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Obituary Notices :—

OBITUARY NOTICES OF FELLOWS DECEASED.

Charles Brooke was born on the 30th June, 1804. He was the son of the well-known mineralogist Mr. H. J. Brooke. His early education was carried on at Chiswick, under Dr. Turner; after this he studied at Rugby; and from thence he went to Cambridge. He remained there five years, and graduated in Arts, taking honours as a Wrangler. During the latter part of this period he studied medicine, and his professional education was completed at St. Bartholomew's. He lectured for one or two sessions on surgery, at Dermott's School, and afterwards held positions on the surgical staff of the Metropolitan Free Hospital and the Westminster Hospital. At the latter he continued to lecture till a short time before his death. In 1844 the Royal College of Surgeons conferred on him an Honorary Fellowship; in 1847 he was elected Fellow of the Royal Society, at the evening meetings of which he was a very regular attendant, and frequent speaker, especially on physical questions. He belonged to the Meteorological and Royal Microscopical Societies, and occupied the President's chair in each of these bodies; he also at various times served on the management of the Royal Institution, and on the Council of the Royal Botanical Society. In addition to these he was connected with many philanthropic and religious societies; and was a very active member of the Victoria Institute and Christian Medical Association.

He was much esteemed in his profession as a surgeon; and is known as the inventor, forty years ago, of the "bead suture," which was a great step in advance in the scientific treatment of deep wounds.

His published papers and lectures have almost always pertained to the department of physics, mathematical and experimental; and his more especial work was the inventing or perfecting of apparatus. His papers date back to 1835, when he wrote upon the Motion of Sound in Space; but the work upon which his reputation mainly rests was published between 1846 and 1852. This was the invention of those self-recording instruments which have been adopted at the Royal Observatories of Greenwich, Paris, and other meteorological stations. They consisted of barometers, thermometers, psychrometers, and magnetometers which registered their variations by means of photography. His method obtained the premium offered by the Government for such apparatus, as well as a Council Medal from the Jurors of the Great Exhibition. These instruments record automatically, by very beautiful arrangements, the continual changes in the dip and declination of the needle, and in the intensity of the magnetic force. This could not be arrived at by any amount of personal observation; and the movements of a spot of light upon the revolving sheet of chemically prepared paper are more to be trusted than the perceptions of the human eye. The adoption of these instruments at the Greenwich Observatory rendered it possible to reduce the staff of assistants and to dispense with night duty, while they revealed magnetic disturbances of a character which otherwise would probably have escaped notice. The perfecting of these apparatus involved many considerations both of a mathematical, physical, and chemical nature, which will be found detailed in the "British Association Reports," from 1846 to 1849, and in the "Philosophical Transactions" of 1847, 1850, and 1852.

Mr. Brooke also studied the theory of the microscope, and was the author of some inventions which facilitated the shifting of lenses, and improved the illumination of the bodies observed. He also applied his improved methods to the investigation of some of the best known test objects of the microscope. (See "Proc. Roy. Soc.," vol. vii, p. 139.) In later years his papers were of a more theoretical character, bearing upon the dynamical theory of electricity, on the nature of electric energy, and on the supposititious character of that ether which is assumed to pervade all matter and space. These are to be found principally in the "Phil. Mag." for 1866, and in the "Proc. Roy. Soc.," vol. xv. He was also the author of various papers and lectures bearing on the relations of natural science and religion.

He is, however, most popularly known by means of the "Elements of Natural Philosophy," originally compiled by Dr. Golding Bird, but which after the death of that physician was re-written and enlarged by Mr. Brooke on bringing out the fourth edition. Since that time new editions have been constantly called for; the results of more recent discovery have been carefully added, and the book has remained a favourite introduction to the study of the physical sciences, especially in the medical schools.

Mr. Brooke died at Weymouth on the 17th of May, 1879, leaving a widow and family, and many friends who recognized him as one of the most kindly and modest of men. He will be remembered on account of his pleasant conversation, or his thoughtful advice, or the quiet influence of his private life, by many who knew little or nothing of his varied acquirements and ingenious inventions.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

December 18, 1879.

THE 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 Secular Changes in the Elements of the Orbit of a Satellite revolving about a Tidally Distorted Planet." By G. H. DARWIN, F.R.S. Received December 8, 1879.

(Abstract.)

This memoir is a continuation of four previous papers on similar subjects.*

As the investigation is entirely analytical, and is rather long and complicated, it seems useless to attempt a detailed abstract, and I shall therefore confine myself to giving an outline of the results, and a few remarks on the method employed. I also here partially replace the analytical treatment of the paper itself by general reasoning, so as to give some idea of the physical causes which underlie the definite results of analysis. These general considerations are not, however,

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* 1. "On the Bodily Tides of Viscous and Semi-elastic Spheroids, and on the Ocean Tides upon a Yielding Nucleus."

2. "On the Precession of a Viscous Spheroid, and on the Remote History of the Earth."

3. "On Problems connected with the Tides of a Viscous Spheroid."

These three will appear in the "Phil. Trans." for 1879.

4. "The Determination of the Secular Effects of Tidal Friction by a Graphical Method." "Proc. Roy. Soc.," vol. xxix, 1879.

strictly appropriate to an abstract, since they do not occur in the paper, and merely serve as a rough substitute for analysis.

In this and the previous papers it is supposed that tides are raised in a planet by its satellites, and the problem is to determine the various effects which result from the friction of those tides.

The hypothesis generally adopted in these papers is that the planet is a viscous body, and that the tides are a bodily distortion of the whole mass of the planet, but nearly all the results would also follow from the friction of oceanic tides upon a rigid nucleus.

The investigation is principally directed towards the case of the earth, sun, and moon, and the phraseology of the paper is taken from our own planet and satellite; but the methods may be extended to the other planets.

The subject will be most easily explained by inverting the order of the paper, and by beginning with the sketch of the results.

Sketch of Results.

At the present time the moon revolves round the earth in 27·3 days. The orbit has an eccentricity of $\frac{1}{18}$, and is inclined at an angle of 5° 9′ to a certain plane, which is said to be "proper to the orbit." This proper plane is inclined to the ecliptic at an angle of about 8″, and intersects the ecliptic in the equinoctial line; it lies on the same side of the ecliptic as the earth's equator.

In this statement the "periodic inequalities" of the moon's motion are neglected.

In this and the previous papers it is proved that frictional tides in the earth are causing, and must have caused, changes in the configuration of the system. The changes in the past may be summarised as follows:—

1. The lunar period must have been shorter in the past, and may be traced back from the present 27·3 days, until initially the moon revolved round the earth in from 2 to 4 hours.

2. The inclination of the orbit to the proper plane must have been larger in the past, and may be traced back from the present 5° 9′ until it was 6° or 7°. This 6° or 7° was a maximum inclination, and in the more remote past the inclination was less, and initially was very small, or zero.

3. The inclination of the proper plane to the ecliptic must have been greater in the past, and may be traced back from the present 8″, until it was in very early times about 11° 45′. It is possible that initially this inclination was less, and that the 11° 45′ of inclination was a maximum value.

4. The eccentricity of the orbit must have been smaller in the past. Either at one time it had a minimum value, before which it had a

maximum value, and again earlier it was very small, or zero; or else the maximum value never occurred, and the eccentricity has always been increasing. The history of the eccentricity depends on the nature of the tides in the earth, but the former of these alternatives seems the more probable.

We will now consider the earth.

At the present time the earth rotates in 24 hours, its equator is inclined at an angle of about 9″ to a plane, which is called in this paper "the proper plane of the earth." This proper plane is inclined at an angle of 23° 28′ to the ecliptic, and its intersection with the ecliptic is the equinoctial line.

(In the ordinary mode of statement the proper plane is called the mean equator, and the true equator is described as nutating about the mean equator with a period of 19 years, and an amplitude of 9″.)

It is here proved that the frictional tides in the earth have caused changes, which may be summarised as follows:—

5. The earth's period of rotation, or the day, must have been shorter in the past, and it may be traced back from the present value of 24 hours, until initially it was from 2 to 4 hours in length. It was then identical with the moon's period of revolution as described in (1).

6. The inclination of the equator to "the earth's proper plane," must have been larger in the past, and may be traced back from the present value of 9″, until it was about 2° 45′. This 2° 45′ was a maximum inclination, and in the more remote past the inclination was less, and initially it was very small, or zero.

7. The inclination of "the earth's proper plane" to the ecliptic must have been smaller in the past, and may be traced back from its present value of 23° 28′, until initially it was 11° 45′, or perhaps somewhat less. It was then identical with the proper plane of the lunar orbit; and this is true whether or not 11° 45′ was a maximum inclination of the lunar proper plane to the ecliptic, as described in (1).

The preceding statements may be subject to varieties of detail, according to the nature of the tides raised in the earth, but the above is a summary of what appears to be the most probable course of evolution.

The hypothesis which is suggested as most probable is, that the more recent changes in the system have been principally due to oceanic tidal friction, and that the more ancient changes were produced by bodily tidal friction.

These seven statements, when taken together, exhibit the earth and moon initially nearly in contact; the moon always opposite the same face of the earth, or moving very slowly relatively to the earth's surface; the whole system rotating in from 2 to 4 hours, about an axis inclined to the normal to the ecliptic at an angle of 11° 45′, or somewhat less; and the moon moving in a circular orbit, the plane of which is nearly coincident with the earth's equator.

This initial configuration suggests that the moon was produced by the rupture, in consequence of rapid rotation or other causes, of a primeval planet, whose mass was made up of the present earth and moon. The coincidence is noted in the paper, that the shortest period of revolution of a fluid mass of the same mean density as the earth, which is consistent with an ellipsoidal form of equilibrium, is 2 hours 24 minutes; and that if the moon were to revolve about the earth with this periodic time, the surfaces of the two bodies would be almost in contact with one another.

Tidal friction is a *vera causa*, and the only postulates of this theory of the evolution of our system are lapse of time, and the non-existence of sufficient diffused matter to materially affect the motions of the moon and earth through space.

The systems of the other planets of the solar system are reviewed from the point of view of this tidal theory of evolution, and it is found that there are many confirmatory circumstances, and none which appear condemnatory. But as the present investigation only treats of a planet with a single satellite, it necessarily leaves many points untouched. In relation to this theory, the most interesting points are the satellites of Mars, and the inclinations of the orbits of Jupiter's satellites to their proper planes.

Notes on the Method of Investigation—General Reasoning in Substitution for Analytical Treatment.

The following are the titles of the several parts into which the paper is divided:—

I. The theory of the disturbing function.

II. Secular changes in the inclination of the orbit of a satellite.

III. The proper planes of the satellite, and of the planet, and their secular changes.

IV. Integration for changes of the inclination of the orbit, and of the obliquity of the ecliptic.

V. Secular changes in the eccentricity of the orbit of a satellite.

VI. Integration for changes of eccentricity of the orbit.

VII. Summary and discussion of results.

VIII. Review of the tidal theory of evolution as applied to the earth, and the other members of the solar system.

These titles indicate the method of treatment.

The application of the method of the disturbing function to the present problem has certain peculiarities. The attraction of the tides raised in the planet by a satellite is the cause of the perturbation of the satellite's motion. Now the state of tidal disturbance of the planet depends upon the position of the satellite, therefore the elements of the satellite's orbit will appear in the disturbing function, as representing the state of tidal disturbance in the planet; but these elements also appear as representing the position of the satellite as a body whose motion is perturbed. In order to apply the method of the disturbing function they ought only to enter in the latter capacity. This difficulty is overcome by supposing the earth to have two satellites; one the tide-raising satellite (called Diana in the memoir), and the other the perturbed satellite or moon. Then, after the application of the method of the disturbing function, the tide-raising satellite is made identical with the moon; and thus the effect of the lunar tides on the moon's motion is determined. Or else the tide-raising satellite is made identical with the sun, so as to find the effect of the solar tides on the moon. The method of the disturbing function is also applied to determine the perturbations of the earth's rotation, and a similar artifice has to be used, because the earth has to be treated in two capacities, *first* as a body in which tides are raised, and *secondly*, as a body whose rotation is perturbed.

The problem is divided into the two following cases:—

1st. Where the lunar orbit is circular, but inclined to the ecliptic.

2nd. Where the orbit is elliptic, but coincident with the ecliptic.

The previous paper on "Precession," dealt with the mean distance of the moon, and with the rotation of the earth and the obliquity of the ecliptic; therefore, in the present paper the inclination and eccentricity afford the principal topics.

The first of these problems occupies the larger part of the paper. If the satellite and planet be the only bodies in existence, the problem of the inclination is not very complicated.

The following considerations (in substitution for the analytical treatment of the paper) will throw some light on the general effects of tidal friction:—

Suppose the motions of the planet and of its solitary satellite to be referred to the invariable plane of the system. The axis of resultant moment of momentum is normal to this plane, and the component rotations are that of the planet's rotation about its axis of figure, and that of the orbital motion of the planet and satellite round their common centre of inertia; the axis of this latter rotation is clearly the normal to the satellite's orbit. Hence the normal to the orbit, the axis of resultant m. of m., and the planet's axis of rotation, must always lie in one plane. From this it follows that the orbit and the planet's equator must necessarily have a common node on the invariable plane.

If either of the component rotations alters in amount or direction, a corresponding change must take place in the other, such as will keep the resultant m. of m. constant in direction and magnitude.

It appears from the previous papers that the effect of tidal friction is to increase the distance of the satellite from the planet, and to transfer m. of m. from that of planetary rotation to that of orbital motion.

If then the direction of the planet's axis of rotation does not change, it follows that the normal to the lunar orbit must approach the axis of resultant m. of m. By drawing a series of parallelograms on the same diameter and keeping one side constant in direction, this may be easily seen to be true.

The above statement is equivalent to saying that the inclination of the satellite's orbit will decrease.

But this decrease of inclination does not always necessarily take place, for the previous investigations show that another effect of tidal friction may be to increase the obliquity of the planet's equator to the invariable plane, or in other words to increase the inclination of the planet's axis to the axis of resultant m. of m.

Now if a parallelogram be drawn with a constant diameter, it will easily be seen that by increasing the inclination of one of the sides to the diameter (and even decreasing its length), the inclination of the other side to the diameter may also be increased.

The most favourable case for such a change is when the side whose inclination is increased is nearly as long as the diameter. From this it follows that the inclination of the satellite's orbit to the invariable plane may increase, and also that the case when it is most likely to increase is when the m. of m. of planetary rotation is large compared with that of the orbital motion. The analytical solution of the problem agrees with these results, for it shows that if the viscosity of the planet be small the inclination of the orbit always diminishes, but if the viscosity be large, and if the satellite moves with a short periodic time (as estimated in rotations of the planet), then the inclination of the orbit will increase.

These results serve to give some idea of the physical causes which, according to the memoir, gave rise to the present inclination of the lunar orbit to the ecliptic. For the analytical investigation shows that the inclination of the lunar orbit to its proper plane (which replaces the invariable plane when the solar attraction is introduced) was initially small, that it then increased to a maximum, and finally diminished, and that it is still diminishing.

But the laws above referred to would, by themselves, afford a very unsatisfactory explanation of the inclination of the lunar orbit, because the sun's attraction was found to be a matter of much importance.

It was stated above, that if the viscosity of the planet be small, the inclination of the orbit of the solitary satellite to the invariable plane will always diminish; but when solar influence is introduced, the corresponding statement is not true with regard to the inclination of the lunar orbit to the proper plane, for during one part of the moon's history, the inclination to the proper plane would have increased, even if the viscosity of the earth had been small.

It does, however, follow, from the analytical investigation, that if the lunar orbit was primitively coincident with the earth's equator, then the present *amount* of inclination of the lunar orbit to the ecliptic (viz., 5° 9′) is not explicable on the hypothesis of small viscosity of the earth, but is explicable if we suppose that the viscosity of the earth has always been large, as it certainly is at present. The theory which gives the most perfect account of the present amount of inclination of the lunar orbit is, that the more recent changes in the system have been principally due to oceanic tidal friction, and the more ancient principally to bodily tidal friction, with a large degree of viscosity of the earth's mass.

The presence of the sun rendered it expedient to divide the problem of the inclination into three cases:—

1st. When the solar influence is large, as it is at present.

2nd. When the solar influence is small, or *nil.* This is the case to which the above general considerations apply.

And the third case is intermediate between the first and second cases.

For the third case the theory of the proper planes of the moon and earth had to be investigated, and the problem resolved itself into the determination of the secular changes of the positions of the two proper planes, and of the inclinations of the planes of motion of the two parts of the system to their respective proper planes.

The questions involved in these three cases are, however, so complex that it does not seem advisable to enter on them in this abstract.

The second of the two problems, that of the eccentricity of the orbit, is also treated by the method of the disturbing function.

The result, for a viscous planet, shows that in general the eccentricity of orbit will increase; but if the obliquity of the planet's equator be nearly 90°, or if the viscosity be so great as to approach perfect rigidity, or if the periodic time of the satellite (measured in rotations of the planet) be short, the eccentricity will slowly diminish.

When the viscosity is small the law of variation of eccentricity is very simple, and it appears that if eleven periods of the satellite occupy a longer time than eighteen rotations of the planet, the eccentricity increases, and *vice versâ.* Hence, in the case of small viscosity, a circular orbit is only dynamically stable if the eleven periods are shorter than the eighteen rotations.

In the history of a single satellite revolving about a planet of small viscosity, the periods of rotation and revolution start from identity and end with identity; hence the eccentricity rises from zero to a maximum, and ultimately decreases to zero again.

It is also proved, that in the history of a single satellite revolving about a planet of large viscosity, the eccentricity rises very rapidly to a maximum, decreases slowly to a minimum, and then increases again; but the actual degree of viscosity has an important influence on the results.

The following considerations (in substitution for the analytical treatment of the paper) throw some light on the physical causes of these results.

Consider a satellite revolving about a planet in an elliptic orbit, with a periodic time which is long compared with the period of rotation of the planet; and suppose that frictional tides are raised in the planet.

The major axis of the tidal spheroid always points in advance of the satellite, and exercises a force on the satellite which tends to accelerate its linear velocity.

When the satellite is in perigee the tides are higher, and this disturbing force is greater than when the satellite is in apogee.

The disturbing force may, therefore, be represented as a constant force, always tending to accelerate the motion of the satellite, and a periodic force which accelerates in perigee and retards in apogee. The constant force causes a secular increase of the satellite's mean distance and a retardation of its mean motion.

The accelerating force in perigee causes the satellite to swing out further than it would otherwise have done, so that when it comes round to apogee it is more remote from the planet. The retarding force in apogee acts exactly inversely, and diminishes the perigeean distance. Thus, the apogeean distance increases and the perigeean distance diminishes, or in other words, the eccentricity of the orbit increases.

Now consider another case, and suppose the satellite's periodic time to be identical with that of the planet's rotation. Then when the satellite is in perigee it is moving faster than the planet rotates, and when in apogee it is moving slower; hence at apogee the tides lag, and at perigee they are accelerated. Now the lagging apogeean tides give rise to an accelerating force on the satellite, and increase the perigeean distance, whilst the accelerated perigeean tides give rise to a retarding force, and decrease the apogeean distance. Hence in this case the eccentricity of the orbit will diminish.

It follows from these two results that there must be some intermediate periodic time of the satellite, for which the eccentricity does not tend to vary.*

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* The substance of the preceding general explanation was suggested to me in con-

But the preceding general explanations are in reality somewhat less satisfactory than they seem, because they do not make clear the existence of certain antagonistic influences.

Imagine a satellite revolving about a planet, and subject to a constant accelerating force, which we saw above would result from tidal reaction.

In a circular orbit a constant tangential force makes the satellite's distance increase, but the larger the orbit the less does the given force increase the mean distance. Now the satellite, moving in the eccentric orbit, is in the apogeean part of its orbit like a satellite moving in a circular orbit at a certain mean distance, but in the perigeean part of the orbit it is like a satellite moving in a circular orbit but at a smaller mean distance; in both parts of the orbit it is subject to the same tangential force. Then the distance at the perigeean part of the orbit increases more rapidly than the distance at the apogeean part. Hence the constant tangential force on the satellite in the eccentric orbit will make the eccentricity diminish. It is not clear from the preceding general explanation, when this cause for decreasing eccentricity will be less important than the previous cause for increasing eccentricity.

The disturbing causes which tend to make the eccentricity diminish are (i) the principal semi-diurnal tide, (ii) the "faster elliptic semi-diurnal tide," (iii) the "elliptic monthly tide." The increase of eccentricity depends entirely on (iv) the "slower elliptic semi-diurnal tide." If the periodic time of the satellite be long, as measured in rotations of the planet, the importance of the tides (i), (ii), and (iv) are as the numbers 4, 1, and 49 respectively, and the importance of (iii) is very small; if the satellite were to move faster, the importance of (iv) would decrease, and that of (i), (ii), and (iii) would increase.

In the outline of results at the beginning of the abstract, it was stated that the periodic times of revolution and rotation of the moon and earth might be traced back to a common period of from 2 to 4 hours. In the paper on "Precession" the common period was found to be a little over 5 hours in length; but that result was avowedly based on a partial neglect of the sun's attraction. In this memoir certain further considerations are adduced, which show that, while the general principle remains intact, yet the common period of revolution of the earth and moon must initially have been shorter than 5 hours to an amount, which is uncertain but is probably large. The period of from 2 to 4 hours is here assigned, because it is mechanically impossible for the moon to revolve about the earth in less than 2 hours, and it is uncertain how the rupture of the primeval planet took place.

versation by Sir William Thomson, when I mentioned to him the results at which I had arrived.

It is hardly possible that such general reasoning, as has been above applied to the two problems of the present paper, could ever have led to a discovery of the laws of change of the system. This kind of consideration is, however, of some interest as throwing light on the definite results already attained by the accurate methods of analysis.

II. "On Buff's Experiments on the Diathermancy of Air." By JOHN TYNDALL, F.R.S. Received December 10, 1879.

Two years ago, Dr. Hofmann drew my attention to a forthcoming paper by Professor Buff, to which he obviously and naturally attached considerable importance. The paper appeared in the "Philosophical Magazine" for December, 1877. Being much occupied at the time with other matters, I merely glanced at its conclusions, and then laid it aside until I should be able to read it carefully, and, if necessary, to examine it experimentally. Last summer, I, for the first time, read the paper through, and I have recently, more than once, repeated its perusal—reflecting on its methods and conclusions, to the best of my ability, wherever they appeared dark to me.

The principal result of the paper is, that a stratum of dry air, 45 millims. thick, "absorbs from 50 to 60 per cent. of the rays of heat which it receives from a source heated to the temperature of boiling water." The experiments whereby I sought to show the absorption of radiant heat, by a stratum of dry air more than thirty times the thickness of that employed by Professor Buff, to be sensibly *nil*, are, at the same time, pronounced "unreliable."

I once ventured te express the opinion that 10 per cent. of the radiation from the earth is absorbed by the aqueous vapour contained in the first 10 feet of air. The late Professor Magnus urged against me at the time, that were so much heat lodged in so thin a stratum, the deposition of dew would be impossible. Urged against the conclusion that not 10 per cent., but 60 per cent. of the earth's radiation is absorbed, not within 120 inches, but within 2 inches of the earth's surface, the argument of Professor Magnus would have serious force. Under the covering assigned to it by Professor Buff, our planet ought never to suffer from rapid nocturnal chill.

Professor Buff's intention, at starting, was to investigate the conduction of heat by gases, and he employed for this purpose an apparatus similar to that of Professor Magnus, the dangers attending the use of which I have frequently pointed out. A glass cylinder, with its lower edge ground level, was mounted on the plate of an air-pump. Cemented on to the open top of the cylinder was a brass vessel with a polished horizontal bottom, which was heated by the pouring of hot water into the vessel. From this source the heat proceeded through air or any other gas introduced into the cylinder underneath. To measure the heat transmitted, a flattened thermo-electric couple was fixed, in the first experiment, at a distance of 23 millims. below the polished surface; two wires being carried from the couple through the plate of the air-pump to a suitable galvanometer.

It immediately appeared that the experiments were vitiated by the heat conducted from the source down the glass cylinder. Below the level of the thermo-electric couple this heat was even sensible to the touch. Currents, Professor Buff rightly inferred, would, under the circumstances, be unavoidable. He would have free *convection*, whereas his object was to investigate *conduction.* He further concluded, also rightly, that such currents were present in the experiments of Magnus,* although through the disposition of his apparatus, "he probably failed to appreciate the importance" of this source of error.

Thus warned, Professor Buff changed his mode of experiment, and the change will be best described and illustrated in his own words:—"I had observed," he says, "that if the hot liquid in the brass vessel was allowed to cool freely, the astatic needle, nevertheless, after some time, assumed a maximum deflection, indicating a state of equilibrium between the influx and loss of heat. It is known that tolerably strong deflections of the needle of a highly sensitive galvanometer correspond to but slight alterations of temperature at the soldered junction. If, therefore, we know the temperature of the brass vessel (or rather that of its bottom) at the commencement of the experiment, and the temperature of its fluid contents for which the deflection of the needle is greatest, then the thermal equilibrium,† upon which the highest temperature at the soldered junction depends, must be very nearly proportional to the difference of these two temperatures of the brass vessel, at least so long as the thermal differences are small.

"Accordingly the brass vessel, at the commencement of the experiment, was filled with water at such temperature that the needle pointed to 0°, and remained stationary. This having been effected, part of the cold water was taken out and replaced by hot water; the temperature of the mixture was observed, and simultaneously the time of pouring in was noted. The temperatures were taken by a

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* The existence of these currents was not only inferred, but they were rendered visible to the eye many years ago, by means of the illuminated fumes of chloride of ammonium. See section headed "Proof of Convection," Contributions to Molecular Physics, p. 381.

† I have found some difficulty in seizing the exact connexion between this expression and the words which follow it.

thermometer, whose bulb was in contact with the bottom of the brass vessel.

"The following table gives the chief results of some of the experiments carried out in this manner. The figures of the column marked T record for each of the stated gases the temperature of the water:— First, at the commencement of the experiment; secondly, after mixing the hot and cold water; and thirdly, at the moment when the deflection of the needle was at its maximum. The differences between the first and third figures are given under *t*, whilst τ shows the corresponding deflections, whose real and comparable values are placed in brackets underneath them. . . . . The figures under *z* express in minutes the time from the beginning of the experiment until the junction reached its maximum temperature. The tension of the gases in the interior of the glass cylinder is given in millimetres under *p*; and, finally, the last column contains comparable values expressing the intensity of the thermal action at the junction."

	T.	*t.*	τ.	*z.*	*p.*	$\frac{100\,\tau}{t}$.
Dry air	19·8	..	0	0	760	
	56·0					
	47·7	27·9	50·5	22	,,	356·6
			(99·25)			
Air vacuum.........	18·4	..	0	0	15	
	54·6					
	47·0	28·6	48·0	15	,,	309·5
			(88·5)			
Hydrogen	22·5	..	0	0	760	
	57·5					
	50·5	28·0	57·8	17	,,	466·4
			(130·6)			
Rarefied hydrogen...	23·0	..	0	0		
	58·0					
	52·1	29·1	53·0	14	,,	378·0
			(110·0)			

I interpret thus the method here pursued:—Suppose, for the sake of simplicity, the thermometric zero at the bottom of the brass vessel to correspond, at starting, with the galvanometric zero, and supposing the temperature of the bottom of the brass vessel to augment within certain limits; then, so long as the differences remained small, the quantity of heat received by the soldered junction would be proportional to the difference of temperature between the junction and the source. When, under such conditions, the galvanometric degrees, or their values, are divided by the thermometric degrees, a constant quotient ought to be obtained. If, on interposing any gas between the source and the junction, the quotient referred to were observed to change, such change would express the action of the gas; while the quotients obtained with different gases would express their relative actions. It is, I suppose, on considerations such as these that Professor Buff has based his column of "comparable values," where the galvanometric value is multiplied by 100 and divided by the thermometric value.

But the actual experiments of Professor Buff are by no means so simple as the ideal ones here described. He brings, first of all, his needle to zero by the introduction of water of the proper temperature into his brass vessel. Removing a portion of this he pours in hot water, and has to wait for 22 minutes when air is in his cylinder, and for 15 minutes when the cylinder is exhausted, before his needle attains its maximum deflection. Here it remains stationary "for 30 minutes and more." Thus while the temperature of the polished brass bottom is steadily sinking from 56° to 47°·7, the deflection is gradually rising, obviously not through the action of what was intended to be the source. Professor Buff then divides his maximum galvanometric deflection, multiplied by 100, by the difference between his initial and his final temperature, and obtains in this way his column of "comparable values."

Though begun with a view to conduction, no inference regarding this subject is drawn from these experiments. We pass, without notice, from conduction to diathermancy, Professor Buff pointing out that his result agrees with that of Magnus in making the denser hydrogen "more diathermanous than the rarefied." Finding, however, in opposition to Magnus, that air showed a similar deportment, being more diathermanous at 760 millims. than at 15 millims., Professor Buff was induced to seek the cause, and if possible avoid the occurrence of so abnormal a result.

Round the cylinder containing his gases he placed a second and wider one, and filled the space between them with cold water to within 7 millims. of the bottom of his brass vessel. The deflection of the needle fell considerably, and the anomaly of dense air being more diathermanous than rarefied air disappeared. Even here, however, Professor Buff detected currents, or a cause of currents, the heat conducted down the interior cylinder rendering the water warm at the top. Lowering his junction to a depth of 45 millims. below the source of heat, he allowed cold water to enter his annular space below and to run off above. Here he halted, as if every source of error had been removed. By means of his water jacket he lessened his radiating zone of glass and diminished the area of the heated vertical surface with which his gases came into contact. It is obvious, however, that no contact of the kind ought to have been at all permitted. The brass plate, which he regarded as his source of heat, must have accepted almost momentarily the temperature of the water above it; still, even in the improved experiments, 8 minutes were required to enable the needle to rise from 0° to 28°·6, the temperature of the presumptive source falling at the same time from 46° to 39°·6. It is on experiments conducted in this way that Professor Buff founds conclusions which, were they correct, would leave eight or nine years of diligent and careful work, on my part, in a very serious plight. Hydrogen gas he alleges to be as diathermanous as a vacuum, while more than 50 per cent. of his radiation is, as before stated, absorbed by 45 millims. of air.

With the fact before him that the deflection of his galvanometer needle was rising while the temperature of his brass plate was sinking, it must have occurred to Professor Buff that not the brass but the glass was his really efficient source of heat. The radiating zone was chilled by his gases, currents were established, and that hydrogen under such circumstances should transfer more heat to his junction than air is only what might be expected from the mobility of the gas.

Professor Buff next lowered his junction to a distance of 100 millims. below his source, and obtained an unexpected result. Instead of the absorption increasing, as it ought to have done when the depth of the absorbing layer was more than doubled, it fell from 50 to 15 per cent. The deflections here were small, and on coating the polished brass surface with lampblack, "though the radiating power of the source of heat must have been six or seven times greater than previously," there was hardly any increase of the deflection. The junction was here acted on simply by the chilled sides of the surrounding vessel. I have failed to extract a clear meaning from Professor Buff's remarks on this point; but, as a matter of fact, when the interior of his cylinder was covered with double cardboard, the absorptive energy of dry air in great part reappeared. The only distinct impression here left upon the mind is that, in Professor Buff's experiments, the surface of his glass cylinder, which ought to have been absolutely neutral, played a most influential part.*

Experimental Examination of Professor Buff's Results.

The disposition of the apparatus devised by me in 1859, when, in relation to radiant heat, the gaseous form of matter was first brought under the yoke of experiment,† is too well known to need description. From the source of heat the rays passed first through a vacuous chamber and then through an experimental tube stopped at both ends

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* Thinking that the original German might enable me to seize Professor Buff's meaning more effectually, I sought for the original paper, but failed to find it. The article appears to have been written and translated specially for the English public.

† Professor Magnus's first paper on radiation through gases was about a year and three-quarters posterior to my first communication to the Royal Society. See "Contributions to Molecular Physics, Historic Remarks on Memoir I," p. 59.

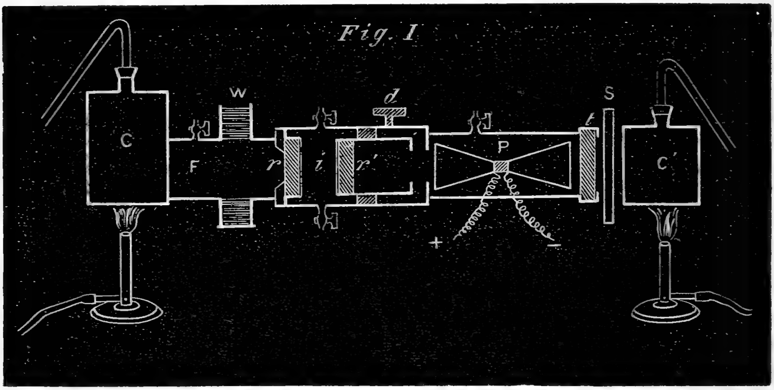
by dry and pellucid plates of rock-salt. The vacuous chamber was clasped at one place by a water jacket, which cut off all conduction from the source. At some distance beyond the end of the tube stood the thermo-pile, furnished with a double cone, with a view to the application of the principle of compensation. The experimenter who does not shrink from the discipline necessary to master it will find this method powerful, exact, and under the most complete control. By it was determined, for the first time, in 1859, the action of a considerable number of gases and vapours, one result of the inquiry being that, while some transparent gases were more impervious than many solids, the dry atmospheric air enveloping our earth was a practical vacuum to the rays of heat.

From this result the experiments of the late Professor Magnus led him to dissent. We once discussed our differences *viva voce;* he urging against me an objection which has been revived by Professor Buff. I had, he said, sent my heat through a vacuous chamber into my experimental tube; but between that tube and my pile there existed a space of air, in which, he asserted, the absorption which I had failed to detect in the tube itself took place. He mentioned a series of experiments which he was then on the point of publishing, and which, he said, proved, not only that the absorption of radiant heat by air was considerable, but also that a layer of air 12 inches thick sufficed to intercept all the rays that air was capable of intercepting. A published account of these results I have never been able to find, but they must have closely resembled those now under discussion.

I asked Professor Magnus whether he thought a stratum of air $\frac{1}{20}$th of an inch in thickness would exert any sensible absorptive action. He promptly replied in the negative. I therefore made the following experiment, which has been overlooked by Professor Buff. Placing the anterior cone of my pile within the experimental tube, I was able to bring its naked face within $\frac{1}{20}$th of an inch of my terminal plate of rock-salt. There was not the slightest alteration of the previously obtained result. Dry air, as before, behaved like a vacuum.

Fig. 1 is a rough vertical section of one of the pieces of apparatus recently arranged with a view of testing Professor Buff's conclusions. F and P are two chambers formed of brass tubing $2\frac{3}{4}$ inches in diameter. The chamber F is soldered to the cube C, one face of which constitutes the source of heat. W is a jacket in which water of the temperature of the surrounding atmosphere constantly circulates, preventing the heat conducted from C from passing further. The chamber F is separated from the chamber P by an intermediate chamber i, bounded by the rock salt plates r, r'. Within the chamber P is the thermo-pile, from which wires pass through the tube to a sensitive galvanometer. S is a screen, and C′ a compensating cube. The chamber P is closed at its end by the rock-salt plate *t*, through which the heat rays from C′ can reach the posterior face of the pile.

Fig. I



Adjusting the screen S so that the rays impinging on both sides of the pile accurately neutralise each other, *the rock-salt plates r, r′, being first in contact*, by means of a screw *d*, one of the plates *r′* is withdrawn from the other, dry air being introduced into the space between them. The layer of air could be gradually augmented in thickness from zero to 3 inches, which exceeds by more than 50 per cent. the thickness of the layer to which Professor Buff ascribes an absorption of 50 or 60 per cent.

The apparatus is delicately constructed, and to avoid the strain upon the plates of rock-salt *r*, *r′*, which would occur if the chambers F and P were exhausted, each of these chambers was filled with dry hydrogen. This, according to Professor Buff, is tantamount to a vacuum, exercising no action whatever on the rays of heat.

Repeated experiments with this apparatus proved the absorption of the layer of dry air in the chamber *i* to be *nil.*

But here another question arises. The rays of heat in these experiments had passed through plates of rock-salt. Supposing, therefore, that air and rock-salt absorb the same rays, the sifting of the rays by the salt would cause the air within the experimental tube to behave as a vacuum. Professor Buff endeavours to prove that this is the case. He first finds the transmission of radiant heat by a plate of clear rock-salt 3 millims. in thickness to be less than 60 per cent. of the entire radiation; and then he finds that the thermal colours of rock-salt and air are "sufficiently similar to explain the phenomena observed by Tyndall."

It is hardly credible that the skilled experimenters who preceded Professor Buff should have failed to detect the high absorption which he ascribes to rock-salt, if it were a fact. Melloni's determination which makes the transmission of a plate of rock-salt 2·6 millims. thick 92·3 per cent. of the total radiation is, in my opinion, much nearer the truth than that of Professor Buff. Be that as it may, I can affirm that in my case the transmission through plates of rock-salt of five times the thickness of that employed by Professor Buff amounts to 80 per cent. of the entire radiation. Subtracting from the 20 per cent. intercepted, 8 per cent. due to reflection at the two surfaces of the salt, we have 12 per cent. remaining for absorption.*

Were the thermal colours of air wholly, instead of partially, as affirmed by Professor Buff, identical with those of rock-salt, the coincidence would fail to account for the high absorption which he ascribes to air. But let us examine whether the alleged coincidence exists at all. To treat this question exhaustively, we must be able to compare the transmission of rock-salt *in vacuo* with its transmission in air. The apparatus employed for this purpose is rendered intelligible by the rough section, fig. 2. From the cube C the experimental tube extends to the plate of rock-salt *t*, which closes the tube. Midway, a rectangular chamber B is introduced, large enough to permit of one of my plates of rock-salt *s* to be raised and lowered within it. It is soldered on to the experimental tube, being exhausted when the tube is exhausted. The plate of salt *s* is clasped by a ring attached to the rod R, which moves air-tight through a stuffing-box in the bottom of the chamber B. The plate of salt *s* is 2¾ inches in diameter; while across the experimental tube, right and left of B, are diaphragms with apertures 1¼ inches in diameter. When the salt is pushed up, it comes between these apertures, the rays of heat passing through the central portion of the salt. The thermo-pile, with its two cones, is placed within the chamber P, while the water-

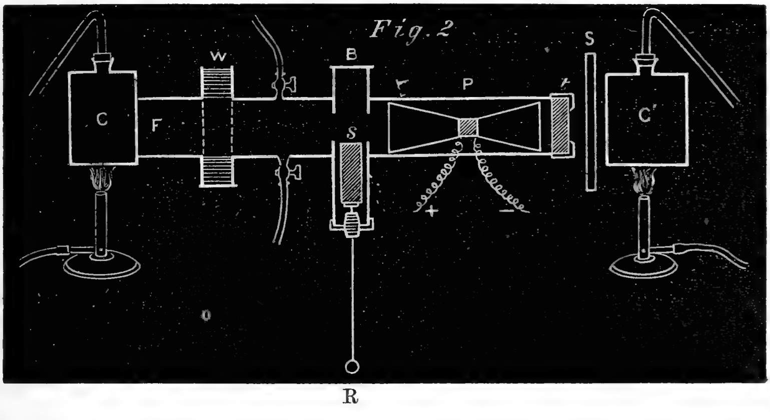


Fig. 2

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* In the case of the second plate of salt we have to deal with surface reflection merely, the heat having been sifted by the first plate.

jacket W plays the part already assigned to it. The method of experiment is now easy of comprehension.

The whole space between C and *t* was, in the first place, rendered as empty as a good air-pump could make it. The radiation from the compensating cube C′ being cut off by the screen S, the total radiation from C was declared by the galvanometer. The plate of salt was then pushed up, and the consequent fall of the deflection was repeatedly noted. Thus the absorption by rock-salt of radiant heat, which in no part of its course had been sifted by air, was determined. Dry air was then permitted to slowly enter the experimental tube until it was filled. The length of air through which the rays had to pass before reaching the pile was ten times that in which Professor Buff had found his large absorption. The sifting of the rays by the air, if Professor Buff be correct, ought here to reduce the absorption of the rock-salt to nothing. As in the former case, the experiment was repeated many times, and on different days. There was no difference between the transmission through the air and salt, and through the vacuum and salt. The air proved absolutely powerless to influence in any sensible degree the colour or quality of the heat. Here is a sample of the results obtained:—

Experimental Tube Exhausted.

Radiation—

Through vacuum, 41°·5; through salt, 37°·5.
" " 41·5; " " 37·5.
" " 41·5; " " 37·5.
" " 41·5; " " 37·5.

The value of 41°·5 is 51·5 units; that of 37°·5 is 41 units—hence the transmission amounts to 80 per cent. of the whole radiation.

Experimental Tube filled with Air.

Radiation—

Salt withdrawn, 39°·5; through rock-salt, 35°.
" " 39·5; " " 35.
" " 39·5; " " 35.
" " 39·5; " " 35.

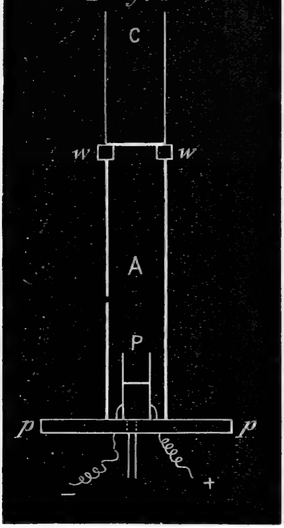
The value of 39°·5 is 46 units; that of 35°·0 is 36·5 units—hence the transmission amounts, as in the vacuum, to 80 per cent. of the whole radiation.

The experiment was checked, not only by repetition, but by the following variation.

By means of the adjusting screen S, the heat from the compensating tube C′ was caused to accurately neutralise that from C, the needle of the galvanometer pointing to zero. When this was done with the experimental tube exhausted, the plate of salt was pushed up. A moderate deflection of the galvanometer was the consequence. Repeated experiments made the heat intercepted by the salt 20 per cent. of the whole radiation. *This was the fraction stopped whether the tube was exhausted or filled with air.* This stoppage of 20 per cent. exactly corresponds with the transmission of 80 per cent. resulting from the previous experiments.

I have also taken the trouble to repeat Professor Buff's experiments with an apparatus similar to his own; taking care, however, to avoid the principal error into which I suppose him to have fallen. A glass cylinder, A, fig. 3, 12 inches long and 2¾ inches in diameter, is mounted on the plate *p*, *p*, of an air-pump. On it is placed a tin vessel C, with a brass bottom, intended to contain the water which warms the bottom, or source of heat. A thermo-pile, P, is mounted on the air-pump plate on which the cylinder stands, one of its faces being presented to the bottom of the tin vessel. The conical reflector is abandoned, a piece of tubing blackened within, and intended to cut off the radiation from the sides of the vessel, being pushed over the pile. Instead of bringing brass and glass into direct contact, as in the apparatus of Professor Buff, a washer, *w*, *w*, of non-conducting india-rubber, an inch and an eighth in thickness, separates the one from the other. There is no chilling by cold water, and the distance of

Fig. 3



the pile from the source renders it difficult for heat to pass by convection from the one to the other.

Following the method pursued by Professor Buff, the radiation through hydrogen was compared with the radiation through air. To make the experiments more strictly comparable, the temperature of the source was in all cases rendered the same. Instead of having to wait 8 minutes for the needle to reach its maximum deflection, it was steady there in less than a quarter of this time. The result was very different from that obtained by Professor Buff. Instead of the one gas absorbing from 50 to 60 per cent. of the radiation, and the other none, no difference whatever was to be detected between the deportment of hydrogen and that of air. Both, as before, proved practical vacua to the rays of heat.

Professor Buff describes other experiments, to one only of which I need refer. He makes "the diathermancy of olefiant gas somewhat higher than that of air." It must have been a strangely defective apparatus which could yield such a result. Air differs as widely from olefiant gas as rock-salt differs from ice. Pursuing Professor Buff's own method of experiment,* but insulating the glass cylinder from the source of heat, the diathermancy of air was found to be perfect, whereas 11 inches of olefiant gas absorbed 33 per cent. of the total radiation.

Such is my examination of the paper to which my attention was directed two years ago by Dr. Hofmann.

III. "On the Photographic Spectra of Stars." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received December 11, 1879.

(Abstract.)

The author presented, in December, 1876, a preliminary note on the subject of this paper, together with a diagram of the spectrum of Vega compared with that of the sun.

The author refers to a paper by Dr. William Allen Miller and himself in 1864, in which they describe an early attempt to photograph the spectra of stars.

Other investigations prevented the author from resuming this line of research until 1875, when a more perfect driving clock, by Grubb, enabled him to take up this work with greater prospect of success.

The author describes the special apparatus and the methods of working which have been employed.

The spectrum apparatus consists of one prism of Iceland spar and

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* A defective method even when every care is bestowed upon it.

lenses of quartz. It is so arranged in the telescope that the image of the star can be brought with certainty upon any part of the slit, and can be kept there during the photographic exposures, by a method of continuous supervision and control. The slit is provided with two shutters, through one-half of which a solar or other spectrum may be taken on the same plate for comparison and the determination of the lines in position in the spectrum.

The photographs were examined and the lines measured by means of a micrometer attached to a microscope of low power. These measures were reduced to wave-lengths by the help of solar and terrestrial spectra, use being made of M. Cornu's map of the ultra-violet part of the spectrum, and of M. Mascart's determination of the wave-lengths of the lines of cadmium.

Photographs have been obtained of the stars Sirius, Vega, α Cygni, α Virginis, η Ursæ Majoris, α Aquilæ, Arcturus, β Pegasi, Betelgeux, Capella, α Herculis, and α Pegasi. Also of the planets Jupiter, Venus, and Mars, and of the different parts of the moon.

The spectra of Sirius, Vega, α Cygni, α Virginis, η Ursæ Majoris, α Aquilæ, and Arcturus are laid down in a map on the scale of M. Cornu's map of the ultra-violet part of the solar spectrum.

The stellar spectra extend from about G to O in the ultra-violet. The stars of the white class are arranged so as to exhibit, first, the remarkable spectrum typical of this class, and then some of the modifications it appears to undergo.

The typical spectrum of this region of this class consists of twelve strong lines winged at the edges. The continuous spectrum extends in the photographs beyond S, but no lines are seen more refrangible than the twelfth line at 3699. Two of these lines agree in position with the hydrogen line (γ) 4340, and the other line at *h*. The third line agrees with H_1. The remaining nine form a group, in which the distance between any two adjacent lines is less as the refrangibility increases, suggesting that they are connected with each other, and represent, probably, one substance.

A very suggestive modification presents itself in the difference of character of the line H_2. In these stars this line is either absent or very thin as compared with its appearance in the solar spectrum. In the spectrum of Arcturus, which belongs to the solar type, this line exceeds in breadth and intensity its condition in the solar spectrum. The white stars may, therefore, be arranged in a series in which the line H_2 passes through different stages of thickness, at the same time that the typical lines become narrower and more defined, and other finer lines present themselves in increasing numbers. Arcturus seems to present a spectrum on the other side of that of the sun in the order of change from the white-star group.

The spectra of the planets were taken on the plan suggested by the author in 1864, in which the planet's spectrum is observed or photographed together with a daylight spectrum. These photographs show no sensible planetary modification of the violet and ultra-violet parts of the spectrum of the planets Venus, Mars, and Jupiter.

Numerous spectra of small areas of the lunar surface have been taken under different conditions of illumination. But the results are negative as to any absorptive action of a lunar atmosphere.

IV. "On a New Method of Spectrum Observation." By J. N. Lockyer, F.R.S. Communicated at the request of the Committee on Solar Physics. Received December 10, 1879.

In anticipation of my report on the Methods of Mapping Spectra, which I have been requested to prepare for the Solar Committee, I beg to present to them the following account of some recent work which has been suggested during the preparation of that report. In the "Phil. Trans." for 1873* I gave an historical account, showing how, when a light source, such as a spark or an electric arc, is made to throw its image on the slit of a spectroscope, the lines had been seen of different lengths, and I also showed, by means of photographs, how very definite these phenomena were. It was afterwards demonstrated that chemical combination or mechanical mixture gradually reduced the spectrum by subtracting the shortest lines, and leaving only the long ones.

On the hypothesis that the elements were truly elementary, the explanation generally given and accepted was that the short lines were produced by a more complex vibration imparted to the "atom" in the region of greatest electrical excitement, and that these vibrations were obliterated or prevented from arising by cooling or admixture with dissimilar atoms.

Subsequent work, however, has shown† that of these short lines *some* are common to two or more spectra. These lines I have called basic. Among the short lines, then, we have some which are basic, and some which are not.

The different behaviour of these basic lines seemed, therefore, to suggest that not all of the short lines of spectra were, in reality, true products of high temperature.

That some would be thus produced and would therefore be common to two or more spectra we could understand by appealing to Newton's rule: "Causas rerum naturalium non plures admitti debere quam quæ et veræ sint et earum phænomenis explicandis sufficiant," and

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* "Phil. Trans.," 1873, p. 254.

† "Proc. Roy. Soc.," vol. xxviii, p. 159.

imagining a higher dissociation. It became, however, necessary to see if the others would also be accounted for.

I have already given to the Royal Society a preliminary account of the extraordinary, because unexpected, phenomena and changes observed in the spectra of vapours of the elementary bodies when volatilised at different temperatures in vacuum tubes. Many of the lines thus seen alone and of surpassing brilliancy are those seen as short and faint in ordinary methods of observation, and the circumstances under which they are seen suggest, if we again appeal to the above rule, that many of them are produced by complex molecules.

In this case the appeal lies to the phenomena produced when organic bodies are distilled at varying temperatures; the simplest bodies in homologous series are those volatilised at the lowest temperatures; so that on subjecting a mixture of two or more liquids to distillation, at the beginning a large proportion of the more volatile body comes over, and so on.

At any particular heat-level, then, some of the short lines may be due to the vibrations of molecular groupings produced with difficulty with the temperature employed, while others represent the fading out of the vibrations of other molecular groupings produced on the first application of the heat.

In the line of reasoning which I advanced a year ago,* both these results are anticipated, and are easily explained. Slightly varying fig. 2 of that paper, we may imagine furnace A to represent the temperature of the jar spark, B that of the Bunsen burner, and C a temperature lower than that of the Bunsen burner.

Then in the light of the paper the lines *b* and *c* would be truly produced by the action of the highest temperature, *c* would be short and might be basic, while of the lines *h* and *m*, *m* would be short and could not be basic, because it is a remnant of the spectrum of a lower temperature.

To make this reasoning valid we must show then that the spark, or better still the arc, provides us with a summation of the spectra of various molecular groupings into which *the solid metal which we use as poles* is successively broken up by the action of temperature.

We are not limited to solid metals; we may use their salts. In this case it is shown in the paper before referred to† that in very many cases the spectrum is one much less rich in lines.

I have therefore attempted to gain new evidence in the required direction by adopting a method of work with a spark and a Bunsen flame, which Colonel Donnelly suggested I should use with a spark and an electric arc. This consists in volatilising those substances which

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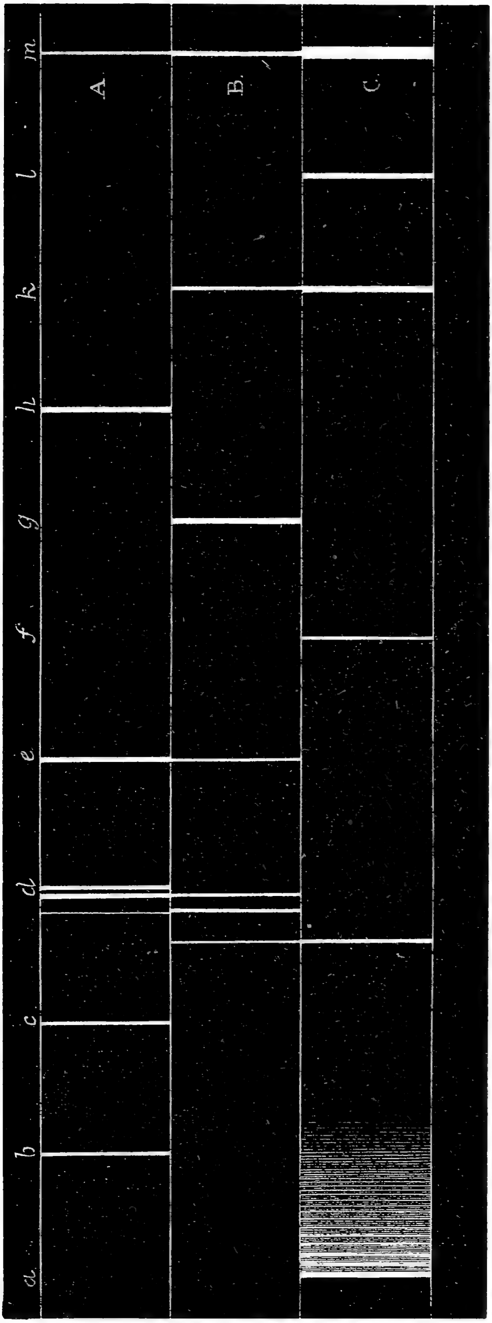
* "Proc. Roy. Soc.," vol. xxviii, p. 162.

† "Phil. Trans.," 1873, p. 258.



A. Highest temperature.

C. Lowest temperature.

give us flame spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilisation, and then after the temperature of the flame has produced all the simplification it is capable of producing.

The results have been very striking; the puzzles which a comparison of flame spectra and the Fraunhofer lines has set us find, I think, a solution; while the genesis of spectra is made much more clear.*

To take an instance, the flame spectrum of sodium gives us, as its brightest, a yellow line, which is also of marked importance in the solar spectrum. The flame spectra of lithium and potassium give us, as their brightest, lines in the red which have not any representatives among the Fraunhofer lines, although other lines seen with higher temperatures are present.

Whence arises this marked difference of behaviour? From the similarity of the flame spectrum to that of the sun in one case, and from the dissimilarity in the other, we may imagine that in the former case—that of sodium—we are dealing with a body easily broken up, while lithium and potassium are more resistant; in other words, in the case of sodium, and dealing only with lines recognised generally as sodium lines, the flame has done the work of dissociation as completely as the sun itself. Now it is easy to test this point, for if this be so then (1) the chief lines and flutings of sodium should be seen in the flame itself, and (2) the spark should pass through the vapour after complete volatilisation has been effected without any visible effect.

Observation and experiment have largely confirmed these predictions. Using two prisms of 60° and a high-power eyepiece to enfeeble the continuous spectrum *of the densest vapour produced at a high temperature*, the green lines, the flutings recorded by Roscoe and Schuster, and another coarser system of flutings, so far as I know not yet described, are beautifully seen. I say largely, and not completely, because the double red line and the lines in the blue have not yet been seen in the flame, either with one, two, or four prisms of 60°, though the lines are seen *during volatilisation* if a spark be passed through the flame. Subsequent inquiry may perhaps show that this is due to the sharp boundary of the heated region, and to the fact that they represent the vibrations of molecular groupings more complex than those which give us the yellow and green lines. The visibility of the green lines, which are short, in the flame, taken in connexion with the fact that they have been seen alone in a vacuum tube, is enough for my present purpose.

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* I allude more especially to the production of triplets, their change into quartets, and in all probability into flutings, and to the vanishing of flutings into lines by increasing the rate of dissociation.

With regard to the second point, the passage from the heat-level of the flame to that of the spark, after volatilisation is complete, produces no visible effect; indicating that in all probability the effects heretofore ascribed to *quantity* have been due to the presence of the molecular groupings of greater complexity. *The more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen.*

Let us now turn to lithium.

Seeing that the red line is absent while the violet lithium line is strong among the Fraunhofer lines, we may imagine that the flame has not done the work of dissociation in the case of lithium so completely as the sun does it, so that (1) the other lines of lithium should not be visible, even with the new precautions, in the flame spectrum, and (2) a passage from the heat-level of the flame to that of the spark after volatilisation should produce the other lines which we know to exist in the spectrum of the metal in the orange, blue, and violet.

Experiment and observation have also confirmed this result, so far as the yellow and blue lines go; that in the violet is difficult of observation.*

We next come to potassium.

The potassium lines usually recorded as not seen in a flame, but which are observed with a spark, are not very brilliant; nor are they strong among the Fraunhofer lines. Seeing therefore that a high temperature does not greatly develop them, we may expect to find them in the flame. They are almost all there when they are looked for with proper precautions, but those in all probability present in the sun are brightened on passing the spark, showing apparently that the flame volatilises with some difficulty the molecule which gives the line in the red.

The flame spectrum of magnesium perhaps presents us best with the beautiful effects produced by the passage from the lower to the

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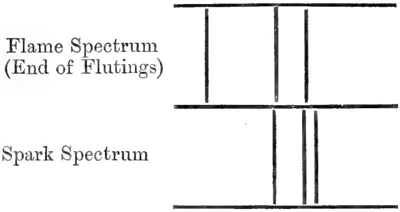
* The way in which the lines ordinarily seen in the flame are unaffected by the spark strikingly reminds me of the following remarks of Ångström and Thalén:— "The Fraunhofer lines can in general be divided, according to their appearance, into two classes: the one sharply defined and tolerably deep black, the other by no means so decidedly marked either as to form or colour. These two different kinds of lines are, as regards their appearance, very happily characterised by the opinion expressed on a former occasion, that the former, especially when the illumination is feeble, look as if they were situated considerably in front of the faint ground on which the latter seem to lie. The most prominent lines of the former class almost all proceed from iron: and those which remain, after the iron-lines are abstracted, belong to the other metals: calcium, manganese, chromium, &c."—("Ångström and Thalén on the Fraunhofer Lines, together with a Diagram of the Violet Part of the Solar Spectrum." Upsala, 1866; p. 5.)

higher heat-level, and shows the important bearing on solar physics of the results obtained by this new method of work.

In the flame the two least refrangible of the components of *b* are seen associated with a line less refrangible, so as to form a triplet. A series of flutings and a line in the blue are also seen.

On passing the spark, all these but the two components of *b* are abolished. We get the wide triplet replaced by a narrow one of the same form, the two lines of *b* being common to both, thus—



When the line in the blue disappears on passing the spark, two new lines are seen. The spark lines are in the sun, but the less refrangible member of the wide triplet and the blue line seen in the flame are absent.

The following are the details of some of the experiments which have been made on the above points :—

Experiment No. I.—Two pieces of platinum wire were supported in a Bunsen flame at a distance from one another of about 3 millims. They were then connected with a Holtz machine, in order that the spark might be passed inside the flame.

An image of the platinums was then thrown on the slit of the spectroscope by means of a lens. The spectroscope used had two dense flint prisms of 60°.

A piece of charcoal soaked in solution of sodic chloride was put into the base of the flame first, and then just below the platinum, and the spectrum observed ; it consisted simply of the yellow line D. The spark was passed and the spectrum again observed ; it now consisted of D plus the lines of hydrogen and some air lines, the red and green Na lines and the green flutings being still absent.

Experiment No. II.—Same arrangements, except that a large induction coil was substituted for the Holtz machine. The same results were obtained with the sodic chloride.

Experiment No. III.—Metallic sodium was next tried. It was found that when the metal was put into the flame just below the platinums the green line and the flutings were seen without the spark, that is, at the ordinary temperature of the flame. On introducing the sodium into the lower part of the flame, the green double (λ 5687·2 and 5681·4) and the flutings were not seen, either with or without the spark.

Experiment No. IV.—Same arrangements as No. II, with metallic sodium, and with a small blowpipe instead of Bunsen.

In this experiment the flame spectrum showed, besides the yellow line (D), the green double (λ 5687·2 and 5681·4), and also the flutings in the green, those in the red being absent. As soon as the spark was passed, the green double (λ 5687·2 and 5681·4) became brighter, while the flutings vanished.

In these observations the sodium was put into the flame *just below the platinums.* When put into the bottom of the flame, the D line was seen alone.

Experiment No. V.—A glass tube ½ inch in diameter was prepared, about 6 inches in length, having two platinums sealed into it at a distance of 4 inches from each other. A bulb was blown at each end, so that the spectrum might be examined with the tube end-on. A piece of sodium was put into the tube, and the latter exhausted with a Sprengel pump. An Argand burner was placed at one end of the tube, in order that the absorption of the vapour, as well as its radiation, might be observed. The metal was then very gradually heated by a Bunsen flame.

After the heating had gone on for about twenty minutes the absorption line of D appeared; this gradually increased in intensity.

On passing the spark along the tube, the bright lines of sodium appeared, the green double (λ 5687·2 and 5681·4), being distinguishable after D had been seen for a little time alone.

The temperature was now increased and the absorption spectrum again examined. The flutings in the green gradually made their appearance, D increasing in intensity, the green line being invisible. Afterwards the flutings in the red came in.

On passing the spark the absorption spectrum, consisting of the red and green flutings disappeared instantaneously, and the green double was seen very bright; after the passage of the spark D dark was much increased in breadth.

The quantity of hydrogen given off during the change prevented the passage of the spark, and the observations had to be discontinued. As soon as some of this had been pumped out the same observations were repeated with the same results.

Experiment No. VI.—An experiment was made with lithic chloride in Bunsen flame, with the same arrangement as in Experiment No. I.

The flame spectrum with the dispersion employed showed no Li line except the red one (λ 6705·2). On passing the spark from the Holtz machine, the yellow line (λ 6102·0) and the blue line (λ 4602·7) appeared as bright as the red line. The same results were obtained on repeating the experiment with the large induction-coil.

Experiment No. VII.—Potassic nitrate was tried by the method previously described in Experiment No. I.

The flame spectrum consisted as usual of the red lines (λ 7697 and 7663) and the blue line (λ 4045), very faint.

The effect of the spark was to bring out the yellow lines (λ about 5800), those in the green (λ about 5340), and the red double (λ 6946 and 6913), out of the flutings visible in the red, the double at 7697 and 7663 not being affected. The experiment was repeated with the induction-coil, and the same observations made, with the additional one that the spark also slightly intensified the blue line.

Experiment No. VIII.—On repeating the experiment with metallic potassium, the same phenomena were more markedly observed, the lines about λ 5800, and other lines more refrangible, were visible as very faint objects in the flame; they were much strengthened, however, by the passage of the spark.

Experiment No. IX.—Some potassium was volatilised by the spark in front of the slit of the sun-spectroscope and comparison of the positions of the lines with the Faunhofer lines made. It is believed that λ 5829·0, 5802·0, 5782·5 are all reversed in the solar spectrum. The less refrangible member of the red double (λ 6946) was next compared, and was undoubtedly absent from the sun. These observations, however, are rendered extremely difficult on account of the fluted appearance of the yellow lines, and must be repeated with a stronger sun and the electric arc. The spectroscope employed had three prisms, one of 60° and two of 45°.

Experiment No. X.—The flame spectrum of magnesium was examined, a green triplet was observed, which was at first sight taken for b. Measurements of the lines, however, showed that the less refrangible member was less refrangible than b, and had a wavelength 5209·8, and that the other two members were b^1 and b^2 respectively. A fresh charge of magnesium was put into the flame and the spark passed; the original triplet was now no longer visible, the line at 5209·8 having vanished, but b^4 was now seen forming with b^1 and b^2 a triplet of similar form on a smaller scale.

According to Thalén, there are three blue lines of magnesium at wave-lengths 4481·0, 4586·5, and 4703·5. These lines were looked for in the flame with and without the spark. Without the spark only one line was visible in this region; its position was found by comparison with the solar spectrum to be at wave-length 4570·3, and coincident with no Fraunhofer line. The passage of the spark abolished this line, at the same time bringing in the two lines given by Thalén at wave-length 4481·0 and 4703·5, both of which are reversed in the solar spectrum.

No line was seen at Thalén's wave-length 4586·5, the nearest approach to which was the line seen at the temperature of the Bunsen flame at wave-length 4570·3, a difference of more than sixteen divisions of the scale.

I am now preparing maps showing the phenomena observed at various heat-levels. I think it is not too much to hope that a careful study of such maps, showing the results already obtained, or to be obtained, at varying temperatures, controlled by observations of the conditions under which changes are brought about, will, if we accept the idea that various *dissociations* of the molecules present in the solid are brought about by different stages of heat, and then reverse the process, enable us to determine the mode of evolution by which the molecules vibrating in the atmospheres of the hottest stars *associate* into those of which the solid metal is composed. I put this suggestion forward with the greater confidence, because I see that help can be got from various converging lines of work. To some of these I may briefly allude here:

1. We have the lines present in the solar spectrum, and absent from it.

Example.—The red potassium line present in the flame is absent from the sun; some of the other lines are present.

2. We have the varying thicknesses of the lines of any one element in the sun to compare with the thicknesses produced at different temperatures in the laboratory.

Example.—The various lines of magnesium, notably *b*, the most refrangible line given by Thalén, and the other blue line.

3. We have the remarkable behaviour of metals vaporised in a vacuum at the lowest temperatures.

Example.—Sodium gives us D, potassium gives us the triplet in the green-yellow; calcium gives us the line in the blue; thus separating those lines from all the others of those metals.

4. We have the remarkable behaviour of the same vapours under like circumstances, the temperature alone being changed; when this is increased lines visible under ordinary conditions are brought in, and are seen in different parts of the tube, so that each line in turn (and therefore, I presume, each molecule which produces it) is separated from those with which it is generally seen in company.

Example.—By increasing the temperature we get the green line of sodium without D, and some of the magnesium lines have been seen separated from the others.

5. We have the power of determining the lower states by means of absorption phenomena, and then of observing the radiation of the vapours produced by the passage of a feeble current of electricity.

Example.—The fluted spectrum of sodium described by Roscoe and Schuster is instantly abolished by this means, and a brightening of the green and a considerable thickening of the dark yellow lines is seen.

6. May we consider the existence of these molecular states as forming a true basis for Dalton's law of multiple proportions? If so, then the metals in different chemical combinations will exist in different molecular groupings, and we shall be able by spectrum observations to determine the particular heat-level to which the molecular complexity of the solid metal, induced by chemical affinity, corresponds.

Examples.—None of the lines of magnesium special to the flame spectrum are visible in the spectrum of the chloride either when a flame or a spark is employed. The facts recorded in my papers, printed in the "Phil. Trans." some years ago, on the spectra of salts and mixtures, seem all explained in this way.

I think then that the method of mapping, to be complete, should not only show the metallic lines as produced at various temperatures compared with the Fraunhofer ones, but that for each metal investigations should be made and recorded for as many heat-levels as possible, and for various chemical groupings such as

CrO
CrO_3
Cr_2O_2
Cr,
Fe_2Cl_6
$FeCl_2$
Fe

to give examples, with a view of investigating the facts, to see whether we can trace a molecular evolution in each case.

Further, the "basic" lines recorded by Thalén will require special study with a view to determine whether their existence in different spectra can be explained or not on the supposition that they represent the vibrations of forms, which, at a early stage of the planet's history, entered into combination with other forms, differing in proximate origin, to produce different "elements."

V. "Note on the Spectrum of Hydrogen." By J. NORMAN LOCKYER, F.R.S. Received December 17, 1879.

Some years ago, in an account of a research undertaken by Dr. Frankland and myself, it was pointed out that in the tubes we then employed the *h* line of hydrogen was only seen when the coil and Leyden jar were so arranged as to produce those effects which are generally supposed to be due to high temperatures. The remark, as we distinctly stated, was only made in connexion with the tubes we then employed, which were of widish bore; it is necessary to mention this because in Germany the remark has been taken to apply to all tubes.

In connexion with my present line of work I have had to return to the spectrum of hydrogen, and I have photographed it, using an end-on tube, which I owe to the kindness of Dr. Monkhoven, whose device it is.* In the spectrum of the Monkhoven tube *h* is very well seen, though not so strong as the line near G.

What I wish, however, to draw special attention to now, is the fact that other lines more refrangible are seen; in the photographs I have obtained, one of them, within the limits of dispersion that I have employed (one dense prism of 60°, collimator 18 inches, and a small Dalmeyer rectilinear lens in the camera), is coincident with a line observed in the spectrum of calcium at λ 3968·0 (H).

More photographs are required to enable me to give the approximate positions of the other lines more refrangible.

VI. "On the Capillary Electroscope." By G. Gore, LL.D., F.R.S. Received December 6, 1879.

The following research was commenced with the hope of being able to make an exact measuring instrument of a modified form which I had devised (see page 37) of the "Capillary Electroscope," and to employ it for the determination of the different degrees of electromotive force of different metals in forming tables of electro-chemical series, with different electrolytic solutions.† With this object in view, I constructed and tried a considerable number of forms of the instrument; and employed a number of liquids, acid, neutral, and alkaline, as conducting media between the mercurial electrodes. In every case, however, an obstacle to uniform and accurate measurement sooner or later arose, viz., unequal adhesion of the mercury to different parts of the capillary tube.

It has been noticed by other investigators that the motion of the slender column of mercury in Lippmann's "Capillary Electrometer" suddenly stops whilst under the electric influence, and this has been supposed to increase the value of the instrument by rendering its indications sensibly instantaneous. According to Lippmann also "when the circuit was closed" by a metallic conductor, "all irregularities at once disappeared" ("Phil. Mag.," April, 1874, p. 284). This sudden stoppage of the mercury, however, whilst under the influence of the current is, I believe, in every case due to a sudden increase of mechanical resistance, usually caused by unequal adhesion

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* It is right to add, however, that this form of tube also suggested itself to Professor Piazzi Smyth quite independently.

† See "On the Chemico-Electric Relations of Metals in Solutions of Salts of Potassium."

of the mercury to different parts of the tube, but sometimes by a minute trace of solid substance in the tube. With a very feeble current this adhesion shows itself in several ways, usually by a more or less sudden retardation or stoppage of the motion of the mercurial column, by a delay in its commencement of motion, or by its non-movement. The current I have employed has, in nearly all cases, been a very feeble one, obtained from two wires, one of copper, and the other of platinum, each about 1 millim. diameter, immersed about 2 or 3 millims. deep in common drinking water. With a very sensitive instrument, and a capillary tube as free from obstruction as could be obtained, this power has been sufficient to move the meniscus 10 or 20 millims.

A common cause of obstruction consists of impurities (especially oxide) in the mercury; the metal therefore should be as pure as possible. I have obtained it sufficiently pure by distilling the ordinary metal twice, then shaking it violently with dilute nitric acid, washing it with distilled water, shaking it again strongly with a dilute solution of potassic cyanide (which very effectually removes oxide), washing it again, and filtering it through a minute hole in the point of a paper funnel. The mercury should be kept in a closed bottle, and be frequently filtered to remove dust from its surface whilst being used, because the most minute speck of solid matter, even that which is invisible to the unassisted eye, is sufficient to stop the capillary tube. The solution also should be kept perfectly clear by means of filtration. Another cause of stoppage is the employment of glass tubing into which dust has been allowed to enter; this may be obviated by employing newly-made tubing, sealed at its ends by wax; with such tubing I have obtained no manifest advantage by washing it with acids, alkalies, ether, &c., nor by allowing a stream of clean mercury to run through it. After having taken all these precautions, there remains a very common cause of stoppage of the motion of the mercury, viz., portions of air or gas adhering to the tube; these consist either of minute films of air, or if the current is too strong, also bubbles of gas liberated from the electrolyte.

One of the most effectual modes of removing the air or gas is to repeatedly cause by mechanical means (such as compression of the pressure-chamber, or by change of position of the instrument from horizontal to vertical and *vice versâ*) the mercury and conducting solution to move freely to and fro through the whole of the fine part of the tube whilst the circuit is closed, and expelling occasionally a few drops of mercury from the end of the tube; employing also an electric current in a suitable direction to assist the motion. If there remain any adhering portions which are not removable by these means, diminished pressure, combined with an electric current from the point of the capillary towards its larger part, and the simultaneous application of a hot plate of metal to the locality of the adhesion, rarely fails to remove them. Notwithstanding, however, all these precautions, the meniscus often subsequently sticks at a particular place without any apparent cause, and without the presence of any body visible by the aid of a microscope magnifying from 30 to 50 diameters. Free use of the instrument reduces many of these adhesions; it also gives rise to fresh ones. Immovable adhesions of the mercury frequently occur, or are subsequently produced by use of the instrument which interfere with the accurate measurement of so feeble an electric power as that mentioned above, and appear to be due to irregularities of smoothness in the glass itself; the best precaution to take in such cases consists in selecting as the sphere of the movement of the meniscus a portion of the capillary tube known to be free from such a source of error; but even such a space does not permanently remain free.

By employing a suitable tube, dilute sulphuric acid as the liquid, and taking all the above precautions, I have constantly found that the mercurial column, instead of moving from point to point with a jerk and stopping suddenly, moved with a gradually diminishing speed until it stopped. The momentary movement and sudden stoppage occurs more readily with a short capillary tube and currents of greater power than that above mentioned, but with a long tube, as free as obtainable from irregular adhesion, a movement of from 10 to 20 millims. not unfrequently occurs, requiring in some cases as much as 80 millims. variation of height of mercury pressure to bring the meniscus back to its original point. If the tube happens to be one of parallel bore, and especially if its lower part enlarges towards its lower extremity, such a current as the one mentioned will cause the meniscus to descend as much even as 5 or 6 centims., and the mercury to run out at the end of the tube. The influence of gravity no doubt operates in all these cases, but with the same electric power, and whilst using a tube the diameter of which does not diminish too rapidly towards its point, the column usually moves more readily towards the end of the capillary than the reverse, whether the instrument be in a vertical or horizontal position. The mercury in all cases tends also to pass from the form of a cylinder to that of a sphere, and if it once gets beyond the end of the tube, it forms a globule which draws the narrower part rapidly into itself by its great cohesion. Similarly if the tube is too taper the mercury tends to draw itself upwards towards the wider cavity.

A good capillary tube is essential; the only way I know of obtaining it is to make a number of tubes and try them until a suitable one is found. Glass tubing, of an external diameter of about 6 to 8 millims., and having a bore of about one-third or one-fourth of a millimetre, is of a very convenient size for making the capillary, because it may be drawn out to a sufficient degree of fineness at one heating; that having a circular bore it is less likely to become clogged by use than that which is oval; and it is an advantage to employ that which has white enamel at the back. Success in making the capillary depends largely upon careful manipulation and the employment of a flame of suitable size; I have used a Bunsen flame about an inch in height and half an inch in diameter, for drawing long capillaries by a single heating, or a very minute flame of a small spirit-lamp for drawing out a short one by a second heating. Within certain limits, I have found that the finer the capillary tube the greater is the movement in it of the meniscus by a given electromotive force, and the greater the pressure required to balance that force; but if the bore of the tube be extremely fine, say ·01 millim., the instrument becomes less sensitive, probably owing to increased adhesion or to increased conduction-resistance of the minute column of solution. A convenient bore is about ·03 to ·04 millim., and an external diameter of ·3 to ·4 millim. Under uniform pressure, the meniscus moves not only through a much greater length, but also at a much faster rate, in the finer portion of a tube of moderate diameter, than it does in the coarser part, and this occurs even in tubes the internal diameter of which diminishes to their very extremity. Tubes of parallel bore are not usually suitable for the instrument (especially if the latter is used in a vertical position), because, instead of presenting a resistance gradually increasing downwards, as they should do, they offer the reverse. The best form of capillary is one which becomes narrower at a sufficiently increasing degree towards its free end. I have employed capillaries varying in length from 5 to 15 centims. with equal advantage.

Considerable pressure is often requisite for filling the capillary tube, and for overcoming obstructions; to produce it, a compressible pressure-chamber is much more convenient than a tall column of mercury. An excellent chamber may be formed of a piece of vulcanized india-rubber tubing, about 6 or 8 centims. long, 3 millims. bore, and 15 millims. external diameter, such as is employed as "engine-packing." It is more convenient to have the platinum wire (for connexion with the column of mercury) inserted through the side of this tube than through that of the glass one, because the former does not so readily cause the wire to be broken. To insert the wire, one end of the tube is stretched open by means of a small pair of pliers; a needle, wet with glycerine, is then forced very obliquely through the india-rubber close to the end of the tube, and after its removal a fine wire may be readily forced through the minute stretched hole and the pliers then withdrawn. After the capillary tube has been fixed in this chamber (which must be previously made scrupulously clean), the end of the chamber adjacent to the terminal and capillary must be securely bound round with wire to prevent leakage. As it often requires considerable pressure to force the mercury through the tube, especially when there exists both a mechanical obstacle and a stoppage of the electric current, a strong pressure screw, with its clamp capable of grasping at least an inch in length of the chamber, is absolutely necessary.

I have tried several plans of mechanically compensating the electro-capillary effect, the most convenient of which is to surmount the pressure-chamber by a vertical glass tube about 30 or 40 centims. high, and 1 millim. bore, with its lower end opening into the chamber, and its upper end also open, but capable of being closed by means of a glass cock, having an open cup at its upper extremity. This arrangement admits of great pressure being applied to the chamber, by means of the screw and clamp, and also of crudely measuring in millimetres height of mercury the compensating pressure necessary to depress the mercury to the neutral point whilst the current is passing. This upper tube also, like the capillary one, has the disadvantage of offering unequal resistance to the passage of the mercury by unequal adhesion, so that a small movement of the screw of the pressure-chamber is not often at once attended by a simultaneous movement of the mercury at the top and bottom of the column.

The voltaic current was very conveniently manipulated by the aid of a small moveable apparatus, so constructed that by turning its handle the first quarter of a revolution, the two wires of the electroscope were connected metallically together, and the current insulated from them; by turning it a second quarter, the current was sent in one direction through the electroscope; by turning it a third, the wires of the electroscope were disconnected and perfectly insulated (the base of the instrument being of ebonite); and by turning it the fourth quarter, the current was sent through the electroscope in a reverse direction. This apparatus may be easily made by prolonging the axis of a common electric reverser, fixing two projecting metal studs on opposite sides of the prolonged part (*but not opposite to each other*), and attaching two vertical springs to two binding-screws fixed on the base, so that when the studs pressed against the upper ends of these springs, the circuit was closed between the ends of the wires attached to the two electrodes.

In the engraving, fig. 1 represents the reverser fixed vertically to a moveable wooden stand. The dotted lines show the directions of four connecting wires at the back of the ebonite base of the instrument. The wires from the voltaic cell are attached to two binding-screws, which are connected with the two metal standards that support the moveable axis. The terminal wires of the electroscope are attached by means of binding screws to two vertical brass springs, so fixed as to press against the two metal studs projecting at right angles from the moveable axis; the terminals are also further con-

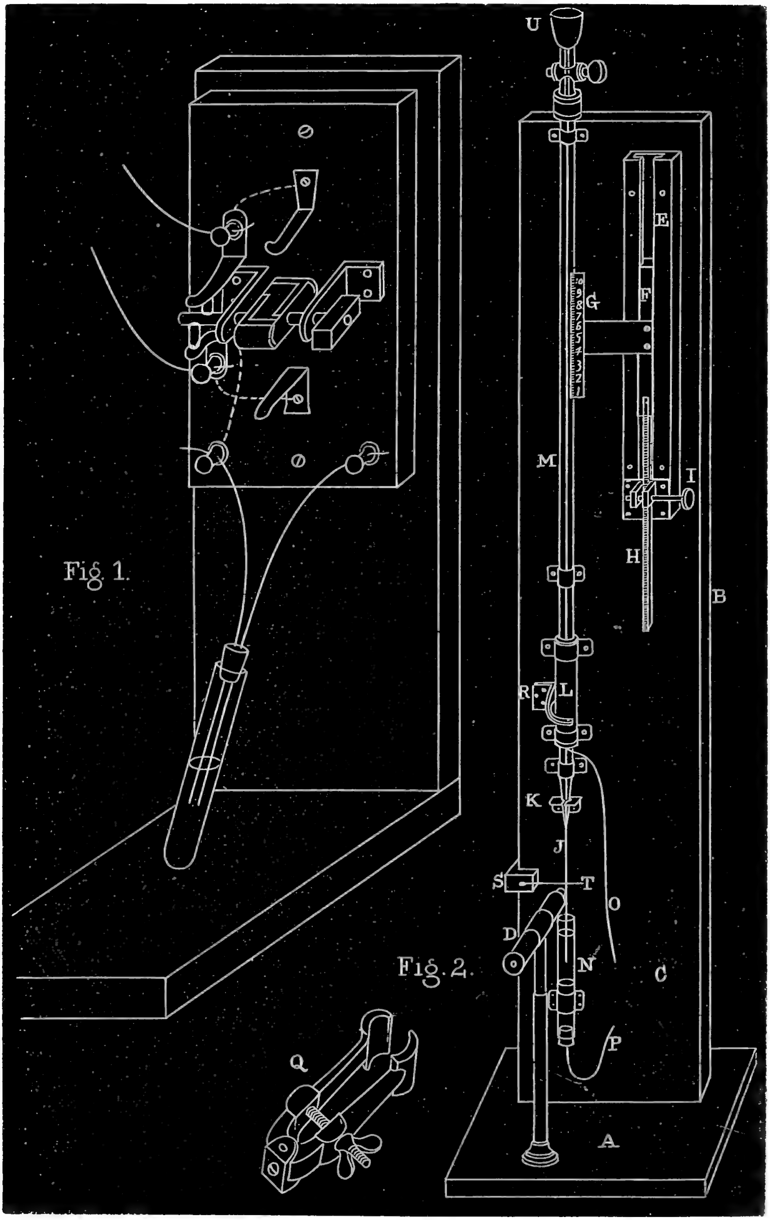


Fig. 1.

Fig. 2.

nected, by means of the wires beneath the base, with the two other and ordinary vertical springs of the instrument, so that by turning the handle they may be disconnected from the studs and connected with those springs, and through them, the axis and standards, with the voltaic cell, and receive a current in either direction, according to the position of the handle. The sketch represents the handle and axis in the above-mentioned first position, with the two wires of the electroscope metallically connected together, but insulated from the cell.

The usual form of the electroscope I have employed is represented in fig. 2. A is a wooden base, B a wooden upright board fixed to it. C is the situation of the reverser. D a microscope, capable of vertical and horizontal movement, having a magnifying power of about 30 to 50 diameters, and provided with a "spider-line." E is a grooved piece of wood fixed to the upright stand. F a wooden slide, with a moveable scale, G, worked by a metal rack H, and pinion with handle, I. J is the capillary, supported by a perforated little brass shelf, K. L is the pressure-chamber. M the glass tube containing the compensating column of mercury. N a thin glass tube, about 5 millims. bore, containing the mercury and conducting solution. O and P are the terminal platinum wires for attachment to the screws of the reverser. Q is a strong clip, shown separate from the instrument. R is a metal support for the clip. S is a moveable slide of cork, carrying a very fine hair, T, for indicating the position of the meniscus. It is not usually necessary to measure the amount of movement of the meniscus. Some cotton wool is placed in the cup, U, and the cup is covered with a lid to exclude dust. A strip of white paper is affixed behind the capillary portion of the tube to form a white background, and the instrument requires to be used in a good light.

Having found by additional experiments that in certain cases the mercury moves in an opposite direction to the electric current, I am now engaged in examining that fact, and in completing an investigation of the causes and conditions of the movements.

VII. "Chemico-Electric Relations of Metals in Solutions of Salts of Potassium." By G. Gore, LL.D., F.R.S. Received December 8, 1879.

The following experiments were made with the object of determining the chemico-electric positions of various metals, &c., in solutions of salts of potassium of various strengths, and at different temperatures; and also with the intention of ascertaining, by the aid of Lippmann's capillary electrometer, the quantitative differences of electromotive force between each two consecutive metals, and thus to construct a series of tables of electromotive forces of the particular solids and liquids employed. But as after making many attempts I was unable to construct such a form of that electrometer as might be relied upon as an accurate measuring instrument, I abandoned the object of measuring the electromotive differences, and proceeded no farther than simply determining the order of such differences in each particular solution. Although I have not been able to carry out the examination as far as I intended, I venture to submit the results to the Royal Society, in the hope that if published they may be of use to other investigators, as similar tables upon a less extensive scale have already proved.

As the true electrical relations of metals in liquids depends largely upon the purity of the substances, I beg to state that the metals and salts employed were, in nearly all cases, the purest obtainable. The tellurium, mercury, and antimony were very highly purified by me; the silicon was prepared by fusing some fine crystals, which I had previously digested with pure hydrofluoric acid, hydrochloric acid, and nitric acid separately. The gold, silver, platinum, palladium, iridium, rhodium, cobalt, tin, and cadmium, were obtained from Messrs. Matthey and Co., and were the purest they prepare. The nickel was refined by Sir J. Mason, and was of a high degree of purity; it had been rolled into a thin strip. The carbon was a rod of a Jablochkoff's candle. The indium was a portion of one of the ingots exhibited at the Paris Exhibition by its discoverers. The gallium was presented to me by M. Lecoq de Boisbaudran, and said to be "nearly pure except traces of zinc." The thallium was supplied to me by Messrs. Hopkin and Williams. The magnesium was probably very pure, and was obtained from the Magnesium Metal Company, from whom I also obtained the bismuth, said to be "highly purified." The aluminium, copper, lead, iron, and zinc wires were of the ordinary qualities.

The salts employed were of considerable purity, and in every case were dissolved in distilled water. To remove any trace of free iodine or acid from the solutions of potassic iodide, or any trace of soluble sulphide contained in those of cyanide of potassium, they were previously well stirred with a rod of aluminium or magnesium.

Every investigator who has made experiments of the present kind is aware that temporary reversals of the current frequently occur in such cases. When the reversals took place immediately upon immersion, the first current was not regarded, because it was probably due to a momentary change of surface tension or of temperature, caused by the physical contact of the solid and liquid prior to chemical action.

A sufficient number of different strengths of solution of each salt were employed to supply a large number of determinations, so as to enable curves to be drawn showing the variation of relative electric positions of different metals with the variations of strength of the liquid. Such curves were drawn, compared with each other, and conclusions inferred respecting similar metals or groups of metals.

In the following tables the numbers at the top of each column represent the numbers of grains of the particular salt dissolved in the quantity of water stated. The amount of water employed in the solutions of the first six tables was 50 cub. centims., and in Tables VII and VIII it was 800 grains.

The specimen of gallium not having been obtained in sufficient time for the whole of the experiments, it was not employed in Tables I and VII and part of Table III. Being also easily fusible it could not be used in hot solutions.

Table No. I.—Potassic Chloride Solutions at 55° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	Sat. sol.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Cd	Tl	Cd	Al	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Tl	Al	Tl	Cd	Al	Al	Al	Al	Al	Al	Al
Al	Cd	Al	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl
In	In	In	In	In	In	In	In	In	In	In
Sn	Pb	Fe	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb
Pb	Fe	Pb	Fe	Sn	Sn	Sn	Sn	Sn	Sn	Sn
Si	Sn	Sn	Sn	Fe	Fe	Fe	Fe	Fe	Fe	Fe
Fe	Co	Si	Si	Co	Co	Co	Co	Co	Co	Co
Co	Si	Co	Co	Si	Si	Si	Sb	Si	Si	Cu
Cu	Sb	Ni	Sb	Sb	Sb	Sb	Cu	Sb	Sb	Sb
Sb	Bi	Cu	Cu	Bi	Cu	Cu	Ni	Cu	Cu	Bi
Bi	Cu	Sb	Ni	Cu	Bi	Bi	Si	Ni	Bi	Si
Hg	Ni	Bi	Bi	Ni	Ni	Ni	Bi	Bi	Ni	Ni
Ni	Te	Te	Te	Te	Te	Te	Te	Te	Ag	Ag
Te	Ir	Hg	Ag	Hg	Ag	Ag	Ag	Ag	Te	Te
Ir	Hg	Pd	Pd	Au	Hg	Ir	Hg	Hg	Hg	Hg
Rh	Rh	Ag	Hg	Ag	Pd	Hg	Ir	Ir	Ir	Ir
Pd	Pd	Au	Au	Pd	Ir	Au	Au	Au	Au	Pd
Au	Au	Ir	Pt	Ir	Au	Pd	Pd	Pd	Pd	Au
Ag	Ag	Pt	Ir	Pt	Pt	Pt	Pt	Pt	Pt	Rh
Pt	Pt	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Pt
C	C	C	C	C	C	C	C	C	C	C

Temporary reversals occurred with Cd and Al in the solution of 200 grs.; Co and Fe in 200 and 240; Pb and Fe in 10; Sb and Cu in 10; Cu and Ni in 20; Tl and Cd in 1; Si and Bi in 160; and Ir and Au in the 160 grs. solution.

Table No. II.—Potassic Chloride Solutions at 100° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	Sat. sol.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Tl	Tl	Tl	Tl	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Al	Al	Cd	Al	Al	Al	Al	Al	Al	Al	Al
Cd	Cd	Al	Cd	Tl	Tl	Tl	Tl	Tl	Tl	Tl
In	In	In	In	In	In	In	In	In	In	In
Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb
Sn	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Sn	Sn	Sn
Fe	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Fe	Fe	Fe
Si	Co	Co	Co	Co	Co	Co	Co	Co	Co	Co
Co	Si	Si	Si	Sb	Si	Si	Sb	Sb	Sb	Sb
Sb	Sb	Sb	Sb	Bi	Sb	Sb	Si	Cu	Cu	Cu
Bi	Bi	Ni	Bi	Si	Cu	Cu	Cu	Bi	Bi	Bi
Cu	Cu	Bi	Cu	Cu	Bi	Bi	Bi	Si	Si	Si
Hg	Ni	Cu	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni
Ni	Te	Te	Te	Te	Te	Te	Te	Te	Ag	Ag
Te	Ir	Ag	Ag	Ag	Ag	Ag	Ag	Ag	Te	Te
Ir	Hg	Pd	Hg	Hg	Ir	Ir	Hg	Hg	Hg	Hg
Rh	Rh	Hg	Ir	Ir	Hg	Hg	Ir	Ir	Ir	Ir
{ Pd	Pd	Au	Rh	Rh	Pd	Pd	Au	Au	Au	Au
{ Au	{ Au	Ir	Pd	Au	Au	Rh	Pd	Pd	Pd	Pd
Ag	{ Ag	Rh	Au	Pd	Pt	Au	Pt	Pt	Rh	Pt
Pt	Pt	Pt	Pt	Pt	Rh	Pt	Rh	Rh	Pt	Rh
C	C	C	C	C	C	C	C	C	C	C

Table No. III.—Potassic Bromide Solutions at 55° F.

Grains. 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	Sat. sol.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Tl	Tl	Tl	Cd	Cd	*Cd	*Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Cd	Cd	Cd	Tl	Tl	Tl	Tl	*Tl	*Tl	*Tl	*Tl	*Tl	*Tl	*Tl
In	In	In	In	In	In	Al	Al	Al	Al	Al	Al	Al	Al
Al	Al	Al	Al	Al	Al	In	In	In	In	In	In	In	Pb
Fe	Fe	Fe	Fe	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	In
Pb	Pb	Pb	Pb	Fe	Fe	Fe	Fe	Sn	Sn	Sn	Sn	Sn	Sn
Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Fe	Fe	Fe	Fe	Fe	Fe
Co	Co	Co	Co	Co	Co	Co	Co	Co	Co	Co	Cu	Cu	Cu
Sb	Sb	Sb	Sb	Sb	Sb	Sb	Cu	Cu	Cu	Cu	Co	Co	Co
Si	Si	Si	Si	Si	Si	Cu	Sb	Sb	Sb	Sb	Sb	Sb	Sb
Bi	Bi	Bi	Bi	Bi	Bi	Si	Si	Si	Si	Si	Si	Si	Si
Ni	Ni	Cu	Cu	Cu	Cu	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi
Te	Cu	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ag	Ag	Ag	Ag	Ag
Cu	Te	Ag	Ag	Ag	Ag	Ag	Ag	Ag	Ni	Ni	Ni	Ni	Ni
Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg
Ag	Ag	Te	Te	Te	Te	Te	Te	Te	Te	Te	Pd	Pd	Pd
Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Te	Au	Au
Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Au	Au	Au	Te	Te
Rh	Au	Au	Au	Au	Au	Au	Au	Au	Pt	Pt	Pt	Pt	Pt
Au	Rh	Rh	Rh	Pt	Rh	Pt	Pt	Pt	Ir	Ir	Ir	Ir	Ir
Pt	Pt	Pt	Pt	Rh	Pt	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C

Table No. II.—Temporary reversals occurred with Cd and Al in solutions of 40, 160, and 200 grs.; Sn and Fe in 40 and 160; Cu and Ni in 20; Si and Cu in 160; and Pd and Pt in 160.

Table No. III.—Temporary reversals occurred with In and Al in solutions of 5, 10, and 20 grs.; Al and Cd in 40; Tl and Al in 160 and 200; Fe and Sn in 1 and 160; Fe and Pb in 1; Fe and Co in 280; Fe and Pd in 10; Au and Pd in 280; Pd and Te in 80 and 200; Au and Rh in 20; Ir and Au in 40, 120, and 160; Bi and Cu in 120; Bi and Si in 160 and 200; and Ni and Ag in 240.

The position of gallium in various columns of this table is indicated by a *; its position is immediately *above* the star.

Table No. IV.—Potassic Bromide Solutions at 100° F.

Grains. 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	Sat. sol.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Tl	Tl	Tl	Tl	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Cd	Cd	Cd	Cd	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl
In	In	In	In	In	In	Al	Al	Al	Al	Al	Al	Al	Al
Al	Al	Al	Al	Al	Al	In	In	In	In	In	In	In	In
Pb	Pb	Fe	Sb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb
Sn	Fe	Pb	Fe	Fe	Fe	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn
Fe	Sn	Sn	Sn	Sn	Sn	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe
Co	Co	Co	Co	Co	Co	Co	Co	Co	Co	Co	Cu	Cu	Cu
Sb	Sb	Sb	Sb	Sb	Sb	Sb	Cu	Cu	Cu	Cu	Co	Co	Co
Si	Si	Si	Si	Si	Si	Cu	Sb	Sb	Sb	Sb	Sb	Sb	Sb
Bi	Bi	Bi	Bi	Bi	Cu	Si	Si	Si	Si	Si	Si	Si	Si
Ni	Ni	Cu	Cu	Cu	Bi	Bi	Bi	Bi	Bi	Bi	Ag	Bi	Ag
Te	Cu	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ag	Ag	Bi	Ag	Bi
Cu	Te	Ag	Ag	Ag	Ag	Ag	Ag	Ag	Ni	Ni	Ni	Ni	Ni
Ag	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg
Hg	Ag	Te	Te	Te	Te	Te	Te	Te	Te	Te	Pd	Pd	Pd
Ir	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Te	Au	Au
Pd	Ir	Ir	Ir	Ir	Au	Ir	Ir	Au	Au	Au	Au	Te	Te
Rh	Au	Au	Au	Au	Ir	Au	Au	Pt	Pt	Ir	Pt	Pt	Pt
Au	Rh	Pt	Rh	Pt	Rh	Rh	Pt	Ir	Ir	Pt	Ir	Ir	Ir
Pt	Pt	Rh	Pt	Rh	Pt	Pt	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C

Temporary reversals occurred with In and Al in solutions of 5, 10, 20, 40, and 80 grs.; In and Pb in 200; Fe and Sn in 20, 80, and 240; Fe and Co in 1; Au and Pd in 80; Pd and Te in 120 and 280; Au and Rh in 5; Ir and Au in 120; Ir and Pt in 200; Ni and Bi in 200; Co and Cu in 240; Ag and Si in 320, and in the saturated solution.

Table No. V.—Temporary reversals occurred with Co and Fe in solutions of 5, 10, 20, and 160 grs.; Ni and Si in 240, 320, 360, and 500; Ni and Pd in 800, 1,000, and saturated solution; In and Fe in 240 and 320; Sb and Ag in 40 and 200; Sb and Hg in 160; Cu and

Table No. V.—Potassic Iodide Solutions at 60° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	400,	500,	600,	800,	1,000,	Sat. solution.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga	Ga
Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl
Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al
Pb	Pd	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb
Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn
In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In
Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Cu	Cu	Cu	Cu	Cu
Co	Co	Co	Co	Co	Co	Cu	Cu	Cu	Cu	Cu	Cu	Cu	Cu	Ag	Ag	Ag	Ag	Ag
Sb	Sb	Cu	Cu	Cu	Cu	Co	Ag	Sb	Co	Ag	Ag	Ag	Ag	Fe	Fe	Hg	Hg	Sb
Cu	Cu	Sb	Sb	Sb	Sb	Sb	Co	Ag	Sb	Co	Sb	Sb	Sb	Sb	Sb	Fe	Sb	Fe
Si	Si	Si	Si	Ag	Ag	Ag	Sb	Co	Ag	Sb	Co	Co	Hg	Hg	Hg	Sb	Fe	Hg
Bi	Bi	Ag	Ag	Si	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Co	Co	Co	Bi	Co	Co
Ni	Ag	Bi	Hg	Hg	Si	Si	Si	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Co	Bi	Bi
Ag	Ni	Ni	Bi	Bi	Bi	Bi	Bi	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si
Hg	Hg	Hg	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni
Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Au	Pd	Pd
Au	Au	Te	Au	Au	Au	Te	Te	Au	Au	Au	Au	Au	Au	Au	Au	Pd	Te	Te
Te	Te	Au	Te	Te	Te	Au	Au	Te	Pt	Te	Pt	Te	Te	Te	Pt	Te	Au	Au
Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Te	Pt	Te	Pt	Pt	Pt	Te	Pt	Pt	Pt
Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir
Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C

Table No. VI.—Potassic Iodide Solutions at 100° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	400,	500,	600,	800,	1,000,	Sat. solution.
Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl	Tl
Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al	Al
Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb	Pb
Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn	Sn
In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In	In
Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Fe	Cu	Cu	Cu	Cu	Cu	Cu
Co	Co	Co	Co	Co	Co	Co	Cu	Cu	Cu	Cu	Cu	Cu	Ag	Ag	Ag	Ag	Ag	Ag
Sb	Sb	Cu	Cu	Cu	Cu	Cu	Co	Co	Co	Co	Ag	Ag	Fe	Fe	Fe	Sb	Fe	Sb
Cu	Cu	Sb	Sb	Sb	Sb	Sb	Sb	Sb	Sb	Ag	Sb	Sb	Sb	Sb	Sb	Fe	Hg	Fe
Si	Si	Si	Ag	Si	Ag	Ag	Ag	Ag	Ag	Sb	Co	Co	Hg	Hg	Hg	Hg	Sb	Hg
Bi	Ag	Ag	Si	Ag	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Hg	Co	Co	Co	Co	Co	Co
Ni	Ni	Hg	Bi	Bi	Si	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi
Ag	Bi	Bi	Hg	Hg	Bi	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si	Si
Hg	Hg	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni
Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd	Pd
Au	Au	Te	Te	Au	Au	Te	Au	Au	Au	Au	Au	Au	Au	Au	Au	Au	Te	Te
Te	Te	Au	Au	Te	Te	Au	Te	Te	Te	Te	Te	Te	Te	Te	Pt	Te	Au	Au
Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Te	Pt	Pt	Pt
Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir
Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C

Table No. VII.—Potassic Cyanide Solutions at 55° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	400,	500,	Saturated solution.
Mg	Al	Mg	Mg	Al	Al	Al	Mg	M	Mg	Mg	Mg	Mg	Zn	Zn	Zn
Zn	Mg	Zn	Zn	Mg	Zn	Zn	Al	Zn	Zn	Zn	Zn	Zn	Cu	Mg	Cu
Al	Zn	Al	Al	Zn	Cu	Mg	Zn	Al	Cu	Cu	Cu	Cu	Mg	Cu	Mg
Cd	Cu	Cu	Cu	Cu	Mg	Cu	Cu	Cu	Al	Al	Al	Al	Cd	Cd	Cd
Cu	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Al	Al	Al
Sn	Sn	Au	Au	Pd	Au	Ag	Au	Au	Au	Ag	Ag	Ag	Pd	Pd	Pd
Tl	Co	Ag	Pd	Ag	Pd	Au	Pd	Ni	Ag	Au	Au	Au	Ni	Ag	Au
Pb	Ni	Sn	Ag	Au	Ag	Pd	Ag	Ag	Pd	Pd	Ni	Ni	Au	Au	Ag
Co	Ag	Co	Co	Co	Ni	Ni	Ni	Pd	Ni	Ni	Pd	Pd	Ag	Ni	Ni
In	In	Ni	Ni	Ni	Co	Hg	Sn	Sn	Sn	Sn	Hg	Sn	Sn	Sn	Sn
Au	Au	Pd	Sn	Pb	{ Pb	Co	Pb	Hg	Hg	Hg	Sn	Hg	Pb	Hg	Hg
Ag	Tl	Tl	Tl	Tl	{ Tl	Sn	Co	Pb	Pb	Pb	Pb	Pb	Co	Pb	Co
Sb	Hg	Pb	Pb	Hg	Hg	Pb	Hg	Co	Co	Co	Co	Co	Hg	Co	Pb
Ni	Pb	In	Hg	Sb	Sb	In	Tl	Sb	Sb	Tl	Sb	Tl	Tl	Tl	In
Hg	Si	Sb	In	Sn	Sn	Tl	In	Tl	Tl	Sb	Tl	Sb	Sb	In	Tl
Pd	Fe	Hg	Sb	In	In	Sb	Sb	In	In	In	In	In	In	Sb	Te
Fe	Pt	Bi	Bi	Bi	Bi	Bi	Te	Te	Te	Te	Te	Te	Te	Te	Sb
Bi	Pd	Te	Fe	Te	Te	Te	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi	Bi
Pt	Sb	Fe	Si	Si	Fe	Fe	Si	Si	Si	Si	Fe	Fe	Fe	Fe	Fe
Te	Bi	Si	Te	Ir	Ir	Si	Fe	Pt	Fe	Fe	Si	Si	Si	Si	Si
Si	Ir	Pt	Pt	Fe	Si	Ir	Ir	Fe	Pt	Pt	Pt	Pt	Pt	Pt	Pt
Ir	Te	Ir	Ir	Pt	Pt	Pt	Pt	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir
Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C

Ag in 160; Tl and Al in 1,000; and with Fe and Hg in the saturated solution.

Table No. VI.—Temporary reversals occurred with Co and Fe in solutions of 1, 5, 10, 40, and 80 grs.; Si and Ni in 160, 200, 280, 320, 360, 400, and 500; Pd and Ni in 800, 1,000, and saturated solution; Hg and Fe in 800; In and Fe in 200; Si and Ag in 10; and Sb and Cu in the 5 grs. solution.

Table No. VII.—Temporary reversals occurred with Zn and Mg in solutions of 40, 80, 120, 160, 240, 280, and 360 grs.; Al and Mg in 40, 120, 160, 240, and 320; Cd and Mg in 400; Cu and Mg in 80, 400, and 500; Pd and Mg in 80; Al and Zn in 5, 40, 80, 120, and 160; Cu and Zn in 80 and 160; Cu and Al in 10, 20, 80, 160, 200, and 280; Cd and Al in 10, 20, 240, 280, and 320; Cd and Cu in 5, 10, 20, and 160; Au and Ag in 10, 20, 80, 160, 240, 280, and 500; Pd and Ag in 10, 40, and 120; Sn and Pd in 200; Sb and Pd in 5; Tl and Pd in 10; Bi and Pd in 1; Fe and Pd in 1; Ni and Pd in 160, 200, and 320; Au and Sn in 20; Ni and Ag in 5, 200, 400, and saturated solution; Co and Ag in 80; Sn and Ag in 5 and 10; Sb and Ag in 1; Ni and Au in 80 and 120; Ni and Sn in 20, 80, 160, 280, and 500; Ni and Pt in 5; Ni and Pb in 10, 40, and 80; Pb and Tl in 10, 20, and 40; Hg and Pb in 160; In and Pb in 5 and 160; In and Bi in 320; Sb and Pb in 240; Sb and Tl in 200 and 500; Sn and In in 80; Sn and Sb in 80; Fe and Bi in 40, 200, 320, 360, and saturated solution; Fe and Si in 160, 200, 500, and saturated solution; Sb and Tl in 280 and 320; Bi and Te in 280; Sb and Bi in 5; In and Au in 5; In and Hg in 10; Tl and Hg in 5; Sn and Pb in 500; Sb and In in 500; Sb and Te in 500; Hg and Sn in saturated solution; Co and Ni in 80, 200, 240, and saturated solution; Co and Pb in 240, 280, and 500; Co and Sn in 280 and saturated solution; Co and Pb in 240, 280, and 500; Co and Sn in 280 and saturated solution; Co and Sb in 360; Co and Tl in 360; Pt and Ir in 10, 80, 160, 240, and 400; Pt and Si in 240, 320, and 400; Fe and Pt in 280 and 360; Pt and Rh in 80; and C and Rh in 280.

Table No. VIII.—Temporary reversals occurred with Zn and Mg in solutions of 120, 200, 240, 280, 320, and 400 grs.; Al and Mg in 40, 200, 320, and saturated solution; Cd and Mg in 500; Cu and Mg in 40, 280, and 320; Al and Zn in 1, 10, 120, 160, and 240; Cu and Al in 10, 20, 160, 200, 320, and 360; Cd and Al in 10; Au and Ag in 240 and saturated solution; Sn and Pd in 200, 500, and saturated solution; Tl and Pd in 10; Au and Pd in 10; Au and Sn in 160 and 240; Ni and Ag in 80, 400, and 500; Sn and Ag in 400 and 500; Ni and Sn in 360; Ni and Pb in 20, 200, and 240; Hg and Pb in 120; Fe and Bi in 500; Fe and Te in 20, 120, and 160; Si and Te in 160; Tl and In in saturated solution; Co and Ni in 1, 80, 120, and saturated solution; Co and Pb in 320 and 360; Co and In in 320 and 400; Co and Sn in

Table No. VIII.—Potassic Cyanide Solutions at 100° F.

Grains 1,	5,	10,	20,	40,	80,	120,	160,	200,	240,	280,	320,	360,	400,	500,	Saturated solution.
Mg	Mg	Mg	Mg	Zn	Zn	Al	Mg	Zn	Mg	Zn	Zn	Mg	Zn	Zn	Zn
Al	Zn	Zn	Zn	Al	Al	Zn	Zn	Mg	Al	Mg	Mg	Zn	Mg	Cu	Cu
Zn	Cd	Al	Al	Mg	Mg	Cu	Al	Al	Zn	Cu	Al	Al	Cu	Al	Al
Cd	Al	Cd	Cu	Cu	Cu	Mg	Cu	Cu	Cu	Al	Cu	Cu	Al	Mg	Mg
Cu	Co	Cu	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd	Cd
Sn	Cu	Sn	Pd	Pd	Pd	Au	Pd	Sn	Pd	Pd	Pd	Pd	Au	Au	Sn
Pb	Ni	Pd	Sn	Ni	Au	Pd	Au	Pd	Sn	Sn	Au	Ag	Pd	Sn	Pd
Tl	Sn	Au	Au	Au	Ni	Ag	Sn	Au	Au	Au	Sn	Sn	Ni	Pd	Au
Pd	In	Ag	Ag	Ag	Ag	Ni	Ag	Ag	Ag	Ag	Ag	Au	Sn	Ni	Ag
In	Au	Co	Co	Co	Pb	Sn	Ni	Ni	Ni	Ni	Ni	Ni	Ag	Ag	Ni
Au	Tl	Ni	Ni	Pb	Co	Co	Co	Co	Hg	Hg	Hg	Pb	Pb	Hg	Hg
Ag	Ag	Tl	Pb	Tl	Tl	Pb	Pb	Pb	Pb	Co	Pb	Hg	Hg	Co	Co
Ni	Pb	Pb	Hg	Sn	Hg	Hg	Hg	Hg	Tl	Pb	In	Co	Tl	Sb	Pb
Co	Pd	In	Tl	Hg	Sn	Tl	Tl	Tl	Co	Tl	Tl	Tl	In	Pb	Tl
Hg	Hg	Sb	In	Sb	Sb	Sb	Sb	Sb	Sb	Sb	Sb	In	Co	Tl	In
Sb	Si	Hg	Sb	In	In	In	Bi	In	In	In	Co	Sb	Sb	In	Sb
Fe	Sb	Bi	Bi	Bi	Bi	Bi	In	Bi	Te	Bi	Bi	Fe	Fe	Fe	Bi
Bi	Bi	Fe	Si	Si	Te	Te	Te	Te	Bi	Te	Te	Bi	Bi	Bi	Fe
Si	Fe	Te	Te	Fe	Fe	Fe	Fe	Si	Fe	Fe	Fe	Te	Te	Te	Te
Te	Te	Si	Fe	Pt	Si	Si	Si	Fe	Si	Si	Si	Si	Pt	Si	Si
Ir	Ir	Ir	Pt	Te	Ir	Pt	Pt	Pt	Pt	Pt	Pt	Pt	Si	Pt	Pt
Pt	Pt	Pt	Ir	Ir	Pt	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir	Ir
Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh	Rh
C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C

5 and 120; Co and Sb in 320 and 400; Co and Tl in 360 and 400; Co and Hg in 320, 360, and 500; Pt and Si in 10; and C and Rh in the 5 grs. solution.

Table No. IX.—In Fused Cyanide of Potassium.

The following was the order obtained by immersing the various elementary substances in cyanide of potassium in a fused state; the first substance being the most positive: magnesium, zinc, aluminium and platinum, nickel, iron, silver, iridium, gold, rhodium, carbon, palladium, cobalt, antimony.

From the notes appended to the foregoing tables, it may be observed—1st, that by far the greatest number of momentary reversals of current took place in the cyanide solutions, especially in the cold ones; 2nd, that the proportions of such reversals to the number of solutions employed were not greatly different in the chloride, bromide, and iodide of potassium; and 3rd, the reversals were not confined to the more electro-positive metals. The most numerous reversals in the cold solutions of potassic cyanide took place with nickel (26), aluminium (21), silver (18), platinum (16), magnesium (15), and zinc (14); and in the hot solutions, with aluminium (16), magnesium (14), and zinc (11); whilst in the cold iodide solutions the most frequent ones were nickel (7), iron (6), silicon (4), and cobalt (4); and in the hot liquids, nickel (10), silicon (8), iron (7), and cobalt (8); most of these occurred in the strong solutions. With the cold bromide mixtures reversals occurred with aluminium (6), iron (5), gold (5), and palladium (4); with the same liquids hot, indium (6), aluminium (5), and iron (4). And with the chloride solutions cold, iron (3); and hot, aluminium (3), and cadmium (3). The behaviour of particular pairs of metals in different liquids with regard to reversals may be ascertained by inspection of the notes.

Certain other general effects were also observed in the results. 1st. Gas carbon was electro-negative to all other bodies in all the solutions employed, whether cold or hot; concentrated or dilute. 2nd. Rhodium was electro-negative to all substances except carbon, in all solutions of either iodide or cyanide of potassium, of all degrees of concentration, and at each temperature. 3rd. Either rhodium or platinum was negative to all bodies except carbon in all the solutions of chloride or bromide of potassium, whether cold or hot. 4th. Magnesium was positive to all the other substances in all the solutions of potassic chloride, bromide, or iodide, at each temperature. 5th. Either magnesium, aluminium, or zinc, was positive to all other metals in solutions of potassic cyanide, whether cold or hot. Thallium appeared to be the most quickly corroded of any metal in all the solutions, and was most manifestly affected in those of potassic iodide; lead was also acted upon, but to a less extent, in that class of liquids.

By drawing lines through all the positions of each particular metal in the tables of solutions of different strengths of either of the salts employed, it was found that the position of pairs of metals of like properties, such as magnesium and zinc, nickel and cobalt, rhodium and iridium or platinum, lead and tin, &c., often varied together, and described similar curves or lines of variation.

A much greater number of variations of relative electric position of the metals occurred in the cold and hot solutions of potassic cyanide than in either those of the chloride, bromide, or iodide, whether cold or hot, and the amounts of such variations were also much greater in the cyanide solutions.

Agitation of the immersed metals is well known to influence the current. Shaking one of the metals reversed occasionally the direction of the current; palladium was the metal which most frequently manifested this effect. The agitation of that metal rendered it either more negative or less positive in a considerable degree, especially in solutions of potassic chloride and cyanide. In those of the bromide, antimony was the metal the current from which was most affected by shaking. The effect of shaking was less in the iodide solutions than in those of the bromide.

VIII. "On the Spontaneous Segmentation of a Liquid Annulus." By A. M. Worthington, M.A. Communicated by Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester. Received December 6, 1879.

In seeking an explanation of the appearance of a definite number of lobes in the liquid annulus which is formed at a certain stage of the splash of a drop that has fallen vertically on to a horizontal plate, I was led to make some experiments on the spontaneous segmentation of such an annulus lying on a plate with a view to ascertaining whether the relation that exists between the dimensions of the annulus and the number of drops into which it will spontaneously split, is the same as for a straight cylinder of liquid under similar conditions. Finding that the relation was the same, it then occurred to me that by liberating an annulus in air without contact with any solid, a direct experimental proof might be obtained of the law of segmentation of a free cylinder.

It has been shown mathematically in various ways, and experimentally by M. Plateau, that the equilibrium of a free cylinder of any liquid, under the influence of surface tension only, becomes unstable as soon as the length exceeds π times the diameter; and it has been regarded as a necessary consequence of this that such a cylinder, if once realised, will spontaneously split into as many equal and equidistant spheres as π times the diameter is contained in the length.

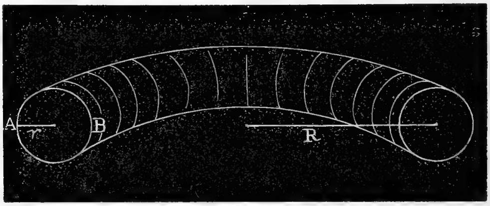
Thus, if n be the number of drops,
l the length,
and d the diameter or thickness of the cylinder, the law of segmentation is expressed by the formula

$$n=\frac{l}{\pi d}.$$

M. Plateau has endeavoured to supply an experimental proof of this by liberating a cylinder of mercury, extended between two amalgamated wires, on a glass plate, but he found that the frictional contact with the plane surface along the length of the cylinder diminished the number of drops, probably because the limits of stability are extended by friction: thus, when the cylinder of mercury rested on a glass plate, the limit of stability appeared to be extended to 6·1 times the diameter, this being the average distance apart at which the drops were formed. So that the law became $n=\frac{l}{6{\cdot}1\times d}$, and it was only by observing the effect of simultaneous contact with a vertical as well as a horizontal plate that a rough estimate could be made of the influence of friction on the number of drops.

By the method of liberation in air that is described in the latter half of this paper all friction is avoided, and the results show a close accordance with theory.

I will first describe experiments with reference to an annulus lying on a plate. It may be well at the outset to remark that an annulus is not, like a cylinder, a figure of equilibrium for a liquid, there being excess of pressure on the outside; this excess, however, will diminish as the radius of the annulus increases,—as, in fact, it approximates to a cylinder. The excess, moreover, will not be a large fraction of the total surface-pressure, if, as in the case we have to deal with, the thickness of the annulus is small in comparison with its width: thus in the figure—



the principal radii of curvature at A are r and $(R+r)$ and at B are r and $-(R-r)$.

$$\therefore \text{ pressure at } A = C + \left(\frac{1}{r} + \frac{1}{R+r}\right)k^2,$$

$$\text{and at } B = C + \left(\frac{1}{r} - \frac{1}{R-r}\right)k^2,$$

where k is a constant, and the difference of pressures at A and B is proportional to $\frac{1}{R+r} + \frac{1}{R-r}$, which is small compared with $\frac{1}{r}$.

It may, therefore, well happen, as we shall see in the sequel that it does, that the annulus will split spontaneously before any appreciable alteration of its form can be caused by this inequality of pressures; and this is the more to be expected if the annulus be lying on a horizontal plate, friction with which impedes the motion of the liquid as a mass.

The method that I have adopted to discover the law of splitting has been to liberate suddenly an annulus of mercury on a horizontal surface of glass or iron.

The apparatus consisted of a circular plate of iron 15 millims. thick and 60 millims. in diameter, the lower surface of which was truly plane, and in it were cut various annular grooves, whose cross section was an exact semicircle.

This plate is placed face downwards on a plane horizontal surface of iron or glass, and mercury is poured into one of the grooves through one of two small vertical holes bored through the plate, the second hole allowing the imprisoned air to escape. After removing any excess of mercury that may remain in the holes, the plate is suddenly lifted by a central handle, and a circle of regularly disposed drops is found lying on the lower plate.

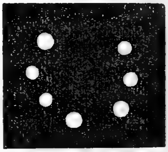
The following is a table of results obtained in this way, which are recorded without any omissions. The size of the grooves was obtained by direct measurement and by weighing the mercury they contained.

A fourth groove of ·7 millim. thickness was found to be too small to use, being very difficult to fill with mercury.

Groove No. 1. Diameter $(d)=$ 2·03 millims.
Principal radius $(R)=25$ "

Method of liberation.	No. of drops.	Remarks.
Plate lifted rapidly....	17	.. Regular, larger at the holes.
" "	18	.. Very regular.
" "	16	.. " "
" "	17	.. " "
" "	18	.. " "
" "	16+(prob. 18)	" "
" "	17	.. Regular; larger at the holes.
" "	19	.. Very regular.
Average....	17·25 (+)	

When a guiding cylinder of cardboard, in which the iron plate could be moved like a piston, was used in order to ensure the plate being lifted vertically, the friction against the cylinder prevented the plate being lifted so quickly, and this at once influenced the number of drops, as will be seen in the following table :—

Method of liberation.	No. of drops.	Remarks.
With well-oiled guiding ring....	16	Regular.
,, ,, ...	16	
,, ,, ...	16	
,, ,, ...	15	
With guiding ring not oiled....	10	5 in opposite quadrants.
,, ,, ...	13	
Lifted slowly on purpose........	10	
,, rather slowly on purpose .	10	
Very slowly on purpose.........	7	Arranged thus: 
,, ,, ...	5	Nearly regular.
,, ,, ...	3	Regular?

2nd Groove. Diameter $(d)=$ 1·04 millims.
Principal radius $(R)=$12·5 ,,

Method of liberation.	No. of drops.
Glass plate, rapid lifting..................	17
,, ,, ...	18
,, ,, ...	17
,, ,, ...	16
,, ,, ...	18
,, ,, ...	16
,, ,, ...	17
,, ,, ...	17
Mean..............	17
Without special precaution as to speed.....	11
,, ,, ,, ...	13
,, ,, ,, ...	12
,, ,, ,, ...	14
,, ,, ,, ...	11
Lifted slowly on purpose.................	10
,, ,, ...	11

3rd Groove. Diameter $(d) = 3{\cdot}17$ millims.
Principal radius $(R) = 22{\cdot}2$ ”

In this case the quantity of liquid was so great that it was found very difficult to lift the plate without giving it an initial disturbance, which caused some of the drops to run together; hence it was impossible to lift the plate as quickly as had been found necessary in the previous cases to secure the maximum number of drops.

Method of liberation.	No. of drops.	Remarks.
On an iron surface, a plate lifted as quickly as was possible without disturbing the liquid..........	7	
” ” ”	8	By computation, some drops having evidently run together.
” ” ”	7	
” ” ”	8	By exact computation from 5 drops.
” ” ”	6	
” ” ”	12	Computed from 5 drops.
” ” ”	9	” ” ”
” ” ”	8	A certain estimate from 6 drops.
” ” ”	7	” ” 5 ”
” ” ”	7	A probable estimate from 5 drops.
” ” ”	8	A probable estimate.
” ” ”	10	” ”
” ” ”	8	” ”
” ” ”	8	
Average....	8(+)	

Before discussing these results, I may remark that the number of drops was practically the same, whether on iron or glass, unless, indeed, the iron was very well rubbed with a dry clean cloth between each experiment. When this was done the number of drops was slightly increased. On one occasion 21 were obtained from the first groove.

In many instances subordinate secondary drops, due to the splitting of the long necks joining the drops before their complete separation, were formed with great regularity; but sometimes these secondary drops were so large as to be easily mistaken for primary drops. In the case of the third groove, it required practice to lift the plate quickly enough to secure the maximum number of drops.

In what follows I shall, for convenience, call the figure that would be formed by bending a cylinder into an annulus, a cylindrical annulus, while the form in which the liquid that has filled a groove is left on the lower plate may be called a semi-cylindrical annulus.

It seems reasonable to suppose that the first thing that will happen to the liquid when liberated will be that it will spring from a semi-cylindrical into a more or less cylindrical annulus, for there is a sharp curvature of the surface at the edges bounding the flat and curved portions, which will *at once* exert a pressure on the liquid, while the force which causes it to split into drops being due to an unstable equilibrium* must begin with a value equal to zero, and will therefore be behindhand in its action.

Since the cylindrical annulus must have the same volume as the semi-cylindrical, its diameter must be $\frac{1}{\sqrt{2}}\times$ diameter of the semi-cylindrical annulus $=\frac{d}{\sqrt{2}}$ according to previous notation.

Hence, if we apply Plateau's formula for the cylinder, we have—

$$n=\frac{l}{6\cdot 1\times\frac{d}{\sqrt{2}}},$$

which gives for the first groove, 17·48 drops.
" " second " 17·38 "
" " third " 10·14 "

The only notable deviation from the number actually observed, is in the case of the third groove, but even here 10·14 is within the limits of observation, and a very possible value, since the plate, as has been explained, could not be lifted up quickly enough to avoid all friction.

The observations, therefore, prove that at least for an annulus of mercury lying on a glass plate, the law of splitting is practically the same as that of a straight cylinder of the same thickness, being expressed by the formula $n=\frac{l}{6\cdot 1\times d}$.

PART II.

In order to effect the liberation of a free annulus in air, the apparatus shown in fig. 1 was used.

A is a circular disk of hard wood, in which two annular grooves were cut, whose cross section was of the form shown in fig. 2, the lower part being circular. The width of the inner groove at the

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* The symmetry of the annulus implies *equilibrium* as regards segmentation.

top was ·4 centim., and of the outer ·625 centim.; the principal diameters being 5·35 centims. and 7·475 centims. respectively. B is a sort of catapult to project the disk vertically downwards, and consists of a short brass tube 3 centims. long, clamped vertically, in which an inner tube *c* slides loosely, but is kept thrust out as far as the slots in the outer tube permit, by the action of a strong spiral spring inside.

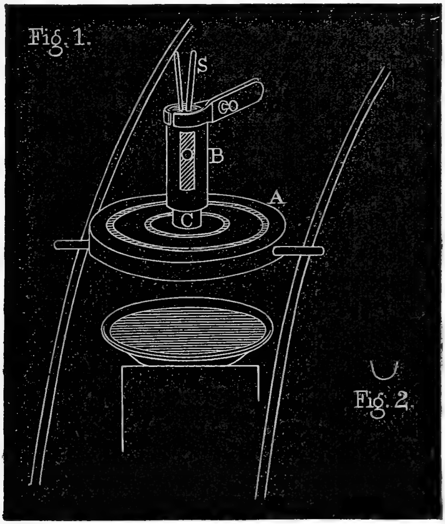
Fig. 1.
Fig. 2.

By looping a string, *s*, over a small central hook in the disk, the latter can be drawn up tight against the end of the outer tube, and held firmly in a horizontal position, and then, after a known weight of mercury has been poured into either groove, the end of the string is let go, and the disk is shot downwards, leaving the mercury annulus in the air. A tray of fine wet sand is placed below the annulus to catch and preserve the impressions of the circle of drops formed, while the disk is steered out of the way by striking with its two projecting arms against the slanting brass rods, as shown in the figure.

The inclination of these rods should be such as to get the disk out of the way as soon as possible, and will depend on the strength of the spring used; the stronger the better.

The object of slanting the sides of the grooves is to prevent the annulus being carried downwards by any friction with the disk.

I have tried smoked glass, lard, and wet sand, for receiving the drops and their impressions—the last is cleanest and answers best; the best consistence is easily found by practice, and depends a little on the depth of fall. If too soft, the impressions of separate drops mingle, if too hard, drops are liable to rebound from their shallow impressions, and to mingle with the neighbouring drops.

If the tray of sand be placed at any considerable distance below the starting point of the annulus, it will be found that the impression made is a single small irregular pit with three or four lobe-like projections, showing that the annulus has contracted as it fell into a single not perfectly symmetrical mass.

This is the natural consequence of the excess of pressure on the outside circumference, which is no longer hindered by any friction, as in the preceding experiments; it is interesting to observe, that as the depth of fall is diminished, the diameter of the whole impression increases, while the number of separate drop-impressions likewise increases, showing that the contraction continues, by reason of impetus, after complete segmentation has taken place, and causes the re-fusion of drops that have at one time been separate; this re-fusion being generally so conducted that the symmetrical arrangement of the drops is tolerably well preserved.

This will be seen from an inspection of the following tables of observations, which are recorded without omissions. A few remarks on the tables are necessary. It often happened that the annulus had not remained circular, but had assumed a more or less elliptical form, as shown by the impressions; the tables contain the measures of the greatest and least diameters in such cases, as well as their mean. These measurements were made with a pair of compasses, and the diameters mentioned joined what were judged to be the middle points of the drop marks on either side of the centre; the measures may be taken as correct to 1 millim., but not beyond.

In most cases the impressions were not equally distinct round the whole of the circumference, but were most distinct and well divided about one end of a diameter, and least so about the other end of the same. When the mark was elliptical this diameter was generally the shortest.

Owing to the fact that each drop did not always remain in the hole it had made in the sand, and that the partitions between neighbouring drop-impressions were often cut through by the neck of liquid that remains till segmentation is complete, it could not be positively asserted in all cases (though it could in some) that the annulus had split into such and such number of drops when it reached the sand at any given depth, but rather that it consisted of a certain countable number of distinct masses of mercury, some of which appeared to consist of two, or in some cases, of three drops, still united by necks of liquid, while the remaining majority consisted of a countable number of separate apparently equal and regularly disposed drops. Then there was in many cases a slight uncertainty as to the exact number, to the extent of one drop more or less, and this could only be eliminated by many observations. As the observations were made before the theoretical number had been calculated, the judgment in estimating doubtful impressions could not have been biassed more than by the inclination to recognise a constantly recurring number of drops.

The re-fusion of the drops, owing to contraction, makes it necessary to take the impression before the annulus has fallen far, if we wish to ascertain the maximum number of drops into which it has originally divided. In the case of the smaller annulus, 9 centims., and in that of the larger, 12 centims., was the least depth of fall that the apparatus permitted, *i.e.*, that was consistent with getting the disk out of the way. The weight of mercury in the first annulus was 26·525 grams, and in the second, 53·05 grams. Assuming that the annulus at once becomes cylindrical on its release, the thickness (d) would be ·3844 centim., and ·4599 centim. in each case respectively, and if the segmentations are determined by the initial dimensions (before contraction) according to the law $n=\frac{l}{\pi d}$, the number of drops in the first case (n_1) will be 13·91, and in the second case (n_2), 16·25. It will be seen by reference to the smallest height of fall in each case that the corresponding experimental values are :—

$$n_1=13$$
$$\text{and } n_2=16{\cdot}1$$

Annulus No. 1.

(Initial diameter=5·35 centims.)

Depth of Fall.	Diameters.			Number of Drops.	Remarks.
	Maximum.	Minimum.	Mean.		
centims.	centims.	centims.	centims.		
47	3·8 (= extreme diameter.)			..	4 lobes.
38	..	..	..	..	Traces of 5 or 6 lobes.
21·5	4·8	3·4	4·1	9	
	4·6	4·2	4·4	9 or 10	
	4·2	3·8	4	10	
	4 5	4	4·25	8 or 9	
				8	
	4·5	4·5	4·5	9	
	4·5	4·5	4·5	8+	
		Mean=	4·3	9(−)	
20·5	4·3	3·9	4·1	9	*Certain.*
18·3	4 8	4·2	4·6	10	Very regular.
	5·15	4·35	4·75	10	Regular.
	5·2	4·25	4 6	10 or 11	Probably 11.
	4·9	4·5	4·7	10	5 very regular.
		Mean=	4·66	10(+)	
15·2	5·5	4 85	5·17	13	Probably; segmentation not complete.
	4·8	4·8	4·8	12	
	4·75	4	4·38	13	Probably.
		Mean=	4·8	13(−)	
9	..	..	..	13	Probably.
				12	(Possibly 13); 9 separate masses; very certain.
				12 or 13	
	4·7	4·7	4·7	13	Probably; 11 masses, or 12, if one not secondary; 7 perfectly symmetrical and equal marks in an exact semicircle.
				13 or 14	
				13 or 14	
	4·9	4·9	4·9	13 or 14	Quite circular; 10 masses.
				12 or 13	11 masses.
		Mean=	4·8	13	

Annulus No. 2.

(Initial diameter = 7·475 centims.)

Depth of Fall.	Diameters.			Number of Drops.	Remarks.
	Maximum.	Minimum.	Mean.		
centims.	centims.	centims.	centims.		
12	6·85	6·85	6·85	17	14 masses; 11 drops perfectly regular.
	6·85	6·85	6·85	16 or 17	Perfectly circular.
				17	Probably.
	6·65	6·65	6·65	16	Probably; 8 in one semicircle; sand rather hard.
	6·65	6·5	6·57	15 or 16	Sand wetter.
	6·65	6·65	6·65	15 or 16	
		Mean =	6·7	16·1	

It will be observed that, in the case of the smaller annulus, the average experimental value of n is nearly a whole drop below the theoretical value. This points to the possibility of contraction before the segmentations were determined. Such contraction would cause a diminution in the value of the ratio $\frac{l}{d}$, both by diminishing the numerator and increasing the denominator of the fraction, and would produce a corresponding diminution in the number of drops.

Since the force of contraction is inversely proportional to the principal radius of the annulus, its effect will be more rapid in the smaller annulus, both on this account and because of the smaller mass of liquid to be set in motion.*

That the annulus sometimes becomes elliptical, is probably due to an uneven distribution of the liquid at starting. When the disk was not quite horizontal the thickest and thinnest portions of the annulus would be opposite to each other, and the segmentation of the latter will be complete before that of the former. This accords with the previous observation that the well and ill-divided parts are always at opposite ends of the same diameter.

To account for the fact that this diameter is the shortest, it would

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* It should be remembered that the contractive force ceases to act on drops which have assumed the spherical form. Hence the total amount of contraction that is found to have taken place at any time after complete segmentation will depend on the velocity acquired while the force acted, and therefore upon the time required for the process of segmentation. This time will evidently be less in the case of a thinner annulus.

be necessary to evaluate the average centripetal velocities during the fall of the portions of the annulus at the positions of maximum, mean, and minimum thickness, for a given distribution of the liquid, which can only be done when the connexion between time of segmentation and thickness is known.

The very fact of segmentation taking place sooner in one portion of the annulus than another is itself a source of asymmetry, as was shown by the experiment of liberating a portion of an annulus, which was found to contract in length and straighten itself with great rapidity.

Thus after a fall of 12 centims., a portion of annulus No. 1, represented in fig. 3, gave on the sand the impression represented in fig. 4.

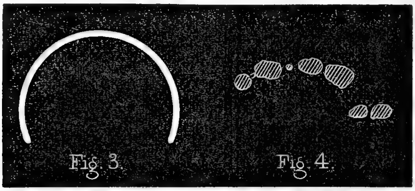

Fig. 3. Fig. 4.

The sharp curvature of the free ends is of course the cause of the shortening. Its influence was often noticed in the case of elliptical marks, inasmuch as the extremities of the row of ill-divided impressions were separated from the extremities of the well-divided portion by spaces rather greater than the average distance between neighbouring drops elsewhere.

It is evident that the method of liberation adopted in these experiments is not applicable to a straight cylinder, which will immediately begin to contract and shorten under the influence of the pressure of the curved ends, unless it be prevented from doing so by being kept stretched between metallic terminals to which it adheres; such terminals would, unless of relatively great mass, require to be fixed to a frame which of course must fall with the cylinder. The impurity introduced by using an amalgamated metal would be a source of error, as M. Plateau has pointed out in his own experiments, which however, might be avoided by adopting the plan mentioned by Professor Guthrie, "Proc. Roy. Soc.," vol. xiii, p. 458, of using platinum, to which, when once rubbed with an amalgam of sodium, mercury will adhere closely and without amalgamation.

The experiment would, in this manner, be probably quite feasible.

I have endeavoured by smoking the wooden disk, to render it unwetable by water, and so to liberate an annulus of that liquid, but so far without success; the water always finds its way here and there through the coating of lamp-black and adheres to the disk.

IX. "On the Histology of *Hydra fusca*." By T. JEFFERY PARKER, B.Sc., Lecturer on Biology in Bedford College, London, and Demonstrator in the Royal School of Mines. Communicated by Professor HUXLEY, Sec. R.S. Received December 11, 1879.

(*From the Biological Laboratory of the Royal School of Mines.*)

[PLATE 1.]

The few observations I have to offer on this much-discussed subject are partly confirmatory of, partly supplementary to, those of Kleinenberg,* they present a certain agreement with those of F. E. Schulze,† while they are, in great measure, distinctly contradictory of the later researches of Korotneff.‡

1. *The Ectoderm and the Muscular Layer.*—The layer of longitudinal fibres between the ectoderm and the endoderm was discovered by Kölliker, who believed that each fibre was in direct connexion with an endoderm cell. Kleinenberg, in teased specimens, saw that the ectoderm cells tapered towards their inner ends, and that each was continued into a simple or branched process, of precisely the same character as the fibres seen in sections: from this observation the important conclusion was arrived at, that the fibres were in direct continuity with the ectoderm cells, thus forming a sort of nascent mesoderm.

Schulze figures the elements of the middle layer as fusiform fibres, with somewhat jagged edges. Korotneff, following Kleinenberg's directions as to methods of preparation, came to the conclusion that the ectoderm cells were expanded (*élargie*) at their inner ends, and that each carried a fusiform refringent fibre, attached by its middle to the enlarged base of the cell, and projecting beyond it in either direction, so that the cell appeared as a lateral appendage (*annexe*) of the fibre, rather than the fibre as a prolongation of the cell.

How M. Korotneff can have come to this conclusion as to the shape of the ectoderm cells, it is rather difficult to imagine; by any ordinary method of preparation it is perfectly easy to satisfy oneself that the ectoderm cells of the body are, as a rule, markedly distinguished from those of the endoderm by the tapering of their inner ends; and, in good specimens, that these ends are continued into longer or shorter filaments.

The question of the exact relations of the fibres is by no means so easy to decide. Anyone working at *Hydra* for a week or two,

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* "Hydra," 1872.

† "Ueber den Bau u. die Entwicklung von Cordylophora lucustris," 1871.

‡ "Histologie de l'Hydre et de la Lucernaire." "Arch. de Zool. exp.," t. v, (1876), p. 369.

and using various methods of preparation, might readily frame a dozen different theories on this point, all equally supported by appearances. But the matter seems to me to be entirely set at rest by thin longitudinal sections of specimens preserved in ammonic bichromate, which reagent usually has the effect of causing a certain amount of separation between the layers. In such sections (fig. 1) the ectoderm cells (*ec.*) are distinctly seen to taper off towards their inner ends; the fibres (*m.p.*) to pass from them, at a sharp angle, towards the endoderm, or, more correctly, towards the supporting lamella; and, in some cases (*e.g.*, the fibre to which the line from *m.p.* points), the fibres can be distinctly traced into the attenuated extremities of the cells.

As to the true nature and functions of these structures, Dr. Kleinenberg calls the ectoderm cell with its filamentous process, a neuromuscle cell; M. Korotneff prefers to name it an epithelio-muscle cell; Prosessor Huxley* considers that the fibres "are solely internuncial in function, and therefore the primary form of nerves." This last view is rendered, to say the least, decidedly improbable, by the great number and the regular disposition of the fibres. It seems, *à priori*, unlikely that an animal devoid of all muscular tissue should have a layer of close-set longitudinal nerve-fibres throughout its whole body, while such an arrangement is perfectly intelligible in a set of specially contractile filaments, developed as a means of rapid retraction of the body.

The term "neuro-muscular" implies, as Kleinenberg explains, that the process only is contractile; the function of the cell itself being merely to receive and transmit impressions. But, as Professor Huxley points out, it is absolutely necessary to assume contractility in the cell proper, to account for the lengthening of the body. The fibres merely have a special degree of contractility assigned to them, in correspondence with the obvious advantage accruing to the animal from the power of instantaneous shortening, the general contractility of the cells serving for extension; this movement being, as observation of a living *Hydra* shows, a comparatively slow one. The fibres must also be of use in the characteristic "looping" movements of the animal.

The simplest and most reasonable way of looking at these structures is that adopted by Dr. Michael Foster, and illustrated in the diagram at the beginning of the third chapter of his "Text-book of Physiology." These show clearly enough that the ectoderm cell of *Hydra*, with its muscular process, is the equivalent of what, in the higher animals, becomes sensory cell, sensory nerve, nerve cell, motor nerve, and muscle cell. So that a fairly logical term might be made by

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* "Anat. of Invert. Animals," p. 64.

combining Kleinenberg's and Korotneff's, and speaking of epithelio-neuro-muscle cell; but, fortunately, it is unnecessary to employ any such cumbersome term, and quite sufficient to speak of ectoderm cell with contractile process.

The interstitial tissue, discovered by Kleinenberg, is quite readily made out in all parts of the body except the proximal end, where nematocysts are also absent. It is not mentioned by Korotneff, and, indeed, its existence would be impossible if the large ectoderm cells had the shape described by him.

I have found no interstitial cells in the tentacles (fig. 5); this would seem to show that the ordinary ectoderm cells may also be the mother cells of the nematocysts. The ectoderm cells of the tentacles also differ from those of the body from the fact that their nuclei are non-nucleolate, resembling indeed the nucleoli of the body cells, rather than their nuclei (fig. 5).

2. *The Supporting Lamella.* This structure is clearly distinguished by Schulze and by Korotneff, the latter of whom, however, figures it* as almost equal in thickness to the diameter of an ectoderm cell! Kleinenberg states that the muscular processes are imbedded in a structureless cementing substance, and that this, continued beyond the muscular layer on the endoderm side, forms a layer—the "Stützlamelle" of Reichert—which can sometimes be obtained as a separate structure.

This description by no means expresses the distinctness of the supporting lamella. In specimens preserved in osmic acid, or ammonium bichromate, without subsequent treatment with alcohol, it is easy, by teasing with fine needles, to detach shreds of considerable extent, more or less free from attached muscular fibrils and from cells of the interstitial tissue (fig. 2).

3. *The Endoderm.*—The ciliation of the endoderm is a question about which there has been a good deal of discussion. Schulze figures a single flagellum to each cell, as seen in optical section of the tentacle. Kleinenberg was unable to demonstrate the existence of flagella in the uninjured animal, or in preserved specimens, but in transverse sections of the living animal, he observed one or two cilia, in connexion with more or fewer of the cells, and noticed that they were not fixed structures, but were occasionally retracted, and then protruded again, the cells at the same time sending out pseudopodial processes.

It is quite easy to confirm this observation; the slow lashing movement of the flagelliform cilia, their continual disappearance and reappearance in fresh places can be made out without difficulty. But the best notion of the characters and relation of the cilia is obtained by teasing out, or still better, by cutting thin sections of

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* *Loc. cit.*, Pl. 15, fig. 8.

osmic acid specimens. Such preparations quite lead one to think that the endoderm is ciliated throughout; in the sections particularly, (fig. 3), cell after cell is seen bearing one, two, or three cilia. These latter are of great length, in fact nearly or quite as long as the cells to which they are attached; in some cases indeed, they are longer, as, for instance in the cell to the right in fig. 6. I have never seen anything like a "collar" at the base of any of the cilia.

The amœboid character of the endoderm cells, as seen in sections or teased fragments of the living animal, is a well-known fact; but the extent and activity of the amœboid movements during life has not been sufficiently insisted on. In sections of picric acid or ammonic bichromate specimens, large rounded pseudopodia are seen to be given off from the cells into the digestive cavity, sometimes to such an extent as completely to obliterate the latter. The length of the cells may, therefore, vary almost indefinitely; they may be but little longer than the ectoderm cells (fig. 3), or may be two or three times as long (fig. 1). This variation in the size of the endoderm cells, and the consequent variation in the diameter of the digestive cavity, is very marked in my series of sections, nearly all of which are taken from large specimens,* killed in a state of half-extension. When the endoderm cells are fully extended, it is almost impossible to obtain them complete by teasing. They nearly always break across, and can only be obtained in a fragmentary condition.

A very noticeable point about the endoderm cells is the presence in their protoplasm, especially towards the far end, of dark-coloured irregular granules, of various sizes. It has been suggested that these are products of excretion; Kleinenberg makes the important observation that their number varies with the state of nutrition of the animal.

I am convinced that these bodies are food particles, taken into the protoplasm of the cells, from the partially disintegrated bodies of the *Entomostraca* in the digestive cavity. They are of quite the same nature as the contents of the alimentary canal in many of the common *Cladocera* and *Copepoda;* they occur chiefly in the free end of the cell, and in some cases they have all the appearance of being half in and half out of the protoplasm. (See fig. 5.) The particles of the more transparent parts of the body of the Crustaceans will naturally not be so evident in the cell protoplasm; even these, however, can be made out in a *Hydra* in full digestion, when the endoderm cells of the distal or gastric region are completely crammed with transparent spheroids.

The clearest case of ingestion of solid particles is that shown in fig. 1, *d*, when a diatom is seen to be completely imbedded in the protoplasm of a cell.

If this explanation of the dark granules is the correct one, *Hydra*

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* Supplied by Mr. Bolton, of Birmingham.

will have been shown to exhibit a process of alimentation identical with that described by Metschinkoff, in the lower *Turbellaria* and in sponges.* The Russian observer describes the complete obliteration, during digestion, of the digestive cavity in the Turbellarians, and of the canals in the sponges; and, in the former as well as the latter, he has undoubted evidence of the actual ingestion of solid particles by the endoderm cells.

It would seem, therefore, that *Hydra* adds another instance to the two already brought forward by Metschnikoff, of a Metazoon exhibiting what is usually considered to be a distinctively Protozoan mode of digestion. It is quite possible that a preliminary disintegration of the animals taken in is performed by juices secreted by the endoderm cells, but the final digestion seems to take place in the actual protoplasm of the cells, into which the food articles are taken in the solid form.

The endoderm cells of the tentacles resemble those of the proximal and of the body in possessing larger vacuoles (fig. 5). Their nuclei are in some instances, although not constantly, simple and non-nucleolate like those of the ectoderm cells for the same region (fig. 5).

Finally, I have been able fully to confirm Professor Huxley's statement† as to the presence of nematocysts in the endoderm (fig. 1, *n*), a statement which, as far as I am aware, has not been made, with regard to *Hydra*, by any other writer on the subject. This fact is, like the absence of interstitial tissue in the tentacles, an argument against Kleinenberg's view that the tissue is the sole source of the nematocysts.

4. *Methods.*—For sections, the *Hydræ* were either killed with hot water, and placed in Kleinenberg's picric acid for two hours, or were placed alive in ammonic bichromate, 1 per cent.—which always kills them in the half extended condition—and kept in it for two or three days. In either case they were afterwards transferred to 50 per cent. alcohol, and then placed successively in 75 per cent., 90 per cent., and absolute alcohol. The specimens were stained either with carmine or picrocarmine, and imbedded in cacao butter, after soaking for a short time first in oil of cloves and then in melted cacao butter. By this means they became so thoroughly permeated with the imbedding material that they could be cut without the loss of a single section; even longitudinal sections of the tentacles could be made with ease.

For teasing I employed ammonic bichromate, acetic acid (0·5 per cent.), or osmic acid (1 per cent.), and for this purpose the specimens were not transferred to alcohol, but to weak glycerine (equal parts of glycerine and water), in which they were teased out.

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* "Zool. Anzeiger," Bd. I (1878), p. 387, and "Zeitsch. f. wiss. Zool." Bd. xxxii (1879). It need hardly be said that the above view of the physiology of digestion in *Hydra* was suggested by these papers of Metschinkoff's.

† Huxley and Martin, "Elementary Biology," p. 100.

For sections showing the cilia of the endoderm, the *Hydrœ* were kept for twenty-four hours in 1 per cent. osmic acid, then washed, and preserved in weak glycerine until required for cutting. The sections were cut by Dr. Pritchard's very convenient freezing microtome, the specimens being placed in gum water before freezing.

DESCRIPTION OF PLATE.

Reference letters.

ec., ectoderm.
m., middle layer.
m. p., muscular processes.
s. l., supporting lamella.
i. t., interstitial tissue.
ne., nematocysts.
en., endoderm.
f., food particles.
d., diatom included in an endoderm cell.
c., cilia.

Figure 1. Longitudinal section of the body, distal end, (ammonic bichromate—alcohol—eosin). The specimen was killed in full digestion, the endoderm cells being gorged with food particles, amongst which is a diatom frustule. The ectoderm and endoderm are unnaturally separated by the action of the bichromate, and the supporting lamella is not shown. The nuclei are not well shown, the specimen having been unfortunately stained with eosin instead of carmine.

Figure 2. Fragment of the supporting lamella, obtained by teasing, (osmic acid—weak glycerine). The wavy character of many of the adhering muscular processes is well seen.

Figure 3. Longitudinal section of the body, distal end, (osmic acid—weak glycerine), showing particularly the flagella of the endoderm cells. The boundaries of the ectoderm cells are not seen.

Figure 4. Transverse section of the body, proximal end, (picric acid—alcohol—carmine), showing the pseudopodia of the endoderm cells and the included food particles, as also the absence of interstitial tissue and of nematocysts in this region.

Figure 5. Longitudinal section of a tentacle, (picric acid—alcohol—carmine). In this section, selected as showing the food particles in the protoplasm of the endoderm cells, no nematocysts, nor palpocils are to be seen.

Figure 6. Longitudinal section of the body, distal end, to show the typical structure of the body wall.

Figures 1—5 are accurately drawn, each from a single specimen, except for the fact that the diatom *d*, in fig. 1, has been inserted from a similar, but otherwise less favourable specimen. Fig. 6 is a combination of several preparations, being the correct proportions retained.

All the figures were drawn with a Gundlach's $\frac{1}{16}$th immersion (for the use of which I am indebted to Professor Huxley) on Hartnack's small stand, and with his No. 3 eye-piece.

The Society adjourned over the Christmas Recess to Thursday, January 8, 1880.

Parker.

Fig

Fig. 4

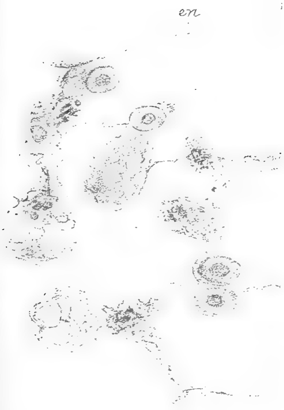

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Fig. 1.

Fig. 6.

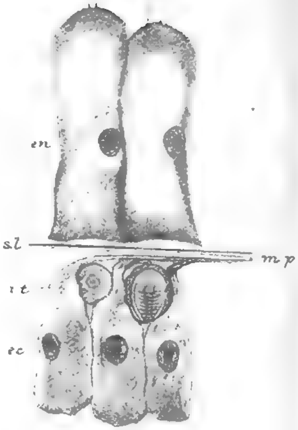
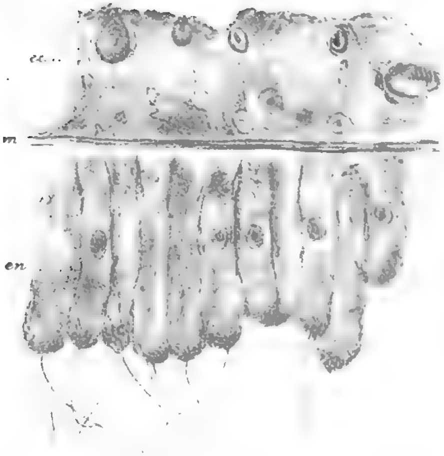


Fig. 3.



Scale
50 m m

Fig. 4.

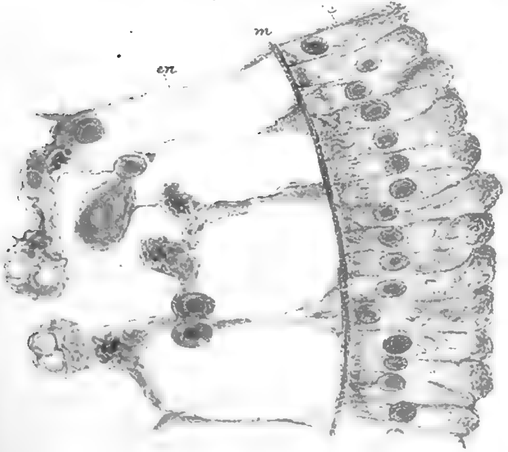


Fig. 5.

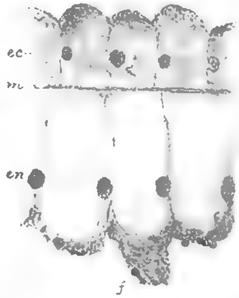
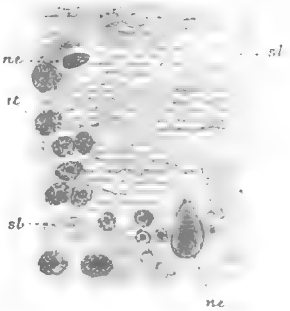


Fig. 2



T.J.P. delᵗ

West Newman & Co lith.

January 8, 1880.

THE PRESIDENT in the Chair.

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

The Right Hon. Thomas George Baring, Lord Northbrook, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

THE BAKERIAN LECTURE—"On the Photographic Method of Mapping the Least Refrangible End of the Solar Spectrum (with a Map of the Solar Spectrum from 7600 to 10750)"—was then delivered by Captain W. DE W. ABNEY, R.E., F.R.S. The following is an abstract:—

The author refers to the sensitiveness of different forms of silver salts when exposed to the action of the spectrum, and shows how he has been able to prepare, by methods indicated, silver bromide which absorbs the red and ultra-red rays, and which is sensitive to these rays.

In his paper he describes the apparatus employed by him in the photography of the invisible least refrangible rays, both with a prismatic, and also with the diffraction apparatus. From photographs taken with the latter, he has constructed a map extending from λ 7600 to λ 10750, which he submits to the Society. He has also compared Lamansky's prismatic thermograph and that of Sir J. Herschel with his photograph of the prismatic spectrum. The paper closes with some theoretical remarks on the silver compounds employed.

January 15, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. "On Chemical Repulsion." By EDMUND J. MILLS, D.Sc., F.R.S. Received January 8, 1880.

While engaged in some researches on the propagation of chemical change, I have incidentally encountered a new order of phenomena, which the title "chemical repulsion" may serve provisionally to designate. A brief outline of the experiments is given in the following paragraphs.

Upon a glass plate, laid in a horizontal position, is poured enough solution of baric chloride to cover it completely to a considerable depth. On this solution is placed another glass plate, provided with a small central perforation; when the two plates are firmly pressed together with the hands, most of the solution is extruded, and only a very thin layer of it left between the plates. All excess of the solution having been removed from the outer surfaces of the plates as well as from the perforation, some dilute hydric sulphate is now introduced into the perforation. This reagent attacks the baric chloride, throwing down a white precipitate of sulphate; and, proceeding partly by diffusion, partly by flow, does not cease to widen in every direction its figure of advance, until the edges of the plates are attained. If the perforation is circular, the figure of advance is circular; in other words, the chemical development of a circle is a circle.

Let us now suppose the two plates to be square and equal, and let the upper one have two circular perforations, equidistant from the centre of the square, and situated upon its diagonal. Let also two circular developments of baric sulphate be caused to proceed, as before, from the two perforations simultaneously. At first, nothing remarkable is observed; but in a short time, the two growing circles begin to exercise a visible retardation on each other's progress; so that the figure of advance is no longer circular, but oval. [This retardation is of course observed only between the perforations; and not outside them, where the motion is entirely free.] As the development of the figures continues, so also does the retardation at their neighbouring edges increase; the final result being (however long the experiment may be prolonged), that the other diagonal of the square is completely and permanently traced out in a line of no chemical action.

The above experiments are of fundamental importance, and they obviously admit of endless variety Of this, a few illustrations may suffice.

If the upper plate have three perforations, situated on the points of a central equilateral triangle, there are three repulsion lines; these end at the centre of the triangle, where they form a trilocular point, and traverse its sides midway at right angles.

When the upper plate has four perforations, situated on the points of a central square, there are four repulsion lines; these end at the centre of the square, where they form a quadrilocular point, and traverse its sides midway at right angles.

A very beautiful modification of the preceding experiment consists in simultaneously developing a circle from a (fifth) central perforation. This last circle has no means of escape from the surrounding four. The result is, that it eventually forms a square figure bounded by repulsion lines, and having four symmetrically situated repulsion lines at its corners.

It is easy to demonstrate that the chemical repulsion in these experiments does not depend upon flow. Two superimposed triangular plates for instance, in neither of which is any perforation, give three repulsion lines on immersion in dilute hydric sulphate. From each corner a line proceeds midway (if the triangle be equilateral) to the centre. In this effect, diffusion is alone concerned.

In addition to hydric sulphate and baric chloride, other pairs of reagents may be used with success; and I anticipate no difficulty in obtaining results in which precipitation is not concerned. A beginning has also been made with experiments in tridimensional development.

The complete explanation of what I have termed "chemical repulsion," will probably demand a varied and considerable amount of experimental work. From some incidents of the investigation, so far as it has hitherto proceeded, I am disposed to believe that the motion in any plane chemical figure is not along the radius, but at right angles to the radius; and this supposition will, if verified, explain the repulsion. The existing results afford proof of the following propositions, viz. :—(1.) *Chemical action can take place at a distance;* and (2.) *Two or more chemical actions identical except in position, completely exclude one another.*

II. "Results of an Inquiry into the Periodicity of Rainfall." By G. M. WHIPPLE, B.Sc., F.R.A.S., Superintendent of the Kew Observatory. Communicated by ROBERT H. SCOTT, F.R.S. Received January 8, 1880.

The exceptionally heavy rainfall of the past spring and summer directed a large amount of attention to the records of rainfall in this country, and more than one investigator stated that he had found a certain periodicity existing in the quantity of rain annually collected.

Dr. Meldrum, Professor Balfour Stewart, Mr. Hennessey, Professor Stanley Jevons, Dr. Hunter, and others, have also widely published theories based upon the assumption that the variation in the yearly amounts of fall depends in some manner upon solar phenomena as exhibited by the changes in the appearance of the sun's surface, thereby indicating a cycle of approximately ten or eleven years' duration; but even among the supporters of this so-termed "sun-spot" theory of rainfall there are differences of opinion as to the exact nature of the influence an increase of sun-spots would exert upon the rainfall of any locality.

Mr. G. J. Symons, in "Nature," vol. vii, p. 143, has partially investigated these theories, and shown the ten-year period does not obtain universally.

After reading a paper by Professor Stewart, in the "Proc. Roy. Soc.," vol. xxix, p. 106, "On a Method of detecting the unknown Inequalities of a Series of Observations," it occurred to me to try the method he employed upon terrestrial magnetic changes, upon the annual values of the rainfall, in order to deduce from the observations themselves the true periodicity, if one existed.

With a view of dealing with the largest mass of material possible, I took the long series of rainfall observations made at Paris from 1689 to 1875, published by M. Marié Davy, in the "Annuaire de l'Observatoire de Montsouris."

These I proceeded to discuss on Professor Stewart's plan, but finding it entailed great labour, and held out little prospect of eventually giving a good result, I soon abandoned it, and after several experiments, adopted the following method, which would detect at once the presence of a cycle of an integral number of years in duration.

Starting with an assumption of a period, which I first made five years in length, and subsequently extended, I grouped all the observations together, first in five-year groups, then in six-year, then in seven-year, and so on, year by year, until I reached thirteen years. The means of these furnished a set of curves, showing the variation from the mean in the amount of annual rainfall for each of the years composing the series under consideration. Perhaps I shall make the operation clearer by explaining in detail the actual method of working finally adopted.

Taking the table of yearly totals, the dates were written down on paper side by side, in horizontal rows, in red chromographic ink, and then the corresponding rainfalls in blue. From this sheet a number of impressions were worked off, each of which was cut into slips, afterwards gummed together, so as to form a long strip, exhibiting in a straight line the whole table of rainfall.

The strips were next divided into short lengths corresponding to the period to be investigated (*e.g.*, for a five-year period, each piece contained the values for five consecutive years), and then gummed on a sheet of paper one beneath the other, so that eventually we had five columns of figures, the means of which represented the average rainfall for each of the years represented by

$$\begin{array}{l}
(1+5)\ldots\ldots(1+2\cdot5)\ldots\ldots(1+3\cdot5)\ldots\ldots(1+n5) \\
(2+5)\ldots\ldots(2+2\cdot5)\ldots\ldots(2+3\cdot5)\ldots\ldots(2+n5) \\
(3+5)\ldots\ldots(3+2\cdot5)\ldots\ldots(3+3\cdot5)\ldots\ldots(3+n5) \\
(4+5)\ldots\ldots(4+2\cdot5)\ldots\ldots(4+3\cdot5)\ldots\ldots(4+n5) \\
(5+5)\ldots\ldots(5+2\cdot5)\ldots\ldots(5+3\cdot5)\ldots\ldots(5+n5)
\end{array}$$

The mean of the whole series also being known, it became possible to form a curve, showing the variation in the amount of rainfall upon the assumption of a five-year period.

By applying the same process to other copies, curves were finally drawn for cycles extending respectively over six, seven, eight, nine, ten, eleven, twelve, and thirteen years, and the points of maximum and minimum noted for each curve.

Tables I to IX, and figs. 1 and 2, exhibit the result of this process as applied to the whole series of observations discussed, including the Paris results.

Next, the whole of the observations were plotted down, and a long curve was drawn through them; from this the maximum years of fall were determined and noted. The dates of these maxima and minima were then compared with the dates of corresponding maxima and minima of a curve based upon the five-year cycle, as determined above, and the number of coincidences and non-coincidences noted.

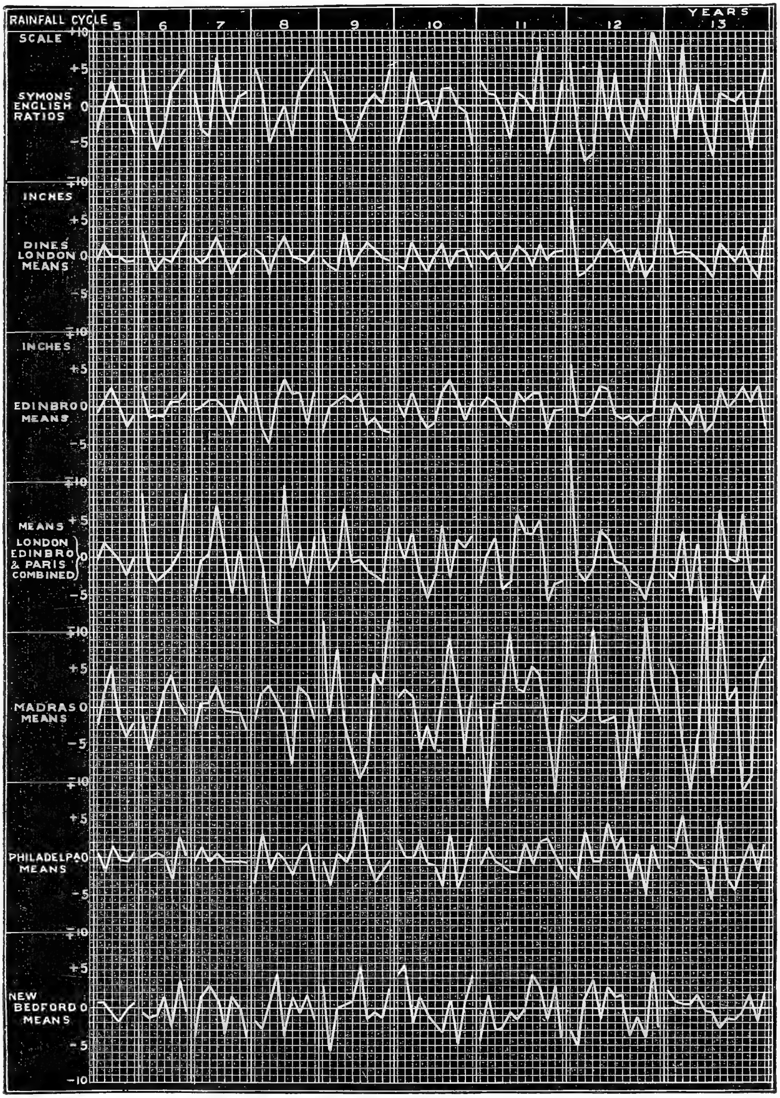
The same operation was performed with each of the cycles, and finally the ratio existing between the number of coincidences and non-coincidences was calculated. The results are given in Table X.

This table shows that in no one case is there any indication of a period of any integral number of years from five to thirteen inclusive running through them.

Hence, whatever period of variation in rainfall there may be, coincident with fluctuations in the spotted surface of the sun, either of ten,

Fig. 1.

Curves of Rainfall Variation in Cycles of 5 to 13 Years.



eleven, or twelve years, this method of treatment shows it to be completely masked (in a long series of observation) by other variations.

The discrepancies exhibited in the first tables obtained made it very desirable to extend the field of inquiry, by including as many observations in the discussion as possible; but having fixed upon the arbitrary limit of fifty years as the least number of years of observations suitable for dealing with after my method, the materials for doing so were not very abundant. Eventually I was able to collect the following ten series :—

Station.	Periods.	No. of years.	Authority.
Paris..........	1689—96, 1699—1754, 1773—97, 1804—75 .	161	Annuaire de l'observatoire de Montsouris, 1879
Padua........	1725 to 1878..........	154	MSS. from P. Denza
England (Symons' table) ..	1726 to 1865..........	140	B. A. Report, 1866
Milan........	1764 to 1878..........	115	MSS. from P. Denza
London......	1813 to 1878..........	66	Dines and Symons
Madras......	1813 to 1877..........	65	"Nature," vol. xviii, p. 565
Philadelphia ..	1810 to 1867..........	58	Smithsonian Tables, p. 97
Edinburgh....	1822 to 1878..........	57	"Nature," vol. xviii, p. 97
New Bedford ..	1814 to 1867..........	54	Smithsonian Tables, p. 90
Rome........	1825 to 1878..........	54	MSS. from P. Denza

To these I added an eleventh one, forming a series by combining together the annual rainfall for 1822 to 1875 at London, Paris, and Edinburgh. This increased the total number of years of observations used in the discussion to 978.

The result of the extended investigation in no way affected the conclusion pointed out by the observations previously treated, viz., that taking the series of annual totals *directly as they stand*, there is no marked indication of the presence of a short cycle to be found. There are a few exceptions, notably in the case of the nine-year period for New Bedford and Philadelphia, the eleven-year cycle for Madras, and the ten-year cycle for London, Paris, and Edinburgh combined, in all of which cases the coincidences but slightly preponderate over the non-coincidences.

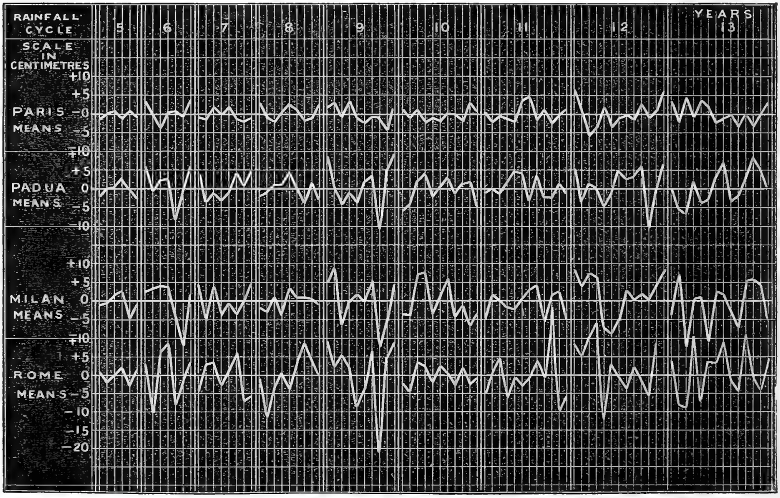
Again on consideration of the differences in Tables I to IX, it will be seen that for the same epoch the curves of variation differ widely for localities comparatively close together. For example, taking the eleven-year cycle for Padua and Milan, stations only about 130 miles apart, both well situated for observing rain, and no mountain range intervening, the variation curves are as follows :—

Year	1800 +11*n*	1801 +11*n*	1802 +11*n*	1803 +11*n*	1804 +11*n*	1805 +11*n*	1806 +11*n*	1807 +11*n*	1808 +11*n*	1809 +11*n*	1810 +11*n*
Padua	−1·3	−0·3	−1·7	+1·1	+4·2	+4·2	−4·9	+3·4	−2·8	−2·8	+1·7
Milan	−5·0	+1·5	+0·2	−1·9	−2·5	0·0	+3·0	+4·7	−5·6	+2·6	+3·3

These show that the years of greatest rainfall at Padua are represented by the formula [1804 or 5+11*n*], and of least by [1806+11*n*], whilst for Milan the maximum occurs at [1807+11*n*], and the minimum at [1808+11*n*].

FIG. 2.

Curves of Rainfall Variation in Cycles of 5 to 13 Years.



Further inspection of the tables will show numerous other instances of incongruity to be found in every one of the cycles, leading forcibly to the conclusion that either no short term of exactly five, six, seven, eight, nine, ten, eleven, twelve, or thirteen years exists in the annual amount of rainfall at any of the stations whose observations have been here discussed, or that the effect of abnormal falls is so great that it cannot be eliminated by upwards of a hundred years' observations.

In any case, I think it may now be stated with certainty that all predictions as to rainy or dry years, based upon existing materials, must in future be considered as utterly valueless.

My thanks are due to Mr. R. H. Scott, Director of the Meteorological Office, for several valuable suggestions, and to Padre F. Denza, Director of the Moncalieri Observatory, for manuscript copies of the Italian observations.

Table I.—5-year Rainfall Cycle.

Stations.		Means.	1800 + 5*n*.	1801 + 5*n*.	1802 + 5*n*.	1803 + 5*n*.	1804 + 5*n*.
Symons' English	Means......	Ratios.	90·8	94·6	97·6	94·4	94·4
	Differences..	94·3	−3·5	+0·3	+3·3	+0·1	+0·1
Dines' London	Means	inches.	23·9	26·3	24·6	24·6	23·8
	Differences..	24·6	−0·7	+1·7	−0·0	−0·0	−0·8
Edinburgh	Means......	26·4	25·4	26·9	29·1	26·6	24·0
	Differences..	..	−1·0	+0·5	+2·7	+0·2	−2·4
London, Edinburgh, and Paris	Means......	70·1	70·0	72·1	71·0	69·9	67·7
	Differences..	..	−0·1	+2·0	+0·9	−0·2	−2·4
Madras	Means......	48·4	46·3	50·2	54·1	47·1	44·3
	Differences ..	..	−2·1	+1·8	+5·7	−1·3	−4·1
Philadelphia	Means.... .	41·6	42·4	39·8	43·2	41·4	41·2
	Differences ..	..	+0·8	−1·8	+1·6	−0·2	−0·4
New Bedford.......................	Means......	41·4	42·2	42·2	41·2	39·8	41·5
	Differences..	..	+0·8	+0·8	−0·2	−1·6	+0·1
Paris	Means......	centims.	47·8	48·7	49·5	47·1	49·4
	Differences ..	48·5	−0·7	+0·2	+1·0	−1·4	+0·9
Padua	Means......	86·0	83·7	85·9	86·2	88·8	85·8
	Differences ..	..	−2·3	−0·1	+0·2	+2·8	−0·2
Milan	Means......	97·5	96·6	96·9	98·7	100·6	92·6
	Differences ..	..	−0·9	−0·6	+1·2	+3·1	−4·9
Rome	Means......	76·2	77·2	74·9	75·8	79·1	73·3
	Differences ..	..	+1·0	−1·3	−0·4	−2·9	−2·9

Table II.—6-year Rainfall Cycle.

Stations.		Means.	1800 + 6*n*.	1801 + 6*n*.	1802 + 6*n*.	1803 + 6*n*.	1804 + 6*n*.	1805 + 6*n*.
Symons' English	Means	Ratios. 94·3	99·5	91·9	88·1	91·5	96·5	98·2
	Differences		+5·2	−2·4	−6·2	−2·8	+2·2	+3·9
Dines' London	Means	inches. 24·6	27·8	23·8	22·4	24·5	23·8	25·5
	Differences		+3·2	−0·8	−2·2	−0·1	−0·8	+0·9
Edinburgh	Means	26·4	28·6	25·1	25·2	25·2	27·0	27·1
	Differences	..	+2·2	−1·3	−1·2	−1·2	+0·6	+0·7
London, Edinburgh, and Paris	Means	70·1	78·2	68·6	67·0	67·8	68·7	70·4
	Differences	..	+8·1	−1·5	−3·1	−2·3	−1·4	+0·3
Madras	Means	48·4	47·4	42·3	47·6	50·9	52·7	49·3
	Differences	..	−1·0	−6·1	−0·8	+2·5	+4·3	+0·9
Philadelphia	Means	41·6	41·4	41·5	42·0	41·6	38·7	44·6
	Differences	..	−0·2	−0·1	+0·4	−0·0	−2·9	+3·0
New Bedford	Means	41·4	41·0	40·2	40·4	42·9	38·8	45·1
	Differences	..	−0·4	−1·2	−1·0	+1·5	−2·6	+3·7
Paris	Means	centims. 48·5	52·0	48·8	44·9	48·7	49·4	47·4
	Differences		+3·5	+0·3	−3·6	+0·2	+0·9	−1·1
Padua	Means	86·0	91·6	85·3	88·1	88·9	77·5	85·1
	Differences	..	+5·6	−0·7	+2·1	+2·9	−8·5	−0·9
Milan	Means	97·5	100·0	101·3	101·7	101·6	94·1	86·1
	Differences	..	+2·5	+3·8	+4·2	+4·1	−3·4	−11·4
Rome	Means	76·1	80·0	65·9	82·7	85·1	68·2	74·8
	Differences	..	+3·9	−10·2	+6·6	+9·0	−7·9	−1·3

Table III.—7-year Rainfall Cycle.

Stations.		Means.	1800 + 7*n*.	1801 + 7*n*.	1802 + 7*n*.	1803 + 7*n*.	1804 + 7*n*.	1805 + 7*n*.	1806 + 7*n*.
Symons' English	Means	Ratios.	96·1	91·2	90·5	100·6	94·4	91·9	95·5
	Differences	94·3	+1·8	−3·1	−3·8	+6·3	+0·1	−2·4	+1·2
Dines' London	Means	inches.	24·7	23·7	24·5	27·1	24·8	22·5	25·1
	Differences	24·6	+0·1	−0·9	−0·1	+2·5	+0·2	−2·1	+0·5
Edinburgh	Means	26·4	25·9	26·4	27·4	27·4	26·3	23·7	28·0
	Differences	..	−0·5	−0·0	+1·0	+1·0	−0·1	−2·7	+1·6
London, Edinburgh, and Paris	Means	70·1	65·1	69·7	70·5	77·1	71·5	65·4	70·9
	Differences	..	−5·0	−0·4	+0·4	+7·0	+1·4	−4·7	+0·8
Madras.	Means	48·4	45·4	49·0	49·1	51·4	48·2	48·0	47·8
	Differences	..	−3·0	+0·6	+0·7	+3·0	−0·2	−0·4	−0·6
Philadelphia	Means	41·6	40·9	43·2	41·0	42·2	41·4	41·1	41·1
	Differences	..	−0·7	+1·6	−0·6	+0·6	−0·2	−0·5	−0·5
New Bedford	Means	41·4	37·5	43·1	44·6	42·5	37·9	42·9	41·6
	Differences	..	−3·9	+1·7	+3·2	+1·1	−3·5	+1·5	+0·2
Paris	Means	cms.	47·5	46·9	50·7	48·4	51·1	48·0	46·8
	Differences	48·5	−1·0	−1·6	+2·2	−0·1	+2·6	−0·5	−1·7
Padua	Means	86·0	90·8	82·0	85·0	82·7	84·4	90·7	86·7
	Differences	..	+4·8	−4·0	−1·0	−3·3	−1·6	+4·7	+0·7
Milan	Means	97·5	102·5	93·5	102·1	94·0	97·9	94·3	98·0
	Differences	..	+5·0	−4·0	+4·6	−3·5	+0·4	−3·2	+0·5
Rome	Means	76·1	70·5	79·1	79·6	74·1	78·2	82·0	69·4
	Differences	..	−5·6	+3·0	+3·5	−2·0	+2·1	+5·9	−6·7

Table IV.—8-year Rainfall Cycle.

Stations.		Means.	1800 + 8*n*.	1801 + 8*n*.	1802 + 8*n*.	1803 + 8*n*.	1804 + 8*n*.	1805 + 8*n*.	1806 + 8*n*	1807 + 8*n*.
Symons' English	Means	Ratios.	99·2	95·3	89·3	92·0	94·4	90·1	95·7	97·9
	Differences .	94·3	+4·9	+1·0	−5·0	−2·3	+0·1	−4·2	+1·4	+3·6
Dines' London	Means	inches.	25·4	25·0	21·9	25·0	27·1	24·9	24·4	23·4
	Differences .	24·6	+0·8	+0·4	−2·7	+0·4	+2·5	+0·3	−0·2	−1·2
Edinburgh	Means	26·4	28·3	24·0	21·4	27·0	29·9	28·3	28·3	24·1
	Differences .	..	+1·9	−2·4	−5·0	+0·6	+3·5	+1·9	+1·9	−2·3
London, Edinburgh, and Paris	Means	70·1	73·1	69·6	61·6	61·1	79·7	68·5	72·0	66·4
	Differences .	..	+3·0	−0·5	−8·5	−9·0	+9·6	−1·6	+1·9	−3·7
Madras	Means	48·4	46·9	50·2	51·4	49·4	47·6	40·7	51·4	50·5
	Differences .	..	−1·5	+1·8	+3·0	+1·0	−0·8	−7·7	+3·0	+2·1
Philadelphia	Means	41·6	38·4	45·0	40·1	42·2	41·6	39·6	42·6	43·6
	Differences .	..	−3·2	+3·4	−1·5	+0·6	−0·0	−2·0	+1·0	+2·0
New Bedford	Means	41·4	39·7	38·7	41·9	46·1	37·5	42·8	40·9	43·3
	Differences .	..	−1·7	−2·7	+0·5	+4·7	−3·9	+1·4	−0·5	+1·9
Paris	Means	cms.	51·4	47·5	45·7	48·7	51·1	49·8	46·5	47·2
	Differences .	48·5	+2·9	−1·0	−2·8	+0·2	+2·6	+1·3	−2·0	−1·3
Padua	Means	86·0	83·7	84·7	86·3	86·8	90·9	86·8	81·7	87·4
	Differences .	..	−2·3	−1·3	+0·3	+0·8	+4·9	+0·8	−4·3	+1·4
Milan	Means	97·5	96·0	94·7	98·9	93·9	101·0	98·8	98·6	97·9
	Differences .	..	−1·5	−2·8	+1·4	−3·6	+3·5	+1·3	+1·1	+0·4
Rome	Means	76·1	76·0	65·1	72·7	77·0	73·4	80·0	85·6	79·5
	Differences .	..	−0·1	−11·0	−3·4	+0·9	−2·7	+3·9	+9·5	+3·4

Table V.—9-year Rainfall Cycle.

Stations.		Means.	1800 + 9*n*.	1801 + 9*n*.	1802 + 9*n*.	1803 + 9*n*.	1804 + 9*n*.	1805 + 9*n*.	1806 + 9*n*.	1807 + 9*n*.	1808 + 9*n*.
Symons' English	Means......	Ratios 94·3	98·9	97·1	92·6	92·4	89·5	92·3	94·9	95·9	94·9
	Differences..		+4·6	+2·8	−1·7	−1·9	−4·8	−2·0	+0·6	+1·6	+0·6
Dines' London.......	Means......	inches. 24·6	24·1	23·1	22·6	27·7	23·1	24·7	26·5	25·3	24·4
	Differences..		−0·5	−1·5	−2·0	+3·1	−1·5	+0·1	+1·9	+0·7	−0·2
Edinburgh..........	Means......	26·4	29·7	26·3	26·8	27·7	27·0	28·1	24·1	24·8	23·3
	Differences..	..	−3·3	−0·1	+0·4	+1·3	+0·6	+1·7	−2·5	−1·6	−3·1
London, Edinburgh, and Paris	Means......	70·1	74·0	68·5	69·5	76·3	69·3	69·7	68·6	68·0	67·0
	Differences..	..	+3·9	−1·6	−0·6	+6·2	−0·8	−0·4	−1·5	−2·1	−3·1
Madras	Means......	48·4	59·9	47·7	56·0	46·9	42·6	39·1	41·0	52·9	51·6
	Differences..	..	+11·5	−0·7	+7·6	−1·5	−5·8	−9·3	−7·4	+4·5	+3·2
Philadelphia	Means......	41·6	41·0	38·3	42·3	41·0	43·3	48·3	42·5	38·5	39·7
	Differences..	..	−0·6	−3·3	+0·7	−0·6	+1·7	+6·7	+0·9	−3·1	−1·9
New Bedford........	Means......	41·4	44·1	35·3	41·3	41·8	42·5	46·8	39·8	40·7	40·2
	Differences..	..	+2·7	−6·1	−0·1	+0·4	+1·1	+5·4	−1·6	−0·7	−1·2
Paris	Means......	cms. 48·5	50·4	51·8	47·8	52·1	47·8	46·2	48·3	47·8	44·4
	Differences..		+1·9	+3·3	−0·7	+3·6	−0·7	−2·3	−0·2	−0·7	−4·1
Padua.............	Means......	86·0	95·2	85·9	81·2	85·7	82·8	87·6	89·9	75·1	91·0
	Differences..	..	+9·2	−0·1	−4·8	−0·3	−3·2	+1·6	+3·9	−10·9	+5·0
Milan	Means......	97·5	102·3	106·9	91·0	97·1	99·8	97·1	103·1	85·4	93·7
	Differences..	..	+4·8	+9·4	−6·5	−0·4	+2·3	−0·4	+5·6	−12·1	−3·8
Rome	Means......	76·1	85·7	78·7	81·3	78·8	68·0	73·2	83·2	54·5	81·7
	Differences..	..	+9·6	+2·6	+5·2	+2·7	−8·1	−2·9	+7·1	−21·6	+5·6

Table VI.—10-year Rainfall Cycle.

Stations.		Means.	1800 + 10*n*.	1801 + 10*n*.	1802 + 10*n*.	1803 + 10*n*.	1804 + 10*n*.	1805 + 10*n*.	1806 + 10*n*.	1807 + 10*n*.	1808 + 10*n*	1809 + 10*n*.
Symons' English	Means	Ratios.	89·1	92·6	98·7	94·3	94·6	92·4	96·6	96·6	94·5	93·8
	Differences	94·3	−5·2	−1·7	+4·4	−0·0	+0·3	−1·9	+2·3	+2·3	+0·2	−0·5
Dines' London	Means	inches.	23·2	26·3	26·8	24·2	22·3	24·6	26·3	22·7	25·0	25·5
	Differences	24·6	−1·4	−1·7	+2·2	−0·4	−2·3	−0·0	+1·7	−1·9	+0·4	+0·9
Edinburgh	Means	26·4	26·8	25·0	28·2	25·2	23·5	24·2	28·5	30·0	28·0	24·6
	Differences	..	+0·4	−1·4	+1·8	+1·2	−2·9	−2·2	+2·1	+3·6	+1·6	−1·8
London, Edinburgh, and Paris	Means	70·1	73·0	70·0	73·2	67·8	64·7	67·5	74·2	68·4	72·4	71·4
	Differences	..	+2·9	−0·1	+3·1	−2·3	−5·4	−2·6	+4·1	−1·7	+2·3	+1·3
Madras	Means	48·4	50·0	51·0	49·9	45·1	46·1	43·1	49·4	57·7	51·7	42·2
	Differences	..	+1·6	+2·6	+1·5	−3·3	−2·3	−5·3	+1·0	+9·3	+3·3	−6·2
Philadelphia	Means	41·6	44·2	41·7	41·7	44·3	41·2	40·7	38·0	44·8	37·8	41·2
	Differences	..	+2·6	+0·1	+0·1	+2·7	−0·4	−0·9	−3·6	+3·2	−3·8	−0·4
New Bedford	Means	41·4	45·6	47·0	39·6	43·0	40·7	39·3	38·2	42·5	36·6	42·6
	Differences	..	+4·2	+5·6	−1·8	+1·6	−0·7	−2·1	−3·2	+1·1	−4·8	+1·2
Paris	Means	cms.	49·6	48·1	50·1	46·7	47·1	46·1	49·2	48·9	47·7	52·1
	Differences	48·5	+1·1	−0·4	+1·6	−1·8	−1·4	−2·4	+0·7	+0·4	−0·8	+3·6
Padua	Means	86·0	80·6	82·0	87·9	90·5	83·1	86·7	89·6	84·5	87·1	88·6
	Differences	..	−5·4	−4·0	+1·9	+4·5	−2·9	+0·7	+3·6	−1·5	+1·1	+2·6
Milan	Means	97·5	94·1	93·9	104·5	104·6	94·1	98·8	103·8	93·3	96·8	91·1
	Differences	..	−3·4	−3·6	+7·0	+7·1	−3·4	+1·3	+6·3	−4·2	−0·7	−6·4
Rome	Means	76·1	74·6	71·8	79·8	79·4	73·6	79·3	77·5	72·5	78·8	73·0
	Differences	..	−1·5	−4·3	+3·7	+3·3	−2·5	+3·2	+1·4	−3·6	+2·7	−3·1

Table VII.—11-year Rainfall Cycle.

Stations.		Means.	1800 + 11*n*.	1801 + 11*n*.	1802 + 11*n*.	1803 + 11*n*.	1804 + 11*n*.	1805 + 11*n*.	1806 + 11*n*.	1807 + 11*n*.	1808 + 11*n*.	1809 + 11*n*.	1810 + 11*n*.
Symons' English	Means	Ratios.	97·5	96·0	95·8	94·0	89·8	96·0	95·4	93·6	101·1	88·1	90·7
	Differences ..	94·3	+3·2	+1·7	+1·5	−0·3	−4·5	+1·7	+1·1	−0·7	+6·8	−6·2	−3·6
Dines' London	Means	inches.	25·5	24·2	25·2	22·6	23·5	26·0	25·3	23·2	26·2	24·2	25·2
	Differences ..	24·6	+0·9	−0·4	+0·6	−2·0	−1·1	+1·4	+0·7	−1·4	+1·6	−0·4	+0·6
Edinburgh	Means	26·4	26·2	27·5	27·0	24·8	24·0	28·4	27·2	28·0	28·2	23·6	25·8
	Differences ..	..	−0·2	+1·1	+0·6	−1·6	−2·4	+2·0	+0·8	+1·6	+1·8	−2·8	−0·6
London, Edinburgh, and Paris	Means	70·1	67·0	70·2	72·6	65·8	66·8	76·0	73·2	73·0	75·0	64·4	66·5
	Differences ..	..	−3·1	+0·1	+2·5	−4·3	−3·3	+5·9	+3·1	+2·9	+4·9	−5·7	−3·6
Madras	Means	48·4	48·3	35·0	49·0	49·0	58·3	50·8	50·5	54·3	52·8	45·0	36·8
	Differences ..	..	−0·1	−13·4	+0·6	+0·6	+9·9	+2·4	+2·1	+5·9	+4·4	−3·4	−11·6
Philadelphia	Means	41·6	40·5	43·0	41·2	40·4	39·8	39·6	43·8	40·6	43·8	44·2	42·0
	Differences ..	..	−1·1	+1·4	−0·4	−1·2	−1·8	−2·0	+2·2	−1·0	+2·2	+2·6	+0·4
New Bedford	Means	41·4	37·2	43·0	58·5	38·6	41·0	39·8	41·8	45·8	44·4	40·2	44·6
	Differences ..	..	−4·2	+1·6	−2·9	−2·8	−0·4	−1·6	+0·4	+4·4	+3·0	−1·2	+3·2
Paris	Means	cms.	49·4	46·4	48·1	47·1	46·3	52·2	53·1	46·4	49·9	45·3	48·9
	Differences ..	48·5	+0·9	−2·1	−0·4	−1·4	−2·2	+3·7	+4·6	−2·1	+1·4	−3·2	+0·4
Padua	Means	86·0	84·7	85·7	84·3	87·1	90·2	90·2	81·1	89·4	83·2	83·2	87·7
	Differences ..	..	−1·3	−0·3	−1·7	+1·1	+4·2	+4·2	−4·9	+3·4	−2·8	−2·8	+1·7
Milan	Means	97·5	92·5	99·0	97·7	95·6	95·0	97·5	100·5	102·2	91·9	100·1	100·8
	Differences ..	..	−5·0	+1·5	+0·2	−1·9	−2·5	0·0	+3·0	+4·7	−5·6	+2·6	+3·3
Rome	Means	76·1	70·6	77·2	81·0	69·8	75·4	72·6	74·6	80·8	75·0	95·2	66·0
	Differences ..	..	−5·5	+1·1	+4·9	−6·3	−0·7	−3·5	−1·5	+4·7	−1·1	+19·1	−10·1

Table VIII.— 12-year Rainfall Cycle.

Stations.		Means.	1800 +12*n*.	1801 +12*n*.	1802 +12*n*.	1803 +12*n*.	1804 +12*n*.	1805 +12*n*.	1806 +12*a*.	1807 +12*n*.	1808 +12*n*.	1809 +12*n*.	1810 +12*n*.	1811 +12*n*.
Symons' English ..	Means......	Ratios.	100·2	90·8	86·8	87·9	100·3	92·4	98·6	93·1	89·5	95·4	92·7	104·0
	Differences..	94·3	+5·9	− 3·5	−7·5	−6·4	+6·0	−1·9	+4·3	−1·2	−4·8	+1·1	−1·6	+9·7
Dines' London	Means......	inches.	30·8	22·3	22·6	23·6	25·8	27·2	25·4	25·6	22·2	25·4	21·4	23·4
	Differences..	24·6	+6·2	−2·3	−2·0	−1·0	+1·2	+2·6	+0·8	+1·0	−2·4	+0·8	−3·2	−1·2
Edinburgh	Means......	26·4	32·0	25·4	25·2	26·4	29·2	28·8	25·2	24·7	25·2	23·7	24·8	25·4
	Differences..	..	+5·6	−1·0	−1·2	−0·0	+2·8	+2·4	−1·2	−1·7	−1·2	−2·7	−1·6	−1·0
London, Edinburgh, and Paris........	Means......	70·1	85·0	68·1	66·8	68·8	74·0	73·0	69·7	69·2	67·2	66·5	64·4	68·4
	Differences..	..	+14·9	−2·0	−3·3	−1·3	+3·9	+2·9	−0·4	−0·9	−2·9	−3·6	−5·7	−1·7
Madras	Means......	48·4	47·2	46·5	47·2	58·8	46·5	47·0	47·6	37·2	48·2	41·4	60·2	52·0
	Differences..	..	−1·2	−1·9	−1·2	+10·4	−1·9	−1·4	−0·8	−11·2	−0·2	−7·0	+11·8	+3·6
Philadelphia	Means......	41·6	40·2	38·6	44·8	41·0	41·0	46·0	42·6	44·4	38·5	42·2	36·4	43·2
	Differences..	..	−1·4	−3·0	+3·2	−0·6	−0·6	+4·4	+1·0	+2·8	−3·1	+0·6	−5·2	+1·6
New Bedford	Means......	41·4	38·5	36·5	42·8	45·0	39·8	44·2	43·0	43·2	37·5	40·2	37·5	46·2
	Differences..	..	−2·9	−4·9	+1·4	+3·6	−1·6	+2·8	+1·6	+1·8	−3·9	−1·2	−3·9	+4·8
Paris	Means......	cms.	55·4	49·0	42·7	45·3	51·0	44·6	47·9	48·6	47·2	51·6	47·9	49·9
	Differences..	48·5	+6·9	+0·5	−5·8	−3·2	+2·5	−3·9	−0·6	+0·1	−1·3	+3·1	−0·6	+1·4
Padua..	Means......	86·0	92·8	82·3	87·3	86·3	80·1	83·6	90·5	88·6	89·1	91·4	74·8	86·6
	Differences..	..	+6·8	−3·7	+1·3	+0·3	−5·9	−2·4	+4·5	+2·6	+3·1	+5·4	−11·2	+0·6
Milan	Means......	97·5	106·6	101·6	104·9	103·3	90·8	89·3	93·5	101·0	98·2	99·8	97·8	82·5
	Differences..	..	+9·1	+4·1	+7·4	+5·8	−6·7	−8·2	−4·0	+3·5	+0·7	+2·3	+0·3	−15·0
Rome	Means......	76·1	85·0	61·2	86·0	90·4	63·2	79·2	76·0	71·7	78·5	78·5	74·5	69·2
	Differences..	..	+8·9	−14·9	+9·9	+14·3	−12·9	+3·1	−0·1	−4·4	+2·4	+2·4	−1·6	−6·9

Table IX.—13-year Rainfall Cycle.

Stations.		Means.	1800 + 13*n*.	1801 + 13*n*.	1802 + 13*n*.	1803 + 13*n*.	1804 + 13*n*.	1805 + 13*n*.	1806 + 13*n*.	1807 + 13*n*.	1808 + 13*n*.	1809 + 13*n*.	1810 + 13*n*.	1811 + 13*n*.	1812 + 13*n*.
Symons' English ..	Means....	Ratios. 94·3	99·2	89·9	102·4	91·9	97·1	90·7	87·7	96·2	95·5	95·1	96·3	89·1	95·4
	Differences		+4·9	−4·4	+8·1	−2·4	+2·8	−3·6	−6·6	+1·9	+1·2	+0·8	+2·0	−5·2	+1·1
Dines' London	Means....	inches. 24·6	28·2	24·8	25·0	25·0	24·4	23·4	22·0	26·6	25·2	23·8	25·8	23·2	22·2
	Differences		+3·6	+0·2	+0·4	+0·4	−0·2	−1·2	−2·6	+2·0	+0·6	−0·8	+1·2	−1·4	−2·4
Edinburgh	Means....	26·4	24·2	26·7	25·7	24·0	26·7	23·0	24·2	29·0	26·2	27·2	29·2	27·0	29·2
	Differences	..	−2·2	+0·3	−0·7	−2·4	+0·3	−3·4	−2·2	+2·6	−0·2	+0·8	+2·8	+0·6	+2·8
London, Edinburgh, and Paris	Means....	70·1	72·2	73·2	73·5	64·7	72·0	60·5	60·5	76·2	70·2	69·6	76·0	67·2	64·0
	Differences	..	−2·1	−3·1	+3·4	−5·4	+1·9	−9·6	−9·6	+6·1	+0·1	−0·5	+5·9	−2·9	−6·1
Madras	Means....	48·4	54·8	53·2	43·8	37·4	45·0	62·4	39·2	63·2	49·4	51·2	37·4	39·2	52·8
	Differences	..	+6·4	+4·8	−4·6	−11·0	−3·4	+14·0	−9·2	+14·8	+1·0	+2·8	−11·0	−9·2	+4·4
Philadelphia	Means....	41·6	43·2	42·8	47·2	41·0	40·5	40·5	36·0	47·0	39·0	37·5	41·2	43·6	39·6
	Differences	..	+1·6	+1·2	+5·6	−0·6	−1·1	−1·1	−5·6	+5·4	−2·6	−4·1	−0·4	+2·0	−2·0
New Bedford......	Means....	41·4	43·5	42·4	42·0	42·0	43·2	41·2	41·0	38·7	40·0	40·0	41·0	43·2	39·5
	Differences	..	+2·1	+1·0	+0·6	+0·6	+1·8	−0·2	−0·4	−2·7	−1·4	−1·4	−0·4	+1·8	−1·9
Paris	Means....	cms. 48·5	51·7	45·5	53·5	47·2	52·1	50·0	46·5	47·8	48·7	45·1	48·6	45·4	48·8
	Differences		+3·2	−3·0	+5·0	−1·3	+3·6	+1·5	−2·0	−0·7	+0·2	−3·4	+0·1	−3·1	+0·3
Padua	Means....	86·0	86·1	80·6	80·1	88·0	81·9	82·1	88·2	92·7	82·7	83·8	87·5	93·9	90·5
	Differences	..	+0·1	−5·4	−5·9	+2·0	−4·1	−3·9	+2·2	+6·7	−3·3	−2·2	+1·5	+7·9	+4·5
Milan	Means....	97·5	101·8	105·3	84·9	98·1	98·4	85·7	100·5	99·4	94·8	90·6	103·0	103·2	101·5
	Differences	..	−4·3	+7·8	−12·6	+0·6	+0·9	−11·8	+3·0	+1·9	−2·7	−6·9	+5·5	+5·7	+4·0
Rome	Means....	76·1	79·6	67·2	66·8	87·0	68·7	78·8	78·5	84·8	73·7	69·5	87·7	75·7	71·6
	Differences	..	+3·5	−8·9	−9·3	+10·9	−7·4	+2·7	+2·4	+8·7	−2·4	−6·6	+11·6	−0·4	−4·5

Table X.—Rainfall Periodicity.

Comparison of calculated epochs of maximum and minimum annual rainfall with the actual maxima and minima observed.

Stations.	Symons' English.			Dines' London.			Edinburgh.			London, Edinburgh, and Paris.			Madras.			Philadelphia.			New Bedford.			Paris.			Padua.			Milan.			Rome.			
Years of Observation.	1726 to 1865 (140).			1813 to 1878 (66).			1822 to 1878 (57).			1822 to 1875 (54).			1813 to 1877 (65).			1810 to 1867 (58).			1814 to 1867 (54).			1689 to 1875 (161).			1725 to 1878 (154).			1764 to 1878 (115).			1825 to 1878 (54),			Totals.
Coincidences =For. Non-coin. =Against.	For.	Against.	Ratio.*	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	For.	Against.	Ratio.	Coincidences =110.
Cycles: 5-year	16	40	25	8	15	19	7	14	20	8	13	16	5	21	42	17	30	18	8	13	16	37	92	25	12	50	42	12	34	28	12	31	26	Non-coins. 277
6-year	20	26	13	13	9	7	6	13	21	11	7	6	6	16	27	12	8	7	18	18	10	35	71	20	30	73	24	12	26	22	13	23	18	175
7-year	31	49	16	14	23	16	13	17	13	13	17	13	5	14	28	11	22	20	14	17	12	25	67	27	9	35	39	23	43	19	9	21	23	226
8-year	29	40	14	15	17	11	12	16	13	20	20	10	6	27	45	22	22	10	22	18	8	31	49	16	21	56	27	15	42	28	8	20	25	207
9-year	9	22	24	14	16	11	9	16	18	10	14	14	11	18	18	15	11	7	16	8	5	24	48	20	24	45	19	19	32	17	12	12	10	163
10-year	23	34	15	16	23	14	8	23	29	14	8	6	4	22	55	15	14	9	17	15	9	29	66	23	26	66	25	15	31	21	10	23	23	229
11-year	35	42	12	10	26	26	9	12	13	8	12	15	6	5	8	11	21	19	16	13	8	25	35	14	24	88	36	19	45	24	10	19	19	194
12-year	42	51	12	15	17	11	9	10	11	12	7	9	7	15	21	21	17	8	14	13	9	37	45	12	28	50	18	13	25	19	16	24	15	145
13-year	40	45	11	11	20	18	14	18	13	17	17	10	13	17	13	17	10	6	8	11	14	42	81	19	17	54	32	15	37	25	11	23	21	182

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* In the columns headed *ratio* are given the computed number of *non-coincidences* for every *ten coincidences*,

III. "Studies on the Electric Arc." By Professor JAMES DEWAR, M.A., F.R.S., Fullerian Professor of Chemistry at the Royal Institution. Received January 8, 1880.

In a former communication on the formation of hydrocyanic acid in the electric arc, a more complete examination of the various reactions taking place in the arc with poles of various kinds, and in presence of different gaseous media, was promised.

Various difficulties have impeded the satisfactory progress of the investigation. During the course, however, of numerous experiments, facts of interest have been recorded, which appear worthy of embodiment in a short paper.

Formation of Cyanogen Compounds.

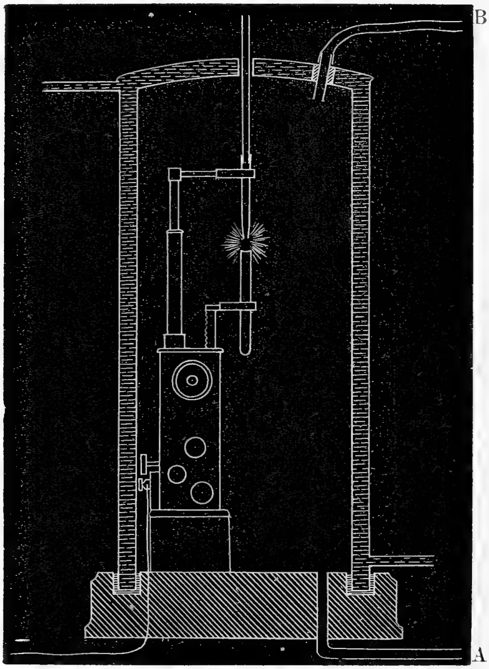
The influence of impurities in the carbon on the production of hydrocyanic acid was first examined. For this purpose, some drilled Siemens' carbons were placed in a porcelain tube, and treated for several days at a white heat with a rapid stream of chlorine, until the greater part of the silica, oxide of iron, alumina, &c., were volatilised in the form of chlorides. Sometimes the carbons had a subsequent treatment with hydrogen, or were directly treated with a current of chlorine while the arc was in operation.

Carbons treated in this way continued to yield hydrocyanic acid, when a steady current of air was drawn through the positive pole as formerly described, even when the same pole had several successive treatments with chlorine during the discharge. Natural graphite poles gave the same result.

As it was evident from the foregoing experiments, that the elimination of a large portion of the impurities had little influence on the production of the hydrocyanic acid, the only other explanation of its formation appeared to be the presence of aqueous vapour, and organic impurities in the air, or a direct formation of cyanogen from carbon and nitrogen. An attempt was made to obtain a pure and dry atmosphere in which such experiments could be carried out. The following apparatus was devised for the purpose.

A tin vessel, fig. 1, about two feet high and one foot in diameter, had an annular space, through which a constant stream of water was kept flowing. This cylinder was placed upon a porcelain stand, having a narrow groove filled with mercury, so as to make an air-tight joint. The lamp was placed inside this vessel, the wires connecting it with the machine being brought through the bottom of the stand. A tube passed through the porcelain base, which allowed a current of dry air to be forced through the vessel. A small aperture in the top of the tin vessel allowed the glass tube coming from the positive pole to pass with little friction, through which the products from the arc were drawn. This annular vessel was very convenient, not only for examining the products formed in the arc, but also those formed outside of it, and the water flowing round it served the double purpose of keeping it cool and enabling a determination of the amount of total radiation in heat units to be made.

Fig. 1.



Vessels containing sulphuric acid and phosphoric anhydride were placed inside the cylinder in order to dry the interior as completely as possible.

Numerous experiments made by forcing perfectly dry air into the vessel through the tube A, and drawing it out by the tube B through a weighed sulphuric acid bulb, gave after an hour a few milligrams of water, owing no doubt to some slight defect in the soldering of the tin, which allowed a capillary film of water to diffuse into the interior space.

When the ordinary Siemens' carbons were burnt in this almost dry atmosphere, the yield of hydrocyanic acid was still very marked, purified carbons yielding the same results.

As the yield of cyanogen compounds did not appear to be diminished, and it appeared almost impossible to get the large volume of air in the tin vessel perfectly dry, another plan was adopted. The poles were enclosed in an egg-shaped glass globe about eight inches long and six inches in diameter, thus diminishing the volume of air to be dried and dispensing with the water covering. The globe, balanced by a system of pulleys, was firmly attached to the lower or negative pole, with which it moved without impeding the action of the lamp.

Dry air was sometimes forced through the negative carbon itself, at other times through a glass tube passing up the side of it into the globe, the products being drawn through the positive pole as before.

As the globe soon became intensely heated, and as a far larger supply of dry air was forced in than was drawn out, it is inconceivable that any moisture could remain near the arc after it had been burning for a few minutes.

Seven consecutive experiments, each of ten minutes' duration, made with the same purified carbon poles, did not show any diminution in the quantity of hydrocyanic acid, unless in one of the experiments, when the arc would not be drawn into the interior of the carbon tube, but persisted in rotating round it.*

The inference drawn from these experiments was, that the drilled carbons, even after prolonged treatment with chlorine, still contained a quantity of combined hydrogen, and a few organic analyses showed that the amount of ash and combined hydrogen in the various samples was never less than about 0·75 of the former and as much as 0·1 of the latter. Poles made with especially purified carbon by Messrs. Siemens for these experiments proved to be no better in respect to the quantity of hydrogen and ash they contained.

The well-nigh impossible problem of eliminating hydrogen from masses of carbon such as can be employed in experiments of this kind, proves conclusively that the inference drawn by Mr. Lockyer,† as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, seeing that undoubtedly hydrogen was present in the carbon, and in all probability nitrogen in the chlorine.

Experiments with Carbon Tubes.

In order to ascertain whether the formation of hydrocyanic acid and acetylene in the arc was really due to transformations induced by some

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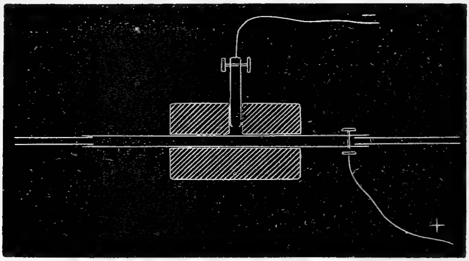
[*] Cyanogen is difficult to recognise in presence of prussic acid when in small quantity, especially when impurities from the carbons complicate the tests. In speaking generally of the formation of this acid in the arc, I do not mean to exclude the possibility of cyanogen being formed as well.

[†] "Note on the Existence of Carbon in the Coronal Atmosphere of the Sun." "Proc. Roy. Soc.," vol. xxvii, p. 308.

occult power located in the arc, or was simply the result of the high temperature attained by the carbons, a series of experiments was made in carbon tubes, the arc being merely used as a means of heating. The following is the method of arranging the arc for this experiment.

A block of limestone about five inches long by three inches thick was drilled horizontally as shown in the drawing, another hole being drilled so as to meet it in the centre of the mass.

Fig. 2.



A drilled purified carbon was placed in the horizontal channel and made the positive pole, the negative pole being a solid rod of carbon passing through the vertical aperture. Gases were passed through the positive carbon, and were thus subjected to the intense heat of the walls of the tube, the arc passing outside.

The walls of the positive carbon burnt through with great rapidity, not lasting as a rule more than fifteen minutes. This action could only be prevented by using much thicker carbons, and consequently reducing considerably the intensity of the heat.

The porosity of the carbons, which allowed a constant diffusion of gases through their walls, was another source of difficulty, which has now been overcome.

On passing a mixture of three volumes hydrogen and one volume nitrogen thoroughly dried through the positive pole, a large yield of hydrocyanic acid was always obtained, and on using equal volumes of hydrogen and nitrogen the quantity was if anything increased.

Pure dry hydrogen by itself gave a trace of hydrocyanic acid, and a considerable quantity of acetylene.

Pure dry air gave no hydrocyanic acid or acetylene; moist air, on the contrary, giving abundance of the former, but only a trace of the latter.

The yield in all these experiments altered considerably with the rate at which the gases were passed, a quick stream always producing more than a slow one, unless when oxygen was present.

Formation of Nitrites in the Arc.

In these experiments the annular vessel was made use of, in which the lamp was allowed to work automatically, often for an hour or two together. A continuous stream of dry air was kept circulating through the interior, being afterwards passed through a series of wash bottles containing dilute caustic soda, or directly through strong sulphuric acid, to absorb the oxides of nitrogen. The nitrous acid was estimated in the former case by titration with permanganate of potash, and the total combined nitrogen by the mercury process.

In this way many experiments were made with a Siemens lamp, both with a long and short arc; Jablochkoff's candles without any insulating material were also employed with the highest intensity current of a De Meritens machine, in order to have the greatest variety in the character of the discharge.

The stream of dry air was forced through at varying degrees of speed, and was found to have a decided effect on the quantity of nitrites produced, the more rapid stream giving the largest yield of nitrites.

The following tables give the quantity produced in a number of different experiments.

The nitrites were calculated as HNO_2.

1. Siemens' machine and lamp. 2. Jablochkoff's candles.

		Nitrates produced in 1 hour.		De Meritens' highest intensity current.
		Siemens' Long arc.	Siemens' Short arc.	
		Mgrms.	Mgrms.	Mgrms.
1st experiment	=	193	28	769
2nd ,, ...	=	804	97	723
3rd ,, ...	=	618	73	1,225
4th ,, ...	=	500	121	548
5th ,, ...	=	622	90	955
6th ,, ...	=	474	85	1,006
7th ,, ...	=	380	..	1,257
8th ,, ...	=	459	..	964
9th ,, ...	=	..	..	664
10th ,, ...	=	..	..	489
11th ,, ...	=	..	..	693
	=	509 mean	..	930 mean.

In these experiments, the total nitrogen estimated by the mercury process was almost identically the same as the amount of nitrogen obtained by a very careful dilution of the acid in a large quantity of water and titration with permanganate, proving that the main product was nitrous anhydride, which may be explained by the fact that the quantity of oxygen in presence of nitrogen in the immediate neighbourhood of the poles is greatly diminished by the combustion of the carbons, or that the nitric peroxide formed is subsequently reduced by contact with the red hot carbon, or other reducing products.

FIG. 3.

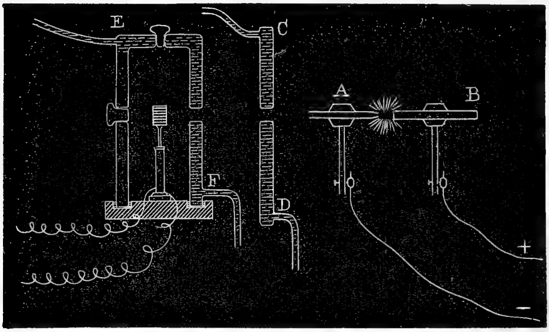
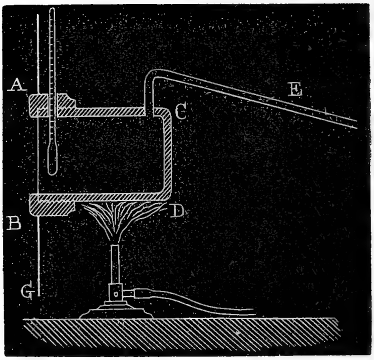


FIG. 4.



Radiation Experiments.

In a report to the British Association* on the determination of high temperatures in the year 1873, it was experimentally proved that the law of Dulong and Petit could not be used for the estimation of high temperatures, seeing that it "gives a far too rapid increase for the total radiation." It was further observed that the variation of the thermo-electric current strength when the same substance radiates at different temperatures, plotted in terms of the temperature, was a

* Report of the Committee for Determining High Temperatures by means of the Refrangibility of the Light evolved by Fluid or Solid Substances. Bradford, 1873. Page 461.

"parabolic curve." Assuming the general accuracy of this law for high temperatures, the total radiation may be taken as nearly proportional to the square of the temperature. From this law the hypothetical temperature of the sun was "estimated as at least 11,000 C." Rosetti has recently made a more elaborate investigation on the subject, and has arrived independently at a formula of a parabolic order. Rosetti * represents his results by the equation

$$\mu = a\mathrm{T}^2(\mathrm{T}-\theta)-b(\mathrm{T}-\theta),$$

where μ is the total radiation measured by thermo-electric current, T° the absolute temperature of the source, θ° that of the medium surrounding the pile, and a and b constants. However well this formula may represent the complete series of the experiments, it is certain that his results for temperatures above 150° may be expressed within the limits of probable error as proportional to the square of the temperature. To be convinced of this, it is sufficient to plot the logarithm of the respective values of the radiation and temperature, when it will be found the results arrange themselves in a straight line, the tangent of which may be 1·9 or 2 for the observations above 150°. Experiments made with the thermopile, surrounded with an annular vessel, through which a continuous current of water at constant temperature is caused to circulate, as represented in fig. 3, where EF represents the section of the vessel, and CD a large water screen, on the same plan, each having a narrow opening, about half an inch in diameter, through which the radiant heat passed to the pile, have confirmed the earlier results. The vessel for holding the mercury or other substance to be heated to different temperatures has a radiating face, which was made of the sheet iron used in the construction of telephone plates, and the thermometer must be placed close to the back of the front surface, and the face guarded with a screen, FG. The tube, CE, is connected with a condenser, when substances at their boiling point are employed for giving fixed points. The form of the apparatus is shown in fig. 4.

This arrangement of the apparatus is necessary in order to get anything like comparable results. The two following tables give the records of two series of experiments, without any correction being made in the numbers representing the deviations of the Thomson galvanometer.

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* "Recherches expérimentale sur la Température du Soleil" (Acad. R. dei Lincei. 1877–78).

Table I.

Temperatnre.	Deviation.	Difference.
80°	32	
90	38·5	6·5
100	44·5	6·0
110	52	6·5
120	59·5	7·5
130	66·5	7·0
140	75·5	9·0
150	85	9·5
160	95·5	10·5
170	106	10·5
180	117	11·0
190	130·5	13·5
200	143·5	13·0
210	158	14·5
220	172·5	14·5
230	188	15·5

Table II.

Temperature.	Deviation.
100°	21
120	29
150	41
160	46
180	57
200	71
220	86
355	240
448	370

If the differences in the galvanometer readings for every ten degrees in the first table be tabulated, it will be observed the second difference may be regarded as constant, considering the errors of observation, so that a parabolic formula can represent the results with sufficient accuracy. These second differences are far more constant than similar numbers deduced from Rosetti's observations, and his more complete formula in terms of the absolute temperature is too extensive, considering the range of the experiments where temperature was accurately known. The results of Table II extend to the boiling points of mercury and sulphur, and the numbers are in near accord with the simple square of the temperature. The alteration in the condition of the radiating surface at high temperatures causes great complications, and until this difficulty is overcome, experiments at higher temperatures must remain uncertain. All the experiments show that for an approach to a knowledge of temperatures beyond the range of our actual thermometric scale, the law given in 1873 is a sufficiently correct reproduction of the facts, considering the limited data at our disposal.

The intensity of the radiation of the positive pole of the Siemens' arc, as compared with the same surface heated with a large oxyhydrogen blowpipe, was determined by employing a hollow negative carbon which allowed the intensely heated surface to radiate directly on to the pile, as shown in fig. 3. A large number of observations have been made by this method at different times, and with slight modifications in the order of the experiments, leading to the average result that the intensity of the total radiation of the positive pole of the Siemens' arc is ten times that of the same substance at the temperature of the oxyhydrogen flame. If we take an average result of nine to one, then we may infer that the temperature of the limiting positive pole is about 6,000° C., seeing that the mean temperature of the oxhydrogen may be taken as 2,000° C. The mean value of the total radiation of the Siemens' arc was determined by observing the rate of flow of the water through the annular vessel, represented in fig. 1, together with the mean increment of temperature. This gave on the average 34,000 gram-units per minute, or a little more than three horse-power.

IV. "On the Spectra of Magnesium and Lithium." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received January 8, 1880.

In a former communication ("Proc. Roy. Soc.," vol. xxvii, p. 350) we described our observations on the absorption spectrum of magnesium, and of magnesium with potassium and sodium, as seen in iron tubes in an atmosphere of hydrogen. These absorptions consisted of—

"(1.) Two sharp lines in the green, of which one, which is broader than the other, and appears to broaden as the temperature increases, coincides in position with the least refrangible of the *b* group, while the other is less refrangible and has a wave-length very nearly 5210. These lines are the first and the last to be seen and very constant, and we at first took them for the extreme lines of the *b* group.

"(2.) A dark line in the blue, always more or less broad, difficult to measure exactly.

"(3.) A third line or band, in the green, rather more refrangible than the *b* group. . . . . The less refrangible edge of this band is sharply defined, and has a wave-length about 5140, and it fades away towards the blue.

"These absorptions are all seen both when potassium and when sodium are used along with magnesium, and may be fairly ascribed to magnesium, or to magnesium together with hydrogen.

"(4). When sodium and magnesium are used together, a dark line with ill-defined edges is seen in the green, with a wave-length about 5300.

"(5.) When potassium and magnesium are used together, a pair of dark lines are seen in the red. The less refrangible of these sometimes broadens into a band with ill-defined edges, and has a mean wave-length of about 6580. The other is always a fine sharp line with a wave-length about 6475.

"(6.) On one occasion, with a mixture of potassium and magnesium, another dark line was seen in the blue, with a wave-length nearly 4820."

We then described the emission spectrum seen when sparks are taken between electrodes of magnesium with a small induction coil without a Leyden jar as consisting of lines with the wave-lengths 4481, 4590, 4570, 4797, 4930, 4945, 4957, 4969, 4980, 4990, 5000, the well-known *b* group, and 5528.

As the absorption lines above described correspond to no known emission lines of magnesium, we could only ascribe their origin to the mixtures employed as distinct from the separate elements, and we were led to investigate the conditions under which corresponding emission lines could be produced.

Shortly afterwards ("Proc. Roy. Soc.," vol. xxvii, p. 494) we described the emission spectrum of sparks from an induction coil taken between magnesium points in an atmosphere of hydrogen, as follows:—

"A bright line regularly appeared with a wave-length about 5210. . . . This line does not usually extend across the whole interval between the electrodes, and is sometimes seen only at the negative electrode. Its presence seems to depend on the temperature, as it is not seen continuously when a large Leyden jar is employed, until the pressure of the hydrogen, and its resistance, is very much reduced. When well-dried nitrogen or carbonic oxide is substituted for hydrogen, this line disappears entirely; but if any hydrogen or traces of moisture be present, it comes out when the pressure is much reduced. In such cases the hydrogen lines C and F are always visible as well. Sometimes several fine lines appear on the more refrangible side of this line between it and the *b* group, which give it the appearance of being a narrow band shaded on that side." "In addition to the above-mentioned line, we observed that there is also produced a series of fine lines, commencing close to the most refrangible line of the *b* group, and extending, with gradually diminishing intensity, towards the blue . . . . . from forty-five to fifty being visible, and placed at nearly equal distances from each other."

These observations, though very briefly described, were the result of much labour and many experiments. Mr. Lockyer, in a recent communication to the Society, has made this particular emission spectrum the basis of a theory of his own, which we regard as being without any foundation in fact; and as he makes no allusion whatever to our observations, although the abstract of our paper appeared in various scientific journals,* or to the grounds on which we have ascribed the spectrum to a mixture of magnesium with hydrogen, we give now some further details of our former experiments, as well as the observations which we have made since.

1. *Observations on the Spark between Magnesium Points in Nitrogen and Carbonic Oxide at Various Pressures.*

The points were pieces of magnesium wire. Round one end of each a platinum wire was tightly coiled and fused into the side of a glass tube. This tube was attached by fusion at one end to another tube filled with phosphoric anhydride, which in turn was connected with a Sprengel pump. The other end of the tube was connected by a thick rubber tube, capable of being closed by a pinchcock, with a gas holder containing nitrogen over strong sulphuric acid. The tube, having been exhausted and filled with nitrogen two or three times, it was found that no line at 5210 was visible in the spark. The tube was now gradually exhausted, and the spark watched as the exhaustion proceeded. No line at 5210 was seen, although the exhaustion was carried nearly as far as the pump would carry it; nor was any hydrogen line (C or F) visible, either with or without the use of a jar. The communication with the gas holder was now opened, and the tube refilled with nitrogen at the atmospheric pressure, a communication was then made with another vessel containing hydrogen, which was allowed to diffuse into the tube for a very short time. On now passing the spark, the line at 5210 at once appeared, although the quantity of hydrogen diffused into the nitrogen must have been very small. The experiments with nitrogen at reduced pressure were repeated several times, with the same result. It was found necessary to have the phosphoric anhydride, as without it traces of moisture were left or found their way through the pump into the tube, and then, when the exhaustion was carried far enough, both the line at 5210 and the hydrogen lines, C and F, made their appearance. We have never, however, been able to detect the line

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* "Nature," vol. xviii, pp. 109, 321. "Proc. Camb. Phil. Soc.," vol. iii, Part V. p. 208.

at 5210, in nitrogen, without being able to detect C or F either at the same time or by merely varying the discharge by means of a Leyden jar.

Experiments made in the same way with carbonic oxide instead of nitrogen led to precisely similar results.

2. *Observations on the Spark between Magnesium Points in Hydrogen at reduced Pressures.*

A tube, similar to those employed with nitrogen and carbonic oxide, was attached at one end to a Sprengel pump and mercury gauge, and at the other to an apparatus for generating hydrogen. Dry hydrogen was passed through for some time, and the connexion with the hydrogen apparatus closed. On sparking with the hydrogen at the atmospheric pressure, the line at 5210 and its attendant series were visible, and were still visible when a small Leyden jar was used with the induction coil, but disappeared almost entirely when a large Leyden jar was used. When the pressure of the hydrogen was reduced to half an atmosphere, the line at 5210 was seen faintly when a large Leyden jar was used, but not the series of fine lines. When the pressure was reduced to 180 millims., the series of fine lines began to show when the large jar was used. By still further reducing the pressure the whole series was permanently visible when the large jar was used; but when the exhaustion was carried still further they grew fainter, and almost disappeared. On gradually readmitting hydrogen, the same phenomena recurred in the reverse order.

3. *Observations on the Arc with Magnesium and Hydrogen.*

The line at 5210 is not seen in the arc in a lime or carbon crucible when magnesium is dropped in without the introduction of hydrogen. If, however, a gentle stream of hydrogen or of coal gas be led in through a perforation in one of the electrodes, the line at 5210 immediately makes its appearance, and, by varying the current, it may be made to appear either bright or reversed. However small the current of hydrogen be made, the line can be detected as long as the current and the supply of magnesium continues, and disappears very quickly when the current ceases.

4. *Observations on the Flame of Burning Magnesium.*

The line at 5210 may often be seen in the flame of magnesium burning in air, but both it and the series of fine lines which accompany it come out with greatly increased brilliance if the burning magnesium be held in a jet of hydrogen, of coal gas, or of steam.

The experiments above described, with nitrogen and carbonic oxide at reduced pressures, are almost if not quite conclusive against the supposition that the line at 5210 is due merely to the lower temperature of the spark in hydrogen. From De La Rue and Müller's observations it would appear that nitrogen at a pressure of 400 millims. should produce much the same effect on the spark as hydrogen at 760 millims. Now the pressures of the nitrogen and carbonic oxide were reduced far below this without any trace of the line in question being visible. Moreover, the magnesium line at 4481, which is not seen in the arc, and may be reasonably ascribed to the higher temperature of the spark, may be seen in the spark at the same time as the line at 5210 when hydrogen is present. Nevertheless temperature does seem to affect the result in some degree, for when a large Leyden jar is used, and the gas is at the atmospheric pressure, the line almost disappears from the spark, to re-appear when the pressure is reduced; but by no variation of temperature have we been able to see the line when hydrogen was carefully excluded.

A line of the same wave-length has been seen by Young in the chromosphere once. Its absence from the Fraunhofer lines leads to the inference that the temperature of the sun is too high (unless at special times and places) for its production. If it be not due to a compound of magnesium with hydrogen, at any rate it occurs with special facility in the presence of hydrogen, and ought to occur in the sun if the temperature were not too high.

We have been careful to ascribe this line and its attendant series to a mixture of magnesium and hydrogen rather than to a chemical compound, because this expresses the facts, and we have not yet obtained any independent evidence of the existence of any chemical compound of those elements. We have independent evidence that mixtures which are not probably chemical compounds favour the production of certain vibrations which are not so strong or are not seen at all when the elements of those mixtures are taken separately. The remarkable absorptions produced by mixtures of magnesium with potassium and sodium above-mentioned belong to this class. We have not been able to obtain the emission spectra corresponding to these absorptions, but in the course of our observations on the arc we have frequently noticed that certain lines of metals present in the crucible are only seen or come out with especial brilliance when some other metal is introduced. This is the case with some groups of calcium lines which are not seen, or barely visible, in the arc in a lime crucible, and come out with great brilliance on the introduction of a fragment of iron, but are not developed by other metals such as tin.

The spectrum of magnesium, as seen in the arc, contains several lines besides those heretofore described. These lines come out brightly, generally considerably expanded, when a fragment of magnesium is dropped into the crucible through which the arc is passing, but rapidly contract and gradually become very faint or disappear entirely.

(1.) A line in the greenish-yellow, wave-length 5710·7. This line is seen not only in the arc but also when sparks are taken from a solution of magnesium chloride. It is a faint line in the latter case, but is seen in carefully purified samples of the chloride, so that we have no doubt that it belongs to magnesium.

(2.) A line, wave-length 4351·2, very close to a chromium line. It is easily distinguished from the latter by its expansion when fresh magnesium is introduced, while the chromium lines show no expansion; it is moreover slightly more refrangible than the chromium line, and as it dies out it is sharply defined on the less refrangible side, diffuse on the other side. This line frequently appears to be accompanied by another weaker line at wave-length 4355, but we have not been able to assure ourselves that the latter is due to magnesium alone. It is certainly developed when magnesium is introduced, and we do not know of any other element to which it can be ascribed; but it behaves very much as some lines of other elements, *e.g.*, the violet lines of lithium mentioned below, coming out brightly when magnesium is freshly put in, and then nearly dying away.

(3.) A violet line, wave-length 4166, identical in position with the sharp edge of one of the so-called carbon bands in that region.

(4.) Another violet line, wave-length 4057·3, easily recognised by its position between two strong iron lines.

These last three lines are conspicuous in Capron's photograph of magnesium in arc. The last (4) may be seen in the spark between magnesium points, and was formerly mistaken by us for the potassium line which is near it. The lines (3) and (4) are so nearly identical in position with two well-known lines of lead, that a suspicion naturally arises that they may be due to the presence of that metal. We can never be sure that the carbons or even the limestone crucibles we have used may not contain traces of lead; but if that be so, and these lines are the consequence, it is certain that these two lines are strengthened in an extraordinary degree by the presence of magnesium, while the other lines of lead are not seen at all. Photographs of the arc taken when a fragment of magnesium is introduced show these lines strongly, while no trace is discernible of other lead lines 4062, 4246, or 4386. Until we get some other evidence of the presence of lead, we must ascribe these lines to magnesium.

There are lines in the "normal solar spectrum" corresponding to all these lines except that at 4166. That at 5710·7 is ascribed to iron by Ångstrom, but neither Thalèn, Kirchhoff, Huggins, nor Boisbaudran give any iron line at this place; and we have not succeeded in seeing any such iron line, but there is an air-line very nearly in that position. Rutherford's photograph of the solar spectrum shows a line very nearly at 4166.

This makes the spectrum of magnesium in the arc consist of lines at 5510·7, 5527·5, the *b* group, 4703·5, 4570·5, 4351·2, 4166, 4057·3, and the triplet beyond H.

On the Spectrum of Lithium.

In the spectrum of lithium we have not heretofore been able to obtain the wave-length of the violet line (at about 4130, Boisbaudran) by reason of its very diffuse character, but we have recently found that by a slight modification of our apparatus we could obtain this line more sharply defined and even reversed. This modification consists in making the opening in the lime small, 5 or 6 millims. in diameter, using small carbons as electrodes, and the current from a De Meritens machine. The line can then be seen close to but on the less refrangible side of the barium line in that region. We make its wave-length 4131·7.

Besides this line we find that lithium gives another line in the arc at 4273. This line comes out conspicuously nearly midway between one of the brightest chromium lines and an iron line when a fragment of lithium is dropped into the crucible. The same line may also be detected in the spark taken between points of fused lithium carbonate; and as a sample of the carbonate very carefully purified shows this line, we have no doubt that it really belongs to lithium.

Both these lines are remarkably strengthened when a fragment of magnesium is dropped into the crucible.

On taking a photograph of the arc when a fragment of lithium is introduced, two more lines come out on either side of H and K. These are generally diffuse, and barely discernible by the eye in the arc when glass prisms are used, but easily seen with a calcite prism and quartz lenses. In the powerful arc of a Siemens machine they are enormously expanded and reversed. From measurements of the photographs the wave-lengths appear to be 3913 and 3984. A yet more refrangible diffuse line appears in the photographs at about 3799.

The lithium lines form a progression not unlike that of the sodium lines, the alternate lines at 4603, 4131·5, 3913 are much more diffuse than the lines at 4972, 4273, and 3984, and approach nearer together as they become more refrangible. There is probably another narrow line at about 3838, amongst the magnesium triplet, which we have not yet made out with certainty, and we may expect to find fainter lines still more refrangible, but as our photographs have been taken with a train of glass prisms they do not show out.

There is a line in the "normal solar spectrum" corresponding to the line at 4273.

While the ratios of the wave-lengths of the red, orange, and green lines of lithium are nearly $\frac{1}{20} : \frac{1}{22} : \frac{1}{27}$, those of the green and blue lines and the two violet lines first above mentioned are nearly $\frac{1}{25} : \frac{1}{27} : \frac{1}{29} : \frac{1}{30}$.

January 22, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. "On certain Definite Integrals." No. 6. By W. H. L. Russell, F.R.S. Received November 15, 1879.

Continuing the methods of the last paper, we obtain the following results:—

$$(101.)\quad \int_{-1}^{+1}\frac{\epsilon^{a+bx+cx^2+ex^3}}{\sqrt{1-x^2}}dx=\pi(\mathrm{A}_0{}^2+\mathrm{A}_1{}^2+\mathrm{A}_2{}^2+\ldots)$$

where A_0, A_1, A_2 are the coefficients of expansion of

$$\epsilon^{\frac{a}{2}+\frac{c}{4}+\left(\frac{b}{2}+\frac{3e}{8}\right)x+\frac{c}{4}x^2+\frac{e}{8}x^3}.$$

Similarly we may find

$$(102.)\quad \int_{-1}^{+1}\frac{\epsilon^{a+bx+cx^2+\cdots+ex^2}}{\sqrt{1-x^2}}\,dx.$$

$$(103.)\quad \int_{0}^{\pi}\frac{\epsilon^{\cos\theta}d\theta}{1-2a\cos\theta+\alpha^2}$$

$$=\frac{\pi}{1-\alpha^2}\left\{1+\frac{1}{2^2}+\frac{1}{1^2\,.\,2^2}\cdot\frac{1}{2^4}+\frac{1}{1^2\,.\,2^2\,.\,3^2}+\frac{1}{2^6}+\ldots\right\}$$

$$+\frac{2\alpha\pi}{1-\alpha^2}\left\{\frac{1}{2}+\frac{1}{1^2\,.\,2}\cdot\frac{1}{2^3}+\frac{1}{1^2\,.\,2^2\,.\,3}\cdot\frac{1}{2^5}+\ldots\right\}$$

$$+\frac{2\alpha^2\pi}{1-\alpha^2}\left\{\frac{1}{1\,.\,2}\cdot\frac{1}{2^2}+\frac{1}{1^2\,.\,2\,.\,3}\cdot\frac{1}{2^4}+\frac{1}{1^2\,.\,2^2\,.\,3\,.\,4}+\ldots\right\}$$

$$+\ldots$$

$$=\frac{2\pi}{1-\alpha^2}\left\{\epsilon^{\frac{\alpha}{2}}+\frac{1}{2\alpha}(\epsilon^{\frac{\alpha}{2}}-1)+\frac{1}{1\,.\,2\,.\,2^2\,.\,\alpha^2}(\epsilon^{\frac{\alpha}{2}}-\frac{\alpha}{2}-1)+\ldots\right\}$$

$$-\frac{\pi}{1-\alpha^2}\left\{1+\frac{1}{2^2}+\frac{1}{1^2\,.\,2^2}\cdot\frac{1}{2^4}+\frac{1}{1^2\,.\,2^2\,.\,3^2}\cdot\frac{1}{2^6}+\ldots\right\}.$$

(104.) $\int_0^\pi \cos\cos\theta d\theta = \int_0^\pi \cos\sin\theta d\theta$

$$=\pi\left\{1-\frac{1}{2^2}+\frac{1}{1^2\,.\,2^2}\cdot\frac{1}{2^4}-\frac{1}{1^2\,.\,2^2\,.\,3}\cdot\frac{1}{2^6}+\ldots\right\}.$$

(105.) $\int_0^\pi \epsilon^{\cos\theta}\,d\theta = \int_0^\pi \epsilon^{\sin\theta}\,d\theta$

$$=\pi\left\{1+\frac{1}{2^2}+\frac{1}{1^2\,.\,2^2}\cdot\frac{1}{2^4}+\frac{1}{1^2\,.\,2^2\,.\,3^2}\cdot\frac{1}{2^6}+\ldots\right\}.$$

In the same manner we may find

(106.) $\int_0^\pi \frac{\epsilon^{\cos\theta}\,d\theta}{(1+2\alpha\cos\theta+\alpha^2)^{\frac{1}{2}}}$.

(107.) $\int_0^\pi \cos r\theta d\theta\,\sqrt{a+b\cos\theta+c\cos^2\theta+c\cos^3\theta}$

$$=\pi(\mathrm{A}_0\mathrm{A}_r+\mathrm{A}_1\mathrm{A}_{r+1}+\mathrm{A}_2\mathrm{A}_{r+2}+\ldots)$$

when A_0, A_1, A_2 . . . are the coefficients of expansion of

$$\sqrt{p+qx+rx^2+sx^3},$$

p, q, r, s being determined by the equations

$$p^2+q^2+r^2+s^2=a+\frac{c}{2}, \qquad 2pq+2qr+2rs=b+\frac{3e}{4},$$

$$2pr+2qs=\frac{c}{2}, \qquad 2ps=\frac{e}{4}.$$

We are able of course to deduce

(108.) $\int_0^\pi d\theta\cos^r\theta\sqrt{}(a+b\cos\theta+c\cos^2\theta+e\cos^3\theta)$,

and similarly the more general integrals

(109.) $\int_0^\pi d\theta\cos^r\theta\sqrt{}(a+b\cos\theta+c\cos^2\theta+\ \ldots\ c\cos^m\theta)$,

and also

(110.) $\int_0^\pi d\theta\cos^r\theta^m\sqrt[m]{}(a+b\cos\theta+c\cos^2\theta+\ \ldots\ +c\cos^m\theta)$.

Since the following integral is known:—

$$\int_0^\omega\frac{dx\sin rx}{\epsilon^{\pi x}-\epsilon^{-\pi x}}=\frac{1}{4}\,\frac{\epsilon^r-1}{\epsilon^r+1},$$

we obtain at once

$$\int_0^\omega \frac{\alpha^r \epsilon^r \sin rx dx}{\epsilon^{\pi x} - \epsilon^{-\pi x}} + \int_0^\omega \frac{\alpha^r \sin rx dx}{\epsilon^{\pi x} - \epsilon^{-\pi x}} = \frac{\alpha^r \epsilon^r}{4} - \frac{\alpha^r}{4}.$$

Hence, if we put

$$\phi(\rho) = \int_0^\omega \frac{dx\,\rho \sin x}{(1-2\rho\cos x+\rho^2)(\epsilon^{\pi x}-\epsilon^{-\pi x})}$$

we have $$\phi(\rho) + \phi\left(\frac{\rho}{\epsilon}\right) = \frac{1}{4}\frac{\rho}{1-\rho} - \frac{1}{4}\cdot\frac{\frac{\rho}{\epsilon}}{1-\frac{\rho}{\epsilon}},$$

so also $$\phi\left(\frac{\rho}{\epsilon}\right) + \phi\left(\frac{\rho}{\epsilon^2}\right) = \frac{1}{4}\frac{\frac{\rho}{\epsilon}}{1-\frac{\rho}{\epsilon}} - \frac{1}{4}\frac{\frac{\rho}{\epsilon}}{1-\frac{\rho}{\epsilon}},$$

and continuing the process

$$(111.)\quad \phi(\rho) = \frac{1}{4}\cdot\frac{\rho}{1-\rho} - \frac{1}{2}\frac{\frac{\rho}{\epsilon}}{1-\frac{\rho}{\epsilon}} + \frac{1}{2}\frac{\frac{\rho}{\epsilon^2}}{1-\frac{\rho}{\epsilon^2}} - \alpha.$$

In like manner we obtain

$$(112.)\quad \int_0^\omega \frac{dx \epsilon^{\rho\cos x}\sin(\rho \sin x)}{\epsilon^{\pi x} - \epsilon^{-\pi x}} = \frac{1}{4}\epsilon^\rho - \frac{1}{2}\epsilon^{\frac{\rho}{\epsilon}} + \frac{1}{2}\epsilon^{\frac{\rho}{\epsilon^2}} - \frac{1}{2}\epsilon^{\frac{\rho}{\epsilon^3}} + \dots$$

In a similar way also we may find the values of

$$(113.)\quad \int_0^\omega \frac{dx}{(1-2\alpha\cos x+\alpha^2)(\epsilon^{\pi x}+\epsilon^{-\pi x})}, \text{ and}$$

$$(114.)\quad \int_0^\omega \frac{dx \epsilon^{\alpha\cos x}\cos(\alpha \sin x)}{\epsilon^{\pi x}+\epsilon^{-\pi x}}.$$

Let us now return for a moment to the equations

$$p^2+q^2+r^2+s^2=\alpha. \qquad 2pq+2qr+2rs=\beta.$$

$$2pr+2qs=\gamma. \qquad 2ps=\delta.$$

Then $\alpha+\beta+\gamma+\delta$ gives us

$$p+q+r+s=\sqrt{(\alpha+\beta+\gamma+\delta)},$$

$\alpha-\beta+\gamma-\delta$ gives us

$$p-q+r-s=\sqrt{(\alpha-\beta+\gamma-\delta)},$$

hence we obtain at once $p+r$, and $q+s$, which will render the solution of the equations easy, at all events, in all possible cases.

I would moreover remark that the validity of the process depends on $\sqrt{(p+qx+rx^2+sx^3)}$ expanding in a converging series, so that the method of evaluation here, α of course in (109), (110), depends on certain conditions, to which the constants in the integrals must be subject.

II. "On certain Definite Integrals." No. 7. By W. H. L. RUSSELL, F.R.S. Received January 6, 1880.

By a development of the methods indicated in the former papers we obtain the following integrals:—

$$(115.)\quad \int_0^{\frac{\pi}{2}} d\theta\,.\,\frac{\cos^n\theta\cos(n-2)\theta+\alpha\cos^{n+1}\theta\cos(n-3)\theta}{\sin^2\theta+(1+\alpha)^2\cos^2\theta}$$

$$=\frac{\pi}{2^n}\left\{\frac{n}{\alpha+2}-\frac{\alpha}{(\alpha+2)^2}\right\}.$$

$$(116.)\quad \int_0^{\frac{\pi}{2}} d\theta\cos^2\theta\,\epsilon^{2x\cos^2\theta}\cos(x\sin 2\theta)=\frac{\pi}{8}(x+2)\epsilon^x.$$

$$(117.)\quad \int_0^{\frac{\pi}{2}} d\theta\,\frac{\cos^n\theta\cos(n-2)\theta+\alpha^3\cos^{n+3}\theta\cos(n-5)\theta}{1+2\alpha^3\cos^3\theta\cos3\theta+\alpha^6\cos^6\theta}$$

$$=\frac{\pi}{2^{n-2}}\left\{\frac{n}{\alpha^3+8}-\frac{3\alpha^3}{(a^3+8)^2}\right\}.$$

$$(118.)\quad \int_0^{\frac{\pi}{2}} d\theta\cos^2\theta\,\epsilon^{\alpha\cos^3\theta\cos3\theta}\cos(\alpha\cos^3\theta\sin3\theta)=\frac{\pi}{8}\left(2+\frac{3\alpha}{8}\right)\epsilon^{\frac{\alpha}{8}}.$$

$$(119.)\quad \int_0^{\frac{\pi}{2}} \frac{\cos^\mu\theta\cos\mu\theta+\alpha\cos^{\mu+1}\theta\cos(\mu-1)\theta}{\sin^2\theta+(\alpha+1)^2\cos^2\theta}d\theta=\frac{\pi}{2^\mu}\,\frac{1}{\alpha+2}.$$

$$(120.)\quad \int_0^{\frac{\pi}{2}} d\theta\,\frac{\cos^\mu\theta\cos\mu\theta+(\alpha+\beta)\cos^{\mu+\theta}\cos(\mu-1)\theta+\alpha\beta\cos^{\mu+1}\theta\cos(\mu-2)\theta}{(\sin^2\theta+(\alpha+1)^2\cos^2\theta)(\sin^2\theta+(\beta+1)^2\cos^2\theta}$$

$$=\frac{\pi}{2^{\mu-1}(\alpha+2)(\beta+2)}.$$

This integral may be written

$$(121.)\quad \int_0^{\frac{\pi}{2}} d\theta\,\frac{\cos^\mu\theta\cos\mu\theta+p\cos^{\mu+1}\theta\cos(\mu-1)\theta+q\cos^{\mu+2}\theta\cos(\mu-2)\theta}{\sin^4\theta+(p^2+2p-2q+2)\sin^2\theta\cos^2\theta+(p+q+1)^2\cos^4\theta}$$

$$=\frac{\pi}{2^{\mu-1}(q+2p+4)}$$

(122.) $$\int_0^{\frac{\pi}{2}} d\theta\, \epsilon^{\alpha \cos^2\theta} \frac{\cos^{\mu-1}\theta}{\sin\theta} \sin(\alpha \sin\theta \cos\theta + \mu\theta) = \frac{\pi\epsilon^{\alpha}}{2}.$$

(123.) $$\int_0^{\frac{\pi}{2}} d\theta \cos\theta \frac{\mu\cos\theta(\beta+\cos\tan\theta) - \lambda\sin\theta\sin\tan\theta}{(1+2\beta\cos\tan\theta+\beta^2)(\lambda^2\sin^2\theta+\mu^2\cos^2\theta)} = \frac{\pi}{2(\lambda+\mu)(\epsilon+\beta}.$$

(124.) $$\int_0^{\pi} \frac{\theta d\theta(1-x^2\cos^2\theta)\sin\theta}{(1+x^2\cos^2\theta)\sqrt{1+x^4\cos^4\theta}} = \frac{\pi}{x\sqrt{2}} . \sin^{-1}\frac{x\sqrt{2}}{1+x^2}.$$

(125.) $$\int_0^{\pi} d\theta\theta\sin\theta \frac{x^{2n}\cos^{2n}\theta}{(a^2+x^2\cos^2\theta)\,..\,(e^2+x^2\cos^2\theta)} = \frac{\pi}{x}\int \frac{dx\,.\,x^{2n}}{(a^2+x^2)\,..\,(e+x^2)}.$$

(126.) $$\int_0^{\pi} \frac{\cos r\theta}{(1-2\alpha\cos\theta+\alpha^2)(1-2\beta\cos\theta+\beta^2)} d\theta$$

$$= \frac{\pi}{(1-\alpha\beta)(\alpha-\beta)} \left\{ \frac{\alpha^{r+1}}{1-\alpha^2} - \frac{\beta^{r+1}}{1-\beta^2} \right\}.$$

(127.) Hence we see the values of

$$\int_0^{\pi} \frac{d\theta\,.\,(f\epsilon^{\theta i}+f\epsilon^{-\theta i})}{(1-2\alpha\cos\theta+\alpha^2)(1-2\beta\cos\theta+\beta^2)}.$$

By a similar method we may find

(128.) $$\int_0^{\pi} \frac{d\theta\cos r\theta}{(1-2a\cos\theta+\alpha^2)(1-2\beta\cos\theta+\beta^2)\ldots(1-2\mu\cos\theta+\mu^2)}.$$

(129.) $$\int_0^{\pi} \frac{d\theta\,.\,\sin(2r+1)\theta}{(\lambda^2\cos^2\theta+\mu^2\sin^2\theta)\sin\theta} = \frac{\pi}{\lambda^2} + \frac{\pi(\mu-\lambda)}{\lambda^2\mu}\left(\frac{\mu-\lambda}{\mu+\lambda}\right)^r.$$

(130.) $$\int_0^{\pi} \frac{d\theta\,.\,\epsilon^{\alpha\cos 2\theta}\sin(\alpha\sin 2\theta+\theta)}{(\lambda^2\cos^2\theta+\mu^2\sin^2\theta)\,.\sin\theta} = \frac{\pi}{\lambda^2}\epsilon^{\alpha} + \frac{\pi(\mu-\lambda)}{\lambda^2\mu}\epsilon^{\alpha\cdot\frac{\mu-\lambda}{\mu+\lambda}}.$$

(131.) We may also find

$$\int_0^{\pi} \frac{d\theta\sin(2r+1)\theta}{\sin\theta(\lambda_1{}^2\sin^2\theta+\mu_1{}^2\cos^2\theta)(\lambda_2{}^2\sin^2\theta+\mu_2{}^2\cos^2\theta)\,..\,(\lambda_n{}^2\sin^2\theta+\lambda_n\cos^2\beta)}.$$

(132.) $$\int_0^{\pi} \frac{\sin r\theta\,.\,d\theta}{1-2\alpha\cos\theta+\alpha^2} = \frac{\pi\alpha^r}{(1-\alpha^2)} - \frac{1}{(1-\alpha^2)} \cdot \left(\alpha^r - \frac{1}{\alpha^r}\right)\log_{\epsilon}\frac{1+\alpha}{1-\alpha}$$

$$+\frac{2}{1-\alpha^2}\left\{\left(\alpha^{r-1}-\frac{1}{\alpha^{r-2}}\right)+\frac{1}{3}\left(\alpha^{r-3}-\frac{1}{\alpha^{r-3}}\right)+\ldots\frac{1}{r-1}\left(\alpha-\frac{1}{\alpha}\right)\right\},$$

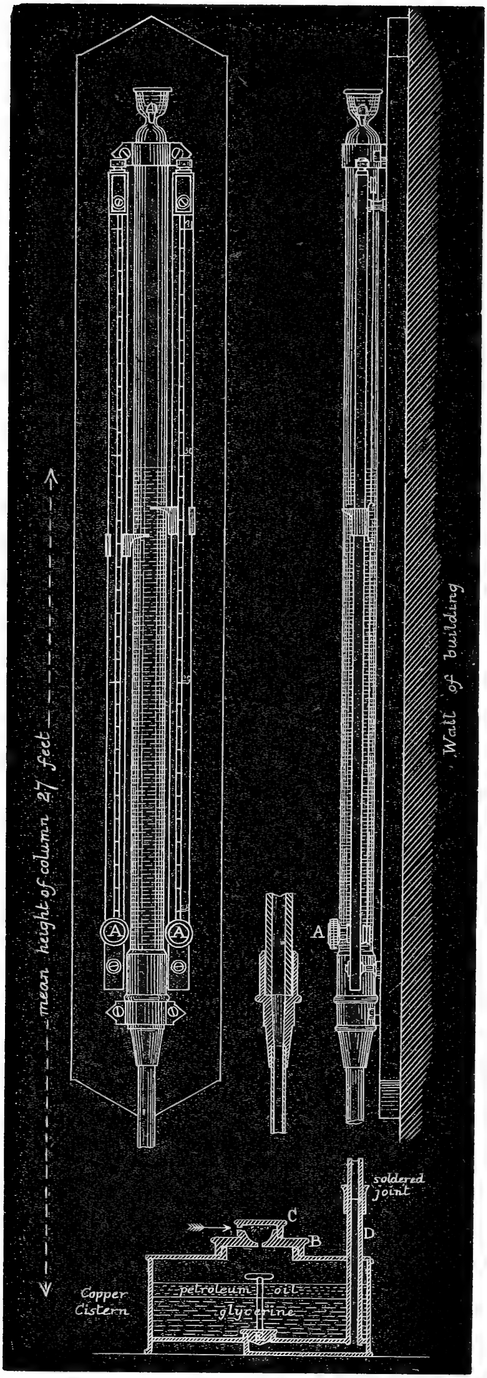
when (r) is even, with a similar expression when (r) is odd. I shall now hope to prove that every function of an algebraical magnitude may be regarded as a centre, from which systems of definite integrals emanate in all directions, like rays from a star, in such a manner, that the value of each integral is equivalent to the original function transformed by a known symbol.

III. "On the Construction of a Glycerine Barometer." By JAMES B. JORDAN. Communicated by Professor STOKES, Sec. R.S. Received January 8, 1880.

Various attempts have been made from time to time to construct barometers with fluids of lower density than mercury, with the view of increasing the range of movement of the column by the direct action of the atmosphere, thereby rendering the variations of pressure more easily visible, without the intervention of mechanical appliances. I have been induced to give the subject attention in the belief that if precise instruments of this class could be made they would prove of scientific value in showing the character of the more minute vibrations of atmospheric pressure, and of practical use at storm stations, collieries, and other situations where it is of importance for the unpractised eye to notice frequently the movements of the column without the careful observation necessary in the reading of an ordinary mercurial barometer.

Water is apparently the most convenient liquid for a long range barometric column, and many water barometers have been constructed, notably that made for the Royal Society in 1830, by Professor Daniell. A water column is, however, of little use for indicating variations of pressure, owing to the effect of changes of temperature on the water vapour existing in the Torricellian vacuum, which often masks the effects which would otherwise be produced by changes of pressure. Having successfully constructed several water barometers, and finding this conflicting action caused by variations of temperature so detrimental to their value, I was induced to experiment with other liquids, and among those tried pure glycerine appeared to me to answer the purpose best. Glycerine from its high boiling point has a very low tension of vapour at the ordinary temperatures of the atmosphere; the length of the column is therefore only altered from changes of temperature by expansion and contraction of the liquid itself, and that in a very small degree, the mean coefficient of absolute expansion between 0° and 100° C. being ·0005455. The specific gravity of the purest glycerine as manufactured by Messrs. Price and Co. is 1·26, or less than one-tenth that of mercury; the mean height of the column is 27 feet at sea level, and a variation of a tenth of an inch in the height of the mercurial column is shown by a change of more than an inch in the glycerine; the boiling point is 440° F., and a very low temperature is required to solidify it. As glycerine absorbs moisture freely if exposed to the atmosphere, the action is prevented by covering the surface of the liquid in the cistern with a shallow layer of heavy petroleum oil, prepared especially for the purpose.

In order to give the method a fair trial, application was made to the Committee of the Government Fund for aiding scientific research,


mean height of column 27 feet
Wall of building
A
A
A
soldered joint
C
B
D
Copper Cistern
petroleum oil
glycerine

and on their recommendation a grant of £30 was made by the Treasury for the purpose. This sum has been expended in the construction of an experimental barometer of this nature, which has been erected at Kew Observatory by the permission of the Kew Committee, who have also sanctioned the taking of a series of observations to extend over a period of twelve months.

The tube forming the body of the instrument is an ordinary composition metal gas-pipe, $\frac{5}{8}$ths of an inch internal diameter, and furnished at the top with a gun-metal socket, into which is cemented a glass tube, 4 feet long, and having an inside diameter of one inch; the upper end is formed in the shape of an open cup and fitted at its neck with an india-rubber stopper. In this tube the fluctuations of the position of the top of the column are observed, and the height read off on brass scales placed on either side of the tube and fitted with indices and verniers, which are moved by means of milled heads placed at the bottom of the scales as shown in the drawing at AA. The scale on the right hand side is divided into inches and tenths of absolute measure, numbering from the level of the liquid in the cistern, while that on the left shows equivalent values reduced to a column of mercury and divided into tenths and hundredths, each hundredth being equal to about one-tenth of an actual inch. The whole of the top fittings, or observing portion of the barometer, is attached to a conveniently constructed back, made of oak and fixed to the wall of an upper room in the observatory building, the main tube being carried down in a direct line through the entrance hall into the barograph room, a distance of 27 feet. This distance was accurately determined by means of a tape measure, the error of which was found by close comparisons with the standard yard preserved at the Observatory. A suitable bracket is here fixed, on the north wall, on which is placed the barometer cistern.

The cistern is a cylindrical vessel of copper tinned inside, 5 inches deep and 10 inches diameter; it is fitted with a screwed cover, as shown at B in the drawing, the air having access only through a small hole in the cap C attached to the cover, which has a recess in it to hold cotton wool for filtering out dust. The main tube is connected with the cistern by attachment (with a soldered joint) to a projecting piece of tube D, which enters the cistern through the bottom and is fitted at its opening with a screwed plug E.

The operation of filling the barometer was performed in the following way:—The quantity of glycerine (coloured red by aniline) having been previously determined, it was heated to a temperature of 100° F., to render it more limpid, thereby enabling the contained air to disengage itself more freely; the whole quantity, about three-fourths of a gallon, was then transferred to the cistern, the plug E removed, and by means of an air-pump connected with the top of the glass tube, the air was exhausted and the liquid forced up the tube by the pressure of the atmosphere, to a height of 323·571 inches, being equivalent to 30 inches of mercury, the Kew standard at the time reading 30·3 inches. The plug in the cistern was now screwed in its place, to support the column, while the air was admitted at the top and the air-pump connexions removed; a sufficient quantity of glycerine to fill the tube was then poured in and the india-rubber stopper inserted. The screw plug being removed for a few seconds to allow the column to fall an inch or two and then replaced, the instrument was allowed to remain until the liquid in it was completely exhausted of its air, which rose slowly to the surface into the Torricellian vacuum above: then the india-rubber stopper was again withdrawn and the tube finally filled up with glycerine, which had been previously exhausted of air under the air-pump receiver: the stopper was now replaced and the cistern plug finally removed, when the column gradually fell until balanced by the pressure of the atmosphere, leaving a small quantity of glycerine in the cup above the stopper, a plate glass cover being placed on the top to keep out dust. The barometer was now complete and it has since continued in operation. Whether it is to be of any scientific or practical value will be proved by the observations which are now being regularly taken under the superintendence of Mr. Whipple, the Superintendent of the Kew Observatory, to whom I am under many obligations for his kind and courteous assistance during the progress of the work. When the observations are completed I shall ask the honour of submitting them to the Royal Society.

IV. "On a Possible Mode of Detecting a Motion of the Solar System through the Luminiferous Ether." By the late Professor J. Clerk Maxwell, F.R.S. In a Letter to Mr. D. P. Todd, of the Nautical Almanac Office, Washington, U.S. Communicated by Professor Stokes, Sec. R.S. Received January 7, 1880.

Mr. Todd has been so good as to communicate to me a copy of the subjoined letter, and has kindly permitted me to make any use of it.

As the notice referred to by Maxwell in the "Encyclopædia Britannica" is very brief, being confined to a single sentence, and as the subject is one of great interest, I have thought it best to communicate the letter to the Royal Society.

From the researches of Mr. Huggins on the radial component of the relative velocity of our sun and certain stars, the coefficient of the inequality which we might expect as not unlikely would be only something comparable with half a second of time. This, no doubt, would be a very delicate matter to determine. Still, for anything we know *à priori* to the contrary, the motion might be very much greater than what would correspond to this; and the idea has a value of its own, irrespective of the possibility of actually making the determination.

In his letter to me Mr. Todd remarks, "I regard the communication as one of extraordinary importance, although (as you will notice if you have access to the reply which I made) it is like to be a long time before we shall have tables of the satellites of Jupiter sufficiently accurate to put the matter to a practical test."

I have not thought it expedient to delay the publication of the letter on the chance that something bearing on the subject might be found among Maxwell's papers.

G. G. STOKES.

(Copy.)

Cavendish Laboratory,
Cambridge,
19th March, 1879.

SIR,

I have received with much pleasure the tables of the satellites of Jupiter which you have been so kind as to send me, and I am encouraged by your interest in the Jovial system to ask you if you have made any special study of the apparent retardation of the eclipses as affected by the geocentric position of Jupiter.

I am told that observations of this kind have been somewhat put out of fashion by other methods of determining quantities related to the velocity of light, but they afford the *only* method, so far as I know, of getting any estimate of the direction and magnitude of the velocity of the sun with respect to the luminiferous medium. Even if we were sure of the theory of aberration, we can only get differences of position of stars, and in the terrestrial methods of determining the velocity of light, the light comes back along the same path again, so that the velocity of the earth with respect to the ether would alter the time of the double passage by a quantity depending on the square of the ratio of the earth's velocity to that of light, and this is quite too small to be observed.

But if JE is the distance of Jupiter from the earth and l the geocentric longitude, and if l' is the longitude and λ the latitude of the direction in which the sun is moving through ether with velocity v, and if V is the velocity of light and t the time of transit from J to E,

$$\mathrm{JE}=[\mathrm{V}-v\cos\lambda\cos(l-l')]\,t.$$

By a comparison of the values of t when Jupiter is in different signs of the zodiac, it would be possible to determine l' and $v\cos\lambda$.

I do not see how to determine λ, unless we had a planet with an orbit very much inclined to the ecliptic. It may be noticed that whereas the determination of V, the velocity of light, by this method depends on the differences of JE, that is, on the diameter of the earth's orbit, the determination of $v \cos \lambda$ depends on JE itself, a much larger quantity.

But no method can be made available without good tables of the motion of the satellites, and as I am not an astronomer, I do not know whether, in comparing the observations with the tables of De Damoiseau, any attempt has been made to consider the term in $v \cos \lambda$.

I have, therefore, taken the liberty of writing to you, as the matter is beyond the reach of anyone who has not made a special study of the satellites.

In the article E [ether] in the ninth edition of the "Encyclopædia Britannica," I have collected all the facts I know about the relative motion of the ether and the bodies which move in it, and have shown that nothing can be inferred about this relative motion from any phenomena hitherto observed, except the eclipses, &c., of the satellites of a planet, the more distant the better.

If you know of any work done in this direction, either by yourself or others, I should esteem it a favour to be told of it.

Believe me,
Yours faithfully,
(Signed) J. Clerk Maxwell.

D. P. Todd, Esq.

January 29, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read :—

I. "English Reproduction Table." By Dr. W. Farr, F.R.S. Received January 15, 1880.

(Abstract.)

This table is a development of the English Life Table, from which the fundamental columns of ly and Py are taken, and is intended to show the constitution of the female population, as regards married and unmarried, and the reproductive power at the several ages.

The female population, Py, is divided into married (uxores) unmarried (filiæ), and widowed (viduæ), according to the proportions of these classes of the various ages at the census of 1871; thus, of a total of 14,936,770, aged 15 and upwards, 7,957,456 are wives, 4,660,311 are spinsters, 2,319,003 are widows.

The offspring (soboles) are derived from Norwegian statistics of the number of children born to married and unmarried women of the several ages, such facts for England not being available.

As the Life Table represents a population, continuing the same in constitution from year to year, the columns sy, $s'y$, and $s''y$, show the *annual* number of children born to women of the several ages and classes; and the total births of live-born children in a year are 1,450,910, of which 55,381 are of illegitimate children.

By means of simple formulæ, the number of children born in or out of wedlock, to any given number of women at a specified age, can be deduced. 1,000 wives aged 20 bear 401 children, while 1,000 unmarried women of this age bear 11 children; at age 30, 1,000 women of each class bear 337 and 29 children; at age 40, 210 and 10 children respectively are born of married and unmarried women.

II. "A Note on Protagon." By ARTHUR GAMGEE, M.D., F.R.S., Brackenbury Professor of Physiology in Owens College, Manchester. Received January 19, 1880.

In 1879, in conjunction with Dr. Ernst Blankenhorn, I communicated to the Royal Society a paper entitled "On the Existence of Liebreich's Protagon in the Brain."*

In that paper we sought to establish the existence of the body which had been described by Dr. Liebreich, but which had by several writers been considered to be a mixture of lecithin and cerebrin. We gave many analyses of several samples of protagon, and pointed out the constancy in the composition of the body when subjected to repeated crystallization from alcohol. I have since the date of the above paper, in conjunction with Herr Adolf Spiegel and Mr. Leopold Larmuth, continued my examination of protagon, and of certain bodies which accompany it. Our researches, which are not yet in a sufficiently advanced state for publication, have in the fullest degree confirmed the conclusions arrived at by the research of which the results have already been submitted to the Society.

The object of the present communication is to notice certain of the statements which have lately been published by J. L. W. Thudichum, M.D.,† to the effect that protagon is an impure body consisting of a mixture of many organic substances, and containing in particular considerable quantities of potassium.

"These inquiries," says Dr. Thudichum, referring to his own re-

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* "Proc. Roy. Soc.," vol. xxix, p. 151.

† "Note and Experiments on the Alleged Existence in the Brain of a Body termed 'Protagon'." "Annals of Chemical Medicine." By J. L. W. Thudichum, M.D. London, 1879. Page 254.

searches, "were already several years ago extended to the product termed 'protagon'; and this, too, was found to contain inorganic constituents irremovable by recrystallization from alcohol, however frequently repeated. It was found more particularly that 'protagon' and the bodies into which it can be separated, according to my researches, always retain considerable quantities of *potassium* in combination. As the quantity of inorganic ingredients in 'protagon' had never been estimated, I prepared a specimen of this matter, and on analysis found it to contain the better part of 1 per cent. of inorganic incombustible matter, phosphoric acid not included, and in this no less than 0·76 *per cent. of the 'protagon' of potassium.*" *

Having described various experiments in which the above impure body is supposed to have been resolved into bodies which Dr. Thudichum has elsewhere described as proximate principles of the brain, he proceeds: "The foregoing data enable us to attribute their proper value to the series of operations by which the advocates of 'protagon' have brought about the concordance of their analyses. The potassium, which, though present in such quantity that if 'protagon' were a unitary body its atomic weight would thereby be fixed, they have not found, is calculated as oxygen; the phosphate of lime which they have not extracted, is made to increase the protagonal phosphorus; the mixture of the myelins, which they have not extracted, and which they are unable to diagnose, is adjusted by solvents to a convenient quantity, and made to represent the constituent phosphorus of protagon . . . . The uniform chemical composition of the brain greatly favours the obtaining from it, by the aid of processes nearly akin to trimming, of extracts of uniform composition; this uniformity can be greatly aided by limitations of the quantities of materials operated upon, and of the quantities and strengths of the solvents; and by careful observance of these limitations, preparations are obtained which present a delusive appearance of definiteness. But this delusion could only be persevered in by persons who are not in the habit of subjecting their products to tests of purity, and who are not acquainted with the necessity, which is imposed upon every conscientious inquirer, of questioning his products and conclusions in a sense adverse to his hypotheses."

After reading the above remarks, I determined upon placing in the

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* The words printed in italics leave some doubt as to the author's meaning. It might be supposed that by "0·76 per cent. of the 'protagon' of potassium" he did not mean 0·76 per cent. of potassium, but much smaller quantities. Lest there should be any doubt on this point it is necessary to quote the following passage, in which the statement is repeated in plainer terms:—"The remaining 30 grms. of protagon once recrystallised (product N 1·2) contained 1·057 per cent. of phosphorus and 0·76 per cent. of potassium." It therefore appears that in the sentence quoted in the text the author means that the potassium amounted to 0·76 per cent. of the protagon.

hands of my friend and colleague, Professor Roscoe, F.R.S., the only sample available of the protagon of which the analyses had been communicated to the Royal Society; this was a twice re-crystallized sample of protagon from ox-brain, which had been employed for analyses 3 and 4. The remaining specimens had been used in experiments on the products of decomposition of protagon.

I requested Dr. Roscoe, to whom I communicated Dr. Thudichum's criticism, to determine the amount of mineral impurities, and especially of potassium present in protagon. He has kindly furnished me with the ensuing report, with which I shall close this communication, as I am convinced that no one will expect me to enter into a polemical discussion with the author of the previously quoted matter.

The Owens College, Manchester,
December, 1879.

I have examined spectroscopically for potash a sample of protagon furnished me by Dr. Gamgee, and labelled "Protagon twice re-crystallized. Blankenhorn." I could not detect any potash by the spectroscope in the incinerated mass from 0·1 grm. of substance. With the carbonized mass obtained from 1·0 grm. of substance I obtained the potassium line (α) very faintly, and from comparative experiments with a dilute solution of a potassium salt, I estimate the quantity of potash in 1 grm. of the substance not to exceed $\frac{1}{20}$ mgrm. The carbonized residue of 1 grm. of protagon was carefully oxidized with pure nitric acid, when a small quantity of fused metaphosphoric acid remained after ignition. This residue weighed 0·0278 grm., corresponding to 1·08 per cent. of phosphorus.*

(Signed) H. E. Roscoe.

III. "On the Induction of Electric Currents in Infinite Plates and Spherical Shells." By C. Niven, M.A., Professor of Mathematics in Queen's College, Cork. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received January 21, 1880.

(Abstract.)

The object of the present paper is to determine the currents which are induced in an infinite plate of uniform conductivity and finite thickness, and in a sphere or spherical shell of any thickness when in the presence of a varying magnetic system: and in any of these bodies when rotating near a constant magnetic system, round an axis which is normal to the faces of the plate or passes through the centre

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* The mean quantity of phosphorus found by Gamgee and Blankenhorn was 1·068 per cent.—A. G.

of the sphere or shell. The case of an infinitely thin plane sheet has been made the subject of a paper by the late Professor Clerk Maxwell, in the "Proceedings of the Royal Society," vol. xx (1872), and Jochmann has also treated the case of a rotating sphere when the inducing magnetism is symmetrical about the axis of rotation, and that of a rotating plate when the mutual induction of the currents is neglected. In the present paper the problems are dealt with generally.

Maxwell's equations of the field are adopted, and the peculiar view which he takes of the electric current renders it necessary to enter with some detail into the general boundary conditions. These are given, first of all, for two substances at rest, which possess both conductivity and specific dielectric capacity: we can then deduce the conditions to be satisfied at the common surface of a conductor and a dielectric.

Let F, G, H be the components, at any point just inside the surface of the conductor, of the vector potential due to all the currents and magnets in the field, $\mathfrak{F}$ the component along the normal (N) to the surface, measured outward from the conductor, ψ the electric potential; and let F′, G′, H′ be the components of the vector potential just outside, the boundary conditions are

$$\mathrm{F}=\mathrm{F}' \ldots \frac{d\mathrm{F}}{d\mathrm{N}}=\frac{d\mathrm{F}'}{d\mathrm{N}}, \ldots \frac{d\mathfrak{F}}{dt}+\frac{d\psi}{d\mathrm{N}}=0.$$

When $\mathfrak{F}=0$, as in the cases treated in the paper, it is shown that ψ must vanish everywhere, and there is no free electricity within the conductor or on its surface.

Infinite Plate.

For the case of an infinite plate, the sealar potential (Ω_0) of the external system and the vector components are given by

$$\Omega_0=\frac{d\mathrm{P}_0}{dz}, \qquad \mathrm{F}_0=-\frac{d\mathrm{P}_0}{dy}, \qquad \mathrm{G}_0=\frac{d\mathrm{P}_0}{dx}, \qquad \mathrm{H}_0=0.$$

The vector potential of the currents in the plate, and the currents themselves are given by

$$\mathrm{F}=-\frac{d\mathrm{P}}{dy}, \qquad \mathrm{G}=\frac{d\mathrm{P}}{dx}, \qquad \mathrm{H}=0,$$

$$u=-\frac{d\Phi}{dy}, \qquad v=\frac{d\Phi}{dx}, \qquad w=0, \text{ where } 4\pi\Phi=-\nabla^2\mathrm{P}.$$

They are also expressed in semipolar co-ordinates ρ, ϕ, z, thus:—

$$\mathrm{F}=-\frac{d\mathrm{P}}{\rho d\phi}, \quad \mathrm{G}=\frac{d\mathrm{P}}{d\rho}, \quad \mathrm{H}=0, \quad u=-\frac{d\Phi}{\rho d\phi}, \quad v=\frac{d\Phi}{d\rho}, \quad w=0,$$

$$\text{where } 4\pi\Phi=-\nabla^2\mathrm{P}.$$

The mode of solution adopted is to consider the variations of the external system as a series of impulses, producing a succession of currents which decay according to the law

$$\frac{\sigma}{4\pi}\nabla^2 \mathrm{P}=\frac{d\mathrm{P}}{dt},\ \sigma \text{ being the conductivity.}$$

If the value of P_0 rise suddenly from 0 to P_0, the corresponding value of P will be $-\mathrm{P}_0$ initially; and, at any subsequent time, will be given, for an external point, on the positive side of the plate, by

$$\mathrm{P}\doteq-\frac{1}{\pi}\Sigma_{m=0}^{m=\infty}\int_0^\pi \cos m\overline{\phi-\phi'}d\phi'\int_0^\infty \kappa\epsilon^{-\kappa(z-b)}\mathrm{J}_m(\kappa\rho)d\kappa\int_0^\infty \rho'\mathrm{J}_m(\kappa\rho')d\rho'\,.\,\mathrm{Z},$$

where

$$\mathrm{Z}=\Sigma\left\{e^{-\lambda t}\frac{2n\cos nb}{2nb+\sin 2nb}\int_{-b}^{+b}\mathrm{P}_0{}'\cos nz'dz'\right.$$
$$\left.+e^{-\lambda' t}\frac{2n'\sin n'b}{2n'b-\sin 2n'b}\int_{-b}^{+b}\mathrm{P}_0{}'\sin n'z'dz'\right\},$$

in which

$$\lambda=\frac{\sigma}{4\pi}(\kappa^2+n^2),\quad n\sin nb-\kappa\cos nb=0.$$

$$\lambda'=\frac{\sigma}{4\pi}(\kappa^2+n'^2),\quad n'\cos n'b+\kappa\sin n'b=0.$$

The origin is midway between the faces of the plate, and the last summation is to be extended over all the values of n and n', derived from these equations.

By putting $\mathrm{R}=\frac{\sigma}{b}$, and making b infinitely small, we deduce the solution for an infinitely thin sheet. The result coincides with Maxwell's.

Sphere or Spherical Shell.

The formulæ adapted to this case are

$$\Omega_0=\frac{d(\mathrm{P}_0 r)}{dr},\qquad \mathrm{F}_0=0,\qquad \mathrm{G}_0=-\frac{d\mathrm{P}_0}{\sin\theta d\phi},\qquad \mathrm{H}_0=\frac{d\mathrm{P}_0}{d\theta},$$

$$\mathrm{F}=0,\quad \mathrm{G}=-\frac{d\mathrm{P}}{\sin\theta d\phi},\quad \mathrm{H}=\frac{d\mathrm{P}}{d\theta},\quad u=0,\quad v=-\frac{d\Phi}{\sin\theta d\phi},\quad w=\frac{d\Phi}{d\theta},$$

and

$$4\pi\Phi=-\nabla^2\mathrm{P},\qquad \frac{\sigma}{4\pi}\nabla^2\mathrm{P}=\frac{d\mathrm{P}}{dt}.$$

[We have here resolved along the elements dr, $rd\theta$, $r\sin\theta d\phi$].

When the shell is of finite thickness, the solution is expressed in terms of spherical harmonics and of the functions which are the solutions of the equation

$$\frac{d^2\mathrm{R}}{dr^2}+\frac{2}{r}\frac{d\mathrm{R}}{dr}+\left(\lambda^2-\frac{n\,.\,\overline{n+1}}{r^2}\right)\mathrm{R}=0.$$

When the shell is infinitely thin, the action of the induced currents is expressed by the following statement, which, it will be seen, is an extension of Maxwell's result for a plane sheet. Divide the time into an infinite number of equal intervals, and at the commencement of any of these let a positive image of the external system be formed at the place occupied by its electric image at the surface. Let the parts of this image move towards the centre in straight lines, so that the logarithmic decrement of their distances from the centre is constant and equal to $\frac{\mathrm{R}}{2\pi a}$ (R being the resistance of the shell and a its radius), and let the intensity of the image increase at each point with a constant logarithmic rate $\frac{\mathrm{R}}{4\pi a}$. At the end of the interval let an exactly equal but negative image be formed in the place of the former and move toward the centre in the same manner, and let these operations be repeated at the beginning and end of every interval: the action of the sheet on external points will be that due to the above train of images.

Induction in a Rotating Conductor.

When the conductor rotates uniformly about an axis of symmetry the currents will become, after a time, steady. In the case of an infinite plate of finite thickness, the vector potential and currents will have the same general forms as before, but the electric potential will not be zero. The solution is now expressed by taking

$$\psi=\sigma\rho\frac{d\chi}{d\rho}, \qquad \Phi=-\frac{d\chi}{d\phi}=-\frac{1}{4\pi}\nabla^2\mathrm{P},$$

$$\sigma\chi=\omega(\mathrm{P}+\mathrm{P}_0), \qquad \frac{\sigma}{4\pi}\nabla^2\mathrm{P}=\frac{\omega d}{d\phi}(\mathrm{P}+\mathrm{P}_0),$$

(w being the angular velocity).

When the inducing magnetism lies all on the positive side of the plate, we may express P_0 within it in the form

$$\Sigma\mathrm{A}\cos m\phi\mathrm{J}_m(\kappa\rho)e^{\kappa z},$$

the origin being on the positive face. The value of P at any external point on the positive side may be expressed by

$$\mathrm{P}=\Sigma\mathrm{C}\cos m\phi\mathrm{J}_m(\kappa\rho)e^{-\kappa z},$$

where $$\frac{\mathrm{C}}{\mathrm{A}}=\frac{\kappa^2-\mu^2}{2\kappa}\cdot\frac{e^{\mu b}-e^{-\mu b}}{\kappa(e^{\mu b}+e^{-\mu b})-\mu(e^{\mu b}-e^{-b})}, \qquad \mu^2=\kappa^2-\frac{2\pi m\omega}{\sigma}\,.\,i.$$

When the plate becomes a thin sheet we may put $\sigma = Rb$, and make b infinitely small; we shall then have

$$R\frac{dP}{dz} - 2\pi\omega\frac{dP}{d\phi} = 2\pi\omega\frac{dP_0}{d\phi}.$$

If we express P and P_0 as functions of ρ, ϕ, $z + \frac{R}{2\pi\omega}\phi$,

$$P = -\int\left(\frac{dP_0}{d\phi}\right)d\phi,$$

$\left(\frac{dP_0}{d\phi}\right)$ being found on the original system of variables. There seems to be a mistake in Maxwell's integration here, but it does not affect the representation of the result by means of the spiral trail of images which he has given.

Spherical Conductor.

In the case of a sphere or spherical shell, the vector potentials and currents will have the same general forms as before, and the solution is contained in the equations

$$\psi = \sigma r \sin\theta\frac{d\chi}{d\theta}, \qquad \Phi = -\frac{d\chi}{d\phi} = -\frac{1}{4\pi}\nabla^2 P,$$

$$\sigma\chi = \omega(P + P_0), \qquad \frac{\sigma}{4\pi}\nabla^2 P = \omega\frac{d}{d\phi}(P + P_0),$$

From the general formulæ expressing completely the effect of the shell on external points, we pass to one whose thickness (c) is infinitely small: putting then $Rc = \sigma$, $\mathfrak{P} = Pr^{\frac{1}{2}}$, $Q = P_0 r^{\frac{1}{2}}$, $\rho = a \log r$, we obtain finally

$$R\frac{d\mathfrak{P}}{d\rho} - 2\pi\omega\frac{d\mathfrak{P}}{d\phi} = 2\pi\omega\frac{dQ}{d\phi},$$

which may be integrated as before. It indicates a spiral trail of images differing somewhat from the former; that due to a single pole will form a curve lying on a cone, and would be, if the cone were developed, a series of portions of an equiangular spiral.

IV. "On the Physical Constants of Liquid Hydrochloric Acid." By GERRARD ANSDELL, F.C.S., Chemical Assistant at the Royal Institution. Communicated by Professor DEWAR, F.R.S. Received January 22, 1880.

In a paper read before the Royal Society, on June 18th, 1879, I described the results of some experiments made with liquid acetylene, obtained by compressing the gas in one of M. Cailletet's ingenious pumps for the liquefaction of gases. The tensions of the saturated vapour then obtained were so entirely different from M. Cailletet's results, that it appeared to me to be possibly due to a slight impurity in the gas, probably a trace of mono-bromethylene which always occurs in the red copper compound of acetylene, and is very difficult to completely eliminate.

I therefore prepared a quantity of the silver compound (which can be obtained entirely free from mono-bromethylene) by passing acetylene prepared in the same way as formerly, viz., by the action of alcoholic potash on di-bromethylene, into an ammoniacal solution of nitrate of silver.

On filling several tubes with acetylene generated by the action of hydrochloric acid on this compound, the tensions were found to be exactly the same as those obtained with the acetylene made from the copper compound, thus proving the correctness of the former experiments.

As the Cailletet pump is so well adapted to the examination of gases in their liquid state, and is at the same time so easily managed, it appeared interesting to determine the physical constants of some other gases, and Professor Dewar suggested an examination of the halogen acids, with regard to their physical relations to each other, more especially as we have absolutely no reliable knowledge as to these numbers.

Faraday was the first to liquefy hydrochloric acid gas in 1823, and he subsequently, in 1848, made a series of determinations of the vapour tensions of the liquid at different temperatures. He also liquefied hydrobromic and hydriodic acids, but gives no account of their properties in a liquid state. Gore ("Proc. Roy. Soc.," vol. xiv), examined very carefully the chemical properties of liquid hydrochloric acid, its solvent action, &c., but made no determinations of its physical constants.

Hydrochloric acid is the only one I have as yet had time to examine, with the following results:—

The gas was made by the action of strong sulphuric acid on dry chloride of ammonium, being afterwards freed from sulphuric acid and dried before entering the tubes. It was found of no advantage to continue the stream of gas for more than six hours, as a constant minimum of air always remained in the tube at the end of this time (about $\frac{1}{400}$th of the total volume of the tube) which it appeared utterly impossible to remove.

The pump was used in the same way as described in my former paper, two iron reservoirs being used, one containing the air manometer, and the other the tube with the gas to be liquefied.

Apart from the mere determination of the vapour tensions, densities, &c., the ratio of the volume of saturated vapour to that of the liquid was considered of the highest importance, as from these numbers the latent heat of transformation and other important data can be easily calculated. For these reasons the gas was examined in rather a different way to the acetylene, the volume to which it had been compressed at the point of liquefaction (or the volume of the saturated vapour) at any given temperature being first accurately determined, and then the pressure increased until the condensed liquid entirely filled the upper part of the tube. The volume of this liquid column was then measured, so that a comparison between the volume of the saturated vapour and the volume of the total condensed liquid was obtained at each temperature.

This fractional volume of the gas at the point of liquefaction was determined by observing when the air manometer ceased to show an increase of pressure on compressing the gas; the volume of the gas at this point being then measured, and the pressure being then gradually released until the air manometer showed a slight decrease of pressure,—the volume of the gas was then again measured at this point, and the mean taken as the point of liquefaction.

The results of the whole series of experiments are recorded in a condensed form in the following table :—

A.	B.	C.	D.	E.	F.
4	137·31	$\frac{1}{38{\cdot}89}$	7·55	18·18	29·8
9·25	118·96	$\frac{1}{45{\cdot}75}$	7·90	15·05	33·9
13·8	103·50	$\frac{1}{53{\cdot}19}$	8·35	12·39	37·75
18·1	91·77	$\frac{1}{61{\cdot}17}$	8·74	10·50	41·80
22·0	81·19	$\frac{1}{70{\cdot}06}$	9·10	8·92	45·75
26·75	69·69	$\frac{1}{82{\cdot}94}$	9·50	7·33	51·00
33·4	55·75	$\frac{1}{105{\cdot}98}$	10·12	5·50	58·85
39·4	44·85	$\frac{1}{134{\cdot}33}$	10·68	4·19	66·95
44·8	36·34	$\frac{1}{168{\cdot}67}$	11·96	3·03	75·20
48·0	31·33	$\frac{1}{197{\cdot}60}$	12·00	2·61	80·80
49·4	27·64	$\frac{1}{224{\cdot}96}$	12·92	2·13	84·75
50·56	25·70	..	14·30	1·79	85·33
51·00	23·96				

In this table

A=temperature of gas.

B=volume of the saturated vapour at point of liquefaction.

C=fractional volume of the gas at point of liquefaction in relation to the initial volume under one atmosphere of pressure.

D=volume of the condensed liquid.

E=ratio of volume of gas to that of the liquid.

F=pressure in atmosphere.

The critical point was found to be 51°·25 C.

It will be seen from this table, that the volumes of the saturated vapours and liquid gradually approach each other as the temperature nears the critical point, and would undoubtedly become identical, if the experiments could be carried on up to the critical point. The following curves, the first of which represents the change of volume of the saturated vapour and liquid, in relation to the temperature; and the second the ratio between these volumes in relation to the temperature (column E in the table) show these results in a graphical form.

Curves.

The ratio between the volume of the saturated vapour and the volume of the liquid at different temperatures decreases very regularly until within about three degrees of the critical point, where a singular point in the curve occurs, and the ratio approaches unity with great rapidity. The volume of the liquid increases very regularly up to a temperature of about 48° C., and at 51° C., or within 0·25 of a degree of the critical point, the distinction between the saturated vapour and the liquid vanishes, as although liquid is plainly seen to condense on the surface of the mercury, on increasing the pressure the line of demarcation immediately disappears, and it is impossible to say whether the tube is filled with the saturated vapour or the liquid itself; therefore no results could be obtained nearer the critical point than about a fourth of a degree.

Avenarius, in a paper intitled "The Causes which determine the Critical Point" ("Mém. Acad. Sci. St. Pétersbourg," 1876–77), made a number of experiments on ether, and came to the conclusion that the volumes of the saturated vapour and of the condensed liquid at the critical point were not identical. He was under the disadvantage of working at a very high temperature, the critical point of ether being 192°·6 C., which must have been very difficult to keep fixed within half a degree.

My own experiments, which were carried out at a very much lower temperature, and were consequently capable of greater accuracy, on account of the facility with which the temperature could be kept constant to within $\frac{1}{20}$th of a degree, appear to confirm his results, in so far

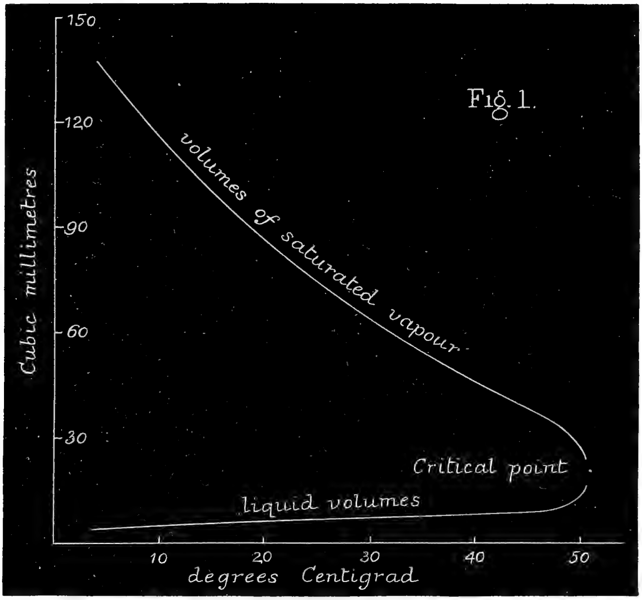
Fig. 1.
150
120
90
60
30
Cubic millimetres
volumes of saturated vapour
Critical point
liquid volumes
10
20
30
40
50
degrees Centigrad

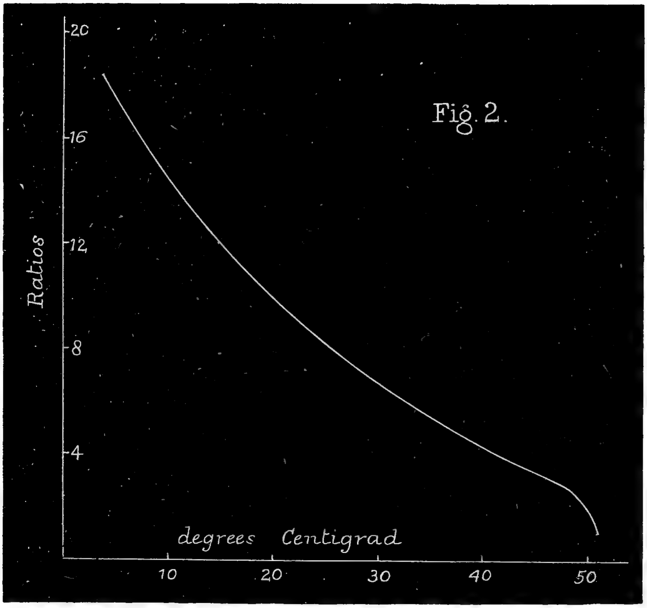
Fig. 2.
20
16
12
8
4
Ratios
degrees Centigrad
10
20
30
40
50

as it is evidently impossible to measure the relative volume of fluid and gas within less than a fourth of a degree of the critical point, and at this place the volumes are certainly unequal. This, however, does not disprove their identity as the critical point.

The density of the liquid at different temperatures was determined in the same way as described in my former paper, and gave the following numbers:—

Temperature.	Density.
0° C.	·908
7·5	·873
11·67	·854
15·85	·835
22·7	·808
33·0	·748
41·6	·678
47·8	·619

This liquid has therefore not quite such a high density as liquid carbonic acid, which is 0·95 at 0° C., and is about twice as high as acetylene, which is 0·450 at the same temperature. It is interesting to note that acetylene is the lightest known fluid substance. On referring to the density of liquid hydrochloric acid given by other authors, Professors Roscoe and Schorlemmer, in their recent treatise on chemistry, describe it as a colourless liquid having a specific gravity of about 1·27. As this value is so much higher than that obtained by myself, and as I feel confident of the accuracy of my results, it seems evident that a mistake has in some way arisen, and that the value given by them refers to the density of the gaseous acid with reference to air, and not to the liquefied gas. Unfortunately Faraday does not seem to have determined its density.

Experiments made to determine the coefficients of compressibility of the fluid at different temperatures were carried on in the same way as formerly detailed. The following are the chief results:—

Mean coefficients of compression at different temperatures. Range of pressure from 52·8 to 208·19 atmospheres.

Temp. of HCl.		Coefficient.
47°·0 C.	=	·00166
41·6	=	·00123
33·0	=	·00096
22·7	=	·000635
15·85	=	·00062
10·5	=	·00054
5·7	=	·000397

It is rather more compressible than liquid acetylene, whose mean coefficient at 22°·3 C is ·00058.

The latent heat of hydrochloric acid and other values depending on thermo-dynamical considerations, will be given in a separate communication.

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"A Magnetic Survey of the Fortieth Parallel in North America between the Atlantic Ocean and the Great Salt Lake, Utah." By T. E. Thorpe, Ph.D., F.R.S. Received March 25: Read May 1, 1879.

A glance at the maps which accompany Sir Edward Sabine's "Contributions to Terrestrial Magnetism," published at various times in the "Philosophical Transactions of the Royal Society," is sufficient to show that considerable as is our knowledge of the magnetic history of our globe, there are still very large areas of the earth's surface about which we have practically no information. A remarkable instance of this fact is afforded by the case of the North American Continent, more especially by that portion belonging to the United States. Magnetic observations have been made, with more or less assiduity, at different places in the Eastern States, for many years past; but over the immense tract of country lying between the Mississippi and the Pacific Ocean—a tract comprising upwards of 200,000 square miles, there is only a single determination of one of the three magnetic elements indicated on Sir E. Sabine's maps, viz., a determination of declination at Salt Lake City. A series of observations was made some years since by United States officers along the Mexican frontier, and a similar series was carried out by the English and American officers employed on the North American Boundary Commission. But the latter observations, I believe, have not yet been published.

My attention was first drawn to this matter by my friend Mr. G. M. Whipple, the Superintendent of the Kew Observatory, in the course of some conversation respecting a journey to Western America which I contemplated making in order to observe the total solar eclipse of last July. As my station was to be in the State of Colorado, which is practically midway between the northern and southern boundaries along which the observations above referred to had been made, it seemed to Mr. Whipple advisable to seize the opportunity to add to our magnetical knowledge of the intervening area to the extent even of securing observations on a single spot only. Further consideration showed that the project might be extended with advantage, and accordingly I offered to make a complete series of observations of the three elements—dip, intensity, and declination, at such times and places as the circumstances of travel would allow. The Kew Committee kindly offered me the loan of the instruments which the Rev. S. J. Perry had employed in Kerguelen's Land and in other places on the occasion of the recent Transit of Venus Expedition; at the same time Professor Balfour Stewart generously undertook to use his influence with the Council of the Owens College to obtain for me the admirable magnetic equipment belonging to that institution. After some experience with both sets of instruments, I decided to accept those belonging to the Owens College, partly because they were of more modern construction than the others, being furnished with certain contrivances and improved modes of adjustment, suggested by Dr. Stewart's extensive experience, which made them more convenient to use, whilst they materially added to the trustworthiness of their indications; and partly (which was of great importance) because the instrumental constants had been comparatively recently determined and verified by Mr. Whipple. The Owens College instruments consisted of a dip-circle by Dover, No. 6; a unifilar magnetometer by Elliott Brothers, No. 27; and a marine chronometer by Frodsham, No. 4066. I would here record my sense of obligation to Professor Stewart and the Council of the Owens College for the readiness with which they placed their instruments at my disposal. My thanks are also due to the Kew authorities for the loan of a couple of tripods. I am specially indebted to Mr. Whipple and to Mr. Figg, the Magnetic Assistant at the Kew Observatory, for instruction in the use of the instruments, for advice in the conduct of the observations, and for assistance in their subsequent reduction.

In addition to the ordinary magnetic instruments, I provided myself with an excellent 7-inch sextant by Casella, a couple of artificial horizons, some spare thermometers, an aneroid for altitudes up to 5,000 feet, and a hypsometric apparatus for greater elevations. The fortunate circumstance that I was accompanied by Dr. Schuster has added, in no small degree, to the completeness of the work; for that gentleman, being provided with one of Pistor and Martin's small prismatic circles, and being very expert in its use, kindly offered to take charge of the observations for time and latitude throughout the journey. It is in great measure due to this assistance that I have been able to develope the original intention into what may be considered as practically a magnetic survey of the fortieth parallel across the American Continent, that is to say, of the district which lies midway between the line of observations already run along the northern and southern boundaries of the United States territory.

The observations were begun at Kew, which was adopted as a base station, and were resumed on the other side of the Atlantic, at New Haven, Connecticut. Determinations were next made at La Junta in South Colorado, the eclipse station selected by Professor Asaph Hall, of the United States Naval Observatory; afterwards at Manitou, at the base of Pike's Peak; at Denver, and at Cheyenne in Wyoming. No further observations were made until we arrived at Salt Lake City, where, thanks to the kindness of President Taylor, we were permitted to make a complete series in an excellent spot in a garden to the side of the late President Young's house. The transit instrument, by Würdermann, of Washington, belonging to the Community, which is mounted in the rear of the Tabernacle, was also placed at our disposal; and we had an opportunity of comparing our chronometers with an instrument (Dent, 2392) which is regularly rated from transit observations made by Mr. Anderson, of the President's Office. Our next station was Ogden, in Northern Utah, at the observatory directed by Lieutenant Wheeler, of the United States Engineers, and on the spot selected by the French astronomers for their observations of the recent Transit of Mercury. The observatory was then in charge of Professor Truman Safford, of Williams College, who also rated our chronometers and otherwise did all in his power to facilitate our work. The next stations were Green River in Wyoming, Grand Island in Nebraska, Council Bluffs in Iowa, Chicago, Allegheny, and Washington. Washington is one of the best observed stations in the States. Mr. Schott, of the United States Coast Survey Department, who is well known as a leading magnetician in America, has here made regular determinations in an observatory in the neighbourhood of the Capitol Hill for many years past, and these he has tabulated and discussed in various publications issued by the Coast Survey authorities. My object in selecting Washington was to obtain an independent check on the indications of my instruments. Mr. Schott was kind enough to allow me to set up my instruments in his observatory, and to afford me such assistance and information as I needed.

I learned that the Coast Survey Department is gradually accumulating data for a discussion of the magnetic history of the continent. It has already published a map of declination for 1870, from which it is evident that much has been done in the determination of this particular element since the date of Sir Edward Sabine's "Memoirs," more especially along the Pacific slope and in the regions to the west and south-west of the Great Salt Lake. As yet, however, no observations have been published relating to any of the districts west of the Mississippi that we visited.

The geographical positions of the several stations, their altitudes above the sea-level, the geological character of the district in which they are situated, together with such details of the exact sites as may be necessary for their identification, are given in Table I. For the greater part of the geological information I am indebted to my colleague, Professor A. H. Green; the rest I owe to the kindness of General Humphreys, Chief of the United States Engineers, who supplied me with the beautiful geological and topographical atlas accompanying Mr. Clarence King's Report of the Geological Exploration of the Fortieth Parallel.

I. *Dip.*

The dip circles at my disposal were Barrow, No. 23, and Dover, No. 6. The late Mr. Welsh had employed the former instrument in

Table I.

Station.	Altitude in feet above sea level.	Latitude. N.	Longitude (from Washington).	Geological Character.	Remarks.
		° ′ ″	h. m. s.		
New Haven, Connecticut	50	41 18 41	0 16 30 E.	Red sandstone, with intrusive trap	In Professor Sillimann's garden. The New Haven and Northampton Railway runs N.N.W. to S.S.E. about 110 yards west of station.
La Junta, Colorado ..	4,190	37 58 55	1 45 57·7 W.	.. .. ..	About 100 paces N.N.W. of Eclipse Station. Dry sandy soil.
Manitou, Colorado	6,300	38 51 55	1 51 25·9 ,,	Limestone and red sandstone	In a grove to the S. of the Cliff House, near the Navajo Soda Spring, and on the edge of the Fontaine-qui-bouille.
Denver, Colorado	5,196	39 45 20	1 51 46·1 ,,	Sands and clays	In Mrs. Craig's garden, to N.W. of town, between the Waterworks and Colorado Central Railway.
Cheyenne, Wyoming ..	6,075	41 07 46	1 51 03·1 ,,	Fine marly sandstones, clays, conglomerates, and limestones	On plot of land between Court House and School House, ¼ mile to rear of railroad station.
Salt Lake City, Utah ..	4,340	40 46 04	2 19 22·4 ,,	Gravels	On rising ground to the east of the President's house, between that and the White House-on-the-Hill, the residence of Mrs. M. A. Young.

Table I—*continued.*

Station.	Altitude in feet above sea level.	Latitude. N.	Longitude (from Washington).	Geological Character.	Remarks.
		° ′ ″	h. m. s.		
Ogden, Utah	4,374	41 13 09	2 19 47·5 W.	Gravels	At Observatory near the thermometer station, about 70 feet N.W. of dome.
Green River, Wyoming ..	6,117	41 31 38	2 9 40·2 ,,	Calcareous shales	In rear of town, to the west of the school house, and directly opposite the front of the Court House, 542 feet N.N.E. of station of U.S. Engineers.
Grand Island, Nebraska..	1,850	40 55 24	1 24 57·7 ,,	.. .. ..	In Mr. Michelson's garden, 655 feet S.S.E. from railroad station U.P.R.R. Dry sandy soil.
Council Bluffs, Iowa ..	1,320	41 15 17	1 15 17·0 ,,	Limestone, with beds of shale and grit	On the edge of large field of sunflowers, 900 feet N. of the Union Pacific Depôt.
Chicago, Illinois	613	41 50 01	0 42 14·7 ,,	Probably limestone	In grounds of Chicago University, 174 feet to the north of the western edge of Observatory tower, and 243 feet from door of Transit House in rear of University buildings.
Allegheny, Pennsylvania	1,145	40 27 36	0 11 50·6 ,,	Sandstones, shales, and coal	In grounds of the Observatory, about 35 feet due north of the dome.
Washington, D. C... ..	70	38 53 12	0 0 0	Gneiss	In Mr. Schott's Magnetic House on the Capitol Hill, 212, First Street, S.E.

his Magnetic Survey of Scotland in 1856–57: it had been carefully examined by Dr. Balfour Stewart for error of axle and evidence of local magnetism in the circle. (See "British Association Reports," Aberdeen, 1859.) After a number of trials with both circles, I decided to make use of the one belonging to Owens College (the Dover circle), principally for the reason that the method of lifting the needles in that instrument seemed to afford more concordant results than that adopted in the other. The Dover circle was provided with two needles of the ordinary form, each 3·5 inches long and 0·27 inch in maximum breadth.

In order to preserve them from rust during the sea-voyage, the needles were carefully wrapped in gutta-percha tissue and placed in a small box filled with soda-lime. This method of preservation was found to be perfectly efficacious: at no time during the journey was the slightest trace of oxidation discernible under the magnifying glass.

The mode of observing was that described by Mr. Welsh in the "Admiralty Manual of Scientific Inquiry," 3rd edition, 1859. Eight readings of the vertical needle gave the magnetic prime vertical. The circle was then turned so that the needle could oscillate in the magnetic meridian. Sets of readings were taken with the face of the circle first east and then west, the face of the needle being towards the face of the instrument; the needle was then reversed on its bearings and again read east and west: afterwards its polarity was reversed by stroking with the bar-magnets and both series of readings were repeated. The needle was raised and lowered several times between each set of readings; these were in all cases repeated, and if a difference of more than 3′ was observed, a fresh set was taken.

A comparison, made at Kew, between the Dover circle and the instrument employed in the monthly observations regularly carried on at that observatory, afforded the following results:—

Kew, June 7, 1878. Time, 9.30 A.M. to 12.26 P.M.

		Needle 1.	Needle 2.	Mean.
		° ′	° ′	° ′
Dover Circle		67 41·8	67 44·1	67 43·0
Kew Circle	1st series	67 43·8	67 44·1	67 44·2
	2nd ,,	67 44·7	67 44·1	

A second set of comparisons at Kew, made after the return from America, gave the following numbers:—

		Needle 1.	Needle 2.	Mean.
		° ′	° ′	° ′
Dover Circle, Oct. 2, 1878		67 45·7	67 41·6	67 43·7
Kew Circle	Sept. 25, ,,	67 43·5	67 43·4	67 43·6
	,, 26, ,,	67 43·8	67 43·8	

These results are perfectly satisfactory, and show that the Dover circle is calculated to afford the true dip at the various stations along the line of survey. The results of the American observations are contained in Table II.

Table II.

Station.	Date. 1878.	Local time.	Needle No. 1.	Needle No. 2.	Mean.
		h. m. h. m.	° ′	° ′	° ′
New Haven ..	July 17	11 0 to 1 3	73 05·8	73 05·0	73 05·4
La Junta	„ 25	9 55 „ 1 25	66 01·3	66 03·2	66 02·3
Manitou	Aug. 4	11 8 „ 4 39	66 58·5	66 59·9	66 59·2
Denver	„ 8	10 27 „ 12 6	67 31·2	67 34·3	67 32·8
Cheyenne	„ 9	3 19 „ 4 23	69 01·5	69 02·5	69 02·0
Salt Lake City.	„ 14	10 13 „ 12 55	67 01·9	67 02·6	67 02·3
Ogden	„ 24	12 31 „ 2 31	67 23·0	67 26·1	67 24·5
Green River ..	„ 26	2 59 „ 3 57	68 20·5	..	68 20·5
Grand Island .	„ 28	3 52 „ 5 24	70 19·1	70 16·2	70 17·7
Council Bluffs.	„ 30	9 26 „ 10 51	71 05·2	71 06·2	71 05·7
Chicago	Sept. 2	12 33 „ 2 21	72 40·1	72 38·7	72 39·4
Allegheny	„ 5	12 15 „ 2 10	72 07·0	72 08·0	72 07·5
Washington ..	„ 8	11 22 „ 1 11	70 47·8	70 45·9	70 46·9

Only a single complete set of observations with needle 1 could be taken at Green River. This was just concluded when a sudden and violent wind-storm, which filled the air with dust and nearly upset the tripods, put an end to the work. In the absence of the requisite data it is scarcely possible to attempt to correct the foregoing numbers for secular variation, even if that should be thought necessary; the time occupied by the survey was hardly long enough to affect the results to any appreciable extent. We may, therefore, safely take 1878·7 as the epoch of all the observations.

Of the thirteen stations named in the foregoing table, New Haven, Washington, Allegheny, and Chicago alone would seem to afford data for a discussion of the yearly rate of change in the inclination; these four towns (if we may consider Pittsburg and Allegheny, which are merely separated by the Monongahela River, as practically identical) are the only ones included in the long list given by Sir Edward Sabine for the epoch 1842·5. (Contributions to Terrestrial Magnetism, No. XIII, "Philosophical Transactions," 1872.) The difference in dip at the four stations at the two epochs may be seen from the following table:—

Station.	Dip 1842·5.	Dip 1878·7.	Diff. of Epoch.	Diff. of Dip.
	° ′	° ′		′
New Haven..	73 30 ..	73 05·4 ..	36·2 years ..	24·6
Washington	71 18 ..	70 46·9 ..	„ ..	31·1
Pittsburg ..	72 40·5 ..	72 07·5 ..	„ ..	33·0
Chicago	72 44·3 ..	72 39·4 ..	„ ..	4·9

No certain conclusion, however, can be drawn from these numbers respecting the extent of the secular change in this element, since, as Mr. Schott's observations at Washington have shown, the dip at that place was on the increase at the epoch 1842·5, and attained a maximum about 1854·8. Doubtless a similar increase occurred at New Haven and Pittsburg (Allegheny) and probably a maximum was also reached at these places at about the same period. The result for Chicago seems exceptional; I am hardly inclined to regard it as due to an error in the determination of the dip at the epoch 1842·5, on account of the number and fairly concordant character of the observations, which were all made at about the time of the first epoch by different observers. These are—

Loomis 1841	72° 48′
Nicollet 1841	72 46
Younghusband 1842 ..	72 39

My own results agree perfectly among themselves: they were taken under favourable circumstances, and so far as I could learn from Professor Colbert, the Director of the Dearborn Observatory—there was nothing in the spot selected for the observations—an open space in the grounds of the University of Chicago—which could at all interfere with the proper direction of the needle. Whether Chicago is affected by any strong local disturbance I am unable to say: the geological character of the district affords no evidence on this point.

In an Appendix to the "United States Coast Survey Report for 1870," Mr. Schott has discussed the secular changes in the declination, dip, and intensity of the magnetic force at Washington. The observations of inclination, which extend over a period of nearly forty years, may be represented by the expression—

$$I = 71^\circ{\cdot}335 - 0{\cdot}000229\ (t - 1855{\cdot}0) - 0{\cdot}000640\ (t - 1855)^2$$
$$- 0{\cdot}0000303\ (t - 1855)^3,$$

which, as the accompanying comparisons show, gives results closely accordant with the observed values.

Date.	Observed Dip.	Computed Dip.	Difference.
1839·2	71°·29	71°·30	−0°·01
1851·5	71·32	71·33	−0·01
1861·6	71·30	71·30	−0·00
1871·4	71·00	71·03	−0·03
1872·5	71·00	70·97	+0·03
1873·5	70·97	70·92	+0·05

This formula, although perfectly applicable between the intervals for which it was originally calculated, no longer accords with the observations of the present epoch. On June 15, 1878, Mr. Schott found the magnetic dip, at his observatory, to be 70°·80: my own observations at the same place, three months later, made it 70°·78; the value calculated from the above formula is 70°·57, or 14′ less than the mean of our observations, an amount far in excess of the error of the observation. It is not at all improbable, as indeed Mr. Schott has already remarked, that although on the whole the dip is decreasing at Washington there are subordinate periods of comparatively short duration during which an increase occurs, the former rate of diminution being afterwards re-established. Inequalities of the same kind, although by no means of the same extent, have also been traced in the observations of inclination made at Toronto and Kew by Sir Edward Sabine and Dr. Balfour Stewart (Sabine, "Phil. Trans., 1863;" Stewart, "Proc. Roy. Soc.," vol. xviii, 1870).

Intensity.

The horizontal component of the earth's magnetic force was determined by the method of vibrations and deflections in the manner described in the "Admiralty Manual of Scientific Inquiry," 3rd edition, 1869. By the aid of the deflections we obtain $\frac{m}{X}$, or the ratio between the magnetic moment of the magnet employed and the horizontal component; from the vibrations we find the product mX, whence either m or X may be obtained. The total force I is obtained by dividing X by the cosine of the observed dip; and from this we readily find the vertical component V from the formula:—

$$V^2 = I^2 - X^2.$$

The final results are stated in British and metrical units; one foot, one second of mean solar time, and one grain being the units of space, time, and mass in the one case; and one millimetre, one second of time and one milligramme being the units in the other. By dividing the latter values by 10, the corresponding values in C. G. S. are also obtained. The factor for converting the British to metric values is 0·46108.

The value of log $\pi^2 K$ at 60° is 1·62832, as determined by Mr. Whipple, at Kew.

The induction coefficient μ is 0·0001615.

The correction for temperature t^c to the adopted standard temperature 35°F., is

$$0{\cdot}0001383\ (t^\circ - 35^\circ) + 0{\cdot}000000607\ (t^\circ - 35^\circ).$$

The correction for error of graduation of the brass deflection bar at 1 foot is −0·000135 foot and at 1·5 foot −0 00087 foot.

The times of vibration given in the table are, in all cases, obtained from the means of 14 observations of the time required by the magnet to make 100 vibrations : no correction has been applied for semi-arc of vibration which at the commencement of the observations was always below 50′ ; at two stations only, viz., at Salt Lake City and at Council Bluffs—where the rate of the chronometer exceeded 3 seconds, was it necessary to apply the correction $1-\frac{s}{86400}$. The observed time of vibration has been also duly corrected for the torsion-force of the suspending thread. The angular value of 1 division of the scale of the vibration magnet is 2′ 12″ : that of the deflection apparatus is 60″·5.

The mean value of P in the formula $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$ was −0·002357.

Tables III and IIIA contain the details of the several observations.

Table III.—Deflection Observations.

Station.	G. M. Time.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$.
	d. h. m.	foot.	°	° ′ ″	
New Haven	July 17 11 14 A.M.	1·0	77·7	16 42 26	9·16182
	11 58 ,,	1·3	78·0	7 30 55	9·16120
	,, 18 4 48 P.M.	1·0	99·8	16 36 07·5	9·16136
	5 28 ,,	1·3	100·0	7 28 53	9·16146
La Junta	,, 25 9 14 A.M.	1·0	93·0	12 03 34	9·02471
	9 54 ,,	1·3	93·6	5 27 51	9·02492
Manitou	Aug. 6 7 05 A.M.	1·0	86·4	12 33 47	9·04109
	7 46 ,,	1·3	86·0	5 40 40	9·04113
Denver	,, 8 11 44 A.M.	1·0	87·7	12 45 52·5	9·04847
	12 14 ,,	1·3	87·9	5 46 52	9·04875
Salt Lake City ..	,, 14 10 06 A.M.	1·0	94·9	12 39 57·5	9·04590
	10 42 ,,	1·3	94·4	5 44 13	9·04609
Ogden	,, 24 11 16 A.M.	1·0	90·0	12 47 36	9·04967
	11 48 ,,	1·3	89·4	5 47 53·5	9·05021
Green River	,, 26 5 39 P.M.	1·0	87·2	13 12 05·5	9·06278
	6 03 ,,	1·3	87·0	5 58 16	9·06263
Grand Island ..	,, 28 8 59 A.M.	1·0	83·6	13 55 31	9·08518
	9 36 ,,	1·3	83·8	6 17 39·5	9·08513
Council Bluffs ..	,, 30 5 30 A.M.	1·0	84·9	14 33 28	9·10420
	5 57 ,,	1·3	84·4	6 34 36	9·10416
Chicago........	Sept. 2 8 50 A.M.	1·0	84·0	15 39 30	9·13500
	9 27 ,,	1·3	83·5	7 03 45	9·13490
Allegheny	,, 5 8 09 ,,	1·0	78·4	15 25 41	9·12820
	8 40 ,,	1·3	79·0	6 57 56·5	9·12849
Washington	,, 8 7 20 ,,	1·0	77·9	14 36 11	9·10483
	7 53 ,,	1·3	78·1	6 35 59	9·10505

Table IIIa.—Vibration Observations.

Station.	Date.	Temperature.	Time of one Vibration.	Log *m* X.	Value of *m*.
	d. h. m.	°	secs.		
New Haven	July 17 11 12 A.M.	75·0	4·4424		
	11 24 ,,	74·7	4·4450	0·33508	
	,, 18 4 05 P.M.	95·0	4·4573		0·5599
	4 17 ,,	95·5	4·4539	0·33464	
La Junta	,, 25 11 04 A.M.	100·0	3·8162		
	11 14 ,,	98·0	3·8162	0·46943	0·5587
Manitou	Aug. 6 6 15 A.M.	85·0	3·8947		
	6 26 ,,	85·7	3·8967	0·45025	0·5568
Denver	,, 8 11 04 A.M.	98·0	3·9296		
	11 15 ,,	97·0	3·9300	0·44388	0·5575
Cheyenne	,, 10 4 20 P.M.	72·0	4·0230		
	4 31 ,,	72·2	4·0229	0·42116	0·5570
Salt Lake City ..	,, 14 11 12 A.M.	96·0	3·9200		
	11 23 ,,	94·8	3·9196	0·44560	
	,, 15 5 05 P.M.	95·0	3·9223		0·5566
	5 16 ,,	94·5	3·9219	0·44499	
Ogden	,, 24 10 43 A.M.	101·5	3·9564		
	10 54 ,,	102·0	3·9559	0·43809	0·5547
Green River	,, 26 5 03 P.M.	87·0	4·0034		
	5 14 ,,	86·7	4·0030	0·42644	0·5553
Grand Island ..	,, 28 7 04 P.M.	95·0	4·1087		
	7 15 ,,	91·5	4·1087	0·40450	0·5557
Council Bluffs ..	,, 30 3 03 P.M.	84·0	4·2010		
	3 16 ,,	83·7	4·2007	0·38439	0·5550
Chicago	Sept. 2 3 06 P.M.	88·0	4·3550		
	3 18 ,,	89·2	4·3549	0·35350	0·5548
Allegheny	,, 5 4 39 P.M.	85·5	4·3193		
	4 51 ,,	80·0	4·3194	0·36013	0·5549
Washington	,, 8 6 37 P.M.	78·0	4·1997		
	6 49 ,,	77·9	4·1996	0·38405	0·5552

The final results of the intensity observations are contained in Table IIIb.

Table IIIB.—Magnetic Intensity.

	British Units.			Metric Units.		
	Horizontal Force.	Vertical Force.	Total Force.	Horizontal Force.	Vertical Force.	Total Force.
New Haven ……	3·8610	12·7001	13·2740	1·7802	5·8558	6·1204
La Junta………	5·2761	11·8717	12·9913	2·4327	5·4738	5 9900
Manitou ………	5·0649	11·9245	12·9555	2·3353	5·4981	5·9735
Denver ………	4·9847	12·0619	13·0513	2·2983	5·5615	6·0177
Cheyenne ……	4·7348	12·3561	13·2322	2·1831	5·6972	6·1011
Salt Lake City …	5·0061	11·8156	12·8324	2·3082	5·4479	5·9168
Ogden ………	4·9440	11·8821	12·8696	2·2796	5·4786	5·9339
Green River ……	4·8069	12·1048	13·0243	2·2164	5·5813	6·0052
Grand Island ……	4·5675	12·7531	13·5463	2·1060	5·8802	6·2459
Council Bluffs …	4·3663	12·7493	13·4763	2·0132	5·8784	6·2137
Chicago ………	4·0671	13·0232	13·6435	1·8753	6·0047	6·2907
Allegheny ……	4·1294	12·8040	13·4534	1·9040	5·9037	6·2031
Washington ……	4·3608	12·5096	13·2479	2·0107	5·7679	6·1083

The following observations of horizontal force in absolute measure were made at Kew with the Owens College unifilar before and after the American observations.

June 7, 1878.—Deflection Observations.

G. M. T.	Distance of centre of magnets.	Temp.	Observed deflection.	$\text{Log } \frac{m}{X}$
h. m.	ft.	°	° ′ ″	
4 24	1·0	77·4	16 29 18	9·15621
5 02	1·3	77·4	7 25 46	9·15610

Vibration Observations.

G. M. T.	Temp.	Time of the vibration.	Log mX.
h. m.	°	sec.	
3 07	74·6	4·4231	0·33899
3 19	74·6	4·4230	

October 2, 1878.—Deflection Observations.

G. M. T.	Distance of centre of magnets.	Temp.	Observed deflection.	$\text{Log. } \frac{m}{X}$
h. m.	ft.	°	° ′ ″	
2 57	1·0	64·7	16 21 33	9·15174
3 24	1·3	64·7	7 22 11	9·15153

Vibration Observations.

G. M. T.	Temp.	Time of the vibration.	Log mX.
h. m.	°	sec.	
2 10	61·7	4·4399	0·33458
2 22	61·7	4·4400	

The magnetic moment of the magnet before and after the survey was as follows :—

			Mean.
Kew, June 7, 1878.—	I.	0·5592	·5592
	II.	0·5592	
Oct. 2, 1878.	I.	0·5535	·5535
	II.	0·5534	

The results of these observations compared with those of the Kew unifilar made at about the same period are as follows :—

		Horizontal force.	Diff.
Kew Unifilar.	May 27, 1878.	3·8958	
Owens College Unifilar ..	Sept. 7, ,,	r=3·9030	+0·0074
		r^1=3·9034	
Kew Unifilar	Sept. 24, 1878.	3·8962	
Owens College Unifilar ..	Oct. 2, ,,	r=3·9032	+0·0074
		r^1=3·9041	

Combining these results with the respective values for the inclination obtained by the several instruments, we have the following independent determinations of the magnetic intensity, in absolute measure, at Kew :—

		Horizontal.	Vertical.	Total.
Kew Instruments ..	May, 1878.	3·896	9·516	10·283
Owens College Instruments	June ,,	3·903	9·525	10·293
Kew Instruments . ..	Sept. ,,	3·896	9·513	10·280
Owens College Instruments	Oct. ,,	3·904	9·531	10·299

Table IV shows the values in British units of the total force at such of the above stations as are mentioned in Sir Edward Sabine's "Contributions to Terrestrial Magnetism," No. XIII ("Philosophical Transactions, 1872,") at the two epochs 1842·5 and 1878·7.

Table IV.

	1842·5.	1878·7.	Diff.
Pittsburg	13·5	13·45	—·05
New Haven ..	13·37	13·27	—·10
Chicago	13·78	13·64	—·14
Washington ..	13·5	13·25	—·25

According to Mr. Schott, the horizontal force at Washington has been steadily increasing since 1853·5. From observations made since 1842·5, he finds that the value of this element may be represented by the formula

$$X=4{\cdot}270+0{\cdot}00084\,(t-1855{\cdot}0)+0{\cdot}000248\,(t-1855{\cdot}0)^2.$$

For the present epoch the calculated value is 4·4292. My determinations gave 4·3608, in close accordance with Mr. Schott's observation made on June 15, 1878, of 4·366. The difference between the observed and computed values affords further evidence of the fact pointed out by Mr. Schott, that the secular changes in the neighbourhood of Washington are of a mixed progressive, and periodic character. The divergences exhibited both in this case and in that of the dip, further show how very unsafe it is to employ numerical formulæ of this kind beyond the limits of the time within which the observations upon which they are based are comprised.

Declination.

The unifilar magnetometer was provided with a mirror and horizontal telescope in accordance with the plan first proposed by Dr. Lloyd in the "Proceedings of the Royal Irish Academy" for 1858; hence the observations could be carried out in the manner described by Mr. Welsh in the "Admiralty Manual of Scientific Inquiry."

As a rule, six readings of the position of the magnet were taken; three of these were with the scale erect and three with the scale inverted: at Cheyenne, however, owing to lack of time, only four observations were possible.

Care was always taken that the transit-mirror was in adjustment before commencing the observations. The operation of adjustment for collimation-error was greatly facilitated by a neat contrivance which Dr. Balfour Stewart had caused to be introduced in the original construction of the instrument. In order to eliminate any error which might arise from the plane of the mirror not being parallel to its axis, the observations were always made first with the mirror in its normal position and then after reversal on its bearings. Care, too, was always taken to remove, as far as practicable, all torsion from the suspending thread. When time permitted, the magnet was replaced by a torsion-plummet of equal weight, the position of which was noted after an interval of an hour or so, a suitable correction being applied to the original observation if it were found that any torsion still remained in the thread.

As the correct determination of the sun's azimuth was mainly dependent upon an accurate knowledge of local time, every opportunity was taken to ascertain the errors and rates of the chronometers employed. In order to guard against the possible accidents of travel over a country so rough and difficult as that of many parts of Western America, I provided myself with three of these instruments. That lent to me by Dr. Stewart was a large-sized marine chronometer, by Frodsham, No. 4,066; in addition, I hired a pocket instrument from the same makers, No. 5,674, which had behaved well as a "hack" chronometer for some months previously: this was always carried horizontally in its case and guard-box. For the third instrument I was indebted to Mr. George Salt, of Saltaire, who is well-known for his horological skill and knowledge; it was a remarkably well-finished instrument by Kuhlberg, No. 2,918, and was specially compensated for fluctuations of temperature. The Owens College instrument, however, went so well throughout the entire journey that I used it exclusively, relying on the others mainly for comparisons. At several of the stations, viz., at Ogden, Chicago, Allegheny, and Washington I was able, thanks to the Directors of the observatories at those places, to make comparisons with standard instruments; at La Junta the errors and rates were furnished to me by Professor Hall; on other occasions I obtained the time from Dr. Schuster's observations.

Table V.

Station.	Date.	Error on G. M. T.	Daily rate.	Remarks.
		m. s.		
Leeds.........	May 21·04	1 48·0 slow	s.	Comparisons with Post Office 1 p.m. time signal.
,, ...	June 3·04	1 30·0 ,,	+1·38	
,, ...	,, 9·04	1 23·8 ,,	+1·03	
,, ...	,, 14·09	1 18·9 ,,	+0·97	Comparisons with P. O. time signal and Frodsham 5,674.
,, ...	,, 22·15	1 09·3 ,,	+1·19	
,, ...	,, 26·47	1 04·4 ,,	+1·14	
S.S. "Germanic"	July 4·04	0 57·0 ,,	+0·99	Time gun at Liverpool.
,,	,, 12·26	0 49·3 ,,	+0·94	Ship's chronometer Bassett 555.
New York.....	,, 20·31	0 40·3 ,,	+1·11	Negus standard clock.
La Junta......	,, 25·17	0 39·7 ,,		Comparison with Negus 1,340. Observations furnished by Prof. Asaph Hall.
,, ...	,, 27·43	0 34·5 ,,	+2·30	
,, ...	,, 30	0·27·9 ,,	+2·36	
Manitou	Aug. 4 to 6		+2·55	Sextant observations by Dr. Schuster.
Denver	,, 8·29	0 05·7 ,,		
Salt Lake City	,, 17·31	0 33·1 fast	+3·00	
Ogden........	,, 23·67	0 49·6 ,,	+2·59	Comparison with Negus 1,481 by Prof. Truman Safford.
,, ...	,, 24·69	0 52·1 ,,	+2·50	
Green River ...	,, 26·29	0 58·4 ,,		Sextant observations by Dr. Schuster.
Grand Island ..	,, 28·28	1 10·8 ,,		
Council Bluffs .	,, 30·27	1 17·8 ,,	+3·50	
Chicago.......	Sept. 1·69	1 24·4 ,,	+2·73	Comparison with observatory clock by Prof. Colbert.
,, ...	,, 2·67	1 27·0 ,,	+2·64	
Allegheny	,, 5·46	1 34·0 ,,	+2·59	Observatory clock.
Washington ...	,, 7·11	1 38·3 ,,	+2·60	Naval observatory clock, Professor Harkness.

Table V shows the error and rate of chronometer Frodsham 4066, during the course of the journey.

Comparative observations were made at Kew before and after the survey between the unifilar employed in the regular monthly determinations at that observatory and the Owens College instruments, with the following results :—

		Declination.
		° ′ ″
Kew Declinometer	May 29, 1878.	19 13 36 W.
,, ,, ...	,, 30, ,,	19 17 44
Owens College Declinometer..	June 7, ,,	19 12 03
Kew Declinometer	Sept. 25, ,,	19 12 10
,, ,, ...	,, 26, ,,	19 13 33
Owens College Declinometer..	Oct. 3, ,,	19 14 01

The observations for the determination of the astronomical meridian at the several stations are given in Table VI.

Table VI.

Station.	Date.	Local apparent time of passage of ⊙ centre.	⊙ Circle reading.	⊙ Bearing.	Meridian reading of circle.
		h. m. s.	° ′ ″	° ′ ″	° ′ ″
New Haven..	July 18	7 44 24·6 A.M.	30 06 35	90 04 50 E.	300 01 45
La Junta ..	,, 29	5 40 54·4 P.M.	203 10 42·5	77 48 22 W.	280 03 01
Denver ..	Aug. 8	9 23 00·2 A.M.	37 44 22	113 18 13 E.	284 26 09
Cheynne ..	,, 10	11 15 38·3 ,,	10 55 22·5	156 11 16 ,,	214 44 16
Salt Lake City	,, 15	10 33 12·8 ,,	247 00 10	138 27 17 ,,	288 32 53
Green River	,, 26	9 00 14·9 ,,	27 08 43	115 09 23 ,,	271 59 20
Grand Island	,, 28	10 11 12·0 ,,	76 13 46	134 46 10 ,,	301 27 36
Council Bluffs	,, 30	7 37 12·8 ,,	45 26 56	99 27 40 ,,	305 59 16
Chicago ..	Sept. 2	4 10 39·0 P.M.	193 11 46	102 56 00 W.	296 07 46
Allegheny ..	,, 5	4 28 49·3 ,,	20 18 38	99 58 11 ,,	120 16 49
,,	,, ,,	.. ..	.. ..	.. ..	120 22 37*
Washington	,, 8	.. ..	.. ..	.. ..	115 18 37*

* Observatory fixed marks.

The corresponding azimuthal readings of the magnet are contained in Table VIA.

Table VIa.

Station.	Date.	Time.	Circle reading.	Declination, correct for torsion.
		h. m. h. m.	° ′ ″	° ′ ″
New Haven	July 18	8 22— 8 47 A.M.	291 20 38	8 40 34 W.
,, ...	,, ,,	6 11— 6 30 P.M.	291 23 07	8 41 55 ,,
La Junta........	,, 29	4 55— 5 20 ,,	294 06 56	14 03 55 E.
Denver..........	Aug. 8	12 17—12 34 ,,	299 08 57	14 43 25 ,,
Cheyenne........	,, 10	10 41—10 44 A.M	230 17 27	15 33 11 ,,
Salt Lake City ...	,, 15	8 22 – 8 40 ,,	305 20 59	16 48 06 ,,
Green River	,, 26	7 41— 8 03 ,,	288 57 09	16 57 49 ,,
Grand Island	,, 28	10 35—10 53 ,,	314 19 15	12 51 39 ,,
Council Bluffs....	,, 30	7 00— 7 19 ,,	316 38 58	10 39 42 ,,
Chicago	Sept. 2	10 27—10 45 ,,	300 40 51	4 33 05 ,,
Allegheny	,, 5	10 26—10 48 ,,	117 59 51	2 22 46 W.
,, ...	,, 5	4 52— 5 08 ,,	117 49 44	2 20 24 ,,
Washington.. ...	,, 8	10 08—10 33 ,,	111 36 20	3 42 51 ,,

I have not attempted to correct these results for diurnal range. Accurate observations of daily change have been made at Philadelphia, Washington, and Key West in Florida, and on the assumption that the range of the daily variation is inversely proportional to the horizontal force, the data derived from these observations may perhaps be considered as fairly applicable to any place in the United States. But such corrections are at best very unsatisfactory, as the epochs of the extreme positions are subject to very great fluctuations on account of disturbances: these are usually most prevalent at about the time at which my observations were made, viz., August and September. The magnet, however, was always quiet during the time of observation; hence the correction due to diurnal range would seldom exceed 3′ or 4′.

Observations of declination have been made in the district of Columbia, in which Washington is situated, from as far back as 1792. These have been collected and discussed by Mr. Schott in the memoir above referred to, and represented by him by the formula—

$$\text{(I.)} \qquad D = 1^{\circ}{\cdot}79 + 1^{\circ}{\cdot}90 \sin (1{\cdot}5n - 24^{\circ}{\cdot}1),$$

in which n is the number of years since 1830, and D the declination, + when west, expressed in degrees.

The annual variation v was given by the formula—

$$\text{(II.)} \qquad v = 2'{\cdot}985 \sin (1{\cdot}5n + 66^{\circ}).$$

The preceding formula gives the value of the declination for 1870 as 2° 54′, and for 1880 as 3° 16′. The observed declination, however, already considerably exceeds the latter value. Mr. Schott informs me that an observation made on June 15, 1878, gave 3° 47′; my own determination on September 8, 1878, was 3° 42′·85 (uncorrected for diurnal range).

In a later publication "On the Secular Change of the Magnetic Declination in the United States and other parts of North America" (United States Coast Survey Report for 1874, Appendix No. 8), Mr. Schott adopts the formula

$$D=1^\circ{\cdot}79+1^\circ{\cdot}90 \sin (1{\cdot}5m+5^\circ{\cdot}9),$$

where m is the number of years and fraction of a year since 1850; but this expression also affords lower values than those actually observed. The value for 1880, for example, is given as 3° 15′·6, and for 1870 as 2° 54′. Mr. Schott has also discussed (*loc. cit.*) the various observations made at New Haven, Connecticut, since 1761, and has represented them by the expression

$$D=+7{\cdot}83+3{\cdot}16 \sin (1{\cdot}4m-21^\circ{\cdot}6),$$

where m has the same signification as in the last formula. The annual change is at present about 4′·3. This would give for 1878·6 the value 8° 45′, which agrees very closely with that actually observed.

Sir Edward Sabine's collections afford the following data for further comparison:—

	Date.	Declination.	Date.	Declination.	Diff. of epoch. Years.	Yearly Change.
		° ′		° ′		′
Salt Lake City	..1850	15 34 E.	1878·7	16 48 E.	28·7	−2·6
Pittsburg	..1840·5	0 08 W.	"	2 21 W.	38·2	+3·5
"	..1845·5	0 33 W.	"	2 21 W.	33·2	+3·3

At Salt Lake City I was informed by Mr. Orson Pratt, who led the first band of Mormons into Utah, that he had determined the magnetic declination on what is now the site of the Mormon Tabernacle, and he was so good as to afford me the following extract from his journal:— "July 30, 1847. I took several observations during the day of the sun's azimuths and altitudes, and by means of seven calculations I ascertained the variation of the magnetic needle to be 15° 47′ 23″ E. Instruments, sextant and ordinary azimuth compass."

In concluding, I wish to tender my thanks to the various officials connected with the public service in America with whom I came in contact, for the prompt and courteous manner with which all our requirements relating to the magnetic work were met. To General Smith, commanding at Fort Lyon, in Colorado, we were indebted for the loan of a tent which was placed at our disposal during the whole of our journey; Rear-Admiral Rodgers, the Director of the United States Naval Observatory at Washington, furnished us with maps and letters, and procured us special facilities from certain of the railroad companies, and otherwise showed his interest in the work.

Addendum. April 26, 1879. Received April 28, 1879.

Since sending in the foregoing communication to the Society, I have received the following letter from Mr. C. A. Schott, of the United States Coast Survey Department, respecting the inadequacy of the formula published in the Report of 1870, as representing the values of the magnetic elements at the present time.

It will be noticed that Mr. Schott confirms my observations on this point; and by permission of the Superintendent of the Survey, he sends amended formula for dip, declination and horizontal force which represent with sufficient accuracy both series of observations, viz., his made in June, and mine made in September, 1878.

United States Coast Survey Office,
Washington, March 28th, 1879.

MY DEAR SIR,

I hasten to reply to yours of March 13, respecting magnetic observations in this city. The formulæ in the 1870 Report undoubtedly fail to represent adequately our late observations, a contingency which was referred to in the concluding paragraph of that report: "They should not be used much beyond the time for which they are supported by observations." There is nothing unusual going on in the behaviour of terrestrial magnetism in this locality, and the dip and intensity formulæ may be easily amended to represent our observations, but in regard to the declination there is a disturbing element introduced, viz.: the *change of station in* 1876, which developed a local difference of 20′ as near as I can judge; whether the old or new place is the more normal one I cannot say. There is apparently no difference in dip and intensity. Considering the dissimilarity of the instruments used our observations agree well, indeed they accord within the daily variations of the magnetic force. By permission of the Superintendent of the Survey, I enclose the latest formulæ for declination, dip, and intensity in this place, as well as my observations of last year; they are:—

The magnetic dip observed at Washington between 1839 and 1879 can be represented by the formula—

$$I = 71^\circ{\cdot}335 - 0{\cdot}000229\,(t - 1855{\cdot}0) - 0{\cdot}000640\,(t - 1855{\cdot}0)^2 - 0{\cdot}0000140\,(t - 1855{\cdot}0)^3,$$

which will leave no greater differences than those given by the old formula. (Report of 1870.) The magnetic horizontal intensity observed between 1842 and 1879 is represented by

$$H = 4{\cdot}270 + 0{\cdot}00084\,(t - 1855{\cdot}0) + 0{\cdot}000130(t - 1855{\cdot}0)^2,$$

which will leave differences of about the same character as those given by the old formula.

The very nature of these functions precludes them from answering for any length of time, and as soon as the case becomes ripe, I shall substitute harmonic functions.

The following is an improved formula for the declination at Washington, prepared for a new edition of the paper in the 1874 Report:

$$D = +2^\circ{\cdot}19 + 2^\circ{\cdot}22 \sin (1{\cdot}4m - 6^\circ{\cdot}6),$$

it represents the observations between 1792 and 1879; $m = t - 1850{\cdot}0$.

These observations are difficult to manage on account of considerable local deflection in this vicinity produced by iron (bog-ore) which in many places crops out to the surface.

Our observations compare as follows:

In latitude 38° 53′·2: longitude 77° 00′·4.

		Declin.	Dip.	Hor. Inten.
		° ′	°	
June 14, 15, 17.	C. A. Schott ..	3 47 ..	70·49 ..	4·366
Sept. 8, 1878.	T. E. Thorpe ..	3 43 ..	70·47 ..	4·361

Yours very truly,
CHAS. A. SCHOTT.

Dr. T. E. THORPE,
Yorkshire College, Leeds, England.

February 5, 1880.

THE 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 Spectra of the Compounds of Carbon with Hydrogen and Nitrogen." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 2, 1880.

In a memoir "On the Spectra of Metalloids" ("Nova Acta Reg. Soc. Upsal.," ser. iii, vol. ix), Ångström and Thalèn have made a careful analysis of the different spectra assigned to carbon. They distinguish four sets of groups of shaded bands produced under different circumstances, which they define, besides the line spectrum which they ascribe to carbon itself. Of these four sets of bands, two sets, situated at the extremities of the spectrum, they show to be produced in the combustion of cyanogen, a third to be common to all the hydrocarbons, and the fourth to be produced by carbonic oxide. The first two sets, the third, and the fourth sets respectively, they observed to be produced also in the electric discharge between carbon electrodes according as it took place in nitrogen, hydrogen, or oxygen. Their observations on this subject appear to us almost conclusive. Nevertheless, other observers have, since their publication, maintained different opinions.

The spectrum of hydrocarbons burning in air has been repeatedly described: first by Swan in 1856, and afterwards by Attfield, Watts, Morren, Plücker, Boisbaudran and others, and has been given in detail by Piazzi Smyth ("End on Illumination in Private Spectroscopy"). The characteristic part of this spectrum consists of four groups of bands of fine lines in the orange, yellow, green, and blue respectively, and we refer hereafter to these as the hydrocarbon bands. These four groups, according to Plücker and Hittorf, also constitute the spectrum of the discharge of an induction coil in an atmosphere of hydrogen between carbon electrodes. They are also conspicuous in the electric discharge in olefiant gas at the atmospheric and at reduced pressures.

The descriptions of the other less conspicuous parts of the spectrum of the flame of hydrocarbons are not all concordant. Boisbaudran notices, besides the four hydrocarbon bands, only two hazy bands in the indigo. Watts ("Phil. Mag.," 1869 and 1871) and Piazzi Smyth give the same two bands somewhat wider, and resolved into a series of fine lines. These two observers are in substantial agreement about this part of the spectrum, but Piazzi Smyth notices in addition a faint haze in the red below C.

Plücker and Hittorf notice the entire absence in the flame of olefiant gas of the two bright groups of lines (blue and violet as described below) characteristic of the flame of cyanogen, and the presence of a series of dark lines on a violet background in a position intermediate between those of the two cyanogen groups. A similar description is given by Morren ("Ann. Ch. et Phys.," Mars, 1865). Neither of these observers notices the two hazy bands above-mentioned. The descriptions given by these authors of this series of dark lines appear to relate to something not seen by the other observers. They resemble in some respects the description of the corresponding part of the spectrum of the electric discharge in vapour of sulphur, and we think it highly probable that these lines were due to some compound of sulphur derived from the sulphuric acid employed in preparing the gas.

Several observers also have described the spectrum of the flame of burning cyanogen. Faraday, as long ago as 1829, called the attention of Herschel and Fox Talbot to it, and the latter, writing of his observations ("Phil. Mag.," ser. iii, vol. iv, p. 114), points out as a peculiarity that the violet end of the spectrum is divided into three portions with broad dark intervals, and that one of the bright portions is ultra-violet. More recently Dibbits, Morren, Plücker and Hittorf, have particularly described this spectrum. Dibbits ("Pogg. Ann.," 1864) mentions in the cyanogen flame fed with oxygen, a series of orange and red bands shaded on the less refrangible side (*i.e.*, in the opposite way to the hydrocarbon bands), the four hydrocarbon bands more or less developed, a group of seven blue lines, a group of two or three faint blue (indigo) lines, then a group of six violet lines, and, lastly, a group of four ultra-violet lines. When the cyanogen is burnt in air the hydrocarbon bands are less developed, and the three faint indigo lines are scarcely visible, but the rest of the spectrum is the same, only less brilliant.

Plücker and Hittorf ("Phil. Trans.," 1865) state that in the flame of cyanogen burning in air under favourable circumstances, the orange and yellow groups of lines characteristic of burning hydrocarbons are not seen, the brightest line of the green group appears faintly, the blue group is scarcely indicated; but a group of seven fluted bands in the blue, three in the indigo, and seven more in the violet, are well developed, especially the last. When the flame was fed with oxygen instead of air, they state that an ultra-violet group of three fluted bands appeared. They notice also certain red bands with shading in the reverse direction, which are better seen when the flame is fed with air than with oxygen. Other observers give similar accounts, noticing the brilliance of the two series of bands in the blue and violet above mentioned, and that they are seen equally in the electric discharge through cyanogen. All do not notice the ultra-violet group, but this is no doubt owing to the absorption of these rays by the prisms employed, for photographs which we have taken of the spectrum of the cyanogen flame in air show this group strongly developed.

These three bright groups of bands of fine lines are as characteristic of the flame of cyanogen and of the spark in that gas, as the four hydrocarbon bands are of the flame of hydrocarbons.

The flame of carbonic oxide shows nothing but a continuous spectrum. The series of bands seen in the spark in an oxide of carbon are well known, and there is not, so far as we know, any dispute about them. The brightest of them appears to have been seen by some observers in the flame produced by the combustion of other compounds of carbon.

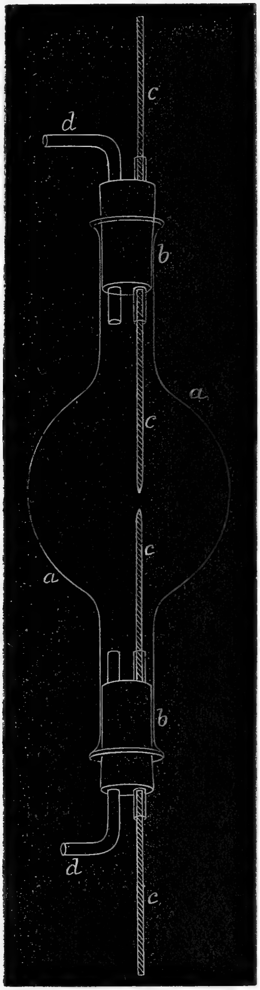
The contention of Ångström and Thalèn is that the channelled spectra of the hydrocarbon and cyanogen flames are the spectra of acetylene and cyanogen, and not of carbon itself, and that in the flame of burning cyanogen we sometimes see the spectrum of the hydrocarbon superposed on that of the cyanogen, the latter being the brighter; and that in vacuum tubes containing hydrocarbons the cyanogen spectrum observed is due to traces of nitrogen.

No chemist who remembers the extreme sensibility of the spectroscopic test, and the difficulty, reaching almost to impossibility, of removing from apparatus and material the last traces of air and moisture, will feel any surprise at the presence of small quantities of either hydrogen or nitrogen in any of the gases experimented on.

Mr. Lockyer ("Proc. Roy. Soc.," vol. xxvii, p. 308) has recently obtained a photograph of the arc in chlorine, which shows the series of fluted bands in the ultra-violet, on the strength of which he throws over the conclusion of Ångström and Thalèn, and draws inferences as to the existence of carbon vapour above the chromosphere in the coronal atmosphere of the sun, which, if true, would be contrary to all we know of the properties of carbon. We cannot help thinking that these bands were due to the presence of a small quantity of nitrogen.

Our faith in the conclusions of Ångström and Thalèn on this subject has been much strengthened by our own observations, which we now proceed to describe.

The following experiments were made with a De Meritens dynamo-electric machine, arranged for high tension, giving an alternating current capable of producing an arc between carbon poles in air of about 10 millims. in length. The carbon poles used were 3 millims. in diameter, and had been previously purified by prolonged heating in a current of chlorine. This treatment, though it removes



a large part of the metallic impurities present in the commercial carbons, will not remove the whole, so that lines of calcium, iron, magnesium, and sodium may still be recognised in the arc. Besides the traces of metallic impurities a notable quantity of hydrogen always remains unremovable by treatment with chlorine; and we are not at present aware of any practicable method by which it can be got rid of.

The arc was taken in different gases inside a small glass globe (*aa* in figure) about 60 millims. in diameter, blown in the middle of a tube. The two ends of the tube (*bb*) were closed with dry corks, through which were passed (1) the carbons (*cc*), inserted through two pieces of narrow glass tubing, and a packing of platinum foil; (2), two other glass tubes (*dd*) through which currents of the gases experimented with were passed.

Spectrum of the Arc in Air.

The arc taken in a globe of air gave a tolerably bright continuous spectrum, superposed on which the green and blue hydrocarbon bands were seen, also the seven bands in the blue (wave-lengths 4600 to 4502, Watts) as in the flame of cyanogen, three faint bands, or lines, in the indigo (wave-lengths about 4364·5, 4371·5, 4381·5), and much more brightly the six bands in the violet (wave-lengths 4220 to 4158, Watts) and five ultra-violet (wave-lengths about 3883·5, 3871, 3862, 3854·5, 3850.) Besides these bands, lines of iron, calcium, and sodium were visible. The arc in this case was practically taken in a mixture of nitrogen and carbonic oxide, for in a short time the oxygen of the air is converted into carbonic oxide, as was shown by the flame which always filled the globe on stopping the arc for a second or two.

Spectrum of the Arc in Carbonic Acid Gas.

On passing through the globe continuously a current of carbonic acid gas, the seven bands in the blue, and those in the violet and ultra-violet gradually died out until they ceased to be at all visible continuously, and when now and then momentarily seen were only just discernible. On the other hand, the hydrocarbon bands, yellow, green, and blue, came out stronger and were even brilliant. Lines of iron and calcium were still visible. On stopping the current of carbonic acid gas and allowing air to diffuse into the globe, the violet and ultra-violet bands soon began to appear and presently became permanent and bright, the hydrocarbon bands remaining bright.

Spectrum of the Arc in Hydrogen.

The globe had next a continuous current of dry hydrogen passed through it. The arc, contrary to what would be expected from the behaviour of the spark discharge in hydrogen, would not pass through more than a very short space, very much less than in air or carbonic acid gas. There was a tolerably bright continuous spectrum, no trace of the seven bands in the blue, or of those in the violet or ultra-violet, and no metallic lines, but a fairly bright line in the red, which we identified, by comparison with the spark in a vacuum tube, with the C line of hydrogen. The F line, identified in like manner, was also seen as a faint diffuse band. This last line was in general overpowered by the continuous spectrum, but was regularly seen, when, from some variation in the discharge, the continuous spectrum became less brilliant. This was the first occasion on which we had seen the hydrogen lines in the arc at all, though Secchi ("Compt. Rend.," 1873) states that he had seen them by the use of moist carbon poles. We tried to repeat his experiment, without success. The hydrocarbon bands in the green and blue were at intervals well seen. Those in the yellow and orange were, owing doubtless to the smaller dispersion of the light in that region, overpowered by the continuous spectrum. Whereas when air and carbonic acid gas were used, the inside of the globe was quickly covered with dust from the disintegrated poles, scarcely any, if any, such dust was thrown off when the arc was passed in hydrogen.

Spectrum of the Arc in Nitrogen.

Nitrogen was next substituted for the hydrogen. A longer arc would now pass, the seven blue, the violet and ultra-violet bands all came out well, at intervals brilliantly. The green and blue hydrocarbon bands were also well developed.

Spectrum of the Arc in Chlorine.

On filling the globe with chlorine and keeping a current of that gas passing through it, the arc would not pass through a greater distance than about 2 millims. No metallic lines were visible. At first the violet bands, as well as the green and blue hydrocarbon bands, were visible, but gradually when the current of chlorine had been passing for some minutes, there was nothing to be seen but a continuous spectrum with the green and blue hydrocarbon bands. Neither of these bands were strong, and at intervals the blue bands disappeared altogether.

Spectrum of the Arc in Carbonic Oxide.

When the globe, previously full of air, was filled with carbonic oxide and a current of that gas passed through it, the arc would not pass through any greater space than in chlorine. There was much continuous spectrum; the yellow, green, and blue hydrocarbon bands were well seen, some of the seven blue bands were just discernible, the violet had nearly, and the ultra-violet quite, gone from sight. No trace of the carbonic oxide bands, as seen in the spark discharge in that gas, was visible. This is the more remarkable since under similar circumstances two of the characteristic lines of hydrogen were seen.

Spectrum of the Arc in Nitric Oxide.

In this gas a very long arc could be obtained. The violet and ultra-violet bands were well seen, the seven blue bands were seen but weaker. The blue and green hydrocarbon bands were also seen well when the arc was short, not so well when the arc was long. The length of the arc did not seem to affect the brightness of the violet bands. Many metallic lines of iron, calcium, and magnesium were seen.

Spectrum of the Arc in Ammonia.

In ammonia only a short arc could be obtained. All the bands were faint, but the seven blue, the violet and ultra-violet bands were always visible.

These experiments with different gases eliminate to a large extent the influence of electric conductivity on the character of the spectrum; but we intend to examine more thoroughly the effect of this variable by observation of the arc in some of these gases under different conditions of pressure.

Spectra of Flames of Carbon Compounds.

Besides the experiments with the arc above described, we have made some observations of the spectra of flames of sundry compounds of carbon.

In the flame of cyanogen, prepared from well dried mercury cyanide, passed over phosphoric anhydride inserted in the same tube, and burnt from a platinum jet fused into the end of the tube, we found, as Plücker and Hittorf had found, that the hydrocarbon bands were almost entirely absent, only the brightest green band was seen, and that faintly. The seven blue and the violet bands on the other hand were, as described by other observers, well developed, the three indigo bands less brightly. A series of bands at the red end, also described by Dibbits and by Plücker and Hittorf, was also visible. These bands are sharply defined on the more refrangible side and fade away on the other side, and extend beyond the orange hydrocarbon bands on the red side. They have not been seen by us in the spectra of the arc, but they may very well have been present in some of those spectra and yet not seen because of the continuous spectrum of the arc, which, not being very much dispersed at the red end, was bright enough to overpower any such bands. This continuous spectrum seems much stronger when well purified carbon electrodes are used than when they contain metallic impurities to the extent commonly present in them. We may remark that in general we see only six bands, or rather six maxima of light, in the violet series, as stated by Dibbits, not seven, as stated by Morren and by Plücker and Hittorf; and in the ultra-violet we always see five maxima of light, instead of four as given by

Dibbits, and three given by Plücker and Hittorf. These ultra-violet bands are well developed in the flame of cyanogen burning in air and are readily photographed, and easily seen when quartz lenses and calcite prisms are employed.

The flame of hydrocyanic acid burning in air showed very much the same as that of cyanogen.

In the flame of a mixture of hydrogen and carbon disulphide, made by passing purified hydrogen through a tube containing carbon disulphide so as to be nearly or quite saturated with the vapour, and burning it in air, no hydrocarbon bands at all could be detected.

Nor could any hydrocarbon bands be detected in the flame of a mixture of carbonic oxide and hydrogen burnt in air.

When a mixture of hydrogen with carbon tetrachloride vapour was burnt, hydrocarbon bands made their appearance, but were rather weak.

The flame of carbonic oxide mixed with vapour of carbon tetrachloride showed faint traces of hydrocarbon bands occasionally, but not continuously.

On the other hand, chloroform, carefully prepared from chloral and fractionated, when mixed with hydrogen, gave, when burnt in air, the hydrocarbon bands very strongly. When it was mixed with carbonic oxide instead of hydrogen it still gave the hydrocarbon bands, but not nearly so strongly as with hydrogen.

On a review of the whole series of observations, certain points stand out plainly. In the first place, the seven blue, the violet, and ultra-violet bands, characteristic of the flame of cyanogen, are conspicuous in the arc taken in an atmosphere of nitrogen, air, nitric oxide, or ammonia, and they disappear, almost, if not quite, when the arc is taken in a non-nitrogenous atmosphere of hydrogen, carbonic oxide, carbonic acid, or chlorine. These same bands are seen brightly in the flames of cyanogen and hydrocyanic acid, but are not seen in those of hydrocarbons, carbonic oxide, or carbon disulphide. The conclusion seems irresistible that they belong to cyanogen; and this conclusion does not seem to us at all invalidated by the fact that they are seen weakly, or by flashes, in the arc or spark taken in gases supposed free from nitrogen, by reason of the extreme difficulty of removing the last traces of air. They are never, in such a case, the principal or prominent part of the spectrum, and in a continuous experiment they are seen to fade out in proportion as the nitrogen is removed. This conclusion is strengthened by the observations of one of us, that cyanogen (or hydrocyanic acid) is generated in the arc in atmospheric air in large quantity. Also in the experiment above described with the arc in nitrogen, we have found that when the current of nitrogen issuing from the globe is passed through a solution of potash the solution soon gives the reactions of a cyanide.

In the next place, the green and blue bands, characteristic of the hydrocarbon flame, are well seen when the arc is taken in hydrogen; but though less strong when the arc is taken in nitrogen or in chlorine, they seem to be always present in the arc, whatever the atmosphere. This is what we should expect, if they be due, as Ångström and Thalèn suppose, to acetylene; for we have found that the carbon electrodes always contain, even when they have been long heated in chlorine, a notable quantity of hydrogen.

In the flames of carbon compounds they by no means always appear; indeed, it is only in those of hydrocarbons or their derivatives that they are well seen. Carbonic oxide and carbon disulphide, even when mixed with hydrogen, do not show them; and if seen in the flames of cyanogen, hydrocyanic acid, and carbon tetrachloride mixed with hydrogen, they are faint, and do not form a principal or prominent part of the spectrum. This is all consistent with the supposition of Ångström and Thalèn. The fact that the bands are not produced even in the presence of hydrogen, when it is not present in the flame in the form of a compound with carbon, is very significant; for we know that acetylene is present, and can easily be extracted from the flame of any hydrocarbon, and that it is formed as a proximate product of decomposition of hydrocarbons by the electric discharge, but we have no evidence that it is producible as a product of direct combination of carbon with hydrogen at the comparatively low temperature of a flame such as we have mentioned.

The hydrocarbon bands are best developed in the blowpipe flame, that is under conditions which appear, at first sight, unfavourable to the existence of acetylene in the flame. We have, however, satisfied ourselves, by the use of a Deville's tube, that acetylene may readily be withdrawn from the interior of such a flame, and from that part of it which shows the hydrocarbon bands most brightly.

The question as to whether these bands are due to carbon itself or to a compound of carbon with hydrogen, has been somewhat simplified by the observations of Watts and others on the spectrum of carbonic oxide. There is, we suppose, no doubt now that that compound has its own spectrum quite distinct from the hydrocarbon flame spectrum. The mere presence of the latter spectrum feebly developed in the electric discharge in compounds of carbon supposed to contain no hydrogen, appears to us to weigh very little against the series of observations which connect this spectrum directly with hydrocarbons.

In the next place, it appears conclusively from the experiments, that the development of the violet bands of cyanogen, or the less refrangible hydrocarbon bands, is not a matter of temperature only. For the appearance of the hydrogen lines C and F in the arc taken in hydrogen, indicates a temperature far higher than that of any flame. Yet the violet bands are not seen in hydrogen at that temperature, while the green bands are well developed. The violet bands are, nevertheless, seen equally well at the different temperatures of the flame, arc, and spark, provided cyanogen be the compound under observation in the flame, and nitrogen and carbon are present together at the higher temperatures of the arc and spark.

The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green band of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connexion the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum.

Apart from the mere relative electric conductivity of gases, it is clear, from the foregoing experiments, that the length and the character of a discontinuous electric discharge in different gases does not follow the law which we should expect. It will require a prolonged series of experiments to arrive at definite conclusions on this matter, but it appears to us, in the mean time, highly probable that one of the main factors in producing these remarkable variations in the arc will be found to be the relative facility with which the carbon electrode combines with a gaseous medium.

The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins ("Proc. Roy. Soc.," xvi, p. 386; xxiii, p. 154; "Phil. Trans.," 1868, p. 555), that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connexion with the conclusions to which our experiments point. Provided we admit that the materials of the comet contain ready formed hydrocarbons and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum, or of metallic lines. Such reactions might be brought about by the tidal disturbances involving collisions and projections of the constituents of the swarms of small masses circulating in orbits round the sun, which we have every reason to believe constitute the cometic structure. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature; and if nitrogen were also present, that we should have the cyanogen spectrum as well. Either then the first supposition is the true one, not disproving the presence of nitrogen; or else the atmosphere which the comet meets is hydrogen and contains no free nitrogen.

II. "On the Epipubis in the Dog and Fox." By T. H. Huxley, Sec. R.S. Received January 30, 1880.

In 1871* I gave a brief description of a structure which I had observed in the dog, in the following terms:—

"In the myology of the dog, the insertion of the tendon of the external oblique muscle of the abdomen presents some interesting peculiarities. The outer and posterior fibres of this muscle end in a fascia, which is partly continued over the thigh as *fascia lata*, and partly forms an arch (Poupart's ligament) over the femoral vessels; by its inner end it is inserted into the outer side of a triangular fibro-cartilage, the broad base of which is attached to the anterior margin of the pubis, between its spine and the symphysis, while its apex lies in the abdominal parietes. The internal tendon of the external oblique unites with the tendon of the internal oblique to form the inner pillar of the abdominal ring, and is inserted into the inner side of the triangular fibro-cartilage. The *pectineus* is attached to the ventral face of the cartilage; the outer part of the tendon of the rectus into its dorsal face; but the chief part of that tendon is inserted into the pubis behind it. This fibro-cartilage appears to represent the marsupial bone, or cartilage, of the Monotremes and Marsupials."

The only reference to this statement which I have met with is by Professor Macalister, in his "Introduction to the Systematic Zoology and the Morphology of Vertebrate Animals" (1878), p. 265:—

"Professor Huxley describes a fibro-cartilaginous 'marsupial' above the pubis, from whose anterior surface the pectineus arises. I have failed to satisfy myself of its existence as a constant structure in many dogs, in the common and Bengal foxes, in the dingo, jackal, *Canis pallipes*, and wolf."

The wording of this passage does not make it quite clear whether the writer has not found the structure in any case, but does not mean to deny that it may occur occasionally in the various *Canidæ* he mentions; or whether he has found it occasionally, but not constantly, in all or some of them.

Under these circumstances, it may be desirable to publish the fact that, having recently dissected, for purposes of comparison, a male and female fox and a male and female dog, I have not had the slightest difficulty in demonstrating the existence of the structure which I described

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* "Manual of the Anatomy of Vertebrated Animals," p. 417.

in 1871, in all four. And the only phrase which appears to require modification in that description is the use of the term fibro-cartilage. I do not remember whether, formerly, I submitted the structure to microscopic examination or not; but in the specimens lately examined, notwithstanding the firmness and density of the triangular plate, it contains no true cartilage cells, but is entirely composed of fibrous bands which lie parallel with one another in the middle of the plate, while, at the thickened edges, they become closely interwoven.

A comparison of this triangular fibrous plate in the fox, with the "marsupial" bones of *Phalangista vulpina*, shows that the fibrous plate in the former animal exactly answers to the basal part of the "marsupial" bone in the latter. It may properly, therefore, be termed the *epipubic ligament*, and must be regarded as a structure of the same order as the rudimentary clavicle and the rudimentary hallux of the *Canidæ;* that is to say, as the remains of an organ which was fully developed in the ancestral forms of that group.

It is interesting to remark, in connexion with this interpretation of the facts, that, in the existing *Thylacinus*, which presents so many curious points of resemblance to the dogs, the epipubis is not ossified. As, however, the *Canidæ* have certainly existed since the Eocene epoch, there is no likelihood of the existence of any direct genetic connexion between the dogs and the Thylacines. The existing carnivorous *Marsupialia* have evidently all proceeded from ancestral forms, characterised by the possession of a thumb-like hallux, a peculiarity which is presented neither by the dogs, when they possess a hallux, nor by any other Carnivora with pentadactyle hind feet. Moreover, the early birth of the young and the development of a marsupium in the female, are evidences of the departure of the existing Marsupialia from the direct line by which the Mammalia have advanced from the ornithodelphous type. That the ancestors of all mammals possessed bony or cartilaginous epipubes is, I think, highly probable, but it does not follow that they had the marsupial method of bearing and nourishing their young.

February 12, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. "Studies on the Chinoline Series." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 10, 1880.

In the first part of this paper which appeared in the "Proceedings" of the Society for 1877, it was shown that the leucoline series of bases occurring in coal tar were really aromatic derivatives, as on oxidation an acid could be obtained which readily decomposed on distilling with an alkali, giving aniline.

The acid, which was named leucolinic acid, had the composition $C_9H_9NO_3$, and may be regarded as a higher homologue of isatic acid, $C_8H_7NO_3$, which is one of the characteristic indigo derivatives. The conclusions of this paper have been confirmed by the synthesis of chinoline by Baeyer from the hydro-carbostyril, and by Koenigs from allylaniline, the latter reaction being similar in principle to the synthesis of pyrrol, described in my former paper, by passing ammonia and a mixture of monobromethylene and acetylene through a red hot tube.

Experiments on the oxidation of leucoline and the alkaloidal bases have been repeated by Hoogewerff, Van Dorp, and Koenigs. These experimenters have found that instead of leucolinic acid being produced, they obtain a variety of pyridene dicarbonic acid, a substance which I had on a former occasion separated from the oxidation products of picoline.

The explanation of the difference of the results is to be found in the fact, that my object was to obtain the first products of oxidation and thus to avoid breaking up the body into simpler constituents.

Both the tar and cinchona bases may be regarded as having a double nucleus of a benzol and pyridene residue, and the latter, being the more stable of the two, derivatives of it are obtained when the oxidation is more complete.

The interest of the subject has been greatly enhanced by the results of Weidel, Schmidt, Ramsay, and Dobbie on the direct oxidation of the cinchona alkaloids.

All these chemists agree that the chief oxidation product has the composition and properties of either a pyridene di- or tri-carbonic acid.

My attention being directed to other scientific subjects, I have thought the results of experiments, which I had intended extending, had better be published, as research in this department of organic chemistry is being vigorously prosecuted, which will in all probability at no distant date result in the synthesis of some important alkaloids.

Oxidation of Cinchona Chinoline.

Chinoline obtained from cinchonine, which had been carefully purified as described in my former paper, was treated with permanganate in the same way as leucoline, in order to see if any acid similar to the leucolinic acid could be obtained. In each operation, about 13 grms. of chinoline was dissolved in dilute sulphuric acid, and a hot solution of 30 grms. of permanganate of potash added. As the chinoline is more readily attacked than the corresponding tar base, to prevent loss the oxidation must be carried out in large basins, as the mass is apt to froth over from the evolution of carbonic acid.

After filtration from the oxide of manganese, the alkaline solution was evaporated to dryness, and extracted with alcohol to separate the sulphate, carbonate, and oxalate of potash.

The soluble potash salt was dissolved in water, excess of sulphuric acid added, and the organic acid extracted by shaking with ether. The yield of acid was small, and as no modification of the process seemed to increase the amount, the substance obtained was chiefly used to determine its composition and general reactions.

Chinolinic Acid.

The acid crystallised in nodular masses very soluble in water. It melts at 143°, or 20° lower than the isomeric leucolinic acid. The silver salt is very insoluble, separating from cold solutions as a flocculent precipitate, which on standing becomes crystalline. When it is recrystallised from a large volume of boiling water, it is obtained in tufts of fine needles. The crystals were always mixed with a slight reddish-brown substance, which appeared to be oxide of silver.

The acid gave no violet colour with ferric chloride, but the ammonia salt gave a violet precipitate, which rapidly became of a reddish-brown colour.

Analysis of the acid obtained from cinchona chinoline and the silver salt yielded the following results :—

	I.	II.
Weight of acid taken	0·2835	0·209
Carbonic acid produced	0·640	0·473
Water	0·134	0·099

Calculated in percentages—

	I.	II.	$C_9H_9NO_3$.
Carbon	61·5	60·5	60·33
Hydrogen	5·2	5·16	5·02

Silver salt—

	I.	II.
Weight of salt	0·2295	0·3235
Carbonic acid produced	0·292	0·454
Water	0·054	0·078
Silver	0·100	0·1375

Calculated in percentages, the results are—

	I.	II.	$C_9H_8AgNO_3$.
Carbon	34·7	34·89	37·76
Hydrogen	2·61	2·68	2·79
Silver	43·56	42·5	37·76

A quantity of the silver salt which had been treated with dilute nitric acid, weighing 0·186, gave 0·081 of silver, or equivalent to 43·5 per cent. A sample recrystallised, weighing 0·1492, gave 0·0655 of silver, or 43·9 per cent. The second fraction, obtained by partial precipitation from the ammonia salt amounting to 0·3033, gave 0·1343 of silver, or 44·2 per cent. The mean of the five analyses giving 43·5 per cent. of silver. Nitrogen determination:—The nitrogen was found on combustion to bear to the carbonic acid the relative volume ratio of 1 to 18·3.

The above analyses of the silver salt prove that it always contains excess of silver, either from a higher substitution product, or from combined oxide. The composition might be represented by eight equivalents of the normal silver salt combining with one of oxide of silver. Although the want of material prevented the examination of a greater number of salts, there can be little doubt that the composition of the acid is $C_9H_9NO_3$.

The acid fused with caustic potash gives aniline, and the silver salt when heated gave a crystalline sublimate with drops of aniline.

The acid dissolves in hot glycerine with a fine violet fluorescence, and begins to decompose, giving off carbonic acid at 180°, together with oily drops of aniline.

Leucolinic acid does not decompose in glycerine before a temperature of 205° is reached; the general decompositions of this acid are clearly identical with that of the isomeric acid obtained from the tar base, and prove conclusively that both bases are intimately related to the aromatic series.

Nature of Tar Leucoline.

According to the researches of Greville Williams the leucoline of tar is distinguished from the corresponding cinchona base by its giving an oily chromate, which in his hands never crystallised.

The leucoline used in my experiments can be shown to be a mixture of at least two isomeric bases, one of which yields a crystalline chromate, which is not however identical with the chromate of the alkaloid base.

The portion boiling between 210° and 220° gave the largest yield of the crystalline chromate.

In no case did the mixture of the base with excess of chromic acid give any crystals until it had stood for several days.

After separating the crystals the free base was obtained by distillation with caustic potash, and gave immediately a well crystallised salt on the addition of a little chromic acid.

The base separated from the uncrystallisable chromate seemed to boil on the whole at a lower temperature.

Analysis of the crystalline chromate obtained from tar leucoline :—

	I.	II.
Weight of chromate taken	0·3285	0·3425
Carbonic acid produced	0·5485	0·573
Water obtained	0·1030	0·112

Calculated in percentages—

	I.	II.	Theory.
Carbon	45·5	54·6	45·34
Hydrogen	3·48	3·62	3·35
Chromic oxide	—	—	31·99

0·223 of the chromate gave 0·0715 chromic oxide, or 32·07 per cent.; and 0·7915 of chromate from another sample gave on ignition 0·260 chromic oxide, or 32·8 per cent. These analyses leave no doubt as to the substance being a pure variety of leucoline.

That there are at least two distinct bases in tar may be proved in other ways.

If leucoline is digested in the water-bath for several days with a large excess of chloride of ethyl, only one of the bases seems to combine with it, forming a reddish-violet solid. If the excess of chloride of ethyl containing the uncombined leucoline is distilled in a current of steam, this portion of the base now yields a crystalline chromate. Similarly, the original leucoline heated in sealed tubes to a temperature of 170° C. with chromic acid in the proportion of 10 grms. to 5 cub. centims. of the base, gave, after separating the oxide of chromium, on distilling caustic potash, the base yielding the crystalline chromate. The alkaline fluid on evaporation to dryness, and heating with soda ime, gave a little pyrrol, but no aniline. The alkaline solution distilled with excess of sulphuric acid gave volatile organic acids. The ordinary leucoline treated directly with iodide of ethyl gave a red viscid mass, containing yellow crystals, which may be separated by successive washings with alcohol. When this iodide of ethyl leucoline was distilled, the free base yielded the crystalline chromate; whereas the leucoline separated from the oily chromate, when heated with iodide of ethyl, gave an intense red-coloured fluid, in which a few crystals appeared after standing for a week or two.

It thus appears by the three methods detailed above, that ordinary leucoline is a mixture of two isomeric bases; and that samples of the tar product may differ considerably in properties.

We can thus easily explain how Hofmann obtained on one occasion a crystalline chromate from the tar bases, which led him to infer that the substance was identical in properties with that derived from the decomposition of the cinchona alkaloids; whereas Greville Williams, working upon a different specimen, could obtain nothing but an oily uncrystallisable salt.

In reality both experimenters described the properties of different specimens of the bases.

Oxidation of Leucoline.

Many attempts to increase the yield of leucolinic acid, by varying the proportions and temperature of the reacting bodies, led to the following results.

The greatest amount of acid is obtained when one part of the base, dissolved in the form of neutral sulphate, is treated with $2\frac{1}{2}$ times its weight of permanganate of potash dissolved in boiling water, the oxidising agent being carefully added to the salt of leucoline. If the action is allowed to proceed in the cold with about four parts of the permanganate to one of base, there is no leucolinic acid formed, all the higher boiling point bases, belonging either to the tar or cinchona series of bases, yielded acid derivatives on oxidation, which, on distillation with soda-lime, gave aniline when the above plan was adopted; if this was not done, the chief product was a member of the pyridine series. This explains why von Dorp, Koenigs, and others have found only pyridine dicarbonic acid without any trace of leucolinic acid.

The crude acid separated from the potash salt or the insoluble lead salt always contains a considerable quantity of a syrupy acid, which does not crystallise unless it is boiled with water for some days. It is possible this substance may be the acid derived from one of the isomeric bases, and that it is slowly changed into the crystalline modification. The crystalline leucolinic acid treated with glycerine gives a substance having the characteristic reactions of indol, but the amount obtained is always small; many attempts made to increase the yield of this body have not been successful. If fused chloride of zinc be employed instead of glycerine, the distillate is hydrochlorate of aniline, without any of the indol substance. Similarly, a solution of the potash salt of the acid, heated to 200° C., gave only aniline, carbonic and acetic acids. On heating the acid with zinc-dust, chinoline was not reproduced, as the product was a crystalline substance which has not been examined.

The decompositions of leucolinic and chinolinic acids point to their being amido-phenylpyruvic acids, and their general relations to nearly allied substances are shown in the following formulæ:—

C_6H_5	$C_6H_4NH_2$	C_6H_4NH	C_6H_4N	$C_6H_4NH_2$
CH	CH	CH	CH	CH_2
CH	CH	CH	CH	CO
CO_2H	CO_2H	CO	CH	CO_2H
Cinnamic Acid.	Amido-cinnamic.	Carbostyril.	Chinoline.	Leucolinic and Chinolinic Acids.

An acid of this character ought to be hydrogenised readily, producing the amido-phenylactic acid which has the same composition and a close relationship to the substance tyrosine, differing only in the amido-group being in this case attached to the phenyl residue instead of to the pyruvic; such isomeric tyrosines are produced by treating leucolinic and chinolinic acids with sodium amalgam. Both products react with the mixed mercurous and mercuric nitrates, giving a red colour, somewhat resembling the well-known tyrosine reaction, the difference between them and ordinary tyrosine being that, on fusion with an alkali, both yield aniline, whereas ammonia and paroxy-benzoic acid are the products obtained from the normal substance.

II. "Note on Electrolytic Experiments." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 10, 1880.

Having had occasion to employ the intermittent current of the de Meritens magneto-electric machine arranged for intensity in connexion with spectroscopic work, the following notes of electrolytic experiments made by means of it may not be without some interest.

The number of intermittent currents taking place in the circuit during the course of the following experiments may be taken as about 300 per second, and as the amount of energy in circulation is very considerable, an examination of its action on electrolytes was of some interest.

Dilute sulphuric acid, of about 1 of acid to 6 of water, used as the electrolyte in a large circular glass vessel, gave the following results:—

On placing two rather large platinum poles, each exposing a surface of about 32 square centims., no decomposition took place, a few bubbles of gas came off at one pole for a short time, which soon ceased. The current was proved to be passing by placing a telephone near the side of the glass vessel, and also by the rapid increase in the temperature of the acid, which very soon reached the boiling point. The absence of decomposition was due to the rapidity with which the current in the de Meritens machine is alternated, the gases given off at each pole being rapidly superposed and combining immediately to form water.

Decomposition was immediately produced by making and breaking the current rapidly with the hand, or better still, by placing a large tuning fork in the circuit, to which was attached a copper rod which dipped in and out of mercury at every vibration of the fork.

When two poles were used, consisting of platinum wire, decomposition went on without intermission at both poles, but on the other hand, when one pole was a plate of platinum and the other a platinum wire, the mixed gases were given off at the wire pole only.

The spectrum of the platinum poles, viewed with a small direct vision spectroscope, was very complicated, showing the H and air lines.

Platinum wire of about ·5 millim. in diameter used as one pole, readily fused when held close to the surface of the liquid, and remained white hot when immersed below the surface, with a continual ejection of the particles of the fluid, and successive small explosions of the mixed gases.

Aluminium wire behaved in a remarkable way when used as one of the poles, the other pole consisting of a large surface of platinum.

On first plunging the wire beneath the surface nothing is observed; almost immediately, however, it becomes intensely illuminated all over the surface. The light seems to emanate from an immense number of points which look like lit up gems. This appears to be due to the formation of oxide of aluminium, which offers a great resistance to the passage of the current and consequently becomes intensely heated, and is pierced or disintegrated at certain points, and this supposition seems to be confirmed by the absence of any incandescence of the wire when hydrochloric acid, caustic potash, or any other medium which dissolves the oxide as soon as formed, is used. If another aluminium wire is substituted for the platinum plate, then either the one or the other becomes illuminated according to the extent of surface of metal immersed, but they are generally not illuminated at the same time, the largest surface always remaining dull; gases are now freely given off, being chiefly hydrogen.

The luminosity of the aluminium wire becomes very much diminished after a short time, as the wire becomes almost completely covered with a uniform layer of the white oxide, and is gradually eaten away. The luminosity can again be produced by cleaning the poles. The same effects took place when dilute hydrochloric acid, or saturated chloride of sodium was substituted for the dilute sulphuric acid. After the current had passed for some time gas was given off at both poles.

The light evolved from the surface of the poles below the surface of the electrolyte was intensely white, and on spectroscopic examination, proved to be the characteristic channeled spectrum of oxide of aluminium, first described by Thalèn. The characteristic metallic lines between the two H lines were faint.

On bringing a piece of thin foil aluminium as one pole near the surface of the acid it is immediately fused, globules of the melted metal flying about in all directions; the aluminium wire is also fused after a short time, and falls through the liquid in white hot globules.

Magnesium wire at one pole, the other being the platinum plate as before, gave an intense green colour below the surface, a spectroscopic examination showing the combined spectrum of the metal and oxide.

It burnt on the surface of the liquid in the same manner as the aluminium, showers of melted metal being thrown off; the action when a saturated solution of chloride of sodium was employed was, if anything, more intense. Similar actions took place with other metals, such as copper and zinc.

A solution of chloride of sodium was rapidly decomposed, abundance of chlorine being set free, when a platinum wire was used as one pole, the larger platinum plate being used for the other. A solution of iodide of potassium behaved in the same way, but was decomposed at a very much slower rate when both the poles were of large surface. A saturated solution of sulphate of copper deposited hardly a trace of copper, when the platinum plate and a thick platinum wire were used as the two poles. Various modifications of these experiments may be made by employing different metals. The intensity current used with a saturated solution of nitrate of potash, has the power of engraving on glass, as described by von Plante.

The determination of the heat evolved from the Jablochkoff candle in the annular calorimeter formerly described, amounted to about 6,000 gram-units per minute, or in other words, it is about $\frac{1}{6}$th of the total radiation of the Siemens machine when the small resistance of the electric arc is alone in the circuit.

On introducing into this circuit containing a Jablochkoff candle the voltameter which was formerly used, and in which no decomposition was apparent when used alone, the total radiation of the candle was very slightly diminished, but the cell showed intermittent bursts of electrolytic gas just as when the tuning fork was used as formerly described. This seemed to be always associated with any quality of intermittence due to the want of regularity in the action of the candle. Assuming that the whole work of the intermittent current was exhausted first in electrolysis, and then in recombination of the separated electrolytes, and that recombination resulted solely in a heating effect localised in the cell, then the elevation of temperature would be a measure of the chemical work done. As, however, the amount of this heating is relatively small in comparison with the current circulating, we must suppose that a considerable portion of the energy of recombination is thrown into the circuit as a new electromotive force. The chief interest in these experiments is the value of the de Meritens machine, as having many of the advantages from Plante's secondary piles, and the facility and ease with which this power can be used. Mr. Spottiswoode's recent experiments have shown that the character of the arc obtained from the de Meritens machine is intermediate in qualities between that of the ordinary arc and that of the induction spark, and may be employed with great advantage in exciting the induction coil. The use of it in spectroscopic work has been fully detailed to the Society in conjoint communications with Professor Liveing. The luminous discharge obtained below the surface of fluids shows another valuable use of the machine in connexion with this subject.

February 19, 1880.

THE PRESIDENT in the Chair.

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

The Right Hon. the Earl of Northbrook was admitted into the Society.

The following Paper was read:—

I. "On some of the Effects produced by an Induction Coil with a De Meritens Magneto-Electric Machine." By WILLIAM SPOTTISWOODE, D.C.L., P.R.S. Received February 17, 1880.

In the "Philosophical Magazine" for November of last year I gave an account of a mode of exciting an induction coil by the direct application of one of M. de Meritens' alternating machines, without the intervention of a contact-breaker or the use of a condenser. The experiments of Professor Dewar on the arc furnished by the machine itself, on its spectrum, and on its behaviour in respect of electrolysis described before the Royal Society (see "Proc. Roy. Soc," vol. xxx, p. 170), have led me to think that an account of some of the peculiarities in the induced discharge, to which the machine gives rise, might be acceptable to the Society.

And, first, as regards the secondary discharge in air. It was mentioned in the paper first quoted that the spark produced by this machine presented an unusually thick yellow flame, and that it was accompanied by a hissing noise different from that commonly heard with a coil excited by a battery. As the machine gives alternate currents, the secondary discharge presents sparks of equal strength in both directions, and the general appearance to the eye is symmetrical in respect of both terminals. The spark was observed in a revolving mirror, first in a vertical and secondly in a horizontal direction. The discharge, although apparently continuous, was immediately seen to be intermittent, with a period in unison with that of the machine. Tongues of flame, leading alternately from one terminal and from the other, crossed the field of view. The length of spark first used (vertically) was about half an inch. When the length was increased to about two inches, the discharge being vertical, flashes or bands of continuous light were seen to traverse the field of view in diagonals of low slope (*i.e.*, nearly horizontally), showing that there were masses of heated matter passing from time to time at moderate velocity between the terminals. From the known period of the machine, and the number of the discharges crossed by these flashes in their passage from terminal to terminal, it was calculated that the time of passage was about ·03 of a second. Occasionally there was a still brighter flash or meteor, which similarly traversed the field, but with a velocity apparently of about double that of the others.

When the discharge was set horizontally, similar phenomena were seen; but at a distance about midway between the two terminals, the flames appeared to meet, and at their point of meeting they showed continuous luminosity throughout. When the flame was blown aside, there appeared occasionally, and at rather long intervals, the true spark, evinced by an irregular bright line, reaching from one terminal to the other.

On observing the discharge in air attentively, it was noticed that whenever a true spark passed vertically its passage was marked, as usual, by an irregular bright line when its path was outside the aureola or flame, but by a similar dark line when its path was within the aureola.

With reference to this phenomenon, it may be observed, in the first place, that in every coil discharge the true spark is always anterior to the aureola, which, in fact, immediately follows it. The true spark of each discharge taken by itself will, therefore, always appear as a bright line. It, however, sometimes happens, as mentioned above, that in the succession of discharges produced by the present method, the flame or aureola of one discharge lasts, at least over part of its length, until the next takes place. When this is the case, the spark of the succeeding discharge will pass partly through air, as usual, and partly through the flame which has lasted over from the preceding discharge. This part of its path will be through a conductor, which may be so far better than the air, as to allow the spark to pass in a non-luminous condition. The phenomenon noticed by Mr. De La Rue, viz., the sudden expansion of a gas at the instant of the passage of an electric discharge through it, may then take place, and the displacement of the flaming gas over that part of the path may give rise to the appearance of the dark line within the area of the aureola. The remaining part of the path, if any, will be bright. It is, however, further to be noticed that the true spark, or those portions of it which are bright, always appears to lie outside, and never within, the aureola. This may arise from the fact that on the extinction of a flame the portions of space immediately outside are for the moment the hottest and most conductive, and that the next spark will consequently choose a path really outside the contour of the flame just extinguished. And as the successive flames do not differ materially in size or shape, the bright spark will appear always to lie outside the flame.

The spectrum of the secondary spark was then examined with terminals of various metals.

Aluminium.—The spectrum showed a faint continuous background with the yellow sodium lines, and faint oxide of aluminium lines. This was with a spark of half an inch. But although the spark was subsequently lengthened, no difference in the spectrum was perceived excepting that the continuous background was rendered more bright.

It would seem that these appearances are due to some such process as the following:—The heat due to oxidation, added to that of the discharge, is sufficient to volatilise the oxide of aluminium; but that in its passage across the interval between the terminals, the oxide becomes so cooled that it gives a continuous spectrum. When the spark was lengthened, the oxide, although perhaps at first more heated than with the shorter spark, had more time to cool.

Magnesium.—In this, as in the former case, we have a faint continuous spectrum as a background, on which were seen the *b* group of magnesium lines. One other line in the blue occasionally flashed out, but was not permanently present. There was also a faint trace of the oxide spectrum. The contrast between the cases of aluminium and magnesium, in respect of the prominence of the oxide, or of the true metallic spectrum, is doubtless due to the fact that in the former case the oxide, and in the latter the metal, is the one which is more easily vaporised. On sending a blast of air on the discharge, the blue line always disappeared; the current of air having lowered the temperature so far as to prevent the vaporisation necessary for its production.

When the spark between magnesium terminals was made to pass through hydrogen, the characteristic lines of hydrogen were seen, apparently owing to a rise in temperature. This, as mentioned below, does not occur with carbon poles.

Platinum.—With terminals of this metal the spectrum was mainly continuous, with the addition of the ubiquitous yellow sodium lines. When the spark was short a few bands were faintly visible, some apparently those of nitrogen, and others in the blue and violet belonging to the oxide of platinum. When the spark was lengthened, these bands disappeared, and nothing but the continuous spectrum (with the D lines) was visible.

It appears from these experiments that the application of the de Meritens machine to the induction coil furnishes us with the means of isolating certain lines of the metallic spectrum from the rest. It has, in fact, enabled us to reduce at pleasure the spectra of aluminium, of magnesium, and of platinum, to their most persistent lines; precisely as had already been noticed as occurring by natural processes in the cases of sodium and of calcium. As a general rule, when the spark is shortened, the metallic, or the oxide, lines come out, according as the one or the other is more easily vaporised; and when it is lengthened they disappear.

From this we may conclude (1) that the discharge which we have been examining, is a real flame with metallic particles passing between the terminals in a solid condition; and (2) that in general the temperature is comparatively low, *i.e.*, that it is insufficient to cause any considerable vaporisation. This is notably the case when the arc is long, and when the matter thrown off from the terminals has sufficient time in its passage to cool.

The spark was then tried between carbon terminals in atmospheres of hydrogen and of carbonic acid. In none of them did the spectrum show any gas lines; but with hydrogen there were faint traces of the hydrocarbon group in the green. In this respect the spark differs from the discharge direct from the machine, inasmuch as the latter gives some of the hydrogen lines in hydrogen and carbon lines in carbonic acid.

When magnesium terminals were used in an atmosphere of hydrogen, the yellow sodium lines, the blue and green magnesium lines, and the red line of hydrogen were visible near the terminals, with a continuous background.*

When the spark was discharged in a magnetic field, known phenomena were reproduced, but owing to the thickness and mass of the flame and the extraordinary strength of the magnetic field, they were exhibited in a state of great splendour.†

When the spark passed in an equatoreal direction the whole flame was spread out in an equatoreal plane, in which heated masses might be seen revolving in one direction or in the other in the neighbourhood of each of the magnetic poles. To give some idea of the actual appearance, it may be mentioned, that a symmetrical spindle-shaped discharge, 15 millims. in length and 3 millims. in thickness, was spread out by the magnet into a disk of about 15 millims. in diameter and 1 millim. in thickness. When the spark passed in an axial direction, or when the poles themselves were made the terminals, the phenomena described in my paper "On an Experiment in Electro-Magnetic Rotation" ("Proc. Roy. Soc.," vol. xxiv, p, 403) were reproduced.

Whatever was the direction of the spark, the resistance due to the magnetic field was such as to extinguish the discharge, provided that

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* When the same discharge was effected in a magnetic field, the only change observed was that the lines became slightly fainter. There was some reason to think that, at the moment of making the magnet current, the lines became decidedly brighter, and that they lost their brilliancy if that current was maintained. But this, perhaps, requires further examination.

† Through the kindness of the authorities of the Royal Institution, Faraday's great electro-magnet, excited by the Siemens machine, was placed at my disposal.

the striking distance was near the limit that it could attain when no magnetic field was present. If a plate of glass was interposed between the poles of the magnet (which were still used as terminals) the yellow flame disappeared, and the spark divided itself into numerous ramifications of true sparks which found their way round the edges of the plate. As soon as the magnet was excited the resistance in the field became so great as to exceed that of the glass plate itself, and the plate was pierced.

Professor Dewar was good enough to measure the efficiency of the secondary discharge, by taking an inch spark in a glass bulb placed in the centre of a calorimeter, in the same way as he had already measured the efficiency of the intermittent current direct from the machine. The former amounted to about 430 gramme-units per minute, while the latter had been found to be 6,000 per minute. The relative efficiency may, therefore, be taken at about 1 : 15. And as the machine was giving about 300 currents per second, this would give for the secondary

$$430 : 60 \times 300 = \cdot 023 \text{ unit per discharge,}$$

and for the primary

$$6000 : 60 \times 300 = \cdot 3 \text{ unit per discharge.}$$

The results in the case of the secondary discharge may not perhaps possess any great degree of accuracy ; but, in the absence of any other information on the question, they may serve to indicate the general scale of magnitude by which the coil discharge is related to that of the machine.

Leaving the subject of the spark from the induction coil, one of the most remarkable effects produced by this machine was the illumination of vacuum tubes by the currents taken simply from the machine. A small sphere of about two inches in diameter, with an air-vacuum, and having two parallel straight terminals reaching nearly across the sphere and about half an inch apart, was (after the first attempt, when there was some difficulty in getting the discharge to pass) readily illuminated. Owing to the alternate currents, both terminals were of course surrounded with the usual blue halo. When the speed of the machine was reduced, the discharge through the tube was not maintained, showing that only that part of the current from the machine which possessed the highest electromotive force, and perhaps also the greatest strength, was sufficient and was therefore actually used for the purpose. As this was apparently only a small fraction of the whole current, we may herein find an explanation of the fact that, compared with the effect from the induction spark, the illumination was moderate, and the heating insignificant. It would perhaps not be easy to establish an accurate comparison between this and other sources of electricity ; but some idea may be conveyed by the fact that, from experiments made with this tube with Mr. De La Rue's chloride of silver battery on a former occasion, and quite independently of the present question, it was estimated that a current having an electromotive force of 400 volts was necessary to effect a discharge.

Other tubes were tried, and were illuminated in the same way.

When the tube was placed in the magnetic field, the discharge underwent the modifications described by Plücker, by Hittorf, and by others. But it may be mentioned as a result of the extraordinary strength of the field here employed that, whenever the tube was so placed that the direction of discharge made any considerable angle with the lines of magnetic force passing through it, the discharge was completely extinguished. When the tube was removed to a weaker part of the field, the discharge reappeared distorted in form, and altered in colour, in a manner known to those who have experimented in this direction.

February 26, 1880.

THE 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 Solubility of Solids in Gases." By J. B. HANNAY, F.R.S.E., F.C.S., and JAMES HOGARTH, F.C.S. Communicated by Professor G. G. STOKES, D.C.L., LL.D., Sec. R.S. Received February 14, 1880.

The research which we now have the honour of submitting to the Royal Society, and of which a preliminary notice was published in the Society's Proceedings,* was instituted with the view of throwing some further light upon what Dr. Andrews calls the "critical state" of matter. In his famous Bakerian lecture, after a very careful discussion of the question, Dr. Andrews says :† "Carbonic acid at 35·5° C., and under a pressure of 108 atmospheres, stands nearly midway between the gas and the liquid, and we have no valid grounds for assigning it to the one form of matter any more than to the other."

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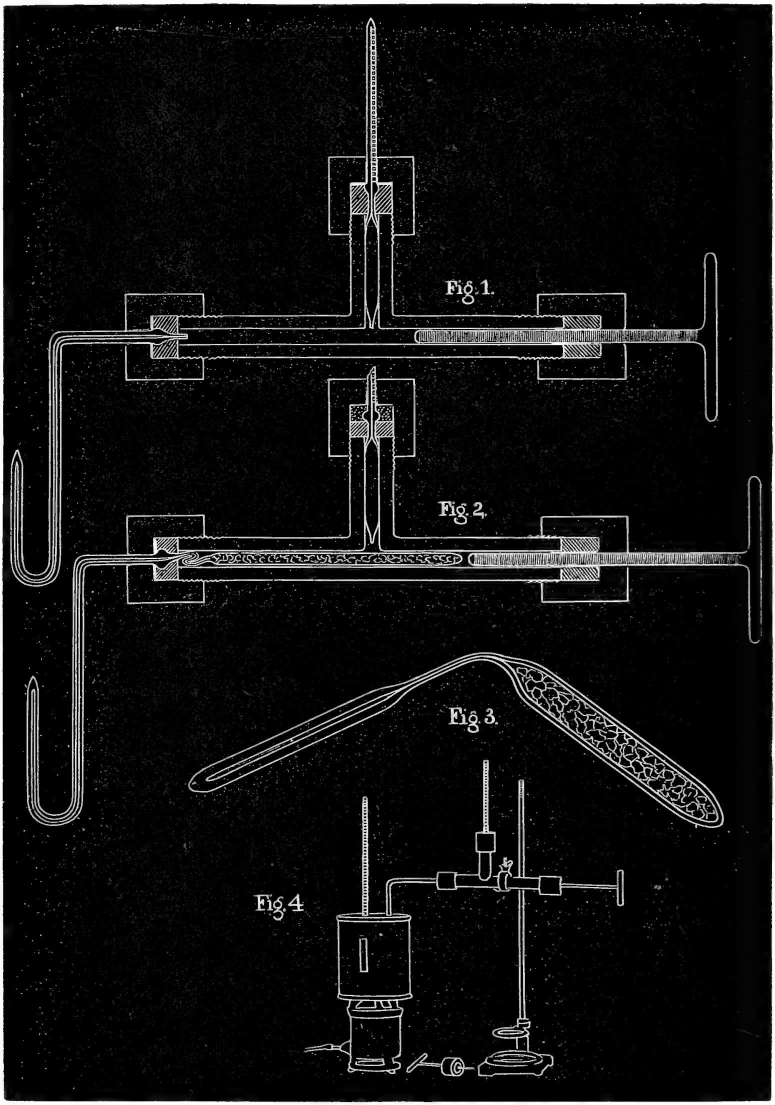
* "Proc. Roy. Soc.," vol. xxix, p. 324.

† "Phil. Trans.," 1869, p. 588.

This conclusion is based on the relation of volume to pressure for different temperatures, and on the failure of optical tests to show any distinction between gas and liquid when pressure is applied above the critical temperature. It occurred to us, that some knowledge might be gained by examining the solvent properties of some fluid for a non-volatile solid, in the passage of the solvent from the liquid to the



gaseous state. The property of dissolving solids being supposed peculiar to liquids, it seemed to us that on passing through the critical state there would be a precipitation of solid, or if on the other hand no separation occurred, a further proof of the perfect continuity of the gaseous and liquid states would be presented. To put the matter to the test, a solution of potassic iodide in alcohol was prepared, and a strong tube filled to about one-half with the solution. After sealing, the tube was placed in an air-bath, and heat applied. No precipitation of solid could be seen even at a temperature of 350° C., more than 100° C. above the critical point of alcohol.

For the examination of the phenomena of solution some form of pressure apparatus was necessary, so that the volume of the fluid experimented on might be altered at pleasure. After several experiments we adopted a modified form of Dr. Andrews' apparatus, which allowed us to change an experimental tube in a few minutes, and showed no sign of leakage at the highest pressure reached (fig. 4). A section of the apparatus is shown in the annexed engraving. It is made of wrought iron tubing such as is used in hydraulic machinery, about $\frac{1}{2}$ inch internal, and 1 inch external diameter. The length of the apparatus is 9 inches, and there is a side tube welded on, measuring about 3 inches, for the insertion of the manometer tube. On the ends of the tube are screwed strong caps, also of wrought iron; Through one of these the pressure screw works, the others serve for the admission of the experimental and manometer tubes. The distinguishing feature of the apparatus is the packing for the screw and tubes. It consists of a solid plug of india-rubber, about $\frac{1}{2}$ inch thick, placed in the hollow cap, so that it serves both for packing the screw or tube, and for making the joint between the cap and the iron tube tight. To prevent the pressure screw from cutting the india-rubber, a lining of thin leather, well soaked in lard, was placed between it and the rubber. The tubes were similarly packed, and to keep them from being forced out through the aperture in the cap, a thickening was made in the glass by fusing and pressing together, and between this bulbous part and the iron a strong leather washer was placed. In some cases, when a very high pressure was wanted, the tube was cemented in with oxychloride of zinc, which effectually prevented it from coming in contact with the iron cap. With this mode of packing the pressure screw did not require to be very fine, the one commonly used being $\frac{1}{4}$ inch in diameter and screwed 30 threads to the inch. The only limit to the pressure was the frailty of the glass tubes; we have, however, attained a pressure of 880 atmospheres as shown by an air manometer, and even at this enormous pressure the screw could be freely moved in either direction without leakage. The apparatus was as tight with mercury as with water, so that there was no necessity for using two liquids as in Dr. Andrews' machine. When a high temperature was required, the experimental tube was bent over as shown in the drawing, and passed into an air-bath formed of two concentric iron cylinders. The cylinders had mica windows, through which the experiment could be observed, and to ensure perfect regularity of temperature, the inner one was lined with cement about $\frac{1}{2}$ inch thick. As in most of our experiments it was quite unnecessary to know the pressure, a simple straight tube was used instead of the T-shaped one described. It was screwed to fit the same caps, and from its simplicity made a very convenient and efficient apparatus.

The experiments with the sealed tube showed us that no deposit of solid occurred above the critical point. With the assistance of the apparatus just described, we were able to show a crystal of potassic iodide dissolving in alcohol gas (using the word gas as Dr. Andrews proposes for a fluid at any temperature above its critical point). Having fixed a tube (internal diameter ·75 mm., external 7 mm.), in the apparatus previously filled with mercury, some alcohol was sucked in by gradually withdrawing the pressure screw. By a further withdrawal of the screw, the little thread of alcohol was moved inwards past a constriction made in the glass tube near the end, and a small fragment of fused potassic iodide dropped in so as to rest on the narrow neck. Sufficient heat was now applied to boil the alcohol and expel the air, and the end of the tube strongly sealed with the blowpipe. The tube being placed in its position in the bath, heat was applied and the pressure regulated so as to keep the alcohol from passing round the bend into the other limb of the U tube. The tube and its contents were thus raised to a temperature of 300° C. (the critical point of alcohol as will be shown is 234°·4 C.), the crystal never having come in contact with the liquid alcohol. Pressure was now applied until the alcohol was reduced to about the volume occupied by it while still liquid, when the fragment of iodide was seen to dissolve gradually and completely. On withdrawing the screw slowly no deposit occurred even when the volume was very considerably increased, but by taking off the pressure very rapidly a crystalline film appeared on the glass, and in some cases, a cloud of fine crystals floating in the menstruum. These crystals could be easily redissolved by again increasing the pressure. The deposit most frequently occurred just at the surface of the mercury, and this we ascribe to a local rarefaction caused by the slightly higher temperature of the mercury. The fact of the existence of solvent power above the critical point being established by this experiment, we next sought to examine the phenomenon by using other solvents and solids. The sealed tube method was found most convenient for this purpose, an experiment with the pressure apparatus being resorted to when we wished to vary the volume. The tube was filled from half to two-thirds with the solution to be tested, and by a few experiments the strength of the solution was so adjusted that no separation of solid took place, when the tube was completely filled by expansion.

The behaviour of a solution of potassic iodide we have already described. Bromide of potassium gave a similar result. The solubility of chloride of calcium in alcohol was found to be uninterrupted by the passage of the alcohol through the critical state. Cobaltous chloride (anhydrous) remained in solution at 320°C. the highest temperature reached in this experiment, and the beautiful blue colour of the solution was unimpaired. Ferric chloride (sublimed) gave a yellow fluid at a temperature of 260° C. This yellow colour rapidly disappeared, and a reddish-coloured powder separated out, which remained insoluble on cooling. On opening the tube a strong ethereal odour was perceived. This was most probably due to the formation of ethylic chloride. Some peculiar phenomena observed when heating the ferric chloride solution deserve description. At a temperature of 120° C. a very strongly coloured liquid layer appeared at the bottom of the tube, while the uppermost layer was perceptibly paler. As the temperature rose the two layers gradually mixed and the fluid was quite homogeneous at and above the critical point.

A solution of ferric chloride in ether was distinctly coloured at least 30° C. above the critical point, but the colour gradually faded, and did not return on cooling. The separation and redissolving of part of the ferric chloride occurred also with ether.

A solution of sulphur in carbon disulphide showed no separation of sulphur when raised 50° C. above its critical point. Of course sulphur at this temperature is a liquid, but its vapour tension is comparatively so small that its solution is worth recording. Selenium also remained in solution in carbon disulphide above its critical point, and separated out in part when the solution cooled. Chemical action evidently took place in this experiment, for on evaporating a portion of the solution after heating, a reddish-yellow residue containing selenium was obtained. The liquid had a distinct horse-radish smell, and on burning gave a dark stain on a piece of porcelain held in the flame. Sulphide of selenium was most probably formed. An attempt was made to dissolve cadmic sulphide in carbon disulphide, but without success. In an experiment with metallic arsenic and carbon disulphide the arsenic seemed to dissolve, but as an orange-coloured film appeared on the glass on cooling, we are inclined to doubt its solubility. When some sulphur was enclosed in the tube with arsenic and carbon disulphide, and the temperature raised above the critical point, solution occurred, and when the temperature was allowed to fall a separation of dense liquid drops took place at the critical point. At a lower temperature these solidify and form reddish-yellow crystals. The apparent solubility of arsenic was thus probably due to its conversion into a sulphide, and the subsequent solution of the sulphide formed.

Carbon tetrachloride was also tried as a solvent, but as the number of substances soluble in this fluid is very small, we can only record a few negative experiments. Cupric chloride did not dissolve, but appeared to be acted on chemically above the critical point, its colour changing to brown. Chromic chloride showed no sign of solution. Zinc oxide did not dissolve, but at a temperature 20° or 30° above the critical point a sudden chemical change occurred, and as a result the critical point was found to be lowered from 280° C. to about 200°. On cooling some transparent globules were seen, and on opening the tube there was an outrush of gas. The transparent globules dissolved in water, and were found to be chloride of zinc.

The use of water as a solvent was quite out of the question, both on account of its high critical point and the corrosion of the glass tubes, so that we were debarred from the use of the liquid whose examination promised most.

The mixture of light paraffins known as paraffin spirit was tried as a solvent for some organic substances. A solution of resin in this liquid remained perfectly bright and without sign of separation at a temperature of 350° to 360°, almost 100° above the critical point.

The examination of liquids to determine their critical temperature and pressure was not exactly in the line of our research, but as Cagniard de la Tour's numbers, so far as we are aware, are the only ones on record, we have made a number of experiments in this direction. To obtain accurate measurements of pressure is a matter of very great difficulty, and as for our purpose these were not required, it would have made our research unnecessarily tedious to attempt it. Our numbers are as follows:—

	Alcohol.	Carbon disulphide.	Carbon tetrachloride.
Mean temperature	234°·6	272°·96	277°·9
Number of experiments	47	12	4
Probable error of a single experiment	2°·06	1°·34	0°·69
Mean pressure	65 atmos.	77·9 atmos.	58·1 atmos.
Number of experiments	14	12	4
Probable error of a single experiment	1·0 atmos.	1·5 atmos.	·55 atmos.

The alcohol used had a specific gravity of ·7949 at 15°·5 C. In the carbon tetrachloride experiments the mercury was attacked, but our numbers were taken as quickly as possible, and before chemical action had taken place to any great extent.

Cagniard de la Tour gives the following results for alcohol and carbon disulphide:—

Alcohol	258°·7 C.	119 atmospheres.
Carbon disulphide.	275°·0 C.	77·8 ,,

The differences between Cagniard de la Tour's numbers and our own in the case of alcohol are no doubt due to the difficulty of obtaining that body anhydrous. As will be seen from the density given, the alcohol used in these experiments contained only a very small quantity of water, and was indeed practically anhydrous.

To determine whether the presence of a solid in solution affected the critical temperature and pressure, a cold saturated solution of potassic iodide in alcohol was prepared. Experiments with this solution gave as the average of three trials—temperature 237°·1 C., pressure 71·1 atmospheres. The probable error of a single experiment is for the temperature 0°·37 C., for the pressure 0·5 atmosphere. The exact amount of solid present in solution when the readings were made it was impossible to determine; for in altering the volume, so as to observe the critical point, a small quantity was deposited on the glass just at the end of the mercury column. To make sure that some salt did remain in solution, we brought the mercury column up so as to cover the crystalline film, and after carefully examining to see that no solid particles were above the mercury, we allowed the fluid to cool under pressure. When cold, the end of the capillary was broken off, and the liquid, which had been shut off by the mercury from contact with the crystalline deposit, received in a watch glass. On evaporation it gave an appreciable residue. These experiments seem to show that a liquid has its critical point raised by the solution in it of a solid.

As the solubility of potassic iodide in alcohol is very slight (1 part in 40 at 13°·5 C.) we thought it advisable to try a solution of chloride of calcium in alcohol, so that the effect of solution on the pressure and temperature might be shown in a more marked degree. On heating a cold saturated solution of calcic chloride (anhydrous), a dense liquid layer was observed to form at the bottom when the temperature reached 230° C.; at 240° C. the line separating the lighter layer of liquid from vapour was very indistinct, and could not be distinguished at all above 250° C.; at 255° C. the dense layer dissolved, but re-appeared when pressure was taken off. When the volume was again reduced, it re-dissolved, and at 270° C. expansion caused a deposition of crystals. In this experiment there was no sharp critical point, and no pressure readings were taken. A dense layer, it will be remembered, was also noticed in the ferric chloride experiment. A combination of the chloride with part of the alcohol evidently occurs, and at a higher temperature diffusion takes place until the fluid is quite homogeneous.

While working with cobaltous chloride an interesting question presented itself to us—Is the absorption spectrum of a substance dissolved in a fluid above its critical point the same as in liquid solution or in the solid state? To answer this question, a solution of anhydrous cobaltous chloride was sealed in a tube as before, and its spectrum observed while heat was applied. No change could be detected in its passage through the critical point, and a careful comparison of the spectra of the same solution at 15° C. and at 300° C. showed no difference beyond the fainter and more nebulous character of the bands caused by expansion. The position of the bands was not changed. Chloride of cobalt, however, was not well fitted for our purpose, the measurement of the bands being rather uncertain on account of the nebulous nature of their edges. At the suggestion of Professor Stokes, we turned our attention to the product of the decomposition of chlorophyll by acids. The beautiful spectrum of a solution of this body in alcohol was quite permanent, even at a temperature of 350° C., although when heated in air it decomposes below 200° C. A direct comparison of the hot and cold solutions failed to show any change in the position of the lines.

A good opportunity of testing the question further seemed to be offered by the blue solution of sodium in liquefied ammonia, described by Gore.* To prepare this, we first tried compression of the gas by the pressure apparatus, but the quantity of liquid got by this method was too small for experiment. We next tried a combination of Gore's method with the pressure apparatus. A tube, shaped as in the drawing, had the wide part charged with chloride of calcium saturated with ammonia, and the end sealed. By applying slight heat, the air was expelled by the liberated ammonia. A small piece of sodium was then placed in a pocket near the end of the experimental tube, and, after sealing, the tube was placed in the pressure apparatus and surrounded with mercury. The iron tube was now heated by flaming with a Bunsen, and the experimental tube cooled in a freezing mixture. Distillation of ammonia commenced, and when a sufficient quantity of liquid was obtained the pressure screw was advanced, until, coming in contact with the wide tube, it broke the narrow neck joining it with the experimental tube, and allowed the mercury to rush in and enclose the liquefied ammonia. Further pressure brought the ammonia in contact with the sodium and solution commenced, but an unforeseen difficulty caused the experiment to fail, for the sodium solution coming in contact with the mercury was rapidly decolorised by it. It was, therefore, necessary to dispense with the use of mercury. Several methods were proposed, and we finally arrived at the following simple and effective one. A piece of tubing, about $\frac{1}{4}$ inch diameter, was joined to a piece of marine barometer tubing, and just beside the join the latter was drawn out to an exceedingly fine bore. The wide tube was now charged with ammonia-saturated chloride of

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* "Proc. Roy. Soc.," vol. xxi.

calcium and the end sealed. A small piece of solid paraffin was then dropped into the capillary tube so that it lay next the drawn out portion. When all air was expelled by heating slightly, a few fragments of sodium were placed in the tube and the whole sealed. The arrangement is shown in the Plate, fig. 3. The end containing the sodium being placed in a freezing mixture, the wide tube was carefully heated until sufficient ammonia was distilled over. When reabsorption commenced the fragment of paraffin was melted by the application of a very small flame, and capillary attraction caused it to run into the fine bore, where it solidified and prevented any passage of ammonia. The tube now containing a solution of sodium in ammonia, was next cut off near the join and the end sealed in the blowpipe flame, the paraffin plug preventing outward pressure. On heating the tube thus prepared in the air bath, the blue colour disappeared before the critical point was reached, and the loss of colour was accompanied by the deposit of a white powder. It was at first thought that this was due to the presence of water in the ammonia, but many experiments, in which every care was taken to have the ammonia perfectly dry, failed to give any other result. In one case only, when the temperature had been raised very rapidly, did the blue colour remain above the critical point, and this only for a few seconds. This chemical action most probably consists in the formation of sodamide, and this view is supported by the fact that one of the tubes opened under mercury gave a quantity of gas not dissolved by water.

As the solution of an alkali metal seemed best adapted for spectroscopic examination, we made an attempt to dissolve sodium in compressed hydrogen. For this purpose a tube with a very large expansion was made, and filled with carefully dried hydrogen. While the stream of gas was still passing, a small piece of sodium was placed in a pocket at the end of the tube, and the aperture closed before the blowpipe. In this experiment we used no manometer, our intention being to increase the pressure slowly and to make it as great as possible. After about half an hour, at a pressure so far as we could judge of about 300 atmospheres, the mercury, which had never come in contact with the solid sodium, was observed to become thick and pasty. At this point the tube burst (no further pressure had been put on) close to the iron cap, and on examining the broken piece small semi-solid globules of mercury were observed adhering to the glass, marking the highest point to which the mercury had reached. These were taken out and placed in water, when small bubbles of gas were observed rising from them. In another experiment a small rod of sodium, quite filling the capillary bore, was observed to be gradually corroded, until only a shell adhering to the glass remained. The very frequent bursting of the tubes, made more frail by the reduction of lead oxide, due to the action of hydrogen on the hot glass, makes the demonstration of the solubility of sodium in hydrogen exceedingly difficult, but the phenomena described, we are convinced, can only be explained on this hypothesis. In several experiments we have seen little pieces of sodium slowly disappear, leaving only the thin shell of oxide formed round them while they were being placed in the tube. There seemed in many cases to be a sudden chemical action, for the tube burst, not while pressure was being applied, but often several minutes after the pressure screw had been advanced. A beam of light passed through the tube showed no absorption when examined by the spectroscope.

Having described our experiments, it remains only for us to consider the state of the solvent at a distance above the critical point. We cannot do better than quote Dr. Andrews' consideration of the subject. He says,* "What is the condition of carbonic acid when it passes at temperatures above 31° C. from the gaseous state down to the volume of the liquid without giving evidence at any part of the process of liquefaction having occurred? Does it continue in the gaseous state, or does it liquefy, or have we to deal with a new condition of matter? If the experiment were made at 100° C., or at a higher temperature, when all indications of a fall had disappeared, the probable answer that would be given to this question is, that the gas preserves its gaseous condition during the compression, and few would hesitate to declare this statement to be true, if the pressures, as in Natterer's experiments, were applied to such gases as hydrogen or nitrogen. On the other hand, when the experiment is made with carbonic acid at temperatures a little above 31° C., the great fall which occurs at one period of the process would lead to the conjecture that liquefaction had actually taken place, although optical tests carefully applied failed at any time to discover the presence of a liquid in contact with a gas. But against this view it may be urged with great force, that the fact of additional pressure being always required for a further diminution of volume, is opposed to the known laws which hold in the change of bodies from the gaseous to the liquid state. Besides, the higher the temperature at which the gas is compressed the less the fall becomes, and at last it disappears." Leaving out of consideration the solubility of sodium in hydrogen, we have had iodide of potassium in solution in alcohol at temperatures much too high for measurement by the mercurial thermometer, and which may safely be estimated at 150° C. above the critical point (=380° C.). We think therefore we are justified in saying that at this high temperature the solvent was truly gaseous. An examination of Dr. Andrews' curves for carbonic acid shows us that at a temperature only 16° C. above the

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* "Phil. Trans.," 1869, p. 575.

critical point, the sudden fall in volume corresponding to the boiling line has disappeared, and although the curve does not coincide with the air curve, it is regular and shows no sign of liquefaction having occurred. Our experiments were made at temperatures further removed from the critical point than this, and as no sudden change in solubility could be detected in passing from the liquid state to these high temperatures, we conclude that, in these experiments, we have further proof of the perfect continuity of the liquid and gaseous states, and also a complete proof of the solubility of solids in gases.

II. "On the Artificial Formation of the Diamond." By J. B. HANNAY, F.R.S.E., F.C.S. Communicated by Professor G. G. STOKES, D.C.L., &c., Sec. R.S. Received February 19, 1880.

(Preliminary Notice.)

While pursuing my researches into the solubility of solids in gases, I noticed that many bodies, such as silica, alumina, and oxide of zinc, which are insoluble in water at ordinary temperatures, dissolve to a very considerable extent when treated with water-gas at a very high pressure. It occurred to me that a solvent might be found for carbon; and as gaseous solution nearly always yields crystalline solid on withdrawing the solvent or lowering its solvent power, it seemed probable that the carbon might be deposited in the crystalline state. After a large number of experiments, it was found that ordinary carbon, such as charcoal, lampblack, or graphite, were not affected by the most probable solvents I could think of, chemical action taking the place of solution.

A curious reaction, however, was noticed, which seemed likely to yield carbon in the nascent state, and so allow of its being easily dissolved. When a gas containing carbon and hydrogen is heated under pressure in presence of certain metals its hydrogen is attracted by the metal, and its carbon left free. This, as Professor Stokes has suggested to me, may be explained by the discovery of Professors Liveing and Dewar, that hydrogen has at very high temperatures a very strong affinity for certain metals, notably magnesium, forming extremely stable compounds therewith.

When the carbon is set free from the hydrocarbon in presence of a stable compound containing nitrogen, the whole being near a red heat and under a very high pressure, the carbon is so acted upon by the nitrogen compound that it is obtained in the clear, transparent form of the diamond. The great difficulty lies in the construction of an inclosing vessel strong enough to withstand the enormous pressure and high temperature, tubes constructed on the gun-barrel principle (with a wrought iron coil), of only half an inch bore and four inches external diameter, being torn open in nine cases out of ten.

The carbon obtained in the successful experiments is as hard as natural diamond, scratching all other crystals, and it does not affect polarised light. I have obtained crystals with curved faces belonging to the octahedral form, and diamond is the only substance crystallising in this manner. The crystals burn easily on thin platinum-foil over a good blowpipe, and leave no residue, and after two days' immersion in hydrofluoric acid they show no sign of dissolving, even when boiled. On heating a splinter in the electric arc, it turned black—a very characteristic reaction of diamond.

Lastly, a little apparatus was constructed for effecting a combustion of the crystals and determining their composition. The ordinary organic analysis method was used, but the diamond crystals were laid on a thin piece of platinum-foil, and this was ignited by an electric current, and the combustion conducted in pure oxygen. The result obtained was, that the sample (14 mgrms.) contained 97·85 per cent. of carbon, a very close approximation, considering the small quantity at my disposal. The apparatus and all analyses will be fully described in a future paper.

Received February 25, 1880.

Extract from a letter from Mr. Hannay, dated 23rd February:—

"I forgot, in the preliminary notice, to mention that the specific gravity of the diamond I have obtained ranges as high as 3·5; this being determined by flotation, using a mixture of bromide and fluoride of arsenic."

Presents, February 5, 1880.

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March 4, 1880.

THE 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 Candidates for election into the Society were read, as follows:—

Alderson, Henry James, Col. R.A.
Allbutt, Thomas Clifford, M.A., M.D., F.L.S.
Attfield, Prof. John, Ph.D., F.C.S.
Ayrton, Prof. William Edward.
Blanford, Henry Francis, F.G.S.
Bristowe, John Syer, M.D., F.R.C.P.
Christie, William Henry Mahoney, M.A., Sec. R.A.S.
Creak, Ettrick William, Staff Commander R.N.
Creighton, Charles, M.D.
Dallas, William Sweetland, F.L.S.
Dallinger, Rev. William Henry.
Day, Francis.
Dobson, George Edward, M.A., M.B., F.L.S.
Douglass, James Nicholas, M.I.C.E.
Dyer, William Turner Thiselton, M.A., F.L.S.
Godman, Frederic Du Cane, F.L.S., F.G.S., M.E.S.
Godwin-Austen, Henry Haversham, Lieut.-Col.
Goodeve, Prof. Thomas Minchin, M.A.
Grantham, Richard Boxall, F.G.S., M.I.C.E.
Graves, The Right Rev. Charles, Bishop of Limerick.
Hannyngton, John Caulfield Major-General.
Heaton, John Deakin, M.D., F.R.C.P.
Hughes, Prof. David Edward.
Jeffery, Henry M., M.A.
Kempe, Alfred Bray, B.A.
Lee, John Edward, F.S.A., F.G.S.
Liversidge, Prof. Archibald, F.G.S., F.C.S., F.L.S.
M'Coy, Prof. Frederick, F.G.S.
McLeod, Prof. Herbert, F.I.C., F.C.S.
Miller, Francis Bowyer, F.C.S.
Moulton, J. Fletcher, M.A.
Niven, Prof. Charles, M.A., F.R.A.S.
Palgrave, Robert Henry Inglis, F.S.S.
Phillips, John Arthur.
Preece, William Henry, C.E.
Priestley, Prof. William Overend, M.D., F.R.C.P.
Rae, John, LL.D.
Ranyard, Arthur Cowper, F.R.A.S.
Rawlinson, Robert, C.B., M.I.C.E.
Reinold, Prof. Arnold William, M.A.
Rennie, George Banks, C.E.
Reynolds, Prof. J. Emerson, M.D.

Spiller, John, F.C.S.
Thudichum, John Louis William, M.D., F.R.C.P.
Tilden, William A., D.Sc.
Tribe, Alfred, F.C.S.
Walsingham, Thomas, Lord.
Ward, James Clifton, F.G.S.
Watson, Rev. Henry William, M.A.
Wilson, Sir Samuel.
Wright, Charles R. Alder, D.Sc.
Wright, Edward Perceval, M.A., M.D., F.L.S.

The following Papers were read:—

I. "Fired Gunpowder. Note on the Existence of Potassium Hyposulphite in the Solid Residue of Fired Gunpowder." By Captain NOBLE (late R.A.), F.R.S., &c., and F. A. ABEL, C.B., F.R.S., V.P.C.S. Received February 24, 1880.

In our second memoir on fired gunpowder we have discussed in detail that part of M. Berthelot's friendly criticism of our first memoir which relates to the potassium hyposulphite found by us, in variable proportions, in our analyses of the solid products obtained by the explosion of gunpowder in the manner described. While pointing out that we had taken every precaution in our power to guard against the production of hyposulphite, by atmospheric action upon the potassium sulphide, during the removal of the hard masses of solid products from the explosion vessel, and had effectually excluded air from them, when once they were removed until they were submitted to analysis, we admitted the impossibility of guarding against the accidental formation of some hyposulphite during the process of removal, especially in some instances in which the structure of the residue had certainly been favourable to atmospheric action, and in which a more or less considerable development of heat had afforded indications of the occurrence of oxidation.

We contended, however, that the method of analysis, and the precautions adopted by us in carrying it out, precluded the possibility of accidental formation of hyposulphite at this stage of our investigations. With respect to the precautions, we could, and still do, speak with perfect confidence; and we certainly have believed ourselves fully justified in being equally confident with respect to the process adopted by us for the determination of the proportions of sulphide and hyposulphite, inasmuch as we accepted and used in its integrity the method published in 1857 by Bunsen and Schischkoff in their classical memoir on the products of explosion of gunpowder, and adopted since that time by several other investigators who have made the explosion of gunpowder the subject of study, and whose results are referred to in our first memoir.

Imposing implicit confidence in the trustworthiness of this method of analysis, emanating as it did from one of the highest authorities in experimental research, we considered ourselves fully justified in maintaining that the very considerable variations in the amount of hyposulphite found in different analyses, carried out as nearly as possible under like conditions, and the high proportion of sulphide obtained in several of those analyses, afforded substantial proof that accidental oxidation during the collection and analysis of the residues was not sufficient to account for all but the very small quantities of hyposulphite, which, in M. Berthelot's view, could have pre-existed in the powder residues. Other facts established by the exhaustive series of experiments detailed in our first memoir, were referred to by us in our second memoir in support of the above conclusion (from which we have still no reason whatever to depart). At the same time we described a series of supplementary experiments which had been instituted by us, with a view to obtain, if possible, further decisive evidence as to the probable proportions of hyposulphite and sulphide actually existing in the residues furnished by the explosion of gunpowder in closed vessels.

In the first place, the residues obtained by the explosion of charges of R.L.G. and pebble powders were submitted to special treatment. Portions of each, consisting exclusively of large masses, were very speedily detached and removed from the explosion vessels, and sealed up in bottles freed from oxygen, having been exposed to the air only for a few seconds. Other portions of the same residues were very finely ground and exposed to the air for 48 hours. As was stated in our recent memoir, the portions of the residues treated in the last-named manner contained very large proportions of hyposulphite (although in one of them there still remained about 3 per cent. of sulphide) while those portions which had been for only a brief period exposed to air (and which presented but small surfaces) were found to contain from 5 to 8·5 per cent. of hyposulphite. As, throughout our entire series of previous experiments, no accidental circumstances had occurred which even distantly approached the special conditions favourable to the oxidation of the sulphide presented in these particular experiments, we considered ourselves fully justified in concluding that the non-discovery of any sulphides in the analyses of residues furnished by the fine grain powder in three out of the whole series of experiments, was not due to accident in the manipulations; and that in those instances, in our several series of experiments, in which large quantities of hyposulphite were found, the greater proportion of that substance must have existed before the removal of the residues from the explosion vessel.

Not suffering the question to rest there, however, we proceeded, in the second place, to adopt new precautions, in two special experiments, for guarding against the possible formation of hyposulphite in the removal of the residues from the explosion vessel, and their preparation for analysis.

Distilled water, carefully freed from air by long continued boiling, was syphoned into the vessel when it had cooled after the explosion, and thus no air was ever allowed to come into contact with the solid products. When the vessel was quite filled with water it was closed, and, after having been left at rest for a sufficient time to allow the residue to dissolve completely, the solution was rapidly transferred to bottles which had been freed from oxygen. These, when completely filled with the liquid, were hermetically sealed until the contents were submitted to analysis in accordance with the usual method, when they furnished respectively 4 and 6 per cent. of hyposulphite. These results corresponded closely to others obtained by the analysis of seven residues obtained in experiments with P., R.L.G., and L.G. powders, in which there were no peculiarities assignable as a possible reason why the proportions of hyposulphite should be so much lower, in these cases, than in other experiments carried out with the same powders under as nearly as possible the same conditions.

By the results obtained under the various conditions pointed out in the foregoing, we were forced to the conclusion that the discovery of a small or a larger proportion of hyposulphite by the analysis of the powder residue, obtained as described, is consequent upon some slight variation (apparently not within the operator's control) attending the explosion itself; but that hyposulphite does exist, though generally not to anything like the extent we were at first led to believe, as a normal and not unimportant product of the explosion of powder in a closed space.

Some time after the submission of our second memoir to the Royal Society, we received a communication from Professor Debus, which has led us to institute a further series of experiments bearing upon this question of the existence of hyposulphite, and the results we have arrived at have led us so greatly to modify our views on this point, that it is our duty to communicate them without loss of time to the Royal Society.

As introductory to these, it is necessary to repeat the account, given in our first memoir, of the method pursued by us for determining the proportions of potassium monosulphide and hyposulphite in a powder residue.

The solution of the residue, prepared by the several methods already described, was separated by filtration, as rapidly as possible, from the insoluble portion, the liquid being collected in a flask in which it was at once brought into contact with pure ignited copper oxide. The solution and oxide were agitated together, from time to time, in the closed flask, the two being allowed to remain together until the liquid was perfectly colourless. In a few instances the oxide was added in small quantities at a time, in others the sufficient excess was added at once, with no difference in the result obtained. The only points in which this method differed from that described by Bunsen and Schischkoff in their memoir, was in the employment of a flask well closed with an india-rubber bung for the stoppered cylinder which was employed by them; and in occasionally curtailing somewhat the prescribed period (two days) for which the liquid and the copper oxide were allowed to remain together, the operation being considered complete when the solution had become colourless. Bunsen and Schischkoff prescribed that the liquid when separated by filtration from the mixed copper oxide and sulphide obtained in the foregoing treatment, is to be divided into seven equal volumes, in one of which the amount of hyposulphite may be most simply estimated by acidifying it with acetic acid, and then titrating with a standard iodine solution. This course was adopted by us, and it will therefore be seen that we departed in no essential point whatever from the method of Bunsen and Schischkoff, which we had considered ourselves fully warranted in adopting, without questioning its trustworthiness.

We were informed, however, last July by Dr. Debus, that in submitting potassium polysulphides to treatment with copper oxide, he had found much hyposulphite to be produced, even when air was perfectly excluded, it having been in the first instance ascertained that the several polysulphides experimented with did not contain any trace of hyposulphite. We proceeded at once to confirm the correctness of his observations, by submitting potassium polysulphides to treatment with copper oxide, proceeding exactly according to the method prescribed by Bunsen and Schischkoff for the treatment of powder residues. In one experiment we obtained as much as 87·1 per cent. of potassium hyposulphite (calculated upon 100 parts of potassium monosulphide). Even in an experiment with pure potassium monosulphide, we obtained 11·6 per cent. of hyposulphite upon its treatment for the usual period with copper oxide.

We next proceeded to convince ourselves that by substituting zinc chloride solution for copper oxide, the sulphur existing in solutions of potassium mono- and poly-sulphides might be abstracted, according to the usual method of operation, without producing more than the very small quantities of hyposulphite ascribable to the access of a little air to the sulphides before or during the method of treatment.

Having confirmed the validity of Dr. Debus' objection to Bunsen and Schischkoff's method, and established the trustworthiness of a modification of that method (zinc chloride being substituted for copper oxide), we proceeded to submit to precisely similar treatment with these two reagents portions of solutions obtained by dissolving, with total exclusion of air (in the manner described in our last memoir and the present note), the residue furnished by special experiments with P., R.L.G., and F.G. powders, exploded under the usual conditions obtaining in our researches, and in quantities ranging from 4,200 to 35,000 grs. (272·2 grms. and 2,268 grms.). The following is a tabulated statement of the results obtained by the two modes of treatment, and of the differences between the proportions of hyposulphite obtained by treatment of portions of one and the same residue with the two different reagents under conditions as nearly alike as possible :—

Table I.

No. of experiment.	Description of powder.	Quantity used.		Density of charge.	Amount of hyposulphite furnished by 100 parts of powder with employment of—		
		Grains.	Grams.		Zinc chloride.	Copper oxide.	Difference.
245	P.	3,396	220·05	0·3	·12	1·93	1·81
241	P.	5,660	366·76	0·5	·07	2·46	2·39
246	R.L.G.	4,200	272·16	0·4	·05	1·43	1·38
244	R.L.G.	5,250	340·19	0·5	·06	1·58	1·52
243	F.G.	4,523	293·41	0·4	·07	1·56	1·49
242	F.G.	6,300	408·23	0·6	·27	2·26	1·99
247	P.	35,000 (5 lb.)	2,267·97	0·23	·78	2·82	2·04

For purposes of comparison, we subjoin a statement of the lowest proportions of hyposulphite furnished by 100 parts of the three powders used in our general series, and also the proportions, similarly expressed, which were obtained in the experiments with sporting and mining powder, the residues of which were dissolved with the same special precautions adopted in the case of the experiments given in Table I.

Table II.

Number of experiment.	Nature of powder.	Amount of hyposulphite in 100 parts of gunpowder used.	Remarks.
7	Pebble.	2·06	Lowest proportions furnished by the respective powders.
44	R.L.G.	1·75	
17	F.G.	3·04	
196	Curtis & Harvey No. 6.	2·28	Special precautions taken in collecting the residue.
194	Mining powder.	2·77	

In reference to the foregoing numerical statements, we have to offer the following observation:—

1. Substituting zinc chloride for copper oxide as the precipitant of the sulphur which existed in the form of sulphide in solutions of powder residues to which air had not had access at all, until the time of its treatment with the zinc chloride, the amount of hyposulphite existing in solution after such treatment was found to range from 0·05 to 0·78 in 100 parts of gunpowder, while the treatment of portions of the same solutions with copper oxide, in the precise manner adopted in our series of experiments, yielded proportions ranging from 1·38 to 2·39 per 100 of powder used. Comparing the results furnished by the two modes of treatment, it will be seen that in the case of the parallel experiments (245), which exhibited the least considerable difference in the amount of hyposulphite found, that existing after the copper oxide treatment was about sixteen times greater, while in the case of the highest difference (Experiment 241) it was about thirty-four times greater than that found after the treatment with the zinc chloride.

2. It would appear from these results that, in four or five out of seven experiments, no hyposulphite, or at any rate only minute quantities, existed in the residues previous to their solution, and although it would seem to have existed in very appreciable amount in two out of seven residues, the highest proportion found after the zinc chloride treatment was less than one-half the lowest proportion found in our complete series of analyses in which the copper oxide treatment was adopted.

3. A comparison of the results among each other leads, therefore, to the conclusion that potassium hyposulphite cannot be regarded as a normal constituent of powder residue (obtained in experiments such as those carried out by us), and that M. Berthelot is correct in regarding this salt as an accidental product, which, if existing occasionally in appreciable amount in the solid matter previous to its removal from the explosion vessel, is formed under exceptional conditions, and then only in comparatively small proportions.

While submitting this as the conclusion to be drawn from our most recent experiments, we are of opinion that the following points deserve consideration in connexion with the question whether hyposulphite may not, after all, occasionally exist, as the result of a secondary reaction, in comparatively large proportion in the explosion vessel before the residue is removed.

It will be observed that although the copper oxide treatment, when applied to the sulphide in the pure condition (*i.e.*, undiluted with the other potassium compounds found in powder residue), gave rise to the production of very large proportions of hyposulphite, when polysulphides were used, the highest proportion of that substance found, after the treatment of the particular residues used in the experiments given in Table I, only amounts to 2·82 per cent. upon the gunpowder (pebble powder) employed, which corresponds to about 14·5 per cent. of the average proportion of monosulphide existing in the residue furnished by that powder. In observing this, it must be borne in mind that the sulphide existing in powder residue is always present, in part, and sometimes to a considerable extent, in the form of polysulphide, also, that the experiments with the sulphides were conducted precisely according to the method pursued in the treatment of the powder residues. It would appear, therefore, as though the mixture of the sulphide with a very large proportion of other salts in solution rendered it less prone to oxidation by the copper oxide than when the undiluted sulphide is submitted to its action.

In comparing with the results furnished by the zinc chloride those obtained by the copper oxide treatment, in the special experiments given above, it is observed that, omitting one exceptional result (No. 241) for which we do not attempt to account, the highest proportions of hyposulphite are furnished by those residues which also gave the highest with the zinc chloride, the differences between the results furnished by the two treatments being likewise the highest in these three cases; so also the lowest proportions furnished by the copper oxide treatment, correspond to the lowest obtained with the zinc oxide, and the differences between the results furnished by the two methods are in the same manner the lowest in these. It would almost appear, therefore, as though the existence of a very appreciable proportion of hyposulphite in the solution of the residue had some effect in promoting the production of hyposulphite when the residue is submitted to treatment with copper oxide.

In a recalculation of the results of our analyses of the powder residues upon the assumption that the whole of the hyposulphite obtained existed originally as monosulphide, it is found that, in several instances in which the proportion of hyposulphite was high, the analytical results are much less in accordance with each other than when it is assumed that the hyposulphite found, or at any rate a very large proportion of it, existed as such in the residue before removal from the explosion vessel. Thus, taking the F.G. series, in which the *mean quantity* of hyposulphite actually found is about double of that obtained either from the pebble or R.L.G. powders, selecting from this series the three experiments which gave the highest proportions of hyposulphite, and calculating in the manner described in our first memoir the total quantities both of solid and gaseous products, first from the basis of the analysis of the solid products, secondly from the basis of the analysis of the gaseous products, and further on the assumption that the hyposulphite found existed as hyposulphite either as a primary or secondary product prior to removal from the explosion vessel, we have as follows:—

Experiment No. 40, F.G. powder. Density ·6, hyposulphite found, 18·24 per cent.

	Calculated solid products. grms.	Calculated gaseous products. grms.
From analysis of solid products	170·268	125·220*
From analysis of gaseous products	172·509*	122·979

Experiment No. 42. F.G. powder. Density ·7, hyposulphite found, 18·36 per cent.

	Calculated solid products. grms.	Calculated gaseous products. grms.
From analysis of solid products	200·191	144·547*
From analysis of gaseous products	200·220*	144·520

Experiment No. 47. F.G. powder. Density ·8, hyposulphite found, 19·95 per cent.

	Calculated solid products. grms.	Calculated gaseous products. grms.
From analysis of solid products	231·652	162·335*
From analysis of gaseous products	229·392*	164.595

Calculating now in the same manner the quantities of solid and gaseous products on the assumption that the hyposulphite found was, prior to removal from the explosion vessel, in the form of mono- or poly-sulphide, we have from the same experiments:—

Experiment No. 40.

	Calculated solid products. grms.	Calculated gaseous products. grms.
From analysis of solid products	157·273	133·842
From analysis of gaseous products	168·136	122·979

Experiment No. 42.

From analysis of solid products	185·914	155·722
From analysis of gaseous products	197·118	144·520

Experiment No. 47.

From analysis of solid products	211·462	176·694
From analysis of gaseous products	223·561	164·595

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* Water included.

Lastly, we still think that the following facts, given in our second memoir, must not be overlooked in considering the question of possible occasional existence of considerable proportions of hyposulphite, viz., that "the production of high proportions of hyposulphite was but little affected by any variations in the circumstances attending the several explosives (*i.e.*, whether the space in which the powder was exploded were great or small), excepting that the amount was high in all three cases when the powder was exploded in the largest space; on the other hand, a great reduction in the size of grain of the gunpowder used appeared to have a great influence upon the production of hyposulphite, as when passing from a very large grain powder (P. or R.L.G.) to a fine grain powder (F.G.). Thus, the production of hyposulphite exceeded 20 per cent. (on the solid residue) in only 3 out of 9 experiments with P. powder, in 3 out of 10, with R.L.G., and in 7 out of 9 with F.G.; while it was below 10 per cent. in 4 out of 9 experiments with P. powder, in 5 out of 10 with R.G.L., and in only 1 out of 9 with F.G. powder." The experiments made with these several powders followed in no particular order, and no circumstance existed in connexion with them to which these great differences in the results obtained could be ascribed.

We append a recalculation of the mean results of our three series of analysis, adding the values of the hyposulphite found, as monosulphide, to the amount of sulphide actually found, and we hope to be allowed to add to our second memoir a similar recalculation of the whole of our analyses.

This recalculation develops (as we pointed out in our second memoir must necessarily be the case) a more or less considerable deficiency of oxygen in the total products of explosion; there is, however, in every instance, also a deficiency of hydrogen, and it may, therefore, be reasonably concluded that the deficiencies in the total quantities of the oxygen and the hydrogen in the powder used, which are unaccounted for in the products found, on the assumption that variable proportions of the total hyposulphite found actually existed in the residues as *sulphide*, are properly accounted for by assuming that the missing quantities of these elements actually existed among the products as water, the amount of which it was obviously impossible to determine.

In conclusion, we have to state that we considered it right, in consequence of the error discovered in the method adopted for the examination for hyposulphite, to repeat the experiments described in our first memoir as having been made by us, with the view of ascertaining whether hyposulphite could exist at temperatures approaching those to which the solid products of explosion were actually subjected in the explosion vessels in our experiments.

To test this point we submitted, for between ten minutes and a

Table III.—Showing the mean composition by weight of the products of combustion of 1 grm. of gunpowder of the undermentioned natures when the whole of the hyposulphite found is supposed originally to have existed in the form of sulphide.

Nature of powder.	Proportions by weight of gaseous products.							Proportions by weight of solid products.									Proportion by weight of gaseous products.	Proportion by weight of solid products.	Water.
	Carbonic anhydride.	Carbonic oxide.	Nitrogen.	Sulphydric acid.	Marsh gas.	Hydrogen.	Oxygen.	Potassium carbonate.	Potassium sulphate.	Potassium monosulphide.	Potassium sulphocyanide.	Potassium nitrate.	Potassium oxide.	Ammonium sesquicarbonate.	Sulphur.	Charcoal.			
Pebble	·2685	·0480	·1122	·0111	·0006	·0006	..	·3258	·0710	·1042	·0014	·0013	..	·0005	·0445	·0008	·4409	·5496	·0095
R.L.G.	·2630	·0422	·1117	·0109	·0008	·0009	·0002	·3415	·0844	·0787	·0013	·0015	..	·0004	·0495	·0004	·4298	·5593	·0111
F.G.	·2689	·0355	·1123	·0101	·0004	·0007	·0003	·2861	·1252	·0999	·0007	·0009	·0056	·0003	·0381	..	·4282	·5569	·0148

quarter of an hour, to the highest heat (about 1,700° C.) of a Siemens regenerative furnace, two platinum crucibles, one filled with powder residue, the other with potassium hyposulphite. At the conclusion of the exposure, and while the crucibles were still red hot, they were plunged into water, deprived of air by long-continued boiling, and at once sealed. The powder residue was found still to contain 1·27 per cent. of hyposulphite, while the crucible with the pure salt consisted of a mixture of sulphate and sulphide, but with an amount of 2·1 per cent. of hyposulphite.

It is probable that, if the exposure had been still longer continued, the hyposulphite would have altogether disappeared, and the experiment can only be taken as proving that the hyposulphite, especially if mixed with other salts, is neither quickly nor readily decomposed, even at very high temperatures.

II. "On the Dynamo-Electric Current and on certain Means to Improve its Steadiness." By C. WILLIAM SIEMENS, D.C.L., LL.D., F.R.S. Received March 1, 1880.

(Abstract.)

The author, after alluding to the early conception by Dr. Werner Siemens of the dynamo-electric or accumulative principle of generating currents, makes reference to the two papers on the subject presented, the one by Sir Charles Wheatstone and the other by himself, to the Royal Society in February, 1867. The machine then designed by him, and shown in operation on that occasion, is again brought forward with a view of indicating the progress that has since taken place in the construction of dynamo-electrical machines, particularly those by Gramme and Siemens-von Alteneck. The paper next points out certain drawbacks to the use of these machines, both of them being subject to the disadvantage that an increase of external resistance causes a falling off of the current; and that, on the other hand, the short circuiting of the outer resistance, through contact between the carbon electrodes of an electric lamp, very much increases the electric excitement of the machine, and the power necessary to maintain its motion, giving rise to rapid heating and destructive sparks in the machine itself.

An observation in Sir Charles Wheatstone's paper is referred to, pointing to the fact that a powerful current is set up in the shunt circuit of a dynamo-electric machine, which circumstance has since been taken advantage of to some extent by Mr. Ladd and Mr. Brush, in constructing current generators.

The principal object of the paper is to establish the conditions under which dynamo-electric machines worked on the shunt principle can be made to give maximum results. A series of tables and diagrams are given, the results of experiments conducted by Mr. Lauckert, electrician, employed at the author's works, which lead up to the conclusion that, in constructing such machines on the shunt principle, the resistance on the rotating helix has to be considerably reduced by increasing the thickness of the wire employed, and that on the magnets has to be increased more than tenfold, not by the employment of thin wire, but by augmenting the length and weight of coil wire employed.

The results of this mode of distributing the resistances is summarised as follows:—

1. That the electro-motive force, instead of diminishing with increased resistance, increases at first rapidly, and then more slowly towards an asymptote.

2. That the current in the outer circuit is actually greater for a unit and a half resistance than for one unit.

3. With an external resistance of one unit, which is about equivalent to an electric arc, when thirty or forty webers are passing through it, 2·44 horse-power is expended, of which 1·29 horse-power is usefully employed, proving an efficiency of 53 per cent., as compared with 45 per cent. in the case of the ordinary dynamo-machine.

4. That the maximum energy which can be demanded from the engine is 2·6 horse-power, so that but a small margin of power is needed to suffice for the greatest possible requirement.

5. That the maximum energy which can be injuriously transferred into heat in the machine itself is 1·3 horse-power, so that there is no fear here of destroying the insulation of the helix by excessive heating.

6. That the maximum current is approximately that which would be habitually used, and which the commutator and collecting brushes are quite capable of transmitting.

Hence the author concludes that the new machine will give a steadier light than the old one with greater average economy of power, that it will be less liable to derangement, and may be driven without variation of speed by a smaller engine; also that the new machine is free from all objection when used for the purpose of electro-deposition.

This construction of machine enables the author to effect an important simplification of the regulator to work electric lamps, enabling him to dispense with all wheel and clockwork in the arrangement. The two carbons being pushed onward by gravity or spring power are checked laterally by a pointed metallic abutment situated at such a distance from the arc itself, that the heat is only just sufficient to cause the gradual wasting away of the carbon in contact with atmospheric air. The carbon holders are connected to the iron core of a solenoid coil, of a resistance equal to about 50 times that of the arc, the ends of which coil are connected to the two electrodes respectively.

The weight of the core (which may be varied), determines the force of current that has to pass through the regulating coil in order to keep the weight in suspension, and this in its turn is dependent upon the resistance of the arc. The result is that the length of the arc is regulated automatically, so as to maintain a uniform resistance signifying a uniform development of light.

III. "On the Influence of Electric Light upon Vegetation, and on certain Physical Principles involved." By C. WILLIAM SIEMENS, D.C.L., LL.D., F.R.S. Received March 1, 1880.

Although according to Ste. Claire Deville the dissociation of CO_2 and H_2O at atmospheric pressure commences at a temperature not exceeding 1,200° C., the reverse action, namely, combustion, continues to be sufficiently active to increase the heat of a flame until a temperature of probably 2,200° C. is reached, of which the Deville oxygen blast and the regenerative gas-furnace furnish examples.

In the working out of a process, by means of which steel and fused iron are produced in large masses on the open hearth of the regenerative gas-furnace, I have had frequent opportunities of observing the utmost limit of temperature practically attainable by means of the combustion of carbonaceous substances. The heat of that furnace is not dependent upon a blast or upon chimney draught, and the pressure within the furnace balances the external atmospheric pressure so completely that the large working doors may be opened occasionally for inspecting the metal. On these occasions it may frequently be observed that serrated clouds of highly heated combustible gases pass through the furnace chamber (as may be seen through obscured or coloured glasses) without suffering apparently any diminution through contact with an excess of heated oxygen, showing that the limit of furnace heat, or the point of complete dissociation, has been nearly reached.

When thus brought face to face with the utmost limit of heat attainable by combustion, it is surprising how slight is the inconvenience (in the absence of heated matter flying about) experienced in approaching the open furnace, and how much higher must be the temperature of the sun or of the electric arc when the effects of distant radiation suffice to break up compounds such as nitrate of silver in a few seconds. Some years ago I undertook certain experimental inquiries having for their object to study the effect of radiant energy upon the ordinary products of combustion, CO_2 and H_2O, presenting them in an extremely rarefied conditon to solar and electric radiation, which inquiries were not however carried far enough to furnish absolute results. They served me, however, for a stepping stone to the subject of my present communication.

The vast development of vegetation proves that dissociation is accomplished freely within the leaf-cells of plants, in which both water and carbonic acid are broken up in order that chlorophyl, starch, and cellulose may be formed. It is well known that this reaction depends upon solar radiation; but the question may fairly be asked whether it is confined to that agency, or whether other sources of light and heat, which in common with the sun exceed the temperature of dissociation, may not be called into requisition, in order to continue the action of growth, when that great luminary has set or is hidden behind clouds?

About two years ago I mentioned to Sir Joseph Hooker, then President of the Royal Society, that I thought the electric arc might be found sufficiently powerful to promote vegetation, and that I should be willing to undertake some experiments on the subject, if he could give me any hope of confirmative results. Sir Joseph Hooker gave me sufficient encouragement to induce me to follow up the subject, and I have since that time gradually matured a plan for conducting the experiment. Operations were commenced only at the beginning of the year, and although the results are necessarily incomplete, they are nevertheless sufficiently positive and remarkable to make them perhaps acceptable to the Royal Society as a preliminary communication on the subject. I was induced to look for interesting results in these experiments on account of the great abundance of blue and actinic rays in the electric arc, upon which its value in photography depends. In experimenting with powerful electric lamps for illuminating purposes, I have been struck moreover by the action produced upon the skin, which is blistered, without the sensation of excessive heat at the time, an effect analogous to that produced by solar rays in a clear atmosphere.

Effect of Radiant Energy on Plants.

The apparatus which has been put up at Sherwood consists—1. Of a vertical Siemens dynamo-machine, weighing 50 kilos., with a wire resistance of 0·717 unit on the electro-magnets. This machine makes 1,000 revolutions a minute, it takes 2 horse-power to drive it, and develops a current of 25 to 27 webers of an intensity of 70 volts. 2. A regulator or lamp, constructed for continuous currents, with two carbon electrodes of 12 millims. and 10 millims. diameter respectively. The light produced is equal to 1,400 candles measured photometrically. 3. A motor, which at present is a 3 horse-power Otto gas-engine, but which it is intended to supersede by a turbine to be worked by a natural supply of water, at a distance of about half a mile from the house.

Experiments on Effect of Electric Light on Plants.

My object in making these experiments was to ascertain whether electric light exercised any decided effect upon the growth of plants. For this purpose I placed the regulator in a lamp with a metallic reflector, in the open air, about 2 metres above the glass of a sunk melon house. A considerable number of pots were provided, sown and planted with quick-growing seeds and plants, such as mustard, carrots, swedes, beans, cucumbers, and melons. The plants could then be brought at suitable intervals under the influence of daylight and electric light, without moving them, both falling upon them approximately at the same angle. The pots were divided into four groups.

1. One pot of each group was kept entirely in the dark.
2. One was exposed to the influence of the electric light only.
3. One was exposed to the influence of daylight only.
4. One was exposed successively to both day and electric light.

The electric light was supplied for six hours, from 5 to 11 each evening, all the plants being left in darkness during the remainder of the night.

In all cases the differences of effect were unmistakable. The plants kept in the dark were pale yellow, thin in the stalk, and soon died. Those exposed to electric light only showed a light-green leaf, and had sufficient vigour to survive. Those exposed to daylight only were of a darker green and greater vigour. Those exposed to both sources of light showed a decided superiority in vigour over all the others, and the green of the leaf was of a dark rich hue.

It must be remembered that, in this contest of electric against solar light, the time of exposure was in favour of the latter in the proportion of nearly 2 to 1, but all allowance made, daylight appeared to be about twice as effective as electric light. It was evident, however, that the electric light was not well placed for giving out its power advantageously. The nights being cold, and the plants under experiment for the most part of a character to require a hot moist atmosphere, the glass was covered very thickly with moisture, which greatly obstructed the action of the light, besides which, the electric light had to pass through the glass of its own lamp.* Notwithstanding these drawbacks, electric light was clearly sufficiently powerful to form chlorophyl and its derivatives in the plants. It was interesting to observe that the mustard-seed stem, when placed obliquely, turned completely towards the light in the course of two or three hours, and that cucumber and melon plants were affected in the same way, though at a slower rate. The cucumber and melon plants which have been

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* Professor Stokes has shown, in 1853, that the electric arc is particularly rich in highly refrangible invisible rays, but that on passing the rays through glass, all those of high refrangibility are found to have been absorbed.

exposed to both day and electric light have made great progress, and my gardener, Mr. D. Buchanan, says that he could not have brought on the latter, without the aid of electric light, during the early winter. Some of these commenced to blossom on the 14th of February.

These preliminary trials go to prove that electric light can be utilized in aid of solar light by placing it over greenhouses, but the loss of effect in such cases must be considerable. I, therefore, directed my observations, in the next place, to the effect of electric light upon plants, when both were placed in the same apartment. A section of the melon house, already referred to (7′ 3″ × 3′ 3″, 2·21 m. × 0·19 m.), was completely darkened by being covered in with thick matting, and was whitewashed inside. The electric light was placed over the entrance door, and shelves were put down, in a horse-shoe form, to receive the pots containing the plants to be exposed to the action of electric light, the plants being placed at distances from the lamp varying from 0·5 metre to 2 metres. Upon the first occasion of trying the naked electric light in this manner, some of the plants, and especially some melon and cucumber plants, from 20 centims. to 40 centims. in height, which were within a metre distance from the lamp, commenced to suffer; those leaves which were directly opposite the light turning up at the edges and presenting a scorched appearance. On subsequent nights, therefore, the stands were so arranged that the distance of the plants from the light varied from 1·5 metres to 2·3 metres. The plants under experiment were divided into three groups; one group was exposed to daylight alone, a second similar group was exposed to electric light during eleven hours of the night, and were kept in the dark chamber during the day time, and the third similar group was exposed to eleven hours' day and eleven hours' electric light. These experiments were continued during four days and nights consecutively, and the results observed are of a very striking and decisive character, as regards the behaviour of such quick-growing plants as mustard, carrots, &c. The experiment was unsatisfactory in this one respect, that during the third night the gas-engine working the dynamo-machine came to a standstill, owing to a stoppage in one of the gas channels, and thus more than half the electric-light influence that night was lost to the plants. But, notwithstanding this drawback, the two groups of plants showed unmistakably the beneficial influence of electric light. The plants that had been exposed to daylight alone (comprising a fair proportion of sunlight) presented their usually healthy green appearance; those exposed to electric light alone were, in most instances, of a somewhat lighter, but, in one instance, of a somewhat darker hue than those exposed to daylight; and all the plants that had the double benefit of day and electric light far surpassed the others in darkness of green and vigorous appearance generally. A fear had been expressed that the melon and cucumber plants, which had been scorched by excess of electric light on the first evening, would droop or die if further exposed to that agency, but I insisted upon their being placed within the influence of the electric light, at a distance from it exceeding 2 metres, and all of them have shown signs of recovery, throwing out fresh leaves and pearls of moisture at their edges. A pot of tulip buds was placed in this electric stove, and the flowers were observed to open completely after two hours' exposure.

One object I had in view in this experiment, was to observe whether the carbonic acid and nitrogenous compounds produced within the electric arc exercised any deleterious action upon the plants. All continuous access of air into the stove was closed, and in order to prevent excessive accumulation of heat, the stove pipes were thickly covered with matting and wet leaves. But although the access of stove heat was thus virtually stopped, the temperature of the house was maintained throughout the night at 72° F., proving that the electric lamp furnished not only a supply of effective light, but of stove heat also. No hurtful effect was moreover observed on the plants from the want of ventilation, and it would appear probable that the supply of pure carbonic acid resulting from the complete combustion of the carbon electrodes at high temperature, and under the influence of an excess of oxygen, sufficed to sustain their vital functions. If the nitrogenous compounds which Professor Dewar has shown to be developed in the electric arc were produced in large quantities, injurious effects upon the plants must undoubtedly ensue, but it can be shown that in a well-conditioned electric lamp, with a free circulation of air round the carbon electrodes, the amount of these products is exceedingly small, and of a different nature than is produced in a confined space. They could not indeed be perceived by their smell in the stove, when all ventilators were closed, and no injurious effects from them have hitherto been observed in the plants.

These experiments are not only instructive in proving the sufficiency of electric light alone to promote vegetation, but they also go to prove the important fact that diurnal repose is not necessary for the life of plants, although the duration of the experiments is too limited perhaps to furnish that proof in an absolute manner. It may, however, be argued from analogy, that such repose is not necessary, seeing that crops grow and ripen in a wonderfully short space of time in the northern regions of Sweden and Norway, and Finland, where the summer does not exceed two months, during which period the sun scarcely sets.

The next step in the course of these experiments was to remove the electric lamp into a palm house, constructed of framed glass, which was 28 ft. 3 in. long, 14 ft. 6 in. wide, and averaging 14 ft. 6 in. (8·62 m. × 14·42 m. × 4·42) in height. In the centre of this house a banana palm, and a few other small palm trees are planted, the sides of the house all round being occupied with a considerable variety of flowering plants. The electric light was fixed as high as practicable at the south corner of the house, in order that its rays might fall upon the plants from a direction and at an angle coincident with those of the sun during the middle of the day. A metal reflector was placed behind the lamp in order to utilize the electric rays as much as possible. Along the eastern side of the house are some young vines, having their roots in a bed outside. Three pots of nectarine plants, just beginning to bud, were placed on the floor at various distances from the electric light, and also some rose plants, geraniums, orchids, &c. The temperature of the house was maintained at 65 F., and the electric lamp was kept alight from 5 p.m. to 6 a.m., for one week, from February 18th to February 24th, excepting Sunday night. The time was hardly sufficient to produce very striking effects, but all the plants continued to present a healthy appearance. Of three Alicante vines, the one nearest the electric light made most progress, and the same could be said of the nectarines and roses. It was observed that other plants, such as geraniums, continued to exhibit a vigorous appearance, notwithstanding the heat of the place. The electric light appeared to impart the vitality necessary to prevent a collapse of the organism through excessive temperature. This experiment is of importance in showing that the electric light if put into conservatories or greenhouses does not injure the plants, but rather improves their appearance and growth. The leaves assume a darker and more vigorous appearance, and it seems that the colouring of the flowers becomes more vivid, but a further period of time is necessary to establish this observation absolutely. The effects producible by electric light in conservatories is very striking, owing to the clear definition of form and colour produced, far exceeding that of ordinary daylight.

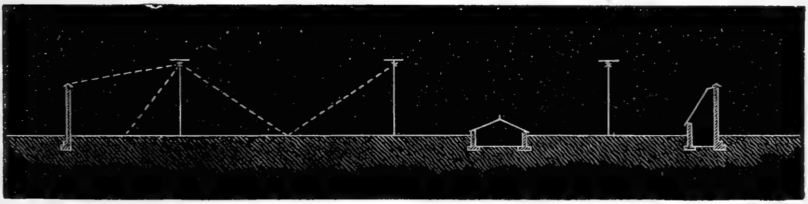
No further results of any particular interest could be expected from a continuance of this experiment, and I decided to try the effect of electric light as a means of promoting growth in the open air and under glass at the same time. The regulator was put back into its first position, 2 metres above the ground, with a sunken melon house on one side, and a sunken house containing roses, lilies, strawberries, and a variety of other plants on the other. The space of ground between these, about 1 metre broad, and 7 metres long, was covered with boxes sown with early vegetables, including mustard, peas, beans, and potatoes, and in order to prevent cold winds from injuring the plants, low protecting walls were put up across the openings of the passage between the two houses. The effect of electric light could thus be observed at the same time, upon the melon and cucumber plants in the one house, upon the roses, strawberries, &c., at an inferior temperature in the other, and upon the early vegetables unprovided with covering.

Some weeks must elapse before any absolute results can be given, but growth is evidently promoted under all these various circumstances. In order to test this clearly, a portion of the plants both under glass and in the open air are shaded from the electric light without removing them from their position of equal temperature, and exposed to solar light during daytime. The effect upon the flowering plants is very striking, electric light being apparently more efficacious to bring them on than daylight. Although the amount of heat given off from the electric arc is not great compared with a gas flame (giving off its products of combustion), yet the rays of intense heat of the arc counteract that loss of heat by radiation from the leaves into space, which during a clear night causes hoar frost. For this reason I expect that electric light may be usefully employed in front of fruit walls, in orchards and in kitchen gardens, to save the fruit-bud at the time of setting; and in this application electric light will probably be found a useful agent not only to promote rapid growth, but to insure a better yield of fruit. Experience alone can determine absolutely the effect of electric light upon the ripening of delicate fruit, but considering its evident power to form chlorophyl, there seems no reason to suppose that its action would not also in this case resemble that of the sun, and that saccharine matter, and more especially the aromatic constituents, would be produced. In this country solar light is too often found insufficient to ripen the fruit or even the wood of fruit trees during the short summer months; and I believe that electric light will be found a most useful auxiliary to solar light to effect the production of ripe and aromatic fruit both under glass and in the open air.

Estimated Cost of Electro-Horticulture.

A very important question is that of the cost of electro-horticulture. This will depend in a great measure upon the cost of the fuel or other source of energy, and upon the scale of application. To work only one electric lamp by means of a small steam or gas engine is expensive both in fuel and in cost of attendance. If steam-power has to be resorted to, an engine of sufficient size should be employed to give economical results per horse-power of energy produced, and the electric arc should be of sufficient brilliancy to give a good effect for the power expended. Experience in electric illumination has established a form and size of machine, both convenient and suitable for the attainment of economical results, viz., the medium dynamo-electric machine, which, if applied to a suitable regulator, produces fully 6,000 candle-power of diffused light with an expenditure of 4 horse-power.

The experiments before given show that the most effective height at which to place the naked electric light of 1,400 candle-power is about 2 metres. By providing a metallic reflector, and thus throwing the major portion of the upward rays down upon the surface to be illuminated that height may be taken at 3 metres. If the electric arc employed was equal to 6,000 candles, the height would be $\frac{\sqrt{6,000}}{\sqrt{1,400}} \times 3 = 6{\cdot}2$ metres, at which such an electric light should be fixed under the protection of a tin plate or other reflector. In operating upon an extended surface several lamps should be placed at such distances apart, as to make the effect over it tolerably uniform. The effect of radiation would be equally distributed over the ground if the radiating centres were placed at distances apart equal to double their height above the ground; for under these circumstances a square foot of surface midway between them would receive from each centre one-half the number of rays falling upon such a surface immediately below a centre. A plant at the intermediate point would, however, have the advantage of presenting a larger leaf surface to the two sources of light; and in order to compensate for this advantage, the light centres may be placed considerably further apart, say at distances equal to three times their elevation, or 18 metres. Nine lights so placed would cover an area 54 metres square, or just about $\frac{3}{4}$ acre. If this space was enclosed with a high fruit wall (as shown with the lamp centres marked in the accompanying sketch), this will also get the full benefit of electric radiation, and would serve at the same time to protect the plants from winds. Protection against injury from this latter cause might be further carried out with advantage by following plan adopted (with excellent results I believe) by Sir William Armstrong, that of subdividing the area under forced cultivation by vertical partitions of glass.



The engine-power necessary to maintain this radiant action would be $9 \times 4 = 36$ horse-power, involving the consumption of $36 \times 2\frac{1}{2} = 90$ lbs. of fuel per hour, or say, for a night of 12 hours (with an allowance of 40 lbs. for getting up steam) 10 cwt., which, at sixteen shillings per ton, would cost eight shillings. This expenditure would not include, however, the cost of carbons and of an attendant, which would probably amount to another eight shillings, making a total of sixteen shillings. If, however, an engine can be found doing other descriptions of work during the day time, the cost of steam-power and attendance for the night-work only would be considerably reduced. In the calculation just given, I have assumed the employment of fuel for the production of mechanical energy, whereas the question will assume a totally different aspect if natural sources of power, such as waterfalls, can be made available within the reasonable distance of half-a-mile. The expenditure for energy will, in that case, be almost entirely saved, and that of attendance be greatly diminished, and under such circumstances it seems probable that electro-horticulture may be carried out with considerable advantage.

The experiments furnish proof that the management of the electrical apparatus presents no particular difficulty, as the gas-engine, dynamo-machine, and regulator have been under the sole management of my head gardener, Mr. D. Buchanan, and his son an assistant gardener. The regulator requires no attention beyond the replacement of carbons every four or five hours, which period may easily be increased to twelve hours, by a slight modification of the lamp.

Conclusions.

The experiments seem to lead to the following conclusions:—

1. That electric light is efficacious in producing chlorophyl, in the leaves of plants, and in promoting growth.

2. That an electric centre of light, equal to 1,400 candles, placed at a distance of 2 metres from growing plants, appeared to be equal in effect to average daylight at this season of the year, but that more economical effects can be attained by more powerful light centres.

3. That the carbonic acid and nitrogenous compounds generated in diminutive quantities in the electric arc, produce no sensible deleterious effects upon plants enclosed in the same space.

4. That plants do not appear to require a period of rest during the twenty-four hours of the day, but make increased and vigorous progress if subjected during daytime to sunlight and during the night to electric light.

5. That the radiation of heat from powerful electric arcs can be made available to counteract the effect of night frost, and is likely to promote the setting and ripening of fruit in the open air.

6. That while under the influence of electric light, plants can sustain increased stove heat without collapsing, a circumstance favourable to forcing by electric light.

7. That the expense of electro-horticulture depends mainly upon the cost of mechanical energy, and is very moderate where natural sources of such energy, such as waterfalls, can be made available.

Since writing the above my attention has been drawn to an article in "Nature," 29th January, 1880, giving interesting observations by

Dr. Schübeler, of Christiania, on "The Effect of Uninterrupted Sunlight on Plants in the Arctic Regions." These observations fully confirm the conclusion indicated by my experiments with electric light. Not only are plants able to grow continuously, according to Dr. Schübeler, but when under the influence of continuous light they develop more brilliant flowers and larger and more aromatic fruit than under the alternating influence of light and darkness, whereas the formation of sugar appears to be dependent chiefly upon temperature.

It would follow from these observations, that with the aid of stoves and electric light, fruit, excelling both in sweetness and aroma, and flowers of great brightness, may be grown without solar aid. Dr. Schubeler mentions that in removing an Acacia plant from the dark, and placing it under the influence of the arctic midnight sun, the leaves opened slowly, and it is interesting to observe that the same effect took place when an *Acacia lophantha* was placed (in the open air) under the influence of my midnight lamp.

March 11, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. THE CROONIAN LECTURE.—"On some Elementary Principles in Animal Mechanics. No. IX. The Relation between the Maximum Work done, the Time of Lifting, and the Weights Lifted by the Arms." By the Rev. SAMUEL HAUGHTON, M.D. (Dubl.), D.C.L. (Oxon.), F.R.S., Fellow of Trinity College, Dublin. Received February 28, 1880.

In the preceding note (No. VIII) I have shown that, in lifting weights at a fixed rate with the arms, until fatigue sets in, the following equation, deduced from the law of fatigue, is complied with, very closely:—

$$\frac{n(1+\beta^2t^2)}{t}=A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where n=number of lifts,

t=time of each lift,

$\left.\begin{array}{l}\beta\\ A\end{array}\right\}$ coefficients to be found from the experiments themselves.

From this equation, and from the experiments themselves, it appeared that there was a particular time of lift—

$$t=\tau,$$

at which the maximum work was done, and that,

$$\tau=\frac{1}{\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

The object of the series of experiments carried on during six months of the winter of 1876–7, and described in the present note, was to find in what manner the co-efficients τ, or β, vary with the weight employed and lifted. The experiments were conducted in every respect as those described in Note VII, the conditions of which I here quote and repeat:—

" The following experiments were made during the last six months upon Dr. Macalister:—

" A pair of dumbbells, held one in each hand, were raised simultaneously, from the vertical to the horizontal position, and again lowered, at a rate regulated by a metronome made for the purpose. No rest was allowed at the beginning or end of the motion, which took place as before, under the following conditions, viz.:—

" 1. To keep time with the metronome.

" 2. To raise the weights in the transverse plane.

" 3. To supinate the hands.

" 4. To abstain from all bending of the knees or spinal column.

" 5. The experimenter not to count the lifts.

" The experiments were made at intervals of never less than 24 hours, so as to avoid all risk of the muscles becoming trained; and on each occasion the weights were lifted until it became impossible to effect another lift, without violating condition No. 4, indicating that other muscles were called in to aid the shoulder muscles already worn out."

I have embodied in this note, the experiments made upon Dr. Macalister in 1875–6, Note VIII, " Proc. Roy. Soc.," vol. xxv, p. 132.

I.

w=9·75 lbs. (1875–6.)

No.	t.	n.
1	0·50 sec.	19·0
2	0·66 ,,	24·6
3	1·00 ,,	26·2
4	1·50 ,,	23·6
5	2·00 ,,	20·0
6	3·00 ,,	14·2
7	4·00 ,,	12·4
8	6·00 ,,	8·5

From these experiments I found A=50·7, β=1·00, τ=1·00 sec.

II.

w=7·75 lbs. (October, 1876.)

t.	n.	t.	n.	t.	n.	t.	n.	t.	n.
sec.		sec.		sec.		sec.		sec.	
0·50	22	0·66	42	0·75	41	1·0	40	2·0	27
,,	25	,,	40	,,	48	,,	37	,,	33
,,	23	,,	44	,,	50	,,	38	,,	31
,,	26	,,	42	,,	47	,,	40	,,	33
,,	26	,,	38	,,	49	,,	36	,,	33
Mean ..	24·4		41·2		47·0		38·2		31·4

In discussing these experiments, it will be found that the first must be rejected, as its results differ from all the others. The rate of work was too fast, and the experiment was stopped by respiratory distress rather than by muscular fatigue.

The other four sets of experiments, when treated as in No. VIII, give the following results :—

β.	$\frac{\delta A}{\Sigma A}$.
1·25	4·59 per cent.
1·29	4·33 ,,
1·30	4·26 ,,
1·31	4·30 ,,
1·33	4·53 ,,
1·40	5·13 ,,

Hence we obtain the constants

A=113·75, β=1·30, τ=0·77 sec.

III.

w=5·87 lbs. (December, 1876.)

t.	*n.*	*t.*	*n.*	*t.*	*n.*
0·50 sec.	60	0·66 sec.	69	1·00 sec.	57
,,	59	,,	71	,,	57
,,	55	,,	78	,,	57
,,	61	,,	71	,,	64
,,	57	,,	70	,,	60
Mean	58·4		71·8		59·0

Discussing these experiments as before, we obtain :—

β.	$\frac{\delta A}{\Sigma(A)}$.
1·50	5·74 per cent.
1·45	5·30 ,,
1·41	5·10 ,,
1·40	5·03 ,,
1·39	5·02 ,,
1·38	5·16 ,,
1·35	5·43 ,,
1·30	5·92 ,,
1·25	6·44 ,,
1·20	6·94 ,,

Hence we obtain the constants

$$A=182{\cdot}2, \qquad \beta=1{\cdot}395, \qquad \tau=0{\cdot}716 \text{ sec.}$$

IV.

w=2·62 lbs. (January, 1877.)

t.	*n.*	*t.*	*n.*	*t.*	*n.*
0·50 sec.	140	0·55 sec.	260	0·66 sec.	209
,,	160	,,	424	,,	215
,,	204	,,	528	,,	225
,,	174	,,	762	,,	260
,,	208	,,	827	,,	227
,,	234	,,	382	,,	216
,,	227	,,	382	,,	268
,,	455	,,	467	,,	569
,,	411	,,	1072	,,	393
,,	507	,,	411	,,	254
Mean	271·8		551·5		283·6

It will be observed that, with light weights, the experiments vary much more than with heavy weights. The discussion of these experiments shows a less satisfactory agreement with equation (1) than the other experiments with heavier weights. The most probable values of the constants are as follows:—

β.	$\frac{\delta A}{\Sigma(A)}$.
1·0	26·5 per cent.
1·1	26·1 "
1·2	25·7 "
1·3	25·4 "
1·4	25·0 "
1·5	24·8 "
1·6	24·8 "
1·7	24·9 "
1·8	25·0 "
1·9	25·1 "
2·0	25·2 "

Hence we find

$$A=1128, \qquad \beta=1{\cdot}55, \qquad \tau=0{\cdot}645.$$

Bringing all the results into one table, we have

No.	w.	A.	β.	τ.
1	9·75 lbs.	50·7	1·000	1·000
2	7·75 "	113·7	1·300	0·770
3	5·87 "	182·2	1·395	0·716
4	2·62 "	1128·0	1·550	0·645

From all these experiments, it appears that the central cubic (1) derived from the law of fatigue, represents fairly the relation between n and t; and that n reaches a maximum for a time of lift τ, which varies with the weight, increasing with it, but not proportionally.

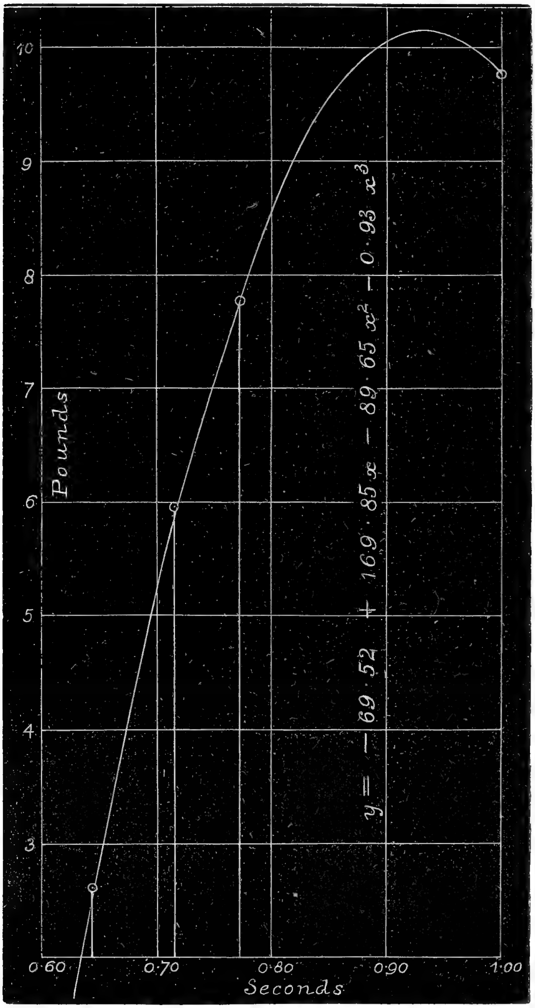
The time of lift corresponding to the maximum work done with each weight, ranges from 0·645 sec. to 1 sec. Within these limits (in order to interpolate) let us assume:—

$$w=a+b\tau+c\tau^2+d\tau^3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Introducing the four results from the table just given, we find:—

$$\begin{aligned} a&=-\ \ 69{\cdot}52, \\ b&=+169{\cdot}85, \\ c&=-\ \ 89{\cdot}65, \\ d&=-\ \ \ 0{\cdot}93, \end{aligned}$$

from which the curve (3) can be plotted.



This curve is shown in the figure, and represents the relation between w and τ within the limits. The ordinate w reaches a maximum, within these limits when—

$$\frac{dw}{d\tau}=0,$$

which gives us

$$w=10\cdot17 \text{ lbs.}$$
$$\tau=0\cdot932 \text{ second.}$$

II. "On the Tonicity of the Heart and Arteries." By W. H. GASKELL, M.D., Trinity College, Cambridge. Communicated by Dr. MICHAEL FOSTER, Prelector of Physiology in Trinity College, Cambridge. Received March 1, 1880.

The author has made a lengthened series of observations on the heart of the frog and tortoise, both on the entire ventricle and on the apex, supplied, by the help of Roy's* tonometer, with saline solutions or with diluted blood (of rabbit, sheep, or bullock).

That the artificial blood solutions differ in their action on the apex from the frog's own blood in degree only and not in kind, and that consequently Bernstein† is wrong in regarding these fluids as specific stimuli giving rise to rhythmic action, is shown by the following facts.

1. The apex of the frog's heart brought to quiescence after Bernstein's method, will beat spontaneously when the pressure in its cavity is increased by clamping the aortic arches.

2. If while the heart still remains in its natural position within the body, the frog's own blood be replaced by the artificial blood solution, and the ventricle be then nipped by fine forceps, the apex will remain quiescent for an indefinite length of time. As soon, however, as the pressure in the cavity is increased, the apex commences to contract, and beats as long as the heart is distended by the increased pressure.

The conclusions to which the author has been led by his experiments are as follows:—

1. The heart possesses, like the arteries, what may be called tonicity, and the variations in this tonicity play an important part in determining the features of the cardiac beat.

The apex, when first tied on to the cannula, the pressure of the blood solution being kept constant at 10 cm. or less, remains quiescent for some time in the position of greatest relaxation. As, however, the blood solution continues to pass through, the lever of the tonometer rises often very appreciably even before the apex begins to beat. This rise, unless otherwise explained, would seem to show that the first effect of the blood solution has been to bring the apex into a condition which is no longer that of greatest relaxation for that particular pressure.

Further, in correspondence with this raising of tone of the cardiac muscle by the artificial blood solution, is the alteration in the character of the beat.

The first beats, whether natural or artificially produced, which occur while the ventricle is still in a somewhat relaxed or atonic condition in

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* "Journ. of Physiology," vol. i, p. 452.

† "Centralblatt f. d. Med. Wissenschaften," 1876, p. 385.

consequence of the operation of being tied on the cannula, are less in height and more pointed in appearance, *i.e.*, remain a shorter time at the height of contraction than those which are obtained after the blood has been passing some time.

2. The tonic condition is in all probability partly due to the alkalinity of the blood.

a. After the ventricle or apex beating with artificial blood solution has been brought to standstill by replacing the blood with "normal" salt solution, then an alkaline salt solution (1 sodium hydrate to 20,000 normal salt solution) causes beats to reappear, and again the ventricle stops beating, but no longer in the position of diastole, the standstill that now occurs is always a systolic not a diastolic standstill.

b. When the ventricle or apex is beating under the influence of an artificial blood solution and the alkaline solution be sent through, then the resulting curve shows how the alkaline standstill is brought about, every contraction is as powerful as with the blood solution, the cavity being closed each time, but every relaxation is less perfect, and of shorter duration than the previous relaxation, while the period of full contraction in each beat is progessively lengthened, the rate of rhythm not being much altered; at last each relaxation becomes invisible, and the lever traces a straight line at the level of the height of full contraction.

c. When the apex is not beating, but is motionless in the position of complete relaxation, as is frequently the case when salt solution alone is sent through, then the alkaline solution brings it very gradually from the position of extreme relaxation to that of extreme contraction without the necessary production of a single beat.

Further, if during this tonic action of the alkaline solution, artificial beats be produced, then, as in the case just mentioned, the time of full contraction in each beat is lengthened as the tonicity rises.

3. Dilute acid solutions, such as lactic acid (1 lactic acid to 20,000 normal saline solution), lower the tonicity of the cardiac muscle.

a. When the lactic acid solution is sent through a beating ventricle or apex, then a curve is produced nearly the exact converse of the curve obtained from alkaline solutions; the separate beats progressively diminish in force, present a pointed appearance, owing to the extreme shortness of the time of full contraction, and very quickly the ventricle or apex remains still in the position of extreme relaxation.

b. When by means of the acid solution the beats have been much lowered in force, then the alkaline solution can bring them back to their original force and character, and then produce its own characteristic effect, and if the acid solution be again sent through, it gradually overpowers the action of the alkali, and the apex or ventricle slowly falls from a condition of extreme contraction to one of extreme relaxation.

c. If the acid solution has brought about a condition of nearly absolute relaxed or atonic standstill, then removal of the acid by salt solution does not improve the beats, and does not raise the tonicity, but immediately the alkaline solution is sent through, the tonicity begins to rise, and the beats are strengthened until the ventricle passes into the condition of systolic or tonic standstill.

d. The reverse case is also true; although salt solution sent through after the alkaline solution will very gradually bring the ventricle from the contracted to the dilated condition, yet the lactic acid solution produces the same effect with much greater rapidity.

e. These very dilute alkali and acid solutions do not produce their effects by permanently injuring the cardiac muscle, for in either case it is possible to restore the beats to their original strength and character by sending the artificial blood solution through.

4. Certain alkaloids act in this respect in the same manner as alkalies, others as acids.

a. Digitalin or antiarin in artificial blood solution or in normal salt solution produce exactly the same effect upon both the beating ventricle and the non-beating apex as the alkaline salt solution.

b. Blood solutions containing muscarin and pilocarpin gradually diminish the force of the beat without necessarily slowing the rate of rhythm until the ventricle stops beating in a condition of relaxation, as in the case of lactic acid solutions; at the same time the beats show all the characters of the atonic beat.

c. Atropin acts in the same direction as alkalies, but only raises the tonicity to a slight extent, at least with the dilute solutions hitherto employed.

5. The tonicity of the muscles of the smaller arteries is increased by alkalies and lowered by acid solutions. This is concluded from the action of the alkaline and lactic acid solutions upon the vessels of the mylohyoid muscle, upon the rate of flow from the abdominal vein when the solutions are sent through the legs alone and upon the mesenteric vessels.

In all cases with the alkali solution the arteries contract, the rate of flow diminishes or ceases altogether, and as long as the alkali solution is passing through the contraction remains; with the lactic acid solution, on the other hand, the arteries dilate to their full extent, the rate of flow is markedly increased, and no constriction takes place unless the alkaline solution be again sent through.

The author reserves for another occasion a fuller description of his results, as well as a discussion of their bearings.

III. "Report on Phyto-Palæontological Investigations of the Fossil Flora of Alum Bay." By Dr. CONSTANTIN BARON ETTINGSHAUSEN, Professor in the University of Graz, Austria. Communicated by Professor HUXLEY, Sec. R.S. Received March 4, 1880.

The white clay of Alum Bay and the fossil plants included in it have been long known. The introduction to the "Monograph on the British Eocene Flora," Palæontographical Society, 1879, p. 12, gives a detailed history of this locality.

The first scientific investigation of the fossil plants of Alum Bay were made by Dr. De la Harpe and Professor Oswald Heer, who enumerated a Flora of about forty species, distributed in several genera.

I have devoted the winter 1879–80 to the investigation of this Fossil Flora at the British Museum, and I have had under examination for this purpose the fossil plants of Alum Bay collected by W. Stephen Mitchell and Mr. H. Keeping, the collections of the Woodwardian Museum at Cambridge, and those of the Museum of Practical Geology, and the collection of Mr. John Starkie Gardner.

The results of this investigation are as follow:—

The Fossil Flora of Alum Bay contains at least 116 genera and 274 species, which are distributed into 63 families. Of these genera 3 belong to the Thallophyta, 2 to the Filices, 5 to the Gymnospermæ, 6 to the Monocotyledons, 28 to the Apetalæ, 15 to the Gamopetalæ, 54 to the Dialypetalæ, and 2 are indeterminable.

A sub-tropical climate, at least, is indicated by many of the Ficus species, and by the Artocarpeæ, Cinchonaceæ, Sapotaceæ, Ebenaceæ, Büttneriaceæ, Bombaceæ, Sapindaceæ, Malpighiaceæ, etc.

The genera which are common to Alum Bay and Sheppey are: *Callitris, *Cupressinites, Sequoia, *Cyperites, Smilax, *Sabal, *Aronium, *Quercus, Juglans, *Laurus, Nyssa, Proteoides, Cinchonidium, Apocynophyllum, Sapotacites, *Diospyros, Symplocos, Magnolia, *Nelumbium, *Hightea, Acer, *Sapindus, *Cupania, Eugenia, *Eucalyptus, *Prunus, Amygdalus, *Podogonium, Leguminosites, *Carpolithes. In the genera to which an asterisk is prefixed, are found species common to Alum Bay and Sheppey. This great number of genera, common to Alum Bay and Sheppey, seems to point to such a close connexion between the two Floras, that it does not appear to me to be advisable to distinguish the leaves of the one from the fruits of the other, even though they cannot be absolutely connected, by separate specific names. I find it is possible, by comparing the leaves and fruits of their nearest living analogues, to unite them in many cases, at least provisionally. For instance, I find that the leaves of one of the Cupanias from Alum Bay approach nearly to those of the existing Cupania glabra, and I should prefer to unite with these Alum Bay leaves the form of Cupania fruit from Sheppey, which most nearly agrees with the same living species, namely, C. glabra, &c.

Liquidambar* and Metrosideros, which are common to Sheppey and Bournemouth, are as yet absent at Alum Bay; on the other hand, we have in Alum Bay the genera Symplocos, Nelumbium, and Hightea, which are common to this locality and Sheppey, but not yet found at Bournemouth.

The small number of ferns and palms, in comparison with the much greater number at Bournemouth and of the latter at Sheppey, is remarkable. This is attributable, I think, only to local physical conditions, and perhaps the same conditions may have led to the much greater prevalence of Dicotyledons in Alum Bay. In the beds of Studland, which are on the same horizon as Alum Bay, remains of palms and ferns abound, though belonging to few species.

Many of the Dicotyledons correspond with Miocene species, and I do not doubt that there is a genetic connexion between them. We have in Alum Bay the precursors of Quercus Lonchitis, Q. mediterranea, Ulmus longifolia, Celtis Japeti, Ficus lanceolata, F. sagoriana, F. arcinervis, F. Rüminiana, Juglans parschlugiana, Salix integra, Nyssa striolata, Grevillea Hæringiana, Persoonia laurina, Olea carneolica, Fraxinus primigenia, F. savinensis, Cinchonidium bilinicum, C. latifolium, Apocynophyllum Amsonia, Symplocos parschlugiana, Ceratopetalum bilinicum, C. radobojanum, Bombax chorisiæfolium, B. salmaliæfolium, Ternstrœmia radobojana, Acer decipiens, Dodonæa Salicites, D. Apocynophyllum, Celastrus Europæus, Ilex stenophylla, Vitis teutonica, Rhus stygia, Amygdalus œningensis, Palæolobium radobojense, Cassia Phaseolites, C. sagoriana, C. Memnonia, C. lignitum, C. stenophylla, Mimosites cassiæformis.

There are also what appear to be certain ancestral species, if I may use the expression, nearly allied to several miocene species, whose characters they unite; for instance, Hiræa intermedia connects H. borealis of the Fossil Flora of Hæring and H. Ungeri of the Fossil Flora of Sotzka; Celastrus Tafnis connects C. Æoli and C. Murchisoni; Celastrus Fenjæ connects C. cassinefolius and C. Bruckmanni; Celastrus Salidæ connects C. protogæus, C. Acherontis, C. deperditus from Hæring, C. Hippolyti from Kutschlin, C. oreophilus from Sotzka and C. stygius from Switzerland; Rhamnus acutangula connects R. paucinervis, R. colubrinoides, R. orbifera, R. œningensis, R. Graefii and R. Decheni; Cissus celastrifolia connects C. rhamnoides and C. celtidifolia; Rhus cyclophylla connects R. Pyrrhæ, R. Brunneri and R. Meriani.

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* The Alum Bay specimen called Liquidambar is only a less palmitid form of Aralia primigenia.

In addition to the great number of Miocene species, whose origin can apparently be traced back to the Eocene, there are not wanting indications that certain Miocene genera were not completely differentiated into genera in the Eocene period. For instance, the genus Castanea, perfectly developed in the Miocene, seems to be represented in the Eocene by a Castanea-like oak, Q. Bournensis, which combines in itself characters common to both genera, which are no longer found united. Another example is afforded by a Pomaderris-like Rhamnus, which, in like manner, seems to show the commencement of the differentiation of Pomaderris from Rhamnus, completed in the Miocene Flora.

I have selected for mention a few from among a great number of new forms, as possessing special interest.

A very distinct Marattia, nearly allied to M. Kaulfussi, J. Smith, is remarkable as being the first species met in the Tertiary. The Celtis is allied to C. Japeti of the Miocene Flora of Parschlug on the one side, and to the living C. australis on the other.

The only Adenopeltis is allied to an American living species. The presence of two species of Banksia is ascertained beyond all doubt, since their seeds also occur; many other leaves formerly named Banksia I now agree may belong to Myrica. The proteaceous Lomatia is represented by a fruit. Very characteristic leaves of Aristolochia and of Alyxia allied to the living Alyxia spicata R. Brown, and a Clerodendron allied to the East Indian C. viscosum Vent. are found, but rarely.

Of Diospyros is found calyx, berry and leaf, the berry occurring also in Sheppey. The solitary species of Diospyros of Alum Bay and that of Sheppey are the same. The only species of Cornus is remarkable as showing perhaps a genetic relation with some of the Miocene species. The leaves of two species of Malvaceæ belong, I think, to two of the species of Hightea from Sheppey, where eight species are found. One of the very characteristic leaflets of Bombaceæ may belong, on account of its venation and form, to the Brazilian genus Salmalia; and very characteristic leaves of Ternstrœmia, nearly allied to a Miocene form, are found. Of Cupania, the fruits of eight species of which are found at Sheppey, I have distinguished six species by their leaves in Alum Bay, and these I provisionally refer to the nearest of the Sheppey species. The only Alum Bay Pistacia is nearly allied to the well-known P. vera. The putamens of two species of Prunus, of which one is common to Sheppey occur, and of Amygdalus three species of fruits are found.

Of the Papilionaceæ I could distinguish thirty-eight species belonging to ten genera. The richest are Cassia and Dalbergia, and many species of them are also found in Hæring and Sotzka.

The comparison of the Fossil Flora of Alum Bay with that of Bournemouth and with the other Tertiary Floras is reserved until further investigations are completed. The most striking fact, however, in connexion with the Alum Bay Flora that my work even at this stage has brought out, is, that more than fifty of the species are common to Sotzka and Hæring, while a lesser number are common to Sézanne, the Lignitic of America, and to other Floras.

It is my pleasing duty to have again to acknowledge my thanks for the very important help the Royal Society has afforded me. I have also to acknowledge my indebtedness for advice and assistance to Sir Joseph Hooker, Dr. Henry Woodward, Mr. William Carruthers, Mr. T. G. Baker, Mr. R. Etheridge, jun., Professor T. M. K. Hughes, and Mr. J. S. Gardner.

Genera and Species of the Fossil Flora of Alum Bay.

THALLOPHYTA.

Rhytisma Eucalypti. *Ett. and Gard.*
" priscum. *Ett. and Gard.*
Sclerotium antiquum. *Ett. and Gard.*

FILICES.

Chrysodium Lanzeanum. *Visian. sp.*
Anemia subcretacea. *Sap. sp.*
Marattia Hookeri. *Ett. and Gard.*

GYMNOSPERMÆ.

Cupressineæ.

Glyptostrobus Europæus. *Brongn. sp.*
Callitris curta. *Bowerb. sp.*
Cupressinites globosus. *Bowerb.*

Abietineæ.

Sequoia Langsdorfii. *Brongn. sp.*
Sequoia Couttsiæ. *Heer.*

Podocarpeæ.

Podocarpus eocenica. *Ung.*

MONOCOTYLEDONES.

Gramineæ.

Arundo Goepperti. *Münst. sp.*

Cyperaceæ.

Cyperites eocenicus. *Ett. and Gard.*

Smilaceæ.

Smilax lancifolia. *Ett. and Gard.*

Palmæ.

Sabal major. *Ung.*
Flabellaria. *sp. adhuc indeterm.*

Aroideæ.

Aronium eocenicum. *Ett. and Gard.*

DICOTYLEDONES.

APETALÆ.

Myriceæ.

Myrica salicina. *Ung.*
„ lignitum. *Ung.*
„ sagoriana. *Ett.*
Myrica Hæringiana. *Ung.*
„ acuminata. *Ung.*
Comptonia acutiloba. *Sternb. sp.*

Cupuliferæ.

Quercus Lyellii. *Heer.*
„ præ-lonchitis. *Ett. and Gard.*
„ lonchitis. *Ung.*
„ prae-mediterranea. *Ett. and Gard.*
Quercus drymeja. *Ung.*
„ Bournensis. *De la Harpe.*
„ viburnifolia. *Lesq.*
Fagus intermedia. *Ett. and Gard.*

Ulmaceæ.

Ulmus antiquissima. *Sap.*
„ plurinervia. *Ung.*
Planera Ungeri. *Ett.*

Celtideæ.

Celtis Woodwardi. *Ett. and Gard.*

Moreæ.

Ficus præ-lanceolata. *Ett. and Gard.*
„ lanceolata. *Heer.*
„ Morrisii. *De la Harpe.*
„ deleta. *Ett. and Gard.*
„ arenacea. *Lesq.*
„ præ-arcinervis. *Ett. and Gard.*
„ rhamnoides. *Ett. and Gard.*
„ Jynx. *Ung.*
„ Wudgæ. *Ett. and Gard.*
„ Falceroni. *Heer.*
Ficus bumeliæfolia. *Ett.*
„ Nerthi. *Ett. and Gard.*
„ Inguionis. *Ett. and Gard.*
„ planicostata. *Lesq.*
„ Reussii. *Ett.*
„ Cisæ. *Ett. and Gard.*
„ Bowerbankii. *De la Harpe.*
„ Granadilla. *Mass. sp.*
„ Hydrarchos. *Ung.*

Artocarpeæ.

Cecropia eocenica. *Ett. and Gard.*
Artocarpidium grandifolium. *Ett. and Gard.*
Artocarpidium integrifolium. *Ung.*

Juglandeæ.

Juglans præ-parschlugiana. *Ett. and Gard.*
Juglans Sharpei. *De la Harpe.*
Juglandites cernuus. *Sap.*

Euphorbiaceæ.

Adenopeltis Alumensis. *Ett. and Gard.*

Salicineæ.

Salix Rhedæ. *Ett. and Gard.*
„ præ-integra. *Ett. and Gard.*
Salix tenuifolia. *Ett. and Gard.*
Populus eocenica. *Ett. and Gard.*

Santalaceæ.

Santalum salicinum. *Ett.*
„ acheronticum. *Ett.*
„ osyrinum. *Ett.*
„ microphyllum. *Ett.*
Nyssa Alumensis. *Ett. and Gard.*
„ Europæa. *Ung.*
„ præ-striolata. *Ett. and Gard.*

Proteaceæ.

Proteoides crassipes. *Ett. and Gard.*
Grevillea Hermionis. „ „
Persoonia eocenica. *Ett.*
Lomatia Britannica. *Ett. and Gard.*
Banksia Ungeri. *Ett.*
„ dillenioides. *Ett.*

Laurineæ.

Laurus primigenia. *Ung.*
„ ocoteides. *Lesq.*
„ Salteri. *De le Harpe.*
„ Agathophyllum. *Ung.*
„ Swozzowiciana. *Ung.*
„ Jovis. *De la Harpe.*
„ vetusta. *Sap.*
„ socialis. *Lesq.*
Laurus Lalages. *Ung.*
„ Haidingeri. *Ett.*
Cinnamomum Rossmæssleri. *Heer.*
„ polymorphum. *A. Braun. sp.*
„ eocenicum. *Ett.*
Daphnogene veronensis. *Mass. sp.*
„ anglica. *Heer.*

Daphnoideæ.

Daphne aquitanica. *Ett.*

Nyctagineæ.

Pisonia eocenica. *Ett.*

Aristolochieæ.

Aristolochia Alumensis. *Ett. and Gard.*

GAMOPETALÆ.

Cinchonaceæ.

Cinchonidium lanceolatum. *Ett. and Gard.*
Cinchonidium præ-bilinicum. *Ett. and Gard.*
Cinchonidium præ-latifolium. *Ett. and Gard.*

Oleaceæ.

Olea Britannica. *Ett. and Gard.*
Notelæa primigenia. *Ett. and Gard.*
Fraxinus Jovis. *Ett. and Gard.*
„ præ-savinensis. *Ett. and Gard.*

Apocynaceæ.

Alyxia Europæa. *Ett. and Gard.*
Apocynophyllum Titaniæ. *Ett. and Gard.*
Apocynophyllum grande. *Ett. and Gard.*
Apocynophyllum Hæringianum. *Ett.*
„ Præ-Amsonia. *Ett. and Gard.*

Convolvulaceæ.

Porana œningensis. *A. Braun.*

Verbenaceæ.

Clerodendron Europæum. *Ett. and Gard.*

Myrsineæ.

Myrsine Erdæ. *Ett. and Gard.*

Sapotaceæ.

Sapotacites eocenicus. *Ett. and Gard.*
„ emarginatus. *Heer.*
„ sideroxyloides. *Ett.*
Bumelia Oreadum. *Ung.*
„ Dryadum. *Ett. and Gard.*

Ebenaceæ.

Diospyros eocenica. *Ett. and Gard.*

Symploceæ.

Symplocos Britannica. *Ett. and Gard.*

Vaccinieæ.

Vaccinium eocenicum. *Ett. and Gard.*

Vaccinium acheronticum. *Ung.*

Ericaceæ.

Andromeda protogæa. *Ung.*

DIALYPETALÆ.

Araliaceæ.

Aralia primigenia. *De la Harpe.*

Corneæ.

Cornus atlantica. *Ett. and Gard.*

Saxifragaceæ.

Callicoma Fornacis. *Ett. and Gard.*
Ceratopetalum crassipes. *Ett. and Gard.*

Ceratopetalum Manni. *Ett. and Gard.*
„ Hæringianum. *Ett.*

Nymphæaceæ.

Nelumbium Buchii. *Ett.*

Nymphæa Doris. *Heer.*

Magnoliaceæ.

Magnolia stygia. *Ett. and Gard.*

Anonaceæ.

Anona elongata. *Ett. and Gard.*

Anona cyclosperma. *Ett. and Gard.*

Büttneriaceæ.

Pterospermum eocenicum. *Ett. and Gard.*

Pterospermites dentatus. *Ett. and Gard.*

Malvaceæ.

Hightea elliptica. *Bowerb.*

Hightea turbinata. *Bowerb.*

Bombaceæ.

Bombax Menjæ. *Ett. and Gard.*
„ Sagorianum. *Ett.*
„ tenuinerve. *Ett. and Gard.*

Salmalia borealis. *Ett. and Gard.*
Sterculia Labrusca. *Ung.*
„ Sigfridi. *Ett. and Gard.*

Ternstrœmiaceæ.

Ternstrœmia eocenica. *Ett. and Gard.*
„ bilinica. *Ett.*

Saurauja robusta. *Sap.*

Tiliaceæ.

Grewiopsis integerrima. *Ett. and Gard.*

Acerineæ.

Acer eocenicum. *Ett and Gard.*
Acer præ-decipiens. *Ett. and Gard.*

Sapindaceæ.

Sapindus eocenicus. *Ett. and Gard.*
„ angustifolius. *Lesq.*
„ falcifolius. *A. Braun.*
„ crassinervis. *Ett. and Gard.*
Cupania lobata. *Bowerb.*
„ subangulata. *Bowerb.*
Cupania tumida. *Bowerb.*
„ depressa. „
„ corrugata. „
„ grandis. „
Dodonea præ-salicites. *Ett. and Gard.*
„ subglobosa. „ „

Malpighiaceæ.

Hiræa intermedia. *Ett. and Gard.*
Malpighiastrum grandifolium. *Ett. and Gard.*
Malpighiastrum banisterinum. *Ett. and Gard.*
Malpighiastrum præ-venosum. *Ett. and Gard.*

Cedrelaceæ.

Cedrela primigenia. *Ett. and Gard.*

Pittosporeæ.

Pittosporum eocenicum. *Ett. and Gard.*

Celastrineæ.

Celastrus Tafnis. *Ett. and Gard.*
„ Fengæ. „ „
„ salidæ. „ „
„ elænus. *Ung.*
„ præ-europæus. *Ett. and Gard.*
Celastrus myricinus. *Ett. and Gard.*
Elæodendron dubium. *Ett.*
Celastrophyllum undulatum. *Ett. and Gard.*

Ilicineæ.

Ilex Atlantica. *Ett. and Gard.*

Rhamneæ.

Zizyphus Ungeri. *Heer.*
„ vetustus. „
„ integrifolius. *Heer.*
„ pachyneuris. *Ett. and Gard.*
Rhamnus acutangula. *Ett. and Gard.*
„ præ-rectinervis. „ „
„ præ-pomaderris. *Ett. and Gard.*

Ampelideæ.

Cissus Auroræ. *Ett. and Gard.*
„ celastrifolia „ „
Vitis præ-teutonica. *Ett. and Gard.*

Anacardiaceæ.

Rhus cyclophylla. *Ett. and Gard.*
„ Atlantidis. „ „
Rhus prisca. *Ett.*
Pistacia Britannica. *Ett. and Gard.*

Myrtaceæ.

Eugenia Apollinis. *Ung.*
Eucalyptus oceanica. *Ung.*
„ Hæringiana. *Ett.*
Myrtus eocenica. *Ett and Gard.*
Callistemophyllum obtusum. *Ett. and Gard.*
Callistemophyllum elegans. *Ett. and Gard.*
„ melaleucæforme. *Ett.*
„ diosmoides. *Ett.*

Amygdaleæ.

Prunus Druidum. *Ett. and Gard.*
,, Pygmæorum. *Ett. and Gard.*
Amygdalus tenue-striata. ,, ,,
Amygdalus præ-œningensis. *Ett. and Gard.*
Amygdalus pereger. *Ung.*

Papilionaceæ.

Glycyrrhiza deperdita. *Ung.*
Physolobium orbiculare. ,,
,, antiquum. ,,
Phaseolites eriosemæfolius. ,,
Dalbergia primæva. ,,
,, Hæringiana. *Ett.*
,, Ungeri. ,,
,, cyclophylla. *Ett. and Gard.*
,, longifolia. ,, ,,
,, eocenica. *Ung. sp.*
Palæolobium Sotzkianum. *Ung.*
,, Hæringianum. ,,
,, heterophyllum. ,,
,, præ-radobojense. *Ett. and Gard.*
Sophora Europæa. *Ung.*
Cæsalpinia æmula. *Heer.*
,, Haidingeri. *Ett.*
Cassia præ-phaseolites. *Ett. and Gard.*
,, Phaseolites. *Ung.*
,, Berenices. ,,
Cassia hyperborea. *Ung.*
,, Fischeri. *Heer.*
,, Ungeri. *De la Harpe.*
,, præ-memnonia. *Ett. and Gard.*
,, Memnonia. *Ung.*
,, præ-sagoriana. *Ett. and Gard.*
,, Sagoriana. *Ett.*
,, pseudoglandulosa. *Ett.*
,, Feroniæ. ,,
,, Zephyri. ,,
,, præ-lignitum. *Ett. and Gard.*
,, præ-stenophylla. ,, ,,
Copaifera prisca. ,, ,,
,, Harpei. ,, ,,
,, Veledæ. ,, ,,
Podogonium Sheppyense. *Ett. and Gard.*
,, obtusissimum. ,, ,,
Leguminosites callisemæfolius. *Ett. and Gard.*
Leguminosites pachyphyllus. *Ett. and Gard.*

Mimoseæ.

Acacia Sotzkiana. *Ung.*
,, Britannica. *Ett. and Gard.*
Mimosites præ-cassiæformis. *Ett. and Gard.*

PLANTÆ IRCERTÆ SEDIS.

Carpolithes elliptico-valvatus. *Ett. and Gard.*
Carpolithes tricoccinus. *Ett. and Gard.*
,, Napæarum. ,, ,,
,, Alumensis. ,, ,,
,, præ-boveyanus. ,, ,,
,, crassipes. ,, ,,
Phyllites cecropioides. ,, ,,
,, ficoides. ,, ,,
,, lantanoides. ,, ,,
,, apocynoides. ,, ,,
,, crassipes. ,, ,,
,, densinervis. ,, ,,
,, Nimrodis. ,, ,,
,, elæocarpoides. ,, ,,
Phyllites syringæformis. *Ett. and Gard.*
,, hederaceus. ,, ,,
,, arbutoides. ,, ,,
,, euphorbioides. ,, ,,
,, Gargantuæ. ,, ,,
,, myrtaceus. ,, ,,
,, Nerthi. ,, ,,
,, franguloides. ,, ,,
,, Veledæ. ,, ,,
,, Freæ. ,, ,,
,, sapindoides. ,, ,,
,, Hilticis. ,, ,,
,, simaruboides. ,, ,,
,, hymenæoides. ,, ,,

March 18, 1880.

THE 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 Structure of the Immature Ovarian Ovum in the common Fowl and in the Rabbit. To which is appended some Observations upon the Mode of Formation of the *Discus Proligerus* in the Rabbit, and of the Ovarial Glands or 'Egg-tubes' in the Dog." By E. A. Schäfer, F.R.S. Received March 3, 1880.

[Plates 2, 3, 4.]

The Ovarian Ovum of the Bird.

In the ovary of a laying hen, Graafian follicles are to be found in almost every stage of growth, and the fundamental parts—germinal spot, germinal vesicle, yolk, and follicular epithelium—exhibit differences both in relative size and in structure at various stages of growth. I will proceed to consider the structure of each fundamental part in detail, as it is presented in sections of the ovary prepared after hardening in picric acid and alcohol, and stained with logwood or with magenta.

Germinal Spot.—In *larger* ovarian ova, those which are already ·5 millim. and upwards in diameter, the germinal spot appears homogeneous and stains of a uniform dark colour with logwood (fig. 1). It is either completely spherical in shape, or somewhat ovoid. In the latter case it is very commonly eccentric and placed close to the wall of the germinal vesicle, with which it may appear to be blended. When spherical it is often situated near the centre of the vesicle; and from its periphery, in what may be considered a typical condition, filaments of great tenuity radiate outwards to the wall of the vesicle, to the inner surface of which they are again attached, and that so closely that it frequently happens that when shrunken from the action of reagents, the filaments may break in the middle rather than suffer themselves to be dragged away from the wall (fig. 1, *g*). These radiating filaments ramify and intercommunicate in such a way as to form a delicate network (intra-nuclear network) which as we have seen serves to connect the germinal spot with the inner wall of the germinal vesicle. The network is hardly at all stained by logwood, thus presenting a marked contrast to the substance of the germinal spot; moreover the nodal points are not marked and the whole structure is comparatively indistinct. It is just such an appearance as can be produced in an albuminous fluid by coagulating reagents, and I am at present unable to decide clearly in my own mind, whether it is natural or has thus been formed artificially. In other ova, chiefly those of smaller size (figs. 2, 3, 4, and 8), the germinal spot is composed of two distinct substances, a homogeneous matrix (*ma.*) staining but slightly, and a number of coarse granules which are darkly stained by the logwood. The granules may be scattered equally thickly over the whole matrix, or they may leave a peripheral layer of the spot clear, or again there may be a clear part in the centre (fig. 3, *c*) as well as at the periphery of the spot(1).* This last condition I have especially noticed in some of the smallest ova. As a rule the intra-nuclear network above described in the larger ova is not to be seen in the smallest ones. This may be accounted for by the assumption that the network is formed subsequently by a protrusion of the homogeneous substance of the spot; *i.e.*, if we regard the network as a natural formation. On the other hypothesis, namely, that it is an artificial production, we must assume that there is less albuminous matter in the fluid which fills the germinal vesicle of the smaller ovum, to what there is precipitated around the spot, and does not adhere together in the form of reticulating filaments. In almost all these smaller ova the germinal spot lies near the wall of the vesicle.

Germinal Vesicle.—In all the ova the wall of the germinal vesicle is a very distinct membrane which becomes stained by logwood, but not very darkly. When a portion of the membrane of the germinal vesicle of one of the larger ova is separated and examined on the flat, it has a finely punctated appearance, and in profile it can be seen that this is due to minute pits with intervening thickenings upon the inner surface of the membrane (fig. 7, *a* and *b*). (2) The action of the reagents employed generally produced a shrinking of the vesicle, so that its outline in many cases became irregular (fig. 3), but where this shrinking has not taken place, it is almost perfectly spherical except in the largest ova. With the exception of the germinal spot the contents of the germinal vesicle in the younger ova remain absolutely clear and unstained, so that we must either assume that there is very little albuminous matter naturally present, or, as above suggested, that this has been precipitated around the germinal spot during the process of hardening the tissue. In one small ovum two

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[*] The small numbers above the line indicate references to the literature of the subject. These will be found at the end of the paper.

germinal vesicles were observed in close contiguity. They were of unequal size, and each had a relatively large macula. In another, a figure of which is given (fig. 8), there are also two vesicles, but one of these is evidently in process of disappearance, for the membrane has become absorbed and the contents of the vesicle, including the macula, are becoming assimilated in appearance to the vitellus.

Yolk.—In all the ova the yolk seems composed of a network of closely interlacing filaments (3). The network may be coarser or finer; it is very fine indeed in the peripheral layers of the largest ova (those above a millimetre in diameter) which generally have a finely granular aspect (fig. 10, I, II); coarser in the smaller ova and in the central parts of the larger, but in the latter the coarse appearance of the network seems chiefly due to the accumulation here of the large spherical yolk globules, which being left clear in the sections, give a reticulated appearance to the substance in which they are imbedded. In the smallest ova there is nothing but clear fluid in the meshes of the network, which is comparatively open. But in almost all there are condensations of the network (fig.6, *c*,) to be seen, in which the filaments are far more closely interlaced, the appearance presented being very like that which might be produced by gathering up a net made of string into a bunch at one part. The filaments of the vitelline substance are but slightly stained with logwood, but since they are more closely placed in the condensations here spoken of, these are more stained than the rest of the yolk. Eventually as the ova increase in size the condensations seem to become more numerous (fig. 5), and to spread, and the vitelline network to become eventually closer and finer everywhere, but especially at the periphery of the ovum and around the germinal vesicle, the deposit of the yolk globules producing, as we have seen, a coarser appearance in the more central parts.

Other condensations (fig. 9) of vitelline substance occur in some ova which are more circumscribed and definite than those just mentioned. They are nucleus-like, spheroidal patches, which stain deeply with magenta, less deeply with logwood, are homogeneous in appearance, and are usually encircled with a corona of short, straight, radiating filaments, which do not interlace with one another like those of the vitelline substance around, but may however (I am not sure of this) be continued into the vitelline network of these *pseudo-nuclei*, as I will term the structures in question. There may be one, two, or even a greater number in the vitellus of the same ovum, with the germinal vesicle of which they have no apparent connexion.

I am unable to conjecture the meaning of these pseudo-nuclei, or to decide whether they are of the same nature as the "yolk-nuclei" which have been mentioned by several observers as occurring in the ova of fish and Amphibia (4).

In the largest ova the yolk exhibits several distinct layers which shade off the one into the other. Close to the surface is a thin layer (fig. 10, I) very clear and almost free from granules, or these are of extreme minuteness; next there is a finely granular looking layer (II), considerably thicker than the last, in the deeper part of which are imbedded the elements of the white yolk (III), which thus forms the third layer, and which passes gradually into the main central substance, or yellow yolk (IV). It is difficult to observe the interlacing filaments of which the vitelline substance is in all probability composed: even with the highest powers the superficial layers of the yolk of these larger ova show little more than an appearance of fine punctation.

Peculiar striated systems of fibres (figs. 12 and 13) are to be seen occasionally in the vitellus of some of the larger ova. They are generally situated near the end or ends (for there may be two such systems in one ovum) of the ovum, and call to mind some of the amphiastral appearances of dividing nuclei. The fibres composing them do not become more darkly stained with magenta than the rest of the vitellus, but they are very obvious both on account of their peculiar arrangement (each system consisting of two triangular halves diverging from a common centre) and on account of the fact that they lie in a substance which is clearer and stains less than the rest of the vitellus.

In the ovum figured (fig. 12) one of these striated systems is situated in a projection of the vitellus. I draw attention to this because it may mean that the vitellus just at the neck of the projection where the system is situated is in a condition of contraction, and that the appearance described is characteristic of that condition. On the other hand, it is just possible that the figures in question are actually karyolitic, and derived from some extruded portion of the contents of the germinal vesicle. In this case the phenomenon is probably connected with the precocious formation of polar globules. I say "precocious," because the ovum in question was far from maturity (5).

Membranes of the Ovum.—I have only one remark to make about the external coverings of the bird's ovarian ovum, and it concerns the *zona radiata*. Waldeyer states that this membrane or layer is derived from cells of the follicular epithelium, and is continuous with their substance (6). I find on the contrary that in good sections the zona is completely distinct from the follicular epithelium, and its outline is more sharply defined externally than internally (fig. 11, *z. r.*). Moreover, where the epithelium has accidentally been broken away, portions of its cells do not adhere to the zona, as they would tend to do if their substance were continuous with it. I am inclined with Balfour (7) to regard the *zona radiata* therefore as a product of the protoplasm of the ovum. I have not been able to gain any information with regard to the origin of the vitelline membrane which might enable me to decide whether it is a product of the yolk or of the follicular epithelium.

Nor can I say whether the clear interval between the follicular epithelium and the *zona radiata* in fig. 11, and which seems to be bridged across here and there by processes of the follicular epithelium cells, is the section of a homogeneous "chorion," or merely a layer of clear coagulated fluid (8).

Epithelium of the Graafian Follicle.—The follicular epithelium of the smallest follicles is a single layer of oblong or cubical cells (figs. 5, 6, 8, 9, *f. e*). As the follicle grows the cells become more columnar. In larger follicles (figs. 10 and 11) the epithelium is two or more cells deep (9). The cells are not arranged in layers, but irregularly disposed. They are for the most part rounded or angular in form, but between these smaller cells other longer ones are met with, which extend through the thickness of the epithelium, and are pyriform in shape, generally with the stalk of the pear directed outwards (fig. 10, *p*). These seem to be the cells which Waldeyer figures as being continuous with the *zona radiata*, but, as will be seen from what has been stated above, I have not been able to confirm their continuity; indeed, the larger end of the cell does not always reach the inner border of the follicular epithelium. On the other hand, the stalk of the pear always extends at least to the outer border of the epithelium, and may even in some cases be traced into the connective tissue coat of the follicle. These cells call to mind the pyriform cells which cover the surface of the developing Elasmobranch ovary, and which also are traceable at their attached smaller ends into the stroma of the organ (10).

Ovarian Ovum of the Rabbit.

The following description is derived from the study of sections of the ovaries of rabbits, which were either full grown or nearly so. The ovaries were removed while still warm and placed in a concentrated solution of picric acid. After hardening in this, they were transferred to alcohol, which was frequently changed. Sections were then prepared and stained either with magenta dissolved in oil of cloves, and subsequently mounted in Canada balsam, or they were stained with Beale's carmine and mounted in glycerine, or sometimes in balsam. In sections of such ovaries, as is well known, permanent ova and Graafian follicles are to be found in every stage of growth.

Germinal Spot.—In small ova the germinal spot, as in the bird, is generally represented by a collection of darkly staining granules, imbedded in a homogeneous, or very finely granular matrix (figs. 14—20). The outline of the macula is generally less well defined than in corresponding ova of the bird, and the shape is more irregular. This granular condition of the macula is not confined to the small ova, for it occurs also in those which are surrounded by a double layer of follicular epithelium (fig. 20), and even in some which are contained in large Graafian follicles in which there is already a considerable accumulation of *liquor folliculi.* In the latter case the matrix abovementioned is absent, and the macula is represented by a number (a dozen or so) of globules of varying size which appear to lie loose within the germinal vesicle (fig. 23, *b*). An intra-vesicular network is sometimes present, and serves to unite the granules of the macula, which are then few in number and individually larger (figs. 21 and 22). It is possible that the homogeneous matrix above described may represent the remains of such a network, the filaments of which have shrunk up into a mass on contact with the hardening reagent; but if we are to assume this for some cases, it is difficult to understand why it should not take place in every case.

Germinal Vesicle.—With the exception of the germinal spot or spots, and the intra-vesicular network when present, the contents of the germinal vesicle are perfectly clear and do not become stained to any appreciable extent. The vesicle is either completely spherical in shape or it may present one or more bulgings (fig. 18), and these bulgings are frequently turned towards the nuclei presently to be described as making their appearance in the peripheral layer of the vitellus. In some of the sections the wall of the germinal vesicle seems to be deficient for a small extent (fig. 14, *v. g.*), as if to permit of an admixture of its contents with the yolk; but this appearance may be due to the wall having become so thinned as to be imperceptible. In some of the smallest Graafian follicles two germinal vesicles are seen within one ovum (fig. 26). It is possible that one of these may afterwards disappear, *i.e.*, that the follicle is really a nest, containing two fused primitive ova; or, on the other hand, that the follicle and ovum are about to divide.

Vitellus.—In small ovarian ova the vitellus, as in the corresponding ova of the bird, is in the form of a comparatively open network of anastomosing filaments (figs. 21, 22). These tend to become collected more thickly together immediately around the germinal vesicle, and also at the periphery of the vitellus. In larger ova a network is no longer recognisable, but the vitellus acquires a uniformly granular aspect, with generally a number of larger and more distinct granules imbedded in it. In maturing ova the vitelline granules are chiefly collected immediately in contact with the inner surface of the *zona pellucida*, but are also distributed throughout the whole vitellus, disposed, it has sometimes appeared to me, in closely anastomosing tracts, leaving clear intervals between.

The *zona pellucida* of the rabbit's ovum is late in becoming formed. Even when the epithelium in the Graafian follicle is composed of two layers of well-marked columnar cells, it is still thin and the radiating pores cannot be detected in it. In later stages of growth the pores are distinct enough, and they often appear to be occupied by minute granules which are like the vitelline granules on the one hand, and, on the other hand, are indistinguishable from those in the layer of granular material which lies between the outer surface of the *zona pellucida* and the innermost layer of cells of the *discus proligerus* (fig. 23, *a*). There can be little doubt that this granular matter is produced by the cells of the discus, and I cannot but conjecture with Waldeyer (11) that the granules in question pass bodily through the pores of the zona to add themselves to the food material of the vitellus. I have seen no appearances such as those represented by Pflüger (12) and Lindgren (13) of the passage of entire cells of the discus through the zona (14).

Follicular Epitheluim.—In the smallest Graafian follicles which are met with in the ovary of the grown rabbit, the epithelium of the follicle consists of a single layer of flattened cells, which tend, however, to be thicker and more cubical on one side of the follicle, that, namely, which is further from the surface of the ovary (fig. 25) (15). As the ovum and follicle grow, the epithelium cells multiply and become uniformly columnar, but long remain in a single layer (figs. 14, 15, 16, 18, 21). Presently another layer is formed within this first one (fig. 17). This inner layer of the follicular epithelium (*discus proligerus*) in the rabbit appears to be formed at the expense of the ovum. At least, this is the only interpretation which I can adopt of the remarkable appearances I am about to describe.

Formation of the Discus Proligerus in the Rabbit.—If we examine those Graafian follicles in the rabbit's ovary whose follicular epithelium is still composed of but a single layer of well-marked columnar cells, it may frequently be observed that one or more nuclei are imbedded in the peripheral layer of the vitellus (fig. 14, *n*), and this part of the vitellus may be marked off from the rest by a sector (fig. 16). The nuclei in question are oval, with their long diameter parallel to the circumference of the ovum, and at right angles therefore to the long axis of the columnar cells and oval nuclei of the follicular epithelium. The protoplasm in which they lie is more sharply marked off from this epithelium than it is from the vitellus, and, indeed, it looks like a part of the latter cut off, for it has the same finely granular appearance, is stained similarly to that with carmine and magenta, and is bounded externally by a direct continuation of the general circumferential line of the ovum. Some ova are met with which have only one of these nuclei, others, in which there are two or more. When more than one, they may make their appearance in different parts of the periphery, and these parts being segmented off, the ovum, from being round may acquire a very irregular shape (fig. 22).

As before mentioned, the germinal vesicle is often bulged towards that part of the periphery of the vitellus where the nuclei are situated (fig. 18), and with care fine striations may occasionally be detected traversing the interval. Such appearances incline one to the belief that the nuclei are derived from the vesicle, but scarcely by a distinct process of cell-division. At least, I have never been able to detect anything in the shape of a division-spindle. The only other possible explanation is that the nuclei and cells in question are derived from cells of the follicular epithelium, which are situated in a different plane from that included in the section, and burrow obliquely between the first formed layer and the vitellus of the ovum, imbedding themselves in the surface of the latter. But this would not account for the sharpness with which they are always marked off from the follicular epithelium, and the fact that both nuclei and cell-substance differ materially in appearance and in their reaction to staining fluids from the nuclei and cell-substance of the first formed layer of epithelium.

However first formed, they soon multiply and spread around the whole of the ovum, so as to form a second complete layer of follicular epithelium, the cells of which in parts may be two deep. They maintain for a time their disposition parallel to the circumference of the ovum and transverse to the direction of the cells of the first formed layer (fig. 17). Moreover, they still exhibit the same behaviour towards staining fluids as the vitellus itself. But as the follicle continues to grow, and as the *zona pellucida* becomes formed between these cells and the vitellus, they acquire a columnar form with the long axis of the cell perpendicular to the surface of the ovum, their contents clear up, and they come to resemble the cells of the first formed layer in every particular (fig. 20).

The only statement that I know of which suggests a parallel between these appearances in the rabbit's ovum and others of a similar nature elsewhere occurs in the description which Kuppfer gives of the formation of an inner layer of follicular epithelium (which is said to develop into the test) from nuclei which make their appearance in the peripheral layer of the vitellus of the ovum of *Ascidia canina* (16). Unless it is permitted to cite the observations of Kleinenberg upon the formation of a layer of cells from the periphery of the ovum of *Hydra*, which cells take no part in the development of the ovum, but are simply transformed into a capsular envelope (17).*

I have not been able to see anything of the kind in the Graafian follicles of other Mammals (cat, dog, man).

Ovarian Ova of the Bitch.

The young ova (figs. 27 to 29) have a germinal vesicle which contains a germinal spot and a beautiful intravesicular network. Attached to one side of the germinal vesicle is the main part of the solid substance of the vitellus, and this vitelline mass is connected

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* There is one important difference between these cases and the appearances in the rabbit's ovum above described, viz., that in them the separation of cells from the vitellus occurs after fertilisation.

to the periphery of the ovum by branching processes. The rest of the contents of the ovum is clear.

Post-Embryonic Formation of Graafian Follicles in the Bitch.

In sections of the ovary of a bitch-pup of eighteen days it is easy to convince oneself of the glandular mode of formation of the Graafian follicles and their contents. Figs. 30 to 33 are representations of such ovarial glands, showing their communication at this period with the surface of the ovary. In fig. 30 this communication is direct, and the whole structure bears a striking resemblance to a racemose gland whose saccules are filled with cells; in fig. 31 the glands are longer and more irregular, and the duct, if we may call it so, does not pass straight to the surface, but in an oblique or even convoluted manner. It is impossible to confound glandular tubes of this description with creases upon the surface of the ovary, as Foulis suggests that Pflüger and Waldeyer have done (18), especially since in many cases where the tubes are cut across, the lumen can be distinctly seen (Fig. 32).

We can see in the figures how portions of these solid columns of cells are becoming cut off into "egg-nests" by ingrowths of the ovarial matrix. In many of these nests the primitive ova are already distinguishable from the other cells by their size and the clearness of their protoplasm. But it often happens that the distinction does not appear until the group of cells which constitutes the nest is completely separated from the rest of the egg-tube. Fig. 24 from the rabbit's ovary illustrates the same fact.

In sections of the ovary of a young but nearly full-grown bitch the same glandular character of the superficial parts of the egg-tubes of Pflüger is evident (fig. 34). (19) In most the open communication with the surface is lost, and the solid ends of the glands, which form the "egg-columns," properly so-called, have also become severed, and have sunk into the ovarial stroma. But the lumen of the tube is still distinct, and the section is wonderfully like that of the duct of a small secreting gland.

Appearances like these seem to me to point very clearly to the conclusion that the typical mode of formation of the Graafian follicles and their contents is by a gland-like ingrowth of the ovarial epithelium, and that the process, which is described by Balfour, as occurring in the Elasmobranch and in the rabbit, is a modification of this typical mode. For it must certainly be admitted that the deeper-seatedness of the columns of germinal epithelium as the ovary advances in development, cannot be wholly due to the outward growth of the ovarian stroma, since we see how that even after the ovary has attained its full size the Graafian follicles (which are derived from these columns), as they successively mature, sink deeper into the substance of the organ.

It will be seen that the view which I have been led to take of the derivation of the ova and epithelium cells of the Graafian follicles, is that both structures originate in common from the germinal epithelium—in the dog (Pflüger, Waldeyer) in the form of ingrowths which may closely simulate the formation of tubular glands (ovarial glands); in the rabbit (Balfour) in the form of columns of cells which are altogether solid, and which thus present a modification of what is in all probability, as H. Ludwig's extensive comparative researches have shown, the typical mode. This is essentially the view first enunciated by Pflüger (20) for the vertebrate ovary, and differs materially from that of Foulis, who contends for the derivation of the follicular epithelium from the connective tissue cells of the ovarian stroma. I think that Dr. Foulis could not possibly have arrived at this result had he at first selected the ovary of the dog for study. Above all the investigation of the human ovary in this respect teems with difficulties, both because it cannot be obtained fresh enough, and on account of the great relative development of the stroma. On Dr. Foulis' hypothesis that the first formed layer of follicular epithelium is only modified connective tissue, the development in the rabbit of the inner follicular epithelium from the ovum itself, as I have described it, would be quite incomprehensible, whereas, if both ova and epithelium have a common origin, there would be no morphological absurdity involved in this apparently exceptional mode of development; especially since, as V. Beneden (21) and Balfour have shown, the permanent ova are for the most part produced by the fusion of two or more cells (primitive ova), which originate from the germinal epithelium in common with the first formed layer.

REFERENCES TO LITERATURE.

1. Compare Eimer (" Untersuchungen ueber die Eier der Reptilien," I. " Archiv für Mikroskopische Anatomie," VIII, p. 218.)

2 Eimer (*loc. cit.*, p. 119) describes actual pores as existing in the much thickened membrane of the germinal vesicle of the ova of some reptiles.

3. This appearance has been remarked by several observers in hardened ova of the fish, reptile, and bird, but evidence is wanting to prove that it may not be a precipitation effect of the hardening reagent.

4. The pseudo-nuclei here described are very different in appearance from the yolk-nucleus which Dr. Allen Thomson has figured in the ovum of the frog (" Cyclopædia of Anatomy and Physiology," Article "Ovum," fig. 65B); nor do they in any way resemble the " Dotterkerne " and "Dotterschorfe," which Eimer describes in reptilian (*loc. cit.*, pp. 225 and 226) and Balfour in Elasmobranch ova ("Structure and Development of the Vertebrate Ovary," " Quarterly Journal of Microscopical Science," vol. xviii, p. 411).

5. The systems of fibres here described may be analogous to the pyramidal systems

of striæ described by A. Schultz, in ovarian ova of the Torpedo. ("Zur Entwickelungsgeschichte des Selachier-eies, "Arch. f. mikr. Anat.," XI, p. 577.)

6. Waldeyer, "Eierstock und Ei," p. 62, and fig. 25.
7. Balfour, *loc. cit.*
8. Gegenbaur ("Eier der Wirbelthiere mit partieller Dotterfurchung, Müller's Archiv." 1861, p. 517) has described a similar homogeneous layer in the reptilian ovum.
9. This is certainly the case in spite of Waldeyer's reiterated assertion to the contrary (*loc. cit.*, p. 58; and article "Eierstock," in Stricker's "Handbuch der Gewebelehre," p. 557).
10. Balfour, *loc. cit.*, p. 391.
11. Waldeyer, "Eierstock und Ei," pp. 40 and 47.
12. E. F. W. Pflüger, "Untersuchungen zur Anatomie und Physiologie der Eierstocke der Säugethiere." 4te. Mittheilung. "Med. Centralzeitung," 1862. No. 90.
13. Hj. Lindgren. "Ueber das Vorhandensein von wirklichen Poren-kanälchen in der *zona pellucida* des Säugethieres," &c. "Arch. f. Anat. u. Phys." Anatomische, Abtheilung, 1877, p. 360.
14. Wagener (Bemerkungen ueber den Eierstock und den gelben Körper," "Arch. f. Anat. u. Phys." Anatomische, Abth. 1879, p. 181) describes and figures a connexion of the cells of the *discus proligerus* with the substance of the vitellus through the pores in the *zona pellucida*.
15. Compare Balfour, *loc. cit.*, p. 427.
16. Kuppfer. "Die Stammverwandtschaft zwischen Ascidien u. Wirbelthieren," "Arch. f. mikr. Anat.," V, 459.
17. N. Kleinenberg. Hydra. Leipzig, 1872.
18. J. Foulis, "On the Development of the Ova and structure of the Ovary in Man and other Mammalia." "Transactions of the Royal Society of Edinburgh," 1875, p. 362.
19. Compare Waldeyer, *loc. cit.*, pp. 30 *et seq.*
20. "Untersuchungen," &c., 3te Mittheilung. "Med. Centralzeitung," 1862, No. 88; and in his important monograph, "Ueber die Eierstocke der Säugethiere und Menschen," 1862.

    F. Valentin ("Ueber die Entwickelung der Follikel in dem Eierstocke der Säugethiere," "Müller's Archiv.," 1838) described the glandular tubes in the ovary, but failed to recognise their connexion with the ovarial epithelium—the true nature of which was not indeed recognised until Waldeyer drew especial attention to it.
21. Ed. van Beneden. "Composition et signification de l'œuf," 1868.

DESCRIPTION OF THE FIGURES.

Figs. 1 to 13 are from sections of hens' ovaries, portions of which were hardened first in picric acid, and subsequently in alcohol. The sections were made after imbedding by the method of soaking with cacao-butter ("Course of Practical Histology," p. 92), and were stained either with logwood or with magenta.

Fig. 1. Germinal vesicle, germinal spot, and intra-vesicular network of a small ovarian ovum (·305 millim. in diameter). Magnified 250 diameters.

m. v. Membrane of germinal vesicle.

g. Break in the intra-vesicular network.

Fig. 2. Germinal spot of small ovum showing matrix and imbedded granules composing the macula.

m. v. Membrane of germinal vesicle to which the macula is adherent.

ma. Homogeneous matrix of germinal spot in which darkly stained granules are imbedded.

Diameter of ovum ·488 millim. Magnified 250 diameters.

Fig. 3. Germinal vesicle and germinal spot of a very small ovum.

m. v., *ma.*, as before.

c. Central clear part of macula. (Diameter of ovum ·122 millim.; of germinal vesicle ·054 millim.) Magnified about 600 diameters.

Fig. 4. *Macula germinativa* of a very small ovum. (Diameter of ovum ·122 millim.; of macula ·049 millim.) Magnified 400 diameters.

Fig. 5. Small Graafian follicle with ovum, showing two or three nucleus-like condensations of the vitellus. (Diameter of ovum ·3 millim.) Magnified 120 diameters.

Fig. 6. Smaller ovum (·264 millim.) than that represented in fig. 5, but more highly magnified, showing a condensation, *c*, of the vitelline network.

Fig. 7. Small piece of the wall of the germinal vesicle of a moderately sized ovum (1·07 millim., germinal vesicle ·195 millim).

(*a.*) On the flat.

(*b.*) Seen in optical section.

The membrane is seen to have a granular appearance.

Fig. 8. Very small ovarian ovum (diameter ·11 millim.) with follicular epithelium, germinal vesicle, and germinal spot; and with a peculiar insulated body in the vitellus, which is apparently the remains of a second germinal vesicle and spot in process of disappearance.

Fig. 9. Part of a small ovum (·3 millim.) with two pseudo-nuclei in vitellus.

f. e. Follicular epithelium.

y. Yolk substance forming a network.

g. v. Part of the germinal vesicle.

Fig. 10. Superficial layers of a comparatively large ovum (1·586 millim.)

f. e. Follicular epithelium.

p. Pyriform cell.

I. Clear superficial layer of vitellus; II. Finely granular layer—its granules are largest near tho first layer; III. White yolk; IV. Superficial part of yellow yolk. Drawn under a magnifying power of about 500 diameters.

Fig. 11. Superficial part of a still larger ovum (2·44 millim.).

f. e. Follicular epithelium.

z. r. *Zona radiata.*

Fig. 12. Thick section of a comparatively large ovum, showing the peculiar systems of fibres described in the text. Slightly magnified (about 10 diameters).

Fig. 13. The upper part of the previous figure. Magnified 40 diameters.

Figs. 14 to 26 are from sections of rabbits' ovaries.

Fig. 14. Small Graafian follicle, the follicular epithelium forming a single layer of columnar cells.

v. g. Germinal vesicle, the wall of which seems to be deficient at this part.

m. Macula, composed of a mass of granules, imbedded in a homogeneous matrix.

n. Nucleus in the periphery of the vitellus. 570 diameters.

Fig. 15. Small Graafian follicle, not yet completely cut off from the rest of the

"Ei-schlauch." The germinal vesicle seems to be connected by fine lines with two small nuclei at the periphery of the vitellus.

Fig. 16. Small Graafian follicle, similar to that represented in Fig. 14. At one side two nuclei are seen in the peripheral part of the vitellus. The area in which they lie is marked off by a line from the rest of the vitellus. Indistinct striæ pass from here towards the germinal vesicle. 570 diameters.

Fig. 17. Graafian follicle, with a complete inner layer of follicular epithelium. This differs markedly from the outer layer in the appearance and arrangement of the cells. The irregular outline of the ovum is to be remarked. Drawn under a magnifying power of about 350 diameters.

Fig. 18. Small follicle, with single layer of epithelium. Germinal vesicle presents a bulging on that side of it which is turned towards a nucleus-like body in the periphery of the vitellus. The figure is otherwise like figs. 14 and 16, but somewhat less magnified.

Fig. 19. Small follicle, showing three cells in periphery of vitellus. Magnified about 240 diameters.

Fig. 20. A Graafian follicle, much further advanced towards maturity than any of those above represented. The cells of the inner layer of the follicular epithelium have by this time assumed their characteristic columnar form and radiating arrangement. The *macula germinativa* is, as before mentioned, formed of a homogeneous matrix, with dark granules imbedded in it. The *zona pellucida* is now distinct. Magnified 570 diameters.

Fig. 21. Small follicle, with ovum. An intra-nuclear network connects a small germinal spot with the wall of the vesicle. The vitellus is in the form of a network of filaments. Magnified about 200 diameters.

Fig. 22. Small follicle, with outer layer of epithelium complete and a few cells of inner layer formed between this and the ovum. The germinal vesicle contains three small germinal spots united by a network. The vitellus also exhibits a network of delicate filaments. Magnified about 450 diameters.

Fig. 23 *a*. A small portion of the *zona pellucida* of an ovum from a large Graafian follicle, together with some of the cells of the *discus proligerus*, which are mixed with it. A layer of granular substance is seen to intervene between these cells and the outer surface of the zona, and granules similar to those of this substance can be traced through pores in the zona into continuity with the granules of the vitellus. Drawn under a power of about 750 diameters.

Fig. 23 *b*. Germinal vesicle of an ovum from a large follicle.

m. v. Membrane of vesicle. The *maculata germinativa* is in the form of a number of spherical globules, one of which is much larger than the rest. The remainder of the contents of the vesicle is clear.

Fig. 24. A row of four so-called "nests" from near the surface of the ovary of a young rabbit. The egg-cell has not yet increased in size beyond the other cells of the nest. Magnified about 450 diameters.

Figs. 25 and 26. Two very small follicles, with young ova. In fig. 26 two germinal vesicles are seen. From the same ovary and magnified to the same extent as fig. 24.

Figs. 27 to 35 are from sections of the bitch's ovary.

Figs. 27, 28, and 29 represent small Graafian follicles, containing young ova. In each the germinal vesicle is seen to contain an intra-vesicular network, and the vitellus is collected into a patch which is in contact with the germinal vesicle, and is connected with the periphery of the ovum by ramified offsets.

Fig. 30. Section of superficial part of ovary of a bitch pup, eighteen days old, showing gland-like connexion of egg-tubes with the surface of the ovary.

str. Ovarial stroma.

Fig. 31. From another section of the same ovary, showing more oblique connexion of egg-tubes with the surface.

o. e. Ovarial epithelium. Stroma not represented.

Fig. 32. From another part, showing two other of the gland-like connexions cut across and obliquely.

Fig. 33. A very oblique connexion, cut longitudinally.

Fig. 34. Section of superficial part of ovary of a young bitch, almost full-grown, showing some of the ovarial glands (connexions of egg-tubes with surface of ovary). Cut across and obliquely.

Fig. 35. A peculiar convoluted tube from the deeper part of the same ovary, almost filled with long, tapering columnar cells.

a. Either the same or a similar tube, cut across.

II. "Researches into the Colouring Matters of Human Urine, with an Account of the Separation of Urobilin." By C. A. MacMunn, B.A., M.D. Communicated by A. Gamgee, M.D., F.R.S., Brackenbury Professor of Practical Physiology and Histology in Owens College, Manchester. Received March 6, 1880.

(Abstract.)

In this paper an account of the spectroscopic and chemical characters of urobilin is given. Urobilin is a pigment which has been diagnosed in urine by means of the spectroscope, and which has not been hitherto isolated.

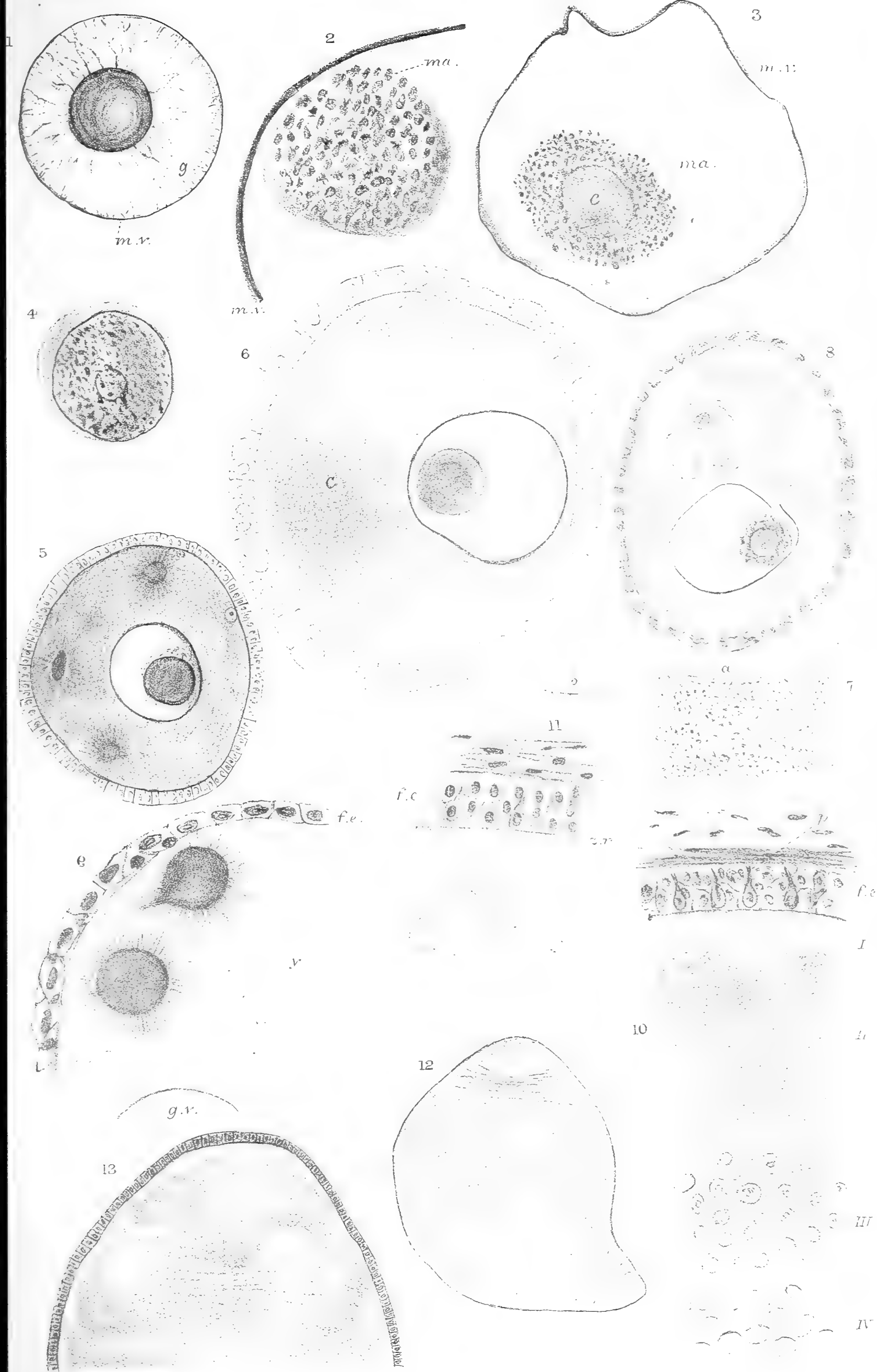
Before its discovery in urine a pigment had been obtained by Jaffé by acting on human bile with nitric acid, and on dog's bile with hydrochloric acid, and subsequent treatment, which in solution gave the same spectrum, and behaved in the same manner with reagents as urine containing urobilin. By an examination of the bile of seventeen animals, I have shown that it is present as such in the bile of all them, but in greatest abundance in the bile of the mouse; and the results are described in the paper, while the spectra are shown in Chart II.

I have also shown that human urine always gives an absorption band at F, which almost always is affected in the same manner by reagents as the pigment got by Jaffé from bile, and that if this band is not so affected another pigment called urolutein is present, which I describe and have mapped in Chart I.

I have succeeded in isolating urobilin by the following method:—Having procured some urine which by preliminary spectroscopic and

Schäfer.

Proc. Roy. Soc. Vol. 30. Pl. 2.



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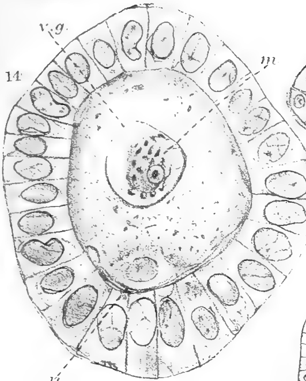
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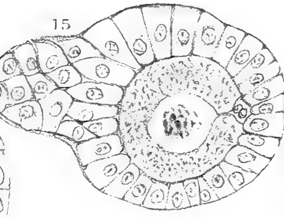
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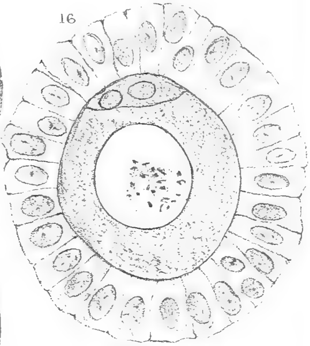
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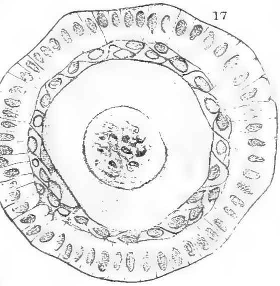
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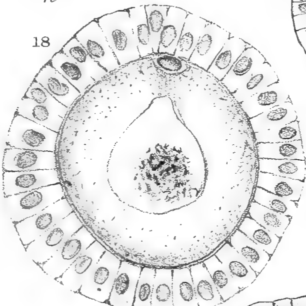
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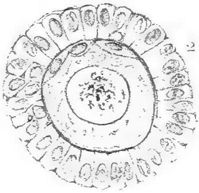
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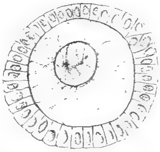
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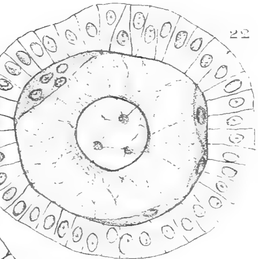
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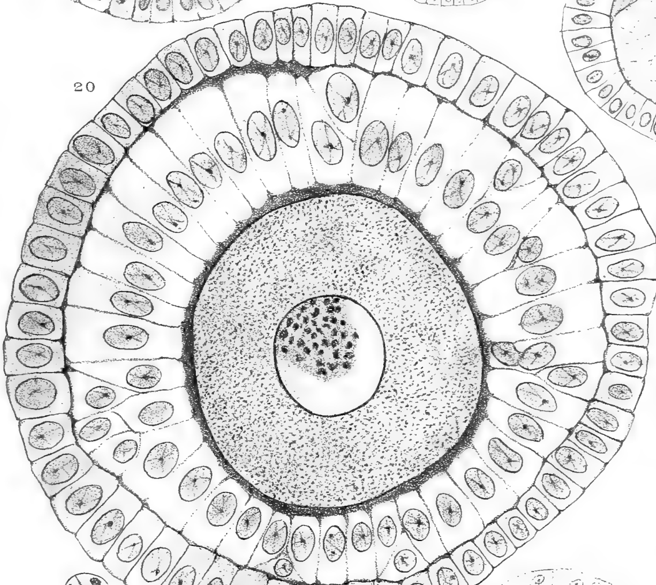
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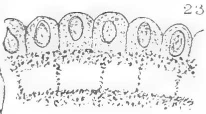
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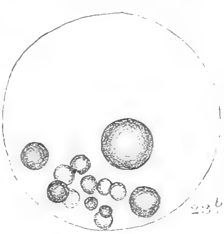
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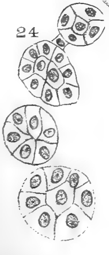
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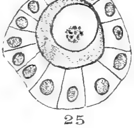
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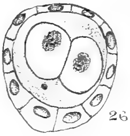
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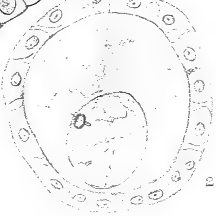
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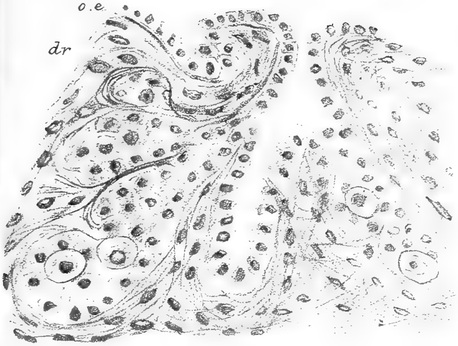

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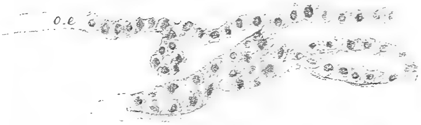

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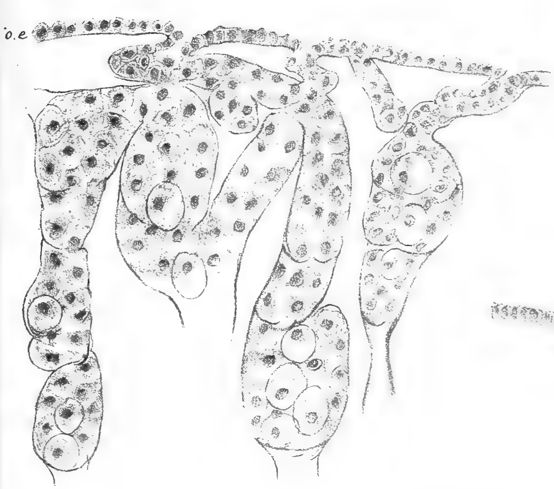

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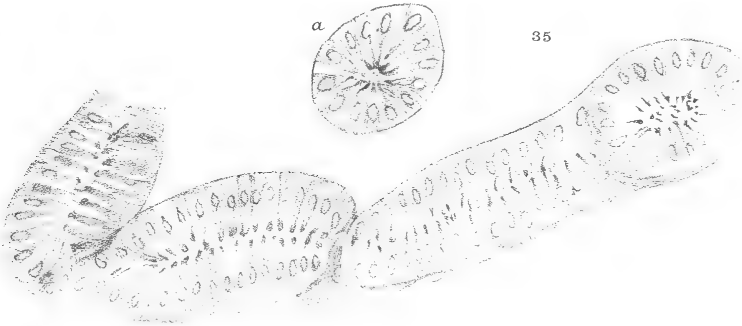

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chemical tests showed a large amount of urobilin, it was precipitated by neutral and basic lead acetate respectively, and filtered; if the filtrate gave an absorption spectrum, it was re-precipitated until the band disappeared, the precipitates were united, extracted with alcohol acidulated either with hydrochloric or sulphuric acid, and filtered. The filtrate was of a fine red colour, giving the dark band at F. This fluid, in small quantities at a time, was put into a separating funnel, a large quantity of water added and then pure chloroform, the whole repeatedly shaken and then allowed to stand. The red chloroform layer was separated off and filtered, when it was again examined and found to give in every instance the original spectrum; the chloroform was now evaporated off and the residue repeatedly dissolved in chloroform, finally on evaporation a brown-red, amorphous, shiny residue was obtained, which was perfectly soluble in alcohol, chloroform, nitric acid, hydrochloric acid, acetic acid, lactic acid, acidulated water, and partly soluble in ether, in water, and in benzol, but quite insoluble in bisulphide of carbon. I fully describe the spectra of the various solutions, which are shown in Chart I, and it will be seen that the same pigment was present in every solution, and since by the action of different solvents and reagents, this pigment could not be separated into more than one, I conclude that pure urobilin was obtained and urobilin only. I have also endeavoured to show that the pigment obtained was in combination with the sulphuric acid, when prepared by the sulphuric acid method, as it was found to contain sulphur, in addition to carbon, hydrogen, oxygen and nitrogen, which was absent and replaced by chlorine when it was prepared by the hydrochloric acid method. The conclusions which this research led me to form were as follows:—

1. That urobilin had been separated from urine.

2. That by the treatment adopted it had been separated in combination with hydrochloric and sulphuric acid respectively.

3. That the spectra of solutions of urobilin obtained by these methods respectively differ in the position of certain feeble bands, but agree in all possessing a black band at F, which can be made to disappear by ammonia in excess, and which is replaced by another band nearer the red end of the spectrum on the addition of sodic hydrate.

4. Urobilin is an amorphous brownish-red pigment, which contains carbon, oxygen, hydrogen and nitrogen. It is soluble in alcohol, chloroform, acidulated water, and acids; partially in ether, benzol, and water, *i.e.*, if the pigment be separated in combination with hydrochloric or sulphuric acid.

5. Urobilin appears capable of existing in different states of oxidation.

6. It is derived from one of the colouring-matters of bile.

7. It appears to be the colouring-matter of the bile of the mouse.

Like hæmoglobin and hæmatin, urobilin appears to be a very unstable body, which easily splits up on treatment with reagents into decomposition products, each giving a peculiar spectrum.

III. "On the Coalescence of Amœboid Cells into Plasmodia, and on the so-called Coagulation of Invertebrate Fluids." By P. GEDDES. Communicated by Professor BURDON SANDERSON, F.R.S. Received March 13, 1880.

[PLATE 5.]

Whether one collects the perivisceral fluid of a sea-urchin or of a worm, or the blood of a crustacean or a mollusc, the same phenomenon is always more or less distinctly to be observed. A kind of coagulation takes place, the fluid separating sooner or later into two portions, which have considerable superficial resemblance to the clot and serum of vertebrate blood.

It is easy to watch the formation of the clot by placing a drop of fresh-drawn fluid upon a cover-glass and inverting this above a glass cell, of which the edge is oiled to prevent evaporation. The drop thus hangs freely and the coagulation can go on without interference.

The phenomena observed in various invertebrates are best understood by reference to the plate. Fig. 1 represents some of the groups into which the amœboid corpuscles of the perivisceral fluid of the earthworm run immediately after drawing. In fig. 2 we have a few adjacent corpuscles from the gill of *Pholas*; in fig. 3 they are commencing to adhere; in 4 and 5 their adhesion is complete; in fig. 6 they have all but completely merged into one mass, which is about to absorb a new-comer; in figs. 8 and 9 the mass is now completely homogeneous, is altering its form and throwing out pseudopodia in all directions. Figs. 10—12 represent the similar union of corpuscles of *Patella*, and figs. 13, 14 those of *Buccinum*.

In *Pagurus* the corpuscles are of two very markedly different kinds, the coarsely and the finely granular. The former are much elongated when freshly drawn, but rapidly become oat- or egg-shaped, and then throw out blunt pseudopodia from any part of their surface. These stages are represented in fig. 15. The finely granular corpuscles, drawn separately at fig. 16, send out filamentous pseudopodia, and alone possess the power of union. Fig. 17 represents a small clot, formed by the union of the finely granular corpuscles, and containing a number of coarsely granular corpuscles, which do not merge into the surrounding mass. The large pseudopodial process of hyaline ectoplasm on the left of the figure is worthy of notice.

Figs. 18—23 show the union of five of the finely granular corpuscles of the common shore-crab (*Carcinus mœnas*). Fig. 24 represents a few of both kinds of corpuscles of *Cancer Pagurus*, and fig. 25 a small clot formed by the union of the finely granular corpuscles. In fig. 26 are represented some of the corpuscles of the common starfish (*Asteracanthion vulgare*), showing the curious looped pseudopodia usual among the Echinoderms. Figs. 27 and 28 are successive drawings of a group of uniting corpuscles from the same preparation. As in fig. 17, the development of large pseudopodia from the completely fused mass is evident.

But it is among the Echinoidea* that the phenomena of corpuscular fusion occur on the most extraordinary scale. A good specimen of *Echinus sphœra* furnishes perivisceral fluid enough to fill an ordinary tumbler. The fluid is at first homogeneous and of a uniform pinkish grey tint, but rapidly becomes cloudy. The cloud contracts constantly, and so becomes gradually darker and of more defined form, and in the course of a few hours shrinks into a small brown pellet. When a drop of this fresh fluid is examined in the moist chamber, it is seen to contain, in addition to its coloured amœboid corpuscles, the same two types of colourless corpuscles, the coarsely and the finely granular, as have just been described in *Pagurus* (figs. 15 and 16). Here too the clot is entirely due to the union of the finely granular corpuscles, which almost instantly run into small heaps, these into larger, and larger ones, and so on until we have a vast amœboid mass, which rapidly differentiates into endoplasm and ectoplasm, the former containing the nuclei and granules of the constituent corpuscles, and the latter, produced by the union of their hyaline ectoplasm, forming a broad clear margin, and sending out pseudopodia, sometimes blunt, but usually filamentous. These pseudopodia frequently run out over such distances that an adequate idea of their extraordinary size could scarcely be given within the limits of the entire plate.

The resemblance of all these cases of the fusion of amœboid cells taken from so many types of Invertebrata is too close to be accidental. All the evidence points to the conclusion that the clot which appears in any invertebrate corpusculate fluid is formed, always partly, and sometimes wholly, by the *fusion* of the finely granular amœboid corpuscles therein suspended.† Thus, while the blood of a lobster is still capable of clotting after removal of the corpuscles by filtration (Frédéricq), the perivisceral fluid of a sea-urchin is not.

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* A detailed account of the corpuscles of *Echinus*, &c., will shortly appear in the "Archives de Zoologie Expérimentale."

† With regard to the amœboid corpuscles of vertebrates, I have not yet been able to make any satisfactory observations. Ziegler has concluded from his researches on the formation of giant cells in exudation liquids (Exp. Untersuch. ueber d. Herkunft d. Tuberkeelmente, &c., Würzburg, 1875) that a coalescence of amœboid cells does actually sometimes take place.

It is impossible to avoid comparing the above-described unions of amœboid cells with those occurring in the ordinary life-history of the Myxomycetes,* and the resemblance is so complete that it seems not only convenient, but necessary to extend the term *plasmodium* to the former.

The formation of plasmodia was at first supposed to be peculiar to the Myxomycetes, but several Rhizopods (*Microgromia*, *Rhaphidiophrys*, *Phonergates*, &c.) have been described, in which a more or less complete cell-fusion has been observed, or perhaps, as I am disposed to think, cell-fusion has been more or less completely observed.

After comparing the descriptions and drawings of these forms† (see Buck's figures of *Phonergates*, copied in the Plate, figs. 30—33) with those of uniting corpuscles already given, the identity of the process of coalescence in the two cases with each other, and with that of a Myxomycete, will be sufficiently obvious.

Finally, although much further observation is desirable, alike upon the undifferentiated cells of Vertebrates and Invertebrates, and upon the lowest plants and animals, it may be safely asserted that all the evidence we possess points to the conclusion that the power of coalescing with its fellows, under favourable circumstances, to form a plasmodium, is at any rate a very widely-spread, if not a general property of the amœboid cell.

The observations were chiefly made last summer at the Scottish Zoological Station at Stonehaven.

EXPLANATION OF THE PLATE.

Fig. 1. Corpuscles of perivisceral fluid of *Lumbricus terrestris* commencing to unite.
Figs. 2—9. Eight successive drawings of the same group of corpuscles (*Pholas*).
Figs. 10—12. Union of group of corpuscles (*Patella vulgata*).
Figs. 13—14. Union of group of corpuscles (*Buccinum undatum*).
Fig. 15. Coarsely granular corpuscles (*Pagurus Bernhardus*).
Fig. 16. Finely granular corpuscles of same animal.
Fig. 17. Small plasmodium (*Pagurus Bernhardus*) formed from finely granular corpuscles, and containing several coarsely granular corpuscles in its interior.
Figs. 18—23. Union of group of corpuscles (*Carcinus Mænas*).
Fig. 24. Group of coarsely and finely granular corpuscles of *Cancer Pagurus*.

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* Sachs. "A Textbook of Botany." De Bary. "Die Mycetozoons." "Zeitsch. f. Wiss. Zool." Bd. X. 1860.

† R. Hertwig. "Ueber *Microgromia socialis*, eine Colonie bildende Monathalamie des süssen Wassers." "Archiv. f. Mikros. Anat." Bd. X. Taf. 1. 1874.

L. Cienkowski. "Ueber einige Rhizopoden und verwandte Organismen." "Archiv. f. Mikros. Anat." Bd. XII. 1876.

E. Buck. "Einige Rhizopodenstudien." "Zeitsch. f. Wiss. Zool." Bd. XXX. 1878. Taf. 2.

P. Geddes del.
West Newman & Co. lith.

Fig. 25. Plasmodium (*Cancer Pagurus*).

Fig. 26. Corpuscles of *Asteracanthion vulgare*, freshly drawn.

Figs. 27—28. Union of a group of corpuscles of *Asteracanthion vulgare.*

Fig. 29. Portion of a plasmodium produced by the union of the finely granular corpuscles of *Echinus sphæra*, showing distinct endoplasm containing the coarsely granular and the coloured corpuscles, and ectoplasm sending out filamentous pseudopodia, which unite with those of free corpuscles.

Figs. 30—33. *Phonergates vorax*, from "Zeitsch. f. Wiss. Zool." Bd. XXX. 1878. Taf. II, figs. 54—57.

All the figures drawn with Verick, Oc. 2, Obj. 7.

IV. "On the Analytical Expressions which give the History of a Fluid Planet of Small Viscosity, attended by a Single Satellite." By G. H. Darwin, F.R.S. Received March 6, 1880.

In a series of papers read from time to time during the past two years before the Royal Society, I have investigated the theory of the tides raised in a rotating viscous spheroid, or planet, by an attendant satellite, and have also considered the secular changes in the rotation of the planet, and in the revolution of the satellite. Those investigations were intended to be especially applicable to the case of the earth and moon, but the friction of the solar tides was found to be a factor of importance, so that in a large part of those papers it became necessary to conceive the planet as attended by two satellites.

The differential equations which gave the secular changes in the system were rendered very complex by the introduction of solar disturbance, and I was unable to integrate them analytically; the equations were accordingly treated by a method of numerical quadratures, in which all the data were taken from the earth, moon, and sun. This numerical treatment did not permit an insight into all the various effects which might result from frictional tides, and an analytical solution, applicable to any planet and satellite, is desirable.

In the present paper such an analytical solution is found, and is interpreted graphically. But the problem is considered from a point of view which is at once more special and more general than that of the previous papers.

The point of view is more general in that the planet may here be conceived to have any density and mass whatever, and to be rotating with any angular velocity, provided that the ellipticity of figure is not large, and that the satellite may have any mass, and may be revolving about its planet, either consentaneously with or adversely to the planetary rotation. On the other hand, the problem here considered is more special in that the planet is supposed to be a spheroid of fluid of small viscosity; that the obliquity of the planet's equator, the inclination and the eccentricity of satellite's orbit to the plane of reference are treated as being small, and, lastly, it is supposed that the planet is only attended by a single satellite.

The satellite itself is treated as an attractive particle, and the planet is supposed to be homogeneous.

The notation adopted is made to agree as far as possible with that of a previous paper, in which the subject was treated from a similarly general point of view, but where it was supposed that the equator and orbit were co-planar, and the orbit necessarily circular.*

The motion of the system is referred to the invariable plane, that is, to the plane of maximum moment of momentum.

The following is the notation adopted:—

For the planet:—

M = mass; a = mean radius; g = mean pure gravity; C = moment of inertia (neglecting ellipticity of figure); n = angular velocity of rotation; i = obliquity of equator to invariable plane, considered as small; $\mathfrak{g} = \frac{2}{5}g/a$.

For the satellite:—

m = mass; c = mean distance; Ω = mean motion; e = eccentricity of orbit, considered as small; j = inclination of orbit, considered as small; $\tau = \frac{3}{2}m/c^3$, where m is measured in the astronomical unit.

For both together:—

$\nu = M/m$, the ratio of the masses; $s = \frac{2}{5}[(a\nu/g)^2(1+\nu)]^{\frac{1}{3}}$; h = the resultant moment of momentum of the whole system; E = the whole energy, both kinetic and potential, of the system.

By a proper choice of the units of length, mass, and time, the notation may be considerably simplified.

Let the unit of length be such that $M+m$, when measured in the astronomical unit, may be equal to unity.

Let the unit of time be such that s or $\frac{2}{5}[a\nu/g)^2(1+\nu)]^{\frac{1}{3}}$ may be unity.

Let the unit of mass be such that C, the planet's moment of inertia, may be unity.

Then we have

$$\Omega^2c^3 = M+m = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

Now, if we put for g its value M/a^2, and for ν its value M/m, we have

$$s = \frac{2}{5}\left\{\left[\frac{aM}{m}\cdot\frac{a^2}{M}\right]^2\frac{M+m}{m}\right\}^{\frac{1}{3}} = \frac{2}{5}\frac{a^2}{m}, \text{ since } M+m \text{ is unity,}$$

and since s is unity, $m = \frac{2}{5}a^2$, when m is estimated in the astronomical unit.

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* "Determination of the Secular Effects of Tidal Friction by a Graphical Method," "Proc. Roy. Soc.," No. 197, 1879.

Again, since $C = \frac{2}{5}Ma^2$, and since C is unity, therefore $M = \frac{5}{2}/a^2$, where M is estimated in the mass unit.

Therefore $Mm/(M+m)$ is unity, when M and m are estimated in the mass unit, with the proposed units of length, time, and mass.

According to the theory of elliptic motion, the moment of momentum of the orbital motion of the planet and satellite about their common centre of inertia is $\frac{Mm}{M+m}\Omega c^2\sqrt{1-e^2}$. Now it has been shown that the factor involving M is unity, and by (1) $\Omega c^2 = \Omega^{-\frac{1}{3}} = c^{\frac{1}{2}}$.

Hence, if we neglect the square of the eccentricity e, the m. of m. of orbital motion is numerically equal to $\Omega^{-\frac{1}{3}}$ or $c^{\frac{1}{2}}$.

Let $x = \Omega^{-\frac{1}{3}} = c^{\frac{1}{2}}$.

In this paper x, the moment of momentum of orbital motion, will be taken as the independent variable. In interpreting the figures given below it will be useful to remember that it is also equal to the square root of the mean distance.

The moment of momentum of the planet's rotation is equal to Cn; and since C is unity, n will be either the m. of m. of the planet's rotation, or the angular velocity of rotation itself.

With the proposed units $\tau = \frac{3}{2}m/c^3 = \frac{3}{5}a^2x^{-6}$, since $m = \frac{2}{5}a^2$; and $\mathfrak{g} = \frac{2}{5}g/a = \frac{2}{5}M/a^3 = \frac{2}{5}m.\ M/ma^3 = \frac{4}{25}\nu/a$.

Also $\tau^2/\mathfrak{g}$ (a quantity which occurs below) is equal to $\frac{9}{4}a^5/\nu x^{12}$.

Now let t be the time, and let $2f$ be the phase-retardation of the tide which I have elsewhere called the sidereal semi-diurnal tide of speed $2n$, which tide is known in the British Association Report on Tides as the faster of the two K tides.

Then if the planet be a fluid of small viscosity, the following are the differential equations which give the secular changes in the elements of the system:

$$\frac{dn}{dt} = -\frac{1}{2}\frac{\tau^2}{\mathfrak{g}}\sin 4f\left(1-\frac{\Omega}{n}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$$\frac{dx}{dt} = \frac{1}{2}\frac{\tau^2}{\mathfrak{g}}\sin 4f\left(1-\frac{\Omega}{n}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

$$\frac{di}{dt} = \frac{1}{4}\frac{\tau^2}{\mathfrak{g}}\sin 4f\left(\frac{i+j}{n}\right)\left(1-\frac{2\Omega}{n}\right) \quad . \quad . \quad . \quad . \quad (4).$$

$$\frac{dj}{dt} = -\frac{1}{4}\frac{\tau^2}{\mathfrak{g}}\sin 4f\,\frac{(i+j)}{x} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

$$\frac{1}{e}\frac{de}{dt} = \frac{1}{4}\frac{\tau^2}{\mathfrak{g}}\sin 4f \, . \, \frac{1}{x}\left(11-\frac{18\Omega}{n}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The first three of these equations are in effect established in my paper on the "Precession of a Viscous Spheroid,"* § 17, p. 497, eq. (80).

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* "Phil. Trans.," Part II, 1879.

The suffix m^2 to the symbols i and N there indicates that the equations (80) only refer to the action of the moon, and as here we only have a single satellite, they are the complete equations. N is equal to n/n_0, so that n_0 disappears from the first and second of (80); also $\mu=1/sn_0\Omega_0^{\frac{1}{3}}$, and thus n_0 disappears from the third equation. $P=\cos i$, $Q=\sin i$, and, since we are treating i the obliquity as small, $P=1$, $Q=i$; also $\lambda=\Omega/n$; the ϵ of that paper is identical with the f of the present one; lastly ξ is equal to $\Omega_0^{\frac{1}{3}}\Omega^{-\frac{1}{3}}$, and since with our present units $s=1$, therefore $\mu\, d\xi/dt=d\Omega^{-\frac{1}{3}}/n_0dt=dx/n_0dt$.

With regard to the transformation of the first of (80) into (4) of the present paper, I remark that treating i as small $\frac{1}{4}PQ-\frac{1}{2}\lambda Q=\frac{1}{4}i(1-2\Omega/n)$, and introducing this transformation into the first of (80), equation (4) is obtained, except that i occurs in place of $(i+j)$. Now in the paper on the "Precession of a Viscous Spheroid" the inclination of the orbit of the satellite to the plane of reference was treated as zero, and hence j was zero; but I have proved in a paper "On the Secular Changes in the Elements of the Orbit of a Satellite revolving about a tidally distorted Planet" (read before the Royal Society on December 18th, 1879, but as yet unpublished) that when we take into account the inclination of the orbit of the satellite, the P and Q on the right-hand sides of eq. (80) of "Precession" must be taken as the cosine and sine of $i+j$ instead of i. Equations (5) and (6) are proved in § 10, Part II, and § 25, Part V of the unpublished paper, and the reader is requested to take them as established.

The integrals of this system of equations will give the secular changes in the motion of the system under the influence of the frictional tides. The object of the present paper is to find an analytical expression for the solution, and to interpret that solution geometrically.

From equations (2) and (4) we have

$$i\frac{dn}{dt}+n\frac{di}{dt}=\frac{1}{4}\frac{\tau^2}{\mathfrak{g}}\sin 4f\left[-(i+j)+2j\left(1-\frac{\Omega}{n}\right)\right].$$

But from (3) and (5) $x\,dj/dt+j\,dx/dt$ is equal to the same expression; hence

$$i\frac{dn}{dt}+n\frac{di}{dt}=x\frac{dj}{dt}+j\frac{dx}{dt}.$$

The integral of this equation is $in=jx$.

or
$$\frac{i}{j}=\frac{x}{n}\quad\dots\dots\dots\dots\quad(7).$$

Equation (7) may also be obtained by the principle of conservation of moment of momentum. The motion is referred to the invariable plane of the system, and however the planet and satellite may interact on one another, the resultant m. of m. must remain constant in direction and magnitude. Hence if we draw a parallelogram of which the diagonal is h (the resultant m. of m. of the system), and of which the sides are n and x, inclined respectively to the diagonal at the angles i and j, we see at once that

$$\frac{\sin i}{\sin j}=\frac{x}{n}.$$

If i and j be treated as small this reduces to (7).

Again the consideration of this parallelogram shows that

$$h^2=n^2+x^2+2nx\cos(i+j),$$

which expresses the constancy of moment of momentum. If the squares and higher powers of $i+j$ be neglected, this becomes

$$h=n+x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8).$$

Equation (8) may also be obtained by observing that $dn/dt+dx/dt=0$, and therefore on integration $n+x$ is constant. It is obvious from the principle of m. of m. that the planet's equator and the plane of the satellite's orbit have a common node on the invariable plane of the system.

If we divide equations (4) and (6) by (3), we have the following results:—

$$\frac{1}{i}\frac{di}{dx}=\frac{1}{2n}\left(1+\frac{j}{i}\right)\frac{n-2\Omega}{n-\Omega} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9).$$

$$\frac{1}{e}\frac{de}{dx}=\frac{1}{2x}\,\frac{11n-18\Omega}{n-\Omega} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10).$$

But from (7) and (8)

$$1+\frac{j}{i}=1+\frac{n}{x}=\frac{h}{x},$$

also $\Omega=x^{-3}$, and $n=h-x$.

Hence (9) and (10) may be written

$$\left.\begin{aligned}\frac{d}{dx}\log i&=\frac{h}{2}\cdot\frac{1}{x(h-x)}\cdot\frac{x^3(h-x)-2}{x^3(h-x)-1}\\ \frac{d}{dx}\log e&=\frac{1}{2x}\cdot\frac{11x^3(h-x)-18}{x^3(h-x)-1}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \qquad (11).$$

Now
$$\frac{h\{x^3(h-x)-2\}}{2x(h-x)\{x^3(h-x)-1\}}=\frac{h}{x(h-x)}+\frac{h}{2}\,\frac{x^2}{x^4-hx^3+1}.$$

Therefore
$$\frac{d}{dx}\log i=\frac{1}{x}+\frac{1}{h-x}+\frac{h}{2}\,\frac{x^2}{x^4-hx^3+1} \quad . \quad . \quad . \quad . \quad . \qquad (12).$$

Also
$$\frac{11x^3(h-x)-18}{2x\{x^3(h-x)-1\}}=\frac{9}{x}-\frac{7}{2}\cdot\frac{x^2(x-h)}{x^4-hx^3+1}.$$

Therefore $$\frac{d}{dx}\log e=\frac{9}{x}-\frac{7}{2}\cdot\frac{x^2(x-h)}{x^4-hx^3+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

These two equations are integrable as they stand, except as regards the last term in each of them.

It was shown in a previous paper that the whole energy of the system, both kinetic and potential, was equal to $\frac{1}{2}\left[n^2-x^{-2}\right]$.*

Then integrating (12) and (13), and writing down (7) and (8) again, and the expression for the energy, we have the following equations, which give the variations of the elements of the system in terms of the square root of the satellite's distance, and independently of the time.

$$\left.\begin{aligned}
\log i&=\log\frac{x}{h-x}+\tfrac{1}{2}h\int\frac{x^2dx}{x^4-hx^3+1}+\text{const.}\\
\log e&=\log x^9-\tfrac{7}{2}\int\frac{x^2(x-h)dx}{x^4-hx^3+1}+\text{const.}\\
j&=\frac{h-x}{x}i.\\
n&=h-x.\\
2E&=(h-x)^2-\frac{1}{x^2}.
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (14).$$

When the integration of these equations is completed, we shall have the means of tracing the history of a fluid planet of small viscosity, attended by a single satellite, when the system is started with any given moment of momentum h, and with any mean distance and (small) inclination and (small) eccentricity of the satellite's orbit, and (small) obliquity of the planet's equator. It may be remarked that h is to be taken as essentially positive, because the sign of h merely depends on the convention which we choose to adopt as to positive and negative rotations.

These equations do not involve the time, but it will be shown later how the time may be also found as a function of x. It is not, however, necessary to find the expression for the time in order to know the sequence of events in the history of the system.

Since the fluid which forms the planet is subject to friction, therefore the system is non-conservative of energy, and therefore x must change in such a way that E may diminish.

If the expression for E be illustrated by a curve in which E is the vertical ordinate and x the horizontal abscissa, then any point on this "curve of energy" may be taken to represent one configuration of the system, as far as regards the mean distance of the satellite.

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* "The Secular Effects of Tidal Friction," &c., "Proc. Roy. Soc.," No. 197, 1879, eq. (4).

Then such a point must always slide down a slope of energy, and we shall see which way x must vary for any given configuration. This consideration will enable us to determine the sequence of events, when we come to consider the expressions for i, e, j, n in terms of x.

We have now to consider the further steps towards the complete solution of the problem.

The only difficulty remaining is the integration of the two expressions in the first and second of (14). From the forms of the expressions to be integrated, it is clear that they must be split up into partial fractions. The forms which these fractions will assume will of course depend on the nature of the roots of the equation $x^4-hx^3+1=0$.

Some of the properties of this biquadratic were discussed in a previous paper, but it will now be necessary to consider the subject in more detail.

It will be found by Ferrari's method that

$$x^4-hx^3+1=\left\{x^2+2x\frac{\lambda^{\frac{3}{2}}-h}{4}+\frac{\lambda^{\frac{3}{2}}-h}{2\lambda^{\frac{1}{2}}}\right\}\left\{x^2-2x\frac{\lambda^{\frac{3}{2}}+h}{4}+\frac{\lambda^{\frac{3}{2}}+h}{2\lambda^{\frac{1}{2}}}\right\}$$

where $\lambda^3-4\lambda-h^2=0$.

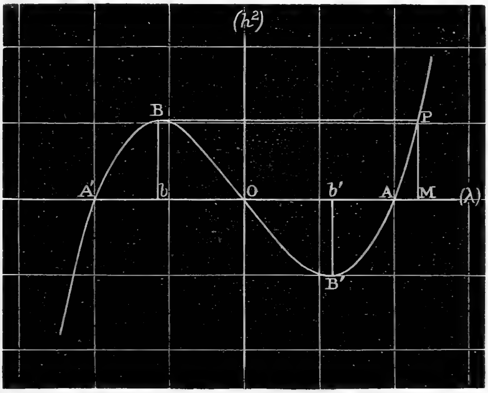
By using the property $(\lambda^{\frac{3}{2}}-h)(\lambda^{\frac{3}{2}}+h)=4\lambda$, this expression may be written in the form

$$\left[\left\{x+\tfrac{1}{4}(\lambda^{\frac{3}{2}}-h)\right\}^2+\left\{\tfrac{1}{4}(\lambda^{\frac{3}{2}}-h)\sqrt{1+2h\lambda^{-\frac{3}{2}}}\right\}^2\right]\times$$
$$\left[\left\{x-\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)\right\}^2+\left\{\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)\sqrt{1-2h\lambda^{-\frac{3}{2}}}\right\}^2\right].$$

Which is of course equivalent to finding all the roots of the biquadratic in terms of h and λ.

Now let a curve be drawn of which h^2 is the ordinate (negative

FIG. 1.



The ordinates are drawn to one-third of the scale to which the abscissæ are drawn.

values of h^2 being admissible) and λ the abscissa; it is shown in fig. 1. Its equation is $h^2=\lambda(\lambda^2-4)$.

It is obvious that $OA=OA'=2$.

The maximum and minimum values of h^2 (viz., Bb, $B'b'$) are given by $3\lambda^2=4$ or $\lambda=\pm 2/3$.

Then $Bb=B'b'=-2^3/3^{\frac{3}{2}}+4\,.\,2/3^{\frac{1}{2}}=(4/3^{\frac{3}{4}})^2$.

Since in the cubic, on which the solution of the biquadratic depends, h^2 is necessarily positive, it follows that if h be greater than $4/3^{\frac{3}{4}}$ the cubic has one real positive root greater than OM, and if h be less than $4/3$, it has two real negative roots lying between O and OA′, and one real positive root lying between OA and OM.

To find OM we observe that since h^2 is equal to $(4/3^{\frac{3}{4}})^2$, and since the root of $\lambda^3-4\lambda-h^2=0$ which is equal to $-2/3^{\frac{1}{2}}$ is repeated twice, therefore, if ϵ be the third root (or OM) we must have

$$\left(\lambda+\frac{2}{3^{\frac{1}{2}}}\right)^2(\lambda-\epsilon)=\lambda^3-4\lambda-\left(\frac{4}{3}\right)^2,$$

whence $(2/3^{\frac{1}{2}})^2\epsilon=(4/3^{\frac{3}{4}})^2$, and ϵ or $OM=4/3^{\frac{1}{2}}$.

Now $OA=2$; hence, if h be less than $4/3^{\frac{3}{4}}$, the cubic has a positive root between 2 and $4/3^{\frac{1}{2}}$, and if h be greater than $4/3^{\frac{3}{4}}$, the cubic has a positive root between $4/3^{\frac{1}{2}}$ and infinity.

It will only be necessary to consider the positive root of the cubic.

Now suppose h to be greater than $4/3^{\frac{3}{4}}$.

Then it has just been shown that λ is greater than $4/3^{\frac{1}{2}}$, and hence (λ being positive) $3\lambda^3$ is greater than 16λ, or $4(\lambda^3-4\lambda)$ greater than λ^3, or $4h^2$ greater than λ^3, or $2h\lambda^{-\frac{3}{2}}$ greater than unity.

Therefore $\{\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)\sqrt{1-2h\lambda^{-\frac{3}{2}}}\}^2=-\{\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)\sqrt{2h\lambda^{-\frac{3}{2}}-1}\}^2$.

Thus the biquadratic has two real roots, which we may call a and b.

Then
$$\mathrm{a}=\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)[1+\sqrt{2h\lambda^{-\frac{3}{2}}-1}];$$
$$\mathrm{b}=\tfrac{1}{4}(\lambda^{\frac{3}{2}}+h)[1-\sqrt{2h\lambda^{-\frac{3}{2}}-1}].$$

It will now be proved that a is greater and b less than $\frac{3}{4}h$.

Now
$$\mathrm{a}> \text{ or } <\tfrac{3}{4}h,$$
$$\text{as } (\lambda^{\frac{3}{2}}+h)[1+\sqrt{2h\lambda^{-\frac{3}{2}}-1}]> \text{ or } <3h,$$
$$\text{as } \frac{(\lambda^{\frac{3}{2}}+h)}{\lambda^{\frac{3}{4}}}\sqrt{2h-\lambda^{\frac{3}{2}}}> \text{ or } <2h-\lambda^{\frac{3}{2}},$$
$$\text{as } \lambda^{\frac{3}{2}}+h> \text{ or } <\lambda^{\frac{3}{4}}\sqrt{2h-\lambda^{\frac{3}{2}}},$$
$$\text{as } \lambda^3+2h\lambda^{\frac{3}{2}}+h^2> \text{ or } <2h\lambda^{\frac{3}{2}}-\lambda^3,$$
$$\text{as } 2\lambda^3+h^2> \text{ or } <0.$$

Since the left hand side is essentially positive, a is greater than $\frac{3}{4}h$.

Again $$\mathrm{b} > \text{ or } < \tfrac{3}{4}h,$$

$$\text{as } (\lambda^{\frac{3}{2}}+h)[1-\sqrt{2h\lambda^{-\frac{3}{2}}-1}] > \text{ or } < 3h,$$

$$\text{as } -\frac{(\lambda^{\frac{3}{2}}+h)}{\lambda^{\frac{3}{4}}}\sqrt{2h-\lambda^{\frac{3}{2}}} > \text{ or } < 2h-\lambda^{\frac{3}{2}}.$$

Since the left-hand side is negative and the right positive, the left is less than the right, and therefore b is less than $\frac{3}{4}h$.

If, therefore, h be greater than $4/3^{\frac{3}{4}}$, we may write

$$x^4-hx^3+1=(x-\mathrm{a})(x-\mathrm{b})[(x-\alpha)^2+\beta^2],$$

where $\mathrm{a}-\frac{3}{4}h$, $\frac{3}{4}h-\mathrm{b}$ are positive, and where α is negative.

We now turn to the other case and suppose h less than $4/3^{\frac{3}{4}}$. All the roots of the biquadratic are now imaginary, and we may put

$$x^4-hx^3+1=[(x-\alpha)^2+\beta^2][(x-\gamma)^2+\delta^2].$$

If α be taken as $-\frac{1}{4}(\lambda^{\frac{3}{2}}-h)$, then γ is $\frac{1}{4}(\lambda^{\frac{3}{2}}+h)$.

Then it only remains to prove that γ is greater than $\frac{3}{4}h$.

Now $$\gamma > \text{ or } < \tfrac{3}{4}h,$$

$$\text{as } \lambda^{\frac{3}{2}} > \text{ or } < 2h,$$

$$\text{as } \lambda^3 > \text{ or } < 4h^2=4(\lambda^3-4\lambda),$$

$$\text{as } 16 > \text{ or } < 3\lambda^2,$$

$$\text{as } 4/3^{\frac{1}{2}} > \text{ or } < \lambda,$$

but it has been already shown that in this case, λ is less than $4/3^{\frac{1}{2}}$, wherefore γ is greater than $\frac{3}{4}h$.

We may now proceed to the required integrations.

First case where h is greater than $4/3^{\frac{3}{4}}$.

Let $$x^4-hx^3+1=(x-\mathrm{a})(x-\mathrm{b})[(x-\alpha)^2+\beta^2],$$

so that the roots are a, b, $\alpha \pm \beta\iota$.

Also let a be the root which is greater than $\frac{3}{4}h$, b that which is less, and let

$$\mathrm{a}=\mathrm{a}_1+\tfrac{3}{4}h, \qquad \mathrm{b}=\tfrac{3}{4}h-\mathrm{b}_1, \qquad \alpha=\tfrac{3}{4}h-\alpha_1.$$

To find the expression for i we have to integrate $\dfrac{x^2}{x^4-hx^3+1}$.

Let $f(x)=(x-\mathrm{a})\psi(x)$, and let $x^2/f(x)=A/(x-\mathrm{a})+\phi(x)/\psi(x)$.

Then $$x^2(x-\mathrm{a})=Af(x)+(x-\mathrm{a})^2\phi(x).$$

Hence $$A=\mathrm{a}^2/f'(\mathrm{a}).$$

If, therefore, $f(x)=x^4-hx^3+1$, $A=1/(4\mathrm{a}-3h)=1/4\mathrm{a}_1$.

Thus the partial fractions corresponding to the roots a and b are

$$\frac{1}{4\mathrm{a}_1}\frac{1}{x-\mathrm{a}}-\frac{1}{4\mathrm{b}_1}\frac{1}{x-\mathrm{b}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15).$$

If the pair of fractions corresponding to the roots $\alpha\pm\beta\iota$ be formed and added together, we find

$$\frac{1}{2(\alpha_1^2+\beta^2)}\frac{-\alpha_1(x-\alpha)+\beta^2}{[(x-\alpha)^2+\beta^2]} \quad . \quad . \quad . \quad . \quad . \quad . \quad (16).$$

The sum of (15) and (16) is equal to $\frac{x^2}{x^4-hx^3+1}$, and

$$\int\frac{x^2dx}{x^4-hx^3+1}=\frac{1}{4\mathrm{a}_1}\log(x\backsim\mathrm{a})-\frac{1}{4\mathrm{b}_1}\log(x\backsim\mathrm{b})-\frac{\alpha_1}{4(\alpha_1^2+\beta^2)}\log[(x-\alpha)^2+\beta^2]$$

$$+\frac{\beta}{2(\alpha_1^2+\beta^2)}\text{arc tan}\,\frac{x-\alpha}{\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17).$$

Substituting in the first of (14) we have

$$i=A\frac{x}{h-x}\cdot\frac{(x\backsim\mathrm{a})^{\frac{h}{8\mathrm{a}_1}}\exp.\left[\frac{h\beta}{4(\alpha_1^2+\beta^2)}\text{arc tan}\,\frac{x-\alpha}{\beta}\right]}{(x\backsim\mathrm{b})^{\frac{h}{8\mathrm{b}_1}}[(x-\alpha)^2+\beta^2]^{\frac{h\alpha_1}{8(\alpha_1^2+\beta^2)}}} \quad . \quad . \quad (18).$$

where A is a constant to be determined by the value of i, which corresponds with a particular value of x.

From the third of (14) we see that by omitting the factor $x/(h-x)$ from the above, we obtain the expression for j.

To find the expression for e we have to integrate $\frac{x^2(x-h)}{x^4-hx^3+1}$.

Now $$x^2(x-h)=\tfrac{1}{4}(4x^3-3hx^2)-\tfrac{1}{4}hx^2,$$
and therefore

$$\int\frac{x^2(x-h)dx}{x^4-hx^3+1}=\tfrac{1}{4}\log\,(x^4-hx^3+1)-\tfrac{1}{4}h\int\frac{x^2dx}{x^4-hx^3+1}.$$

The integral remaining on the right hand has been already determined in (17). Then substituting in the second of (14), we have

$$e=\frac{Bx^9}{(x^4-hx^3+1)^{\frac{7}{8}}}\left(\frac{(x\backsim\mathrm{a})^{\frac{h}{8\mathrm{a}_1}}\exp.\left[\frac{h\beta}{4(\alpha_1^2+\beta^2)}\text{arc tan}\,\frac{x-\alpha}{\beta}\right]}{(x\backsim\mathrm{b})^{\frac{h}{8\mathrm{b}_1}}[(x-\alpha)^2+\beta^2]^{\frac{h\alpha_1}{8(\alpha_1^2+\beta^2)}}}\right)^{\frac{7}{4}} \quad . \quad (19).$$

where B is a constant to be determined by the value of e, corresponding to some particular value of x.

From this equation we get the curious relationship

$$e=\frac{B}{A^{\frac{7}{4}}}\cdot\frac{x^9}{(x^4-hx^3+1)^{\frac{7}{8}}}j^{\frac{7}{4}}\quad .\ .\ .\ .\ .\ .\ .\quad (20).$$

This last result will obviously be equally true even if all the roots of $x^4-hx^3+1=0$ are imaginary.

In the present case the complete solution of the problem is comprised in the following equations :—

$$\left.\begin{aligned}
j&=A\frac{(x\backsim \mathrm{a})^{\frac{h}{8\mathrm{a}_1}}\exp.\left[\frac{h\beta}{4(\alpha_1{}^2+\beta^2)}\operatorname{arc}\tan\frac{x-\alpha}{\beta}\right]}{(x\backsim \mathrm{b})^{\frac{h}{8\mathrm{b}_1}}\left[(x-\alpha)^2+\beta^2\right]^{\frac{h\alpha_1}{8(\alpha_1{}^2+\beta^2)}}}\\
i&=\frac{x}{h-x}j.\\
e&=\frac{B}{A^{\frac{7}{4}}}\frac{x^9}{(x^4-hx^3+1)^{\frac{7}{8}}}j^{\frac{7}{4}}.\\
n&=h-x.\\
2E&=(h-x)^2-\frac{1}{x^2}.
\end{aligned}\right\}\quad .\ .\ (21).$$

It is obvious that the system can never degrade in such a way that x should pass through one of the roots of the biquadratic $x^4-hx^3+1=0$. Hence the solution is divided into three fields, viz., (i) $x=+\infty$ to $x=\mathrm{a}$; here we must write $x-\mathrm{a}$, $x-\mathrm{b}$ for the $x\backsim\mathrm{a}$, $x\backsim\mathrm{b}$ in the above solution; (ii) $x=\mathrm{a}$ to $x=\mathrm{b}$; here we must write $\mathrm{a}-x$, $x-\mathrm{b}$ (this is the part which has most interest in application to actual planets and satellites); (iii) $x=\mathrm{b}$ to $x=-\infty$; here we must write $\mathrm{a}-x$, $\mathrm{b}-x$. When x is negative the physical meaning is that the revolution of the satellite is adverse to the planet's rotation.

By referring to (4) and (6), we see that i must be a maximum or minimum when $n=2\Omega$, and e a maximum or minimum when $n=\frac{18}{11}\Omega$. Hence the corresponding values of x are the roots of the equations $x^4-hx^3+2=0$, and $x^4-hx^3+\frac{18}{11}=0$ respectively.

Since

$$\frac{x^2}{x^4-hx^3+1}=\frac{1}{4\mathrm{a}_1}\frac{1}{x-\mathrm{a}}-\frac{1}{4\mathrm{b}_1}\frac{1}{x-\mathrm{b}}+\frac{1}{2(\alpha_1{}^2+\beta^2)}\frac{-\alpha_1(x-\alpha)+\beta^2}{[(x-\alpha)^2+\beta^2]}.$$

Therefore

$$x^2=\frac{1}{4\mathrm{a}_1}(x-\mathrm{b})[(x-\alpha)^2+\beta^2]-\frac{1}{4\mathrm{b}_1}(x-\mathrm{a})[(x-a)^2+\beta^2]$$

$$+\frac{1}{2(a_1{}^2+\beta^2)}[-\alpha_1(x-a)+\beta^2](x-\mathrm{a})(x-\mathrm{b}).$$

Hence the coefficient of x^3 on the right-hand side must be zero, and therefore $\frac{1}{4\text{a}_1}-\frac{1}{4\text{b}_1}-\frac{\alpha_1}{2(\alpha_1^2+\beta^2)}=0.$

And $\frac{h}{8\text{a}_1}=\frac{h}{8\text{b}_1}+\frac{h\alpha_1}{4(\alpha_1^2+\beta^2)}.$

Now when $x=+\infty$, $\text{arc tan}\,\frac{x-\alpha}{\beta}=\frac{1}{2}\pi$, and when $x=-\infty$, it is equal to $-\frac{1}{2}\pi$.

Hence when $x=\pm\infty$, $j=A\,\text{exp.}\,[\pm\pi h\beta/8(\alpha_1^2+\beta^2)]$, $i=-j$; the upper sign being taken for $+\infty$ and the lower for $-\infty$.

Then since j tends to become constant when $x=\pm\infty$, and since $9-\frac{7}{2}=\frac{11}{2}$, therefore when x is very large e tends to vary as $x^{\frac{11}{2}}$.

If x be very small j has a finite value, and i varies as x, and e varies as x^9.

j, i, and e all become infinite when $x=\text{b}$, and i also becomes infinite when $x=h$.

This analytical solution is so complex that it is not easy to understand its physical meaning; a geometrical illustration will, however, make it intelligible.

The method adopted for this end is to draw a series of curves, the points on which have x as abscissa and i, j, e, n, E as ordinates. The figure would hardly be intelligible if all the curves were drawn at once, and therefore a separate figure is drawn for i, j, and e; but in each figure the straight line which represents n is drawn, and the energy curve is also introduced in order to determine which way the figure is to be read. The zero of energy is of course arbitrary, and therefore the origin of the energy curve is in each case shifted along the vertical axis, in such a way that the energy curve may clash as little as possible with the others.

It is not very easy to select a value of h which shall be suitable for drawing these curves within a moderate compass, but after some consideration I chose $h=2{\cdot}6$, and figs. 2, 3, and 4 are drawn to illustrate this value of h. If the cubic $\lambda^3-4\lambda-(2{\cdot}6)^2=0$, be solved by Cardan's method, it will be found that $\lambda=2{\cdot}5741$, and using this value in the formula for the roots of the biquadratic we have

$$x^4-2{\cdot}6\,x^3+1=(x-2{\cdot}539)(x-{\cdot}826)[(x+{\cdot}382)^2+({\cdot}575)^2].$$

Hence $\text{a}=2{\cdot}539$, $\text{b}={\cdot}826$, $\alpha=-{\cdot}382$, $\beta={\cdot}575$, $\frac{3}{4}h=1{\cdot}95$, and $4\text{a}_1=2{\cdot}356$, $4\text{b}_1=4{\cdot}496$, $\alpha_1=2{\cdot}332$, $\alpha_1^2+\beta^2=5{\cdot}771$.

Then we have

$$\left.\begin{aligned}
j&=A\frac{(2{\cdot}539\backsim x)^{\cdot 552}\,\text{exp.}\left[{\cdot}062\,\text{arc tan}\,(1{\cdot}740x+{\cdot}665)\right]}{(x\backsim{\cdot}826)^{\cdot 289}\,(x^2+{\cdot}765x+{\cdot}477)^{\cdot 1314}}.\\
i&=\frac{x}{2{\cdot}6-x}j.\\
e&=\frac{B}{A^{\frac{7}{4}}}\frac{x^9}{(x^4-2{\cdot}6\,x^3+1)^{\frac{7}{8}}}j^{\frac{7}{4}}.\\
n&=2{\cdot}6-x.\\
2E&=(2{\cdot}6-x)^2-\frac{1}{x^2}.
\end{aligned}\right\}\quad . \quad . \quad (22).$$

The maximum and minimum values of i are given by the roots of the equation $x^4-2{\cdot}6x^3+2=0$, viz., $x=2{\cdot}467$ and $x=1{\cdot}103$. The maximum and minimum values of e are given by the roots of the equation $x^4-2{\cdot}6x+\frac{18}{11}=0$, viz., $x=2{\cdot}495$ and $x=1{\cdot}0095$. The horizontal asymptotes for i/A and j/A are at distances from the axis of x equal to exp. $({\cdot}062\times\frac{1}{2}\pi)$ and exp. $(-{\cdot}062\times\frac{1}{2}\pi)$, which are equal to 1·102 and ·908 respectively.

Fig. 2 shows the curve illustrating the changes of i, the obliquity of the equator to the invariable plane.

The asymptotes are indicated by broken lines; that at A is given by $x={\cdot}826$, and is the ordinate of maximum energy; that at B is given by $x=2{\cdot}6$, and gives the configuration of the system for which the planet has no rotation. The point C is given by $x=2{\cdot}539$, and lies on the ordinate of minimum energy. Geometrically the curve is divided into three parts by the vertical asymptotes, but it is further divided physically.

The curve of energy has four slopes, and since the energy must degrade, there are four methods in which the system may change, according to the way in which it was started. The arrows marked on the curve of obliquity show the direction in which the curve must be read.

Since none of these four methods can ever pass into another, this figure really contains four figures, and the following parts of the figure are quite independent of one another, viz.: (i) from $-\infty$ to O; (ii) from A to O; (iii) from A to C; (iv) from $+\infty$ to C. The figures 3 and 4 are similarly in reality four figures combined. For each of these parts the constant A must be chosen with appropriate sign; but in order to permit the curves in fig. 2 to be geometrically continuous the obliquity is allowed to change sign.

The actual numerical interpretation of this figure depends on the value of A. Thus if for any value of x in any of the four fields the obliquity has an assigned value, then the ordinate corresponding to that value of x will give a scale of obliquity from which all the other ordinates within that field may be estimated.

FIG. 2.

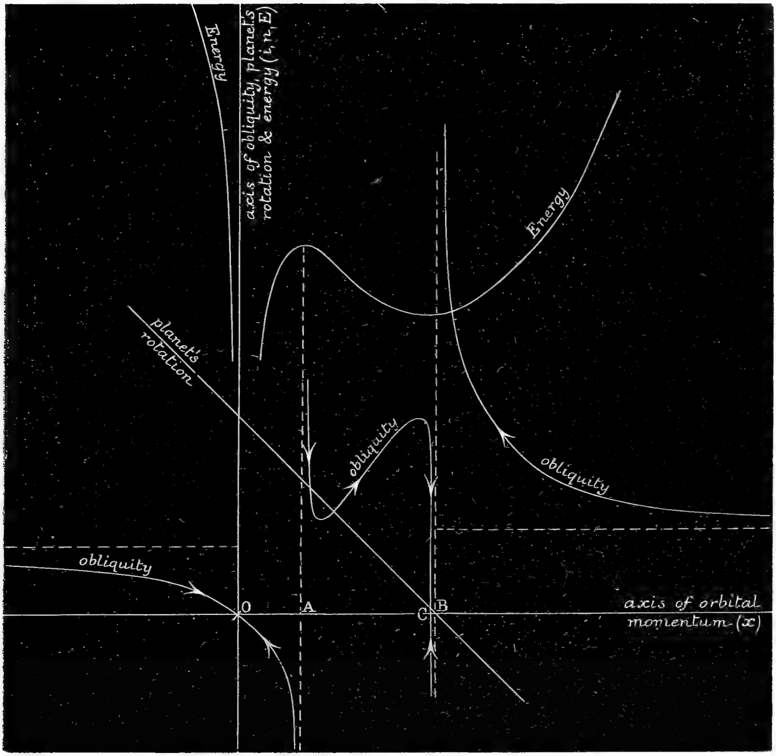


Diagram for Obliquity of Planet's Equator.—First case.

As a special example of this we see that, if the obliquity be zero at any point, a consideration of the curve will determine whether zero obliquity be dynamically stable or not; for if the arrows on the curve of obliquity be approaching the axis of x, zero obliquity is dynamically stable, and if receding from the axis of x, dynamically unstable.

Hence from $x = +\infty$ to B, zero obliquity is dynamically unstable, from $-\infty$ to O and A to O dynamically stable, and from A to B, first stable, then unstable, and finally stable.

The infinite value of the obliquity at the point B has a peculiar significance, for at B the planet has no rotation, and being thus free from what Sir William Thomson calls "gyroscopic domination," the obliquity changes with infinite ease. In fact at B the term equator loses its meaning. The infinite value at A has a different meaning. The configuration A is one of maximum energy and of dynamical equilibrium, but is unstable as regards mean distance and planetary rotation; at this point the system changes infinitely slowly as regards time, and therefore the infinite value of the obliquity does not indicate an infinite rate of change of obliquity. In fact if we put $n=\Omega$ in (1) we see that $di/idt=-\frac{1}{4}(\tau^2/\mathfrak{g})\sin 4f$. However, to consider this case adequately we should have to take into account the obliquity in the equations for dn/dt and dx/dt, because the principal semi-diurnal tide vanishes when $n=\Omega$.

Similarly at the minimum of energy the system changes infinitely slowly, and thus the obliquity would take an infinite time to vanish.

We may now state the physical meaning of fig. 2, and this interpretation may be compared with a similar interpretation in the paper on "The secular effects of tidal friction," above referred to.

A fluid planet of small viscosity is attended by a single satellite, and the system is started with an amount of positive moment of momentum which is greater than $4/3^{\frac{3}{4}}$, with our present units of length, mass and time.

The part of the figure on the negative side of the origin indicates a negative revolution of the satellite and a positive rotation of the planet, but the m. of m. of planetary rotation is greater (by an amount h) than the m. of m. of orbital motion. Then the satellite approaches the planet and ultimately falls into it, and the obliquity always diminishes slowly. The part from O to A indicates positive rotation of both parts of the system, but the satellite is very close to the planet and revolves round the planet quicker than the planet rotates, as in the case of the inner satellite of Mars. Here again the satellite approaches and ultimately falls in, and the obliquity always diminishes.

The part from A to C indicates positive rotation of both parts, but the satellite revolves slower than the planet rotates. This is the case which has most interest for application to the solar system. The satellite recedes from the planet, and the system ceases its changes when the satellite and planet revolve slowly as parts of a rigid body—that is to say, when the energy is a minimum. The obliquity first decreases, then increases to a maximum, and ultimately decreases to zero.*

The part from infinity to C indicates a positive revolution of the satellite, and from infinity to B a negative rotation of the planet, but from B to C a positive rotation of the planet, which is slower than the revolution of the satellite. In either of these cases the satellite approaches the planet, but the changes cease when the satellite and planet move slowly round as parts of a rigid body—that is to say, when the energy is a minimum. If the rotation of the planet be positive, the obliquity diminishes, if negative it increases. If the rotation of the planet be *nil*, the term obliquity ceases to have any meaning, since there is no longer an equator.

* According to the present theory, the moon, considered as being attended by the earth as a satellite, has gone through these changes.

Fig. 3 illustrates the changes of inclination of the satellite's orbit, and may be interpreted in the same way as fig. 2. It appears from the part of the figure for which x is negative, that if the revolution of the satellite be negative, and the rotation of the planet positive, but the m. of m. of planetary rotation greater than that of orbital motion, then, as the satellite approaches the planet, the inclination of the orbit increases, or zero inclination is dynamically unstable. In every other case the inclination will decrease, or zero inclination is dynamically stable.

Fig. 3.

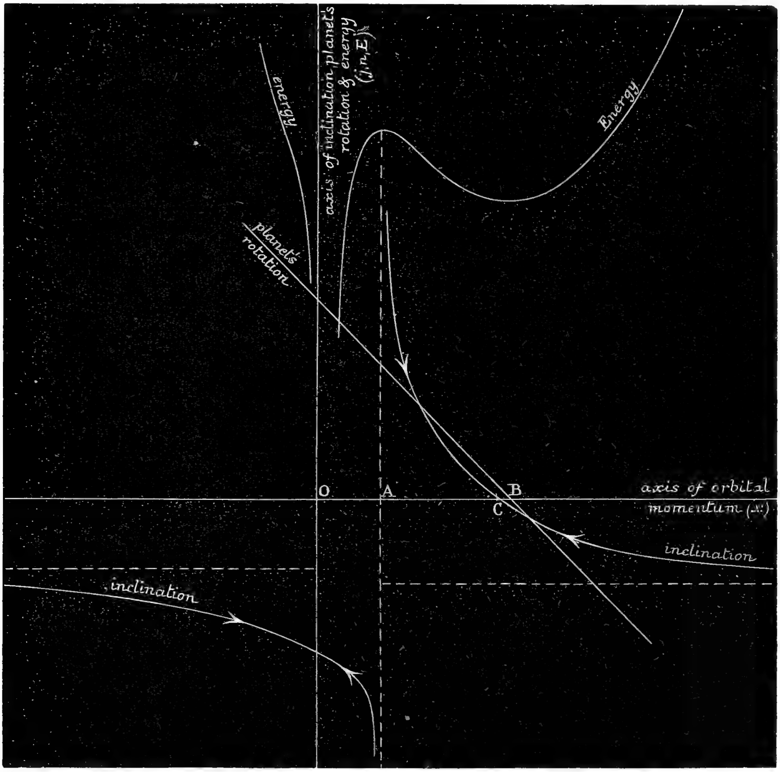


Diagram for Inclination of Satellite's Orbit.—First case.

This result undergoes an important modification when a second satellite is introduced, as will appear in the unpublished paper.

Fig. 4 shows a similar curve for the eccentricity of the orbit. The variations of the eccentricity are very much larger than than those of the obliquity and inclination, so that it was here necessary to draw the ordinates on a much reduced scale. It was not possible to extend the figure far in either direction, because for large values of x, e varies as a high power of x (viz., $\frac{11}{2}$). The curve presents a resemblance to that of obliquity, for in the field comprised between the two roots of the biquadratic (viz., between A and C) the eccentricity diminishes to a minimum, increases to a maximum, and ultimately vanishes at C.

FIG. 4.

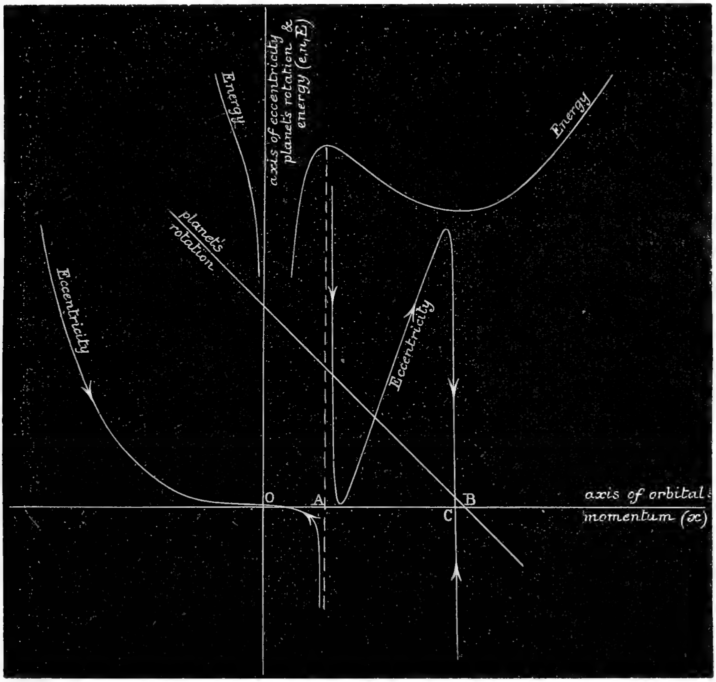


Diagram for Eccentricity of Satellite's Orbit.—First case.

This field represents a positive rotation both of the planet and satellite, but the satellite revolves slower than the planet rotates. This part represents the degradation of the system from the configuration of maximum energy to that of minimum energy, and the satellite recedes from the planet, until the two move round slowly like the parts of a rigid body.

In every other case the eccentricity degrades rapidly, whilst the satellite approaches the planet.

The very rapid rate of variation of the eccentricity, compared with that of the obliquity would lead one to expect that the eccentricity of the orbit of a satellite should become very large in the course of its evolution, whilst the obliquity should not increase to any very large extent. But it must be remembered that we are here only treating a planet of small viscosity, and it will appear, in the unpublished paper above referred to, that the rate of increase or diminution of the eccentricity is very much less rapid (per unit increase of x) if the viscosity be not small, whilst the rate of increase or diminution of obliquity (per unit increase of x) is slightly increased with increase of viscosity. Thus the observed eccentricities of the orbits of satellites and of obliquities of their planets cannot be said to agree in amount with the theory that the planets were primitively fluids of small viscosity, though I believe they *do* agree with the theory that the planets were fluids or quasi-solids of large viscosity.

We now come to the *second case*, where h is less than $4/3^{\frac{3}{4}}$. The biquadratic having no real roots, we may put

$$x^4-hx^3+1=[(x-a)^2+\beta^2][(x-\gamma)^2+\delta^2].$$

It has already been shown that a is negative, and γ greater than $\frac{3}{4}h$.

Let $$a=\tfrac{3}{4}h-a_1, \qquad \gamma=\gamma_1+\tfrac{3}{4}h.$$

Then by inspection of the integral in the first case we see that

$$j=A\frac{[(x-\gamma)^2+\delta^2]^{\frac{h\gamma_1}{8(\gamma_1^2+\delta^2)}}}{[(x-\alpha)^2+\beta^2]^{\frac{h\alpha_1}{8(\alpha_1^2+\beta^2)}}}\times$$
$$\exp.\left[\frac{h\beta}{4(\alpha_1^2+\beta^2)}\text{arc tan}\,\frac{x-\alpha}{\beta}+\frac{h\delta}{4(\gamma_1^2+\delta^2)}\text{arc tan}\,\frac{x-\gamma}{\delta}\right].$$

The rest of equations (21), which express the other elements in terms of j and x, remain the same as before.

By comparison with the first case, we see that

$$\frac{x^2}{x^4-hx^3+1}=\frac{1}{2(\alpha_1^2+\beta^2)}\,\frac{-\alpha_1(x-\alpha)+\beta^2}{(x-\alpha)^2+\beta^2}+\frac{1}{2(\gamma_1^2+\delta^2)}\,\frac{\gamma_1(x-\gamma)+\delta^2}{(x-\gamma)^2+\delta^2}.$$

On multiplying both sides of this identity by x^4-hx^3+1, and equating the coefficients of x^3, we find

$$0=\frac{-\alpha_1}{2(\alpha_1^2+\beta^2)}+\frac{\gamma_1}{2(\gamma_1^2+\delta^2)}.$$

Therefore $$\frac{h\alpha_1}{8(\alpha_1^2+\beta^2)}=\frac{h\gamma_1}{8(\gamma_1^2+\delta^2)}.$$

Thus when x is equal to $\pm\infty$

$$j=A\exp.\left[\pm\frac{\pi h\beta}{8(\alpha_1^2+\beta^2)}\pm\frac{\pi h\delta}{8(\gamma_1^2+\delta^2)}\right],$$

the upper sign being taken for $+\infty$, and the lower for $-\infty$. This expression gives the horizontal asymptotes for j and i.

In order to illustrate this solution, I chose $h=1$, and found by trigonometrical solution of the cubic $\lambda^3-4\lambda-1=0$, $\lambda=2{\cdot}1149$, and thence

$$\left.\begin{aligned}
j&=A\left(\frac{x^2-2{\cdot}038x+1{\cdot}401}{x^2+1{\cdot}038x+{\cdot}714}\right)^{{\cdot}077}\text{exp.}\left[{\cdot}081\text{ arc tan}\left(1{\cdot}500x+{\cdot}778\right)\right.\\
&\qquad\qquad\qquad\qquad\left.+{\cdot}346\text{ arc tan}\left(1{\cdot}659x-1{\cdot}691\right)\right].\\
i&=\frac{x}{1-x}j.\\
e&=\frac{B}{A^{\frac{7}{4}}}\frac{x^9}{(x^4-x^3+1)^{\frac{7}{8}}}j^{\frac{7}{4}}.\\
n&=1-x.\\
2E&=(1-x)^2-\frac{1}{x^2}.
\end{aligned}\right\}\quad(23).$$

When $x=+\infty$, $j/A=1{\cdot}956=-i/A$, and
when $x=-\infty$, $j/A={\cdot}512=-i/A$.

FIG. 5.

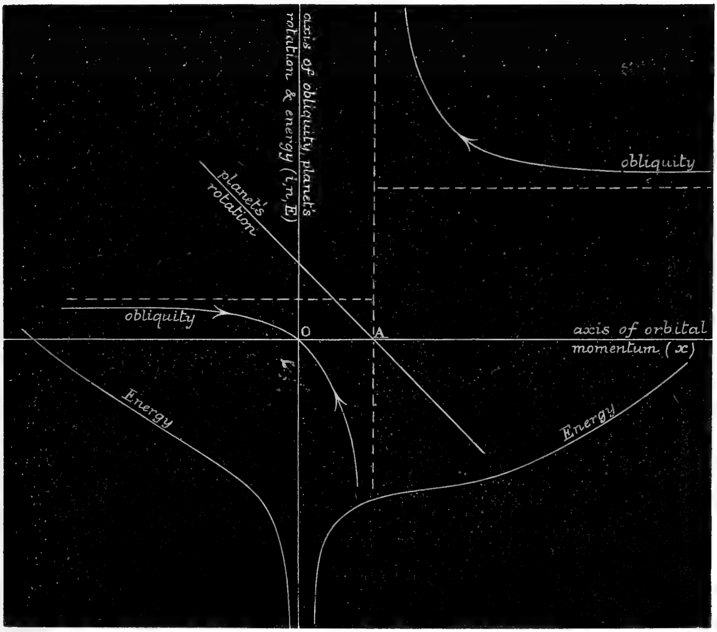


Diagram for Obliquity of Planet's Equator.—Second case.

These solutions are illustrated as in the previous case by the three figures 5, 6, 7. There are here only two slopes of energy, and hence these figures each of them only contain two separate figures.

Fig. 5 illustrates the changes of i, the obliquity of the equator to the invariable plane.

In this figure there is only one vertical asymptote, viz., that corresponding to $x=1$. For this value of x the planet has no rotation, is free from "gyroscopic domination," and the term equator loses its meaning.

The figure shows that if the rotation of the planet be negative, but the m. of m. of planetary rotation less than that of orbital motion, then the obliquity increases, whilst the satellite approaches the planet.

This increase of obliquity only continues so long as the rotation of the planet is negative. The rotation becomes positive after a time, and the obliquity then diminishes, whilst the satellite falls into the planet. In the corresponding part of fig. 2 the satellite did not fall into the planet, but the two finally moved slowly round together as the parts of a rigid body.

If the revolution of the satellite be negative, and the rotation of the planet positive, but the m. of m. of rotation greater than that of revolution, the obliquity always diminishes as the satellite falls in to the planet.

Fig. 6.

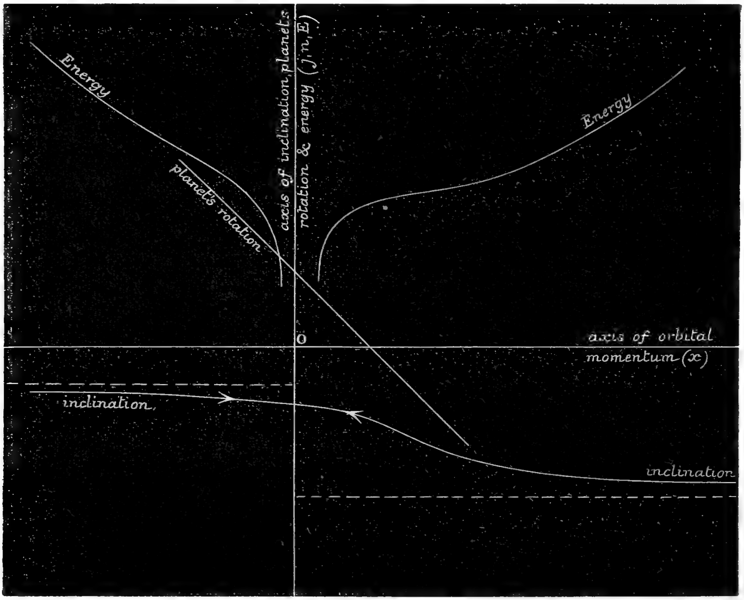


Diagram for Inclination of Satellite's Orbit.—Second case.

Figs. 2 and 5 only differ in the fact that in the one there is a true maximum and a true minimum of obliquity and energy, and in the other there is not so. In fact, if we annihilate the part between the vertical asymptotes of fig. 2 we get fig. 5.

Fig. 6 illustrates the changes of inclination of the orbit. It does not possess very much interest, since it simply shows that however the system be started with positive revolution of the satellite, whether the rotation of the planet be positive or not, the inclination of the orbit slightly diminishes as the satellite falls in.

And however the system be started with negative revolution of the satellite, and therefore necessarily positive rotation of the planet, the inclination of the orbit slightly increases. Fig. 6 again corresponds to fig. 3, if in the latter the part lying between the maximum and minimum of energy be annihilated.

FIG. 7.

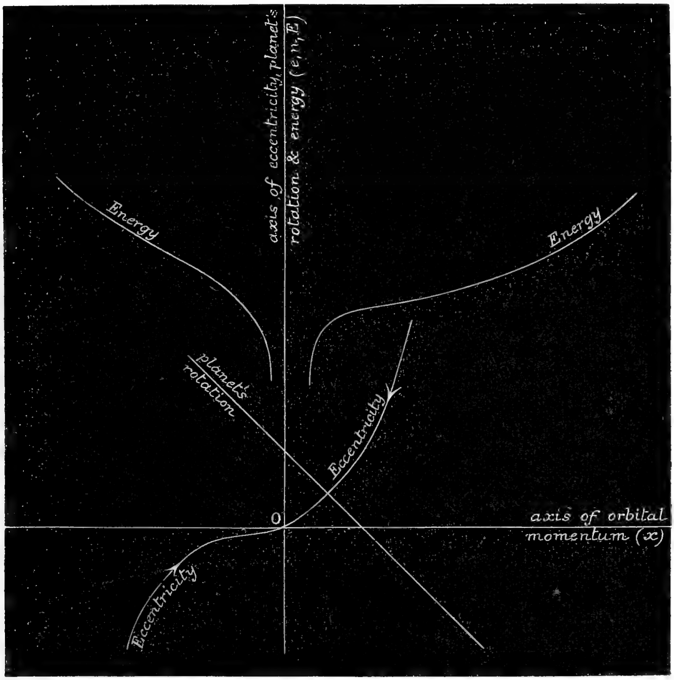


Diagram for Eccentricity of Satellite's Orbit.—Second case.

Fig. 7 illustrates the changes of eccentricity, and shows that it always diminishes rapidly however the system is started, as the satellite falls into the planet. This figure again corresponds with fig. 4, if in the latter the parts between the maximum and minimum of energy be annihilated.

These three figures may be interpreted as giving the various stabilities and instabilities of the system, just as was done in the first case.

The solution of the problem, which has been given and discussed above, gives merely the sequence of events, and does not show the rate at which the changes in the system take place. It will now be shown how the time may be found as a function of x.

Consider the equation

$$\frac{dx}{dt}=\frac{1}{2}\frac{\tau^2}{\mathfrak{g}}\sin 4f\left(1-\frac{\Omega}{n}\right),$$

f is here the angle of lag of the sidereal semi-diurnal tide of speed $2n$; then by the theory of the tides of a viscous spheroid, $\tan 2f=2n/\mathfrak{p}$, where $\mathfrak{p}$ is a certain function of the radius of the planet and its density, and which varies inversely as the coefficient of viscosity of the spheroid.*

Since by hypothesis the viscosity is small, f is a small angle, so that $\sin 4f$ may be taken as equal to $2\tan 2f$. Thus, $\sin 4f/n$ is a constant, depending on the dimensions, density, and viscosity of the planet.

It has already been shown that τ^2 varies as x^{-12}, and $\mathfrak{g}$ is a constant, which depends only on the density of the planet. Hence, the above equation may be written

$$x^{12}\frac{dx}{dt}=\mathrm{K}(n-\Omega),$$

where K is a certain constant, which it is immaterial at present to evaluate precisely.

Since $n=h-x$ and $\Omega=x^{-3}$, we have

$$\mathrm{K}dt=\frac{-x^{15}dx}{x^4-hx^3+1},$$

or

$$\mathrm{K}t=-\int\frac{x^{15}dx}{x^4-hx^3+1}+\text{a const.}$$

The determination of this integral presents no difficulty, but the analytical expression for the result is very long, and it does not at present seem worth while to give the result. The actual scale of time in years will depend on the value of K, and this is a subject of no interest at present.

It will, however, be possible to give an idea of the rate of change of the system without actually performing the integration. This may

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* "On the Bodily Tides of Viscous and semi-elastic Spheroids," &c. "Phil. Trans.," Part I, 1879, p. 13, § 5.

be done by drawing a curve in which the ordinates are proportional to dt/dx, and the abscissæ are x. The equation to this curve is then

$$K\frac{dt}{dx}=\frac{-x^{15}}{x^4-hx^3+1}.$$

The maximum and minimum values (if any) of dt/dx are given by the real roots of the equation

$$11x^4-12hx^3+15=0.$$

One of such roots will be found to be intermediate between a and b, and the other greater than a.

Fig. 8.

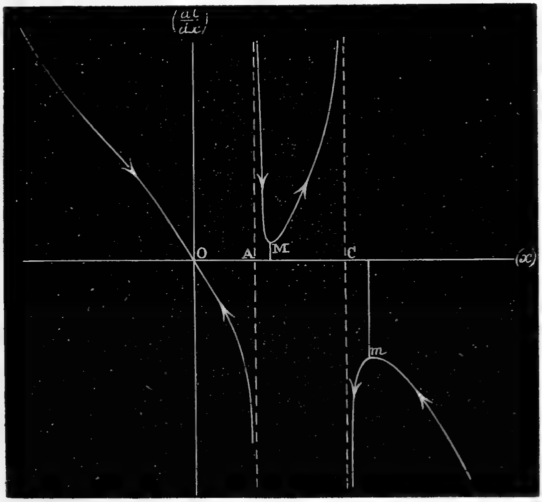

Diagram illustrating the Rate of Change of the System.

Fig. 8 shows the nature of the curve when drawn with the free hand. It was not found possible to draw this figure to scale, because when $h=2{\cdot}6$ it was found that the minimum M was equal to ·85, and could not be made distinguishable from a point on the asymptote A, whilst the minimum m was equal to about 900,000, and could not be made distinguishable from a point on the asymptote C.

The area intercepted between this curve, the axis of x, and any pair of ordinates corresponding to two values of x, will be proportional to the time required to pass from the one configuration to the other.

Where dt/dx is negative, that is to say, when the satellite is falling into the planet, the areas fall below the axis of x. This is clearly necessary in order to have geometrical continuity in the curve.

The figure shows that the rate of alteration in the system becomes very slow when the satellite is far from the planet; this must indeed obviously be the case, because the tidal effects vary as the inverse sixth power of the satellite's mean distance.

V. "On the Modifications of the Spectrum of Potassium which are Effected by the Presence of Phosphoric Acid, and on the Inorganic Bases and Salts which are found in combination with Educts of the Brain." By J. L. W. THUDICHUM, M.D., F.R.C.P.L. Communicated by JOHN SIMON, C.B., F.R.S., &c. Received March 10, 1880.

Among the results of a large investigation on which I have for many years been engaged in regard of the chemistry of the brain, I had been led to conclude that the so-called "protagon" of Oscar Liebreich is not a definite chemical body, but is a variable mixture of several bodies. This conclusion of mine (which agrees with opinions expressed on the same subject by Strecker, Diaconow, and Hoppe-Seyler) was published by me in 1874,* and endeavours to controvert it have since then been made, on several occasions, by Dr. Arthur Gamgee.† Last summer, he brought before the Royal Society‡ his contentions for the chemical individuality of "protagon"; and it fortunately was in my power shortly afterwards to publish evidence, which, I believe, those who will take the trouble to follow it will find quite unanswerable, that Dr. Gamgee's contentions were mistaken.§ Part of my evidence to that effect consisted in showing by quantitative analyses that Dr. Gamgee's so-called "protagon" contains 0·7 per cent. of potassium; secondly, that in connexion with trifling differences in the extraction process, the proportion of potassium in different specimens of "protagon" can be made to range from a trace to 1·6 per cent.; thirdly, that with the variable quantities of potassium the quantities of phosphorus and other ingredients will also vary.

In the last published number, No. 200, p. 111, of the "Proceedings of the Royal Society," I find that Dr. Gamgee has recently brought the question again under notice of the Society, and that, in doing so, he especially rests his case upon the following statement made by his colleague, Professor Roscoe, on the subject of some examinations,

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* "Reports of the Medical Officer of the Privy Council and Local Government Board." New Series. No. III.

† "Zeitschrift für Physiol. Chemie," vol. iii, p. 260; "Ber. Deutsch. Chem. Ges.," 1879, &c.

‡ "Proc. Roy. Soc.," vol. xxix, p. 151.

§ "Annals of Chemical Medicine." Edited by J. L. W. Thudichum. Vol. i, p. 254.

which, at Dr. Gamgee's request, he had made for him: see "Proceedings," vol. xxx, p. 113:—"I have examined spectroscopically for potash a sample of protagon furnished me by Dr. Gamgee, and labelled 'Protagon, twice recrystallised, Blankenhorn.' I could not detect any potash by the spectroscope in the incinerated mass from 0·1 grm. of substance. With the carbonised mass obtained from 1·0 grm. of substance I obtained the potasssium line (α) very faintly, and from comparative experiments with a dilute solution of a potassium salt I estimate the quantity of potash in 1 grm. of the substance not to exceed $\frac{1}{20}$ mgrm. The carbonised residue of 1 grm. of protagon was carefully oxidised with pure nitric acid, when a small quantity of fused metaphosphoric acid remained after ignition. The residue weighed 0·0278 grm., corresponding to 1·08 per cent. of phosphorus.—(Signed) H. E. Roscoe."

As regards the bearing of Professor Roscoe's evidence on the question that has been raised before the Royal Society, and as regards some other relations of the same question, I beg leave to submit to the Royal Society the following observations:—

1. *On the Modifications of the Spectrum of Potassium which are effected by Phosphoric Acid.*

It is well known in spectroscopy that the best spectra of metals are obtained in the easiest manner by aid of their most volatile compounds, such as chlorides; and, on the contrary, that salts which are more or less fixed at high temperatures, such as the silicates and phosphates of alkalies and earths, give either no spectra at all or only very feeble spectra. Thus, if a minute particle of *potassic bromide* be ignited in the aërated gas flame before the slit of the spectroscope, the spectrum of potassium is at once produced in the most brilliant manner: particularly the line in red is brilliant and sharply defined, while the gas flame to the naked eye shows the violet colouring of potassium. But when a similar particle of *potassic phosphate* is ignited under similar conditions, the flame, instead of showing the violet colour of potassium, shows a greenish-yellow colour produced by volatilised phosphoric acid: and in the spectroscope, at first, only the continuous spectrum of phosphoric acid (not to be confounded with the continuous spectrum of potassium) is seen, till gradually, and at a white heat, a feeble potassium line in red, with indistinct edges, is perceived.

Even a large bead of pure potassic phosphate, when ignited before the slit of the spectroscope, never produces, even at white heat, any such intense red potassium line as the smallest bead of potassic chloride, potassic bromide, or potassic nitrate. And, in contrast with the latter salts, which volatilise quickly, the phosphate takes some time before it is completely volatilised.

When such an indolent bead of potassic phosphate which has given a feeble potassium line in red, is dipped into syrupy phosphoric acid and again ignited, the continuous spectrum of volatilising phosphoric acid is again at first obtained, the aërated gas flame to the naked eye is enveloped in a greenish-yellow mantle, and in the spectroscope the continuous spectrum of phosphoric acid only, stretching far into the blue, and without a trace of the potassium line in red, is seen. Gradually the bead flows again as a transparent glass, and then the red potassium line reappears in the spectroscope though without ever reaching the vivacity of the line produced by volatile salts. If now the bead have added to it a particle of a chloride—for example, zinc chloride—and be again ignited, a lively potassium spectrum will be seen in the spectroscope, *i.e.*, the potassium goes away as chloride. That double decomposition with the separation of the products takes little more time than the volatilisation of a bead of potassic chloride of about the same size.

Evidently then, if it were intended to estimate from the spectroscopic signs the concentration of a given potassic solution, it would be necessary to take into account not only the intensity of the potassium line produced during the volatilisation of the solution, but also the length of time during which that line is perceived. If solutions equally concentrated of potassic chloride and potassic phosphate are similarly examined before the spectroscope, the phosphatic solution is found to give the feebler band, but to give it for the longest time; and no spectroscopic observation with regard to such a solution can pretend to any validity for purposes of quantitative comparison unless the observer be equally attentive to the duration as to the intensity of the phenomenon.

Referring now, in the light of these considerations, to Professor Roscoe's letter, I wish first of all to point out that his observation, when made with the carbonised mass from 1 grm. of substance corroborated my assertion that the substance contains potassium. And I may add, by the way, that in my paper on which Dr. Gamgee comments to the Royal Society, I have given sixteen quantitative analyses of "protagon" and its congeners with regard to the quantity of potassium contained in each; having made each determination of potassium by ordinary gravimetric methods, and having weighed the potassium as platinochloride.

Secondly, however, Professor Roscoe estimates that the quantity of potash contained in a gram of Dr. Gamgee's "protagon twice recrystallised" did not exceed $\frac{1}{20}$ mgrm., an estimate which he appears to found on the faintness of the potassium line (α) obtained by him from that substance as compared "with a dilute solution of a potassium salt." This inference I think it necessary to check by the considerations I have before stated. Professor Roscoe states that he obtained from 1 grm. of "protagon" a residue of 0·0278 grm. of metaphosphoric acid, but according to his spectroscopic estimate, less than $\frac{1}{20}$ mgrm. of potassium. What is the value of the latter half of this statement when taken with the context of the other half?

Before Professor Roscoe could possibly obtain from such a substance as he seems to have examined any spectroscopic evidence of the presence of potassium, he would have had to drive away from his platinum wire a relatively enormous amount of free phosphoric acid. He would then have had some potassic phosphate left, which, as we have seen, gives only a feeble and indistinct potassium band in the red, even when a large bead is heated in the flame before the spectroscope, and which, in so extremely small an amount as he must have had, would of necessity give a fallaciously weak reaction.

Further, Professor Roscoe seems not only to have thus overlooked the inhibitive or weakening influence of phosphoric acid in relation to the spectrum of potassium, but has also omitted to give any objective measures of the intensities which he purports to compare, has moreover not recorded the length of time during which the compared phenomena continued, and has not even named what was the particular potassic salt used by him as a standard in his comparison.

Apart, therefore, from the inherently small worth of spectroscopy as a quantitative method, not even those precautions of which the spectroscopic method admitted were employed by Professor Roscoe, and I must therefore respectfully submit my opinion that his experiment is of no practical value.

2. *On the Inorganic Bases and Salts which are found in combination with the Educts of the Brain.*

Frémy, in his research on the brain published in 1841, stated that the body which he had isolated and termed "cerebric acid," was frequently combined with soda and phosphate of lime. Another substance, which he termed "oleophosphoric acid," he assumed to be ordinarily in the state of soda salt, but sometimes combined in part with phosphate of lime. He separated these bases and salts by dissolving the mixture of the two bodies (cerebric and oleophosphoric acid) in boiling absolute alcohol, which had been made slightly acid by sulphuric acid. Sulphates of lime and soda remained suspended mixed with some insoluble organic matter, and were removed by filtration. The cerebric and oleophosphoric acids were in solution, and deposited on cooling. From the mixture oleophosphoric acid was extracted by ether, which left the cerebric acid behind. Frémy further found that cerebric acid combined with all bases, and must be considered a true acid. When heated with dilute solutions of potash, soda, or ammonia it did not dissolve, but combined with each of these bases. The compounds were also obtained by mixing any of these bases with an alcoholic solution of cerebric acid. Thus, he prepared the insoluble barium compound containing 7·8 per cent. of baryta.

Bearing this experience of Frémy in mind during my researches on the chemical constitution of the brain, I invariably examined for inorganic ingredients all the educts which I obtained from the brain.*

Of KEPHALIN (the most important of the phosphorised ingredients of the brain) 10 grms. were dissolved in one litre of water, and after filtration precipitated by hydrochloric acid. The solution of hydrochloric acid and other matters filtered from the precipitated kephalin was evaporated to dryness. A portion was then boiled with solution of baryta, when traces of *ammonia* were evolved. The rest of the residue was then ignited in a platinum dish to destroy all traces of organic matter. The ash was but slightly fusible; was only partially soluble in water, but was easily soluble in water slightly acidified by hydrochloric acid. The solution with ammonia gave an abundant precipitate of *earthy salts*, and the solution assumed a *deep blue colour*. Precipitate and solution were separated by filtration.

The precipitate dissolved readily in a little hydrochloric acid, forming a slightly red solution, suggesting the presence of *iron*, which was confirmed by the sulphocyanide test. In another portion of the solution dilute sulphuric acid gave an abundant precipitate of gypsum, showing presence of *calcium*. In another portion the molybdate test showed the presence of *phosphoric acid*. To the remaining portion a few drops of ferric chloride were added, then sodic carbonate nearly to neutrality, and, lastly, barytic carbonate. The mixture was allowed to stand, filtered, and the filtrate, free from excess of baryta by sulphuric acid, was again filtered, and the filtrate, after supersaturation with ammonia, gave a great precipitate with ammonic oxalate, showing presence of much *calcium*. The filtrate from this calcic oxalate on concentration, and treatment with ammonia, ammonic chloride, and sodic phosphate, gave the precipitate characteristic of *magnesium*. The precipitate produced as above described by baryta carbonate, was boiled with excess of pure soda, and the filtrate was warmed with ammonic chloride; only a slight turbidity ensued, so that it may be assumed that *aluminium* was absent.

The *alkaline filtrate* from the foregoing precipitate by ammonia was tested for lime by oxalate, when a considerable precipitate was produced, showing the presence of *lime uncombined with phosphoric acid*, and which therefore must have been in combination with part of the kephalin. The blue solution was again filtered from calcic oxalate, which had been entirely precipitated, and was acidified with hydrochloric acid. The *copper* was precipitated by hydrothion, the filtrate

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* That part of my experience relating to kephalin and myelin is recorded in "Reports of the Medical Officer of the Privy Council and Local Government Board," new series, No. III (1874), p. 129, and *ibid.*, No. VIII (1876), p. 131.

evaporated to dryness, ignited, and the residue tested for alkalies. This residue exceeded in bulk the bases previously removed. It was fusible with ease, and on solidification became white and crystalline, but interspersed with many red particles of ferric oxide. It was a mixture of *potassic and sodic chlorides,* and of some *ferric oxide,* which, owing to the presence of the chlorides, had escaped precipitation by excess of ammonia.

It was, therefore, proved that the kephalin, obtained by the alcohol and ether process, and purified by solution in water and filtration, as described at length in my researches above quoted, is, at least in part, combined with *ammonium, sodium, potassium, calcium, iron, copper,* and *calcic and magnesic phosphates.* This experience was repeated a great number of times, and, in the aggregate, on several hundred grams of dry kephalin.

In the following experiments MYELIN (the phosphorised ingredient which stands next in importance to kephalin in the chemistry of the brain)* was examined for inorganic bases, in order to obtain some knowledge regarding their relative amounts and nature. The myelin was dissolved in water, precipitated by hydrochloric acid, and the acid solution separated by filtration. The *copper* was removed by hydrothion, the filtrate evaporated to dryness, and the residue ignited in a platinum dish. During this ignition much *ammonic chloride* was given out, and its nature distinctly proved by condensing and analysing it.

The fused mass was dissolved in dilute hydrochloric acid, and the solution so obtained treated with excess of ammonia. The white gelatinous precipitate which was thrown down was scarcely coloured black by ammonic sulphide, indicating the presence of only a trace of iron. The precipitate and filtrate were submitted to analysis, qualitative and quantitative, as follows:—

The precipitate was dissolved in dilute hydrochloric acid, the solution was nearly neutralised by sodic carbonate, after addition of some ferric chloride; an excess or barytic carbonate was now added; the mixture was shaken, allowed to stand, and then filtered. From the concentrated filtrate the barium was then removed by sulphuric acid, and the filtrate was precipitated in the presence of excess of ammonia by ammonic oxalate. The oxalate so obtained was converted by intense ignition into oxide, which weighed 0·1994 grm., equal to 0·1423 grm. *calcium.* The filtrate and washings from the calcic oxalate were treated as usual for the quantation of magnesium, and there was obtained 0·5556 grm. magnesic pyrophosphate, equal to 0·1212 grm. *magnesium.* The precipitate which had been produced by ferric chloride, and sodic and barytic carbonates, was dissolved in

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* Reports, as above, No. III, p. 156.

nitric acid, and the phosphorus estimated by precipitation with ammonic molybdate, and transformation of the precipitate into magnesio-ammonic phosphate, &c. This gave 0·3266 grm. *phosphorus*, equal to 0·7480 grm. phosphoric anhydride.

The filtrate was proved to be free from magnesium and calcium. The potassium contained in it was extracted in the usual manner, by means of platinic chloride, giving 11·2766 grms. of the double salt ($2KCl,PtCl_4$) equal to 3·651 grms. potassic chloride, or 1·911 grm. of *potassium.* The mother-liquor and washings were evaporated to dryness, and the platinum was removed by extracting the ignited mass with water acidified by hydrochloric acid. The extract so obtained was evaporated to dryness and fused; the residue weighed 3·45 grms., being pure sodic chloride equal to 1·356 grm. sodium. The quantities of sodium and potassium thus found were controlled by the analysis of a separate part of the original solution containing their chlorides; and on calculating from the residue obtained on evaporation there should have been found a total quantity of chlorides = 7·09 grms. There was found potassic chloride = 3·651, and sodic chloride = 3·450; together, 7·101 grms.

Assuming the above earthy bases to have originally existed in myelin as tribasic phosphates, and any phosphoric acid remaining over to have been in combination with potassium, then we have,

Calcium .. = 0·1423 grm. existing as $3CaO,P_2O_5$.
Magnesium .. = 0·1212 ,, ,, $3MgO,P_2O_5$.
Potassium .. = 0·5663 ,, ,, $3K_2O,P_2O_5$.
Potassium .. = 1·3447 ,, } combined directly with
Sodium .. = 1·3560 ,, } myelin.
Phosphorus .. = 0·3266 ,, existing as P_2O_5 (0·748 grm.)

and distributed between the calcium, magnesium, and potassium.

In the kephalin research, related above, it was proved that kephalin was partly in combination with calcic oxide or lime, without any other acid being united with that base. In the myelin research it was found that, in myelin, there may be in combination even a much greater quantity of potash and soda than of lime.

A third research was now made on the mixture of phosphorised and nitrogenised substances, which Frémy termed "CEREBRIC ACID," and which later writers have termed "PROTAGON." This, after exhaustion with ether, was redissolved eight times in hot spirit, and after filtration reprecipitated on cooling. It was pressed to remove all mother-liquor, was then suspended in water and heated until it became thoroughly emulged with it; acid, either sulphuric or hydrochloric, was now added, whereupon the organic matter curdled and contracted; it was separated, and the acid solution was treated like the above described similar solution' from kephalin and myelin. Considerable quantities of the same bases and salts as those obtained from kephalin and myelin were isolated, but not examined any further.

It had been ascertained in previous operations on these mixed phosphorised and cerebrin bodies, and was now again observed, that one treatment with acid was insufficient to free them entirely from bases. The bodies which had been treated with acid were, therefore, treated with lead acetate solution in the cold, and were allowed to stand. The liquid which formed was filtered from, and pressed out of the lead-compounds, and on proper treatment yielded as follows:—*sodium*, = 1·78 grm.; *potassium*, = 1·185 grm.; *calcium*, = 0·08 grm.; and *magnesium*, 0·02 grm. The sodium was weighed as chloride; the potassium, 0·856 grm., as bitartrate, and 0·32 grm. as platinic chloride; the calcium as carbonate; and the magnesium as pyrophosphate. The respective preparations are herewith submitted for inspection.

Curious facts which deserve notice are the following. When so-called "protagon" was decomposed by baryta to obtain neurin, it was observed that the platinic chloride salt of the base always *contained potassium*. When the neurin was combined with phosphomolybdic acid, and carefully washed, and then again transformed into platinic chloride salt, it became free from potassium. In both cases the salt was crystalline, but the crystallisation was different in the two cases. The crystals containing potassium were most perfectly formed individuals, sometimes more than half an inch in length and breadth, and an eighth of an inch in thickness, while, on the other hand, the crystals free from potassium were bundles of needles, of lighter aspect and not admitting of angular measurement. The experience shows that the platinic chloride compound of potassic chloride is dimorphous, and in one of its forms is isomorphous with the platinic chloride salt of neurin hydrochlorate. It is further remarkable that the impure isomorphous mixture crystallised in well-defined individuals, while the pure salt crystallised in confused masses and in bundles of needles. The crystals of the perfect type, when powdered and burnt on a platinum wire in an aërated gas flame before the spectroscope, produced the line in red characteristic of potassium. This specimen is herewith submitted for inspection, as also a specimen of the salt free from potassium.

The foregoing data may perhaps contribute to explain some of the discrepancies which have been found between different specimens of neurin platinic chloride, and have caused the inconvenience of a number of different formulæ being attributed to neurin, namely, $C_5H_{15}NO_2$, or $C_5H_{14}NO$, or $C_5H_{14}N$, or $C_5H_{12}N$. They may also explain why neurin from "protagon" has been supposed to consist of a mixture of two bodies, having respectively the formulæ $C_5H_{15}NO_2$, termed oxyneurin, and $C_5H_{15}NO$, termed neurin. According to my own numerous analyses (Reports as above, No. VIII, p. 118, *et seq.*), the principal ammonium base from the phosphorