralty; at this time our extensive Fleet was wholly of wood, partly composed of steam and partly of sailing ships. Our experience of the effects of magnetism in the iron vessels which had been constructed in the Mercantile Marine was extremely limited; the science of the subject, so far as it extended, was alone known to two or three individuals in Europe, and even that science was extremely mistrusted by all practical men, and further, numerous, and especially one awfully fatal shipwreck (that of the "Royal Charter") had occurred to iron merchant vessels, which were officially traced to compass errors.

The force of circumstances at the close of the Russian war, as is well known, rapidly brought about the conviction that our war ships must be iron, and not only this, but that they must be armour-plated, and thus almost suddenly a question presented itself to science and practical knowledge, the difficulties of which have probably rarely been surpassed, viz., how to overcome the intricacies of magnetism in such ships. That the difficulty was surmounted is of course known, though at what cost of close and laborious investigation, theoretical and practical, for years, is known but to few.

That it was successfully and triumphantly overcome is mainly due to two individuals, viz., the late Mr. Archibald Smith, F.R.S., and the subject of this notice; and their names will ever be associated with the solution of a question the importance of which to a maritime nation cannot be over-estimated.

It is unnecessary to enter here into the scientific merits of Archibald Smith, they are of world-wide renown; suffice it for our purpose to say, that he was a mathematician of the highest order, had long gone into the question of terrestrial magnetism in connexion with compass disturbance *con amore*, had cheerfully joined Captain Evans in all his labours, and until his lamented death in 1872, was joint author and fellow worker in all their investigations.

The writer cannot here refrain from quoting words which have already appeared in connexion with this subject, and which so well express the connexion between the two men.*

"The subject was one which called for the combination of practical sagacity and experience, with refined scientific method, and if Archibald Smith was stronger on the one side, Captain Evans was his master on the other; nor was either of them without large powers, even in the special department of their joint labours, in which he owned the supremacy of his friend; it was an undertaking which called for the united efforts of just two such men as were fortunately brought together to do it, and the result has been a triumph to England, and a blessing to the world, which will preserve the memories of its authors as long as the ocean remains the highway of Englishmen and the world."

The first work which Captain Evans produced after his appointment as Chief of the Compass Department, was a chart of the world, showing the curves of equal magnetic declination; by which the navigator was enabled to obtain with approximate accuracy in any part of the ocean the deviation of his compass from the magnetic meridian; this chart was published by the Admiralty in 1858.

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* "Nature," January 14th.

In 1859 he read a paper at the Royal United Service Institution, on the magnetism of iron ships, showing all that had been done up to that date in acquiring a knowledge of their magnetism and the treatment of their compasses.

His next paper was a report to the Admiralty of the magnetic character of the various types of iron ships in the Navy, and of the "Great Eastern" steamship. The results of this paper were to show the best magnetic direction for building an iron ship, the best position for placing her standard and other compasses, and the various sources of error affecting a compass under the most favourable conditions. This report was communicated to the Royal Society, and printed in the Transactions for 1860.

A joint paper on the proper length and arrangement of the needles on a compass card, with exact information as to proper arrangement of magnet and soft iron correctors with respect to it, was communicated to the Royal Society in 1861.

In 1862, the Admiralty published "The Manual for the Deviation of the Compass in Iron Ships;" this important work was the result of the joint labours of Evans and Archibald Smith. It was immediately translated into the principal European languages, and became the text-book of the maritime world. On the introduction of new types of armour-plated ships, new editions of the work became from time to time necessary, and were published.

In 1865 the joint authors produced another important paper on the magnetic character of the armour-plated ships of the Navy up to that date. The novelty of the form of ships thus discussed rendered the results of more than usual interest, and proved among other things with what degree of confidence compasses might be placed in positions where armoured protection would be afforded; this paper was also printed in the Transactions of the Royal Society.

The practicability of determining the magnetic coefficients without the labour of swinging, and the heeling error, without inclining the ship, was also demonstrated, and has been practically adopted in the Navy, with great saving of labour and expense.

In 1865, on the retirement of Captain Becher from the Hydrographic Department, Captain Evans succeeded him as Chief Assistant to the Hydrographer, retaining the position of Chief of the Magnetic Department. From this time his practical duties in regard to compass management devolved on his successor, though he never relaxed his investigations, or wearied in his devotion to the science of terrestrial magnetism, and in 1870, desiring to present the subject of compass deviation in a less elaborate form than had been done in the former publication, he brought out his "Elementary Manual," a work which was very well received, and was also translated by the maritime nations.

In the year 1874, a vacancy occurring for the post of Hydrographer of the Admiralty, he was selected to fill it; his habits of extreme accuracy and method, and his ripe and extended knowledge of the numerous and varied subjects which came within the province of that department to investigate and to deal with, rendered him peculiarly fitted to fulfil the duties of the position, which he continued to perform with equal ability and conscientiousness till within a little more than a year of his death; the multifarious calls of his new office, however, diverted him more and more from exclusive attention to his favourite science, though he still found time to draw up and read before the Royal Geographical Society in 1878, an able and instructive lecture on the Magnetism of the Earth, showing the distribution and direction of its magnetic force, and the changes in its elements, as then known.

Drawn together by the sympathies engendered by similar tastes and pursuits, he and the late Sir Edward Sabine had long been close friends and coadjutors, and during the last years of the labours of that distinguished *savant*, Evans had rendered him assistance in completing his great work, "Contributions to Terrestrial Magnetism," contained in the fifteen numbers of the "Magnetic Survey of the Globe," for the epoch 1842–5.

Science in this country has not always met with official encouragement, or reward, whether in, or outside of the public service, though it is the nation which is chiefly the gainer by it; and it was late in life before the valuable services rendered by Captain Evans to his own and other countries received any kind of recognition, though honorary rewards were proffered by foreign Governments. The explanation is perhaps, that the remedy for a serious difficulty was discovered before its existence had generally made itself felt, and that in the Naval profession it has been always a maxim that impossibilities arise only to be overcome in the ordinary course of duty.

In order to show, however, that these services were appreciated by the department with which he was immediately associated, the writer of this notice is induced to give an extract from a statement which nearly seventeen years ago it became his duty to place before the authorities in regard to them:—

. . . . . "When he found himself in the responsible position of head of the Compass Department, at a time when the complete revolution in shipbuilding raised the serious question whether it was possible so to deal with the magnetic influence of the iron which entered so largely into the construction and fittings of a modern ship as to retain the efficiency of the compass, he readily entered upon a task involving years of close and laborious investigation, experimental and theoretical, of the intricacies of an iron ship's magnetism; nor did he ever shrink from the responsibility of acting on his acquired knowledge, and promptly recommending and carrying out improvements of system consequent on the change of circumstances, viz., an iron fleet superseding a wooden one. The compass system for the secure navigation of the Fleet under its remarkable changes, has grown entirely under his own hands, and has been marked by the following conditions:—

"1st. Entire success; for to the present time not a single ship of the Fleet has been lost or hazarded by default traceable to her compasses.

"2nd. The principles involved are accepted by the navies of Europe and America.

"3rd. The scientific value of his labours has in this country been recognised by his election into the fellowship of the Royal Society.

"4th. The labours, whether experimental, or for purposes of investigation, by seeking the aid of men of science outside the naval profession, have cost the country nothing.

"5th. Officers have been instructed by him, and works on the whole subject written and published, so that the knowledge has been diffused, and the results cannot be lost.

"But in addition to the security of the fleet and the establishment of the foundation of correct principles, he can claim the advantage that has accrued to the State of having by the system pursued, economised the great expenses that must have been incurred, had no system based on pure science existed: I allude to the time and enormous labour involved in swinging and heeling ships; this for all ordinary cases has long been dispensed with, and the swinging alone is confined to times and circumstances, when it is chiefly necessary to give confidence to the officers of the ship.

"There is another point on which these sound principles operate: they effectually bar the door to individuals who come with quasi inventions, sometimes backed by officers of rank, well meaning but necessarily ignorant of the subject, professing to relieve all the troubles of compass management on board ship, and to leap over at a bound the labour of years of investigation, and all past experience! The Admiralty is the special target at which these inventors aim, and the Government cannot be ignorant of a recent case where a very large and well supported demand on their credulity, and the public purse to follow, was defeated by the wise and persistent course of action adopted by Captain Evans; these examples, more or less in their attempts on the national purse and on his responsibilities, have abounded during his tenure of office and mine, sometimes I fear to the no small engenderment of ill will and jealousy, but I am sure not to the diminution of the general respect for his character and recognition of his abilities." . . .

Captain Evans sat for many years on the Council of the Royal

Society, and was more than once a Vice-President. He was also a Fellow of the Royal Astronomical and Geographical Societies; he served for many years as a member of the Meteorological Committee of the Royal Society, and on the change in the constitution of that body he became a member of its Council.

In recognition of his public services the Companionship of the Bath was conferred on him in the year 1873, and in 1881 he was advanced to the Commandership of the same order on the recommendation of the Earl of Northbrook, the First Lord of the Admiralty, under whom he had served during the last five years of his career.

Sir Frederick Evans' last public service after his retirement from the Admiralty in June, 1884, was as the British delegate to the International Congress at Washington, convened for the establishment of a prime meridian, when that of Greenwich was adopted as the starting point from which longitude should in future be universally computed.

He died on the 20th of December, 1885.

G. H. R.

II. *Steel.*

The behaviour of steel varies greatly with the hardness and temper of the metal. More experiments than I have hitherto made would be necessary to establish the general laws with certainty; but my results are consistent with the following conclusions:—

1. In soft steel magnetisation produces elongation, which increases up to a certain value of the magnetising force, and afterwards diminishes. The maximum elongation is less than in the case of iron, and the rate of diminution after the maximum is passed is also less.

2. The critical value of the magnetising force for a steel bar diminishes with increasing hardness of the steel up to a certain point corresponding to a yellow temper, after which it again increases, and with very hard steel becomes very high.

3. In soft steel a strong magnetising force subsequently diminished may cause a greater temporary elongation than the diminished force is capable of producing if applied in the first place.

4. A temporary elongation when once produced in soft steel may be maintained by a magnetising force which is itself too small to originate any perceptible elongation.

III. *Nickel.*

1. Nickel continues to retract with magnetising forces far exceeding those which produce the maximum elongation of iron. The greatest retraction of nickel hitherto observed is more than three times as great as the maximum elongation of iron, and the limit has not yet been reached.

2. A nickel wire stretched by a weight undergoes retraction when magnetised.

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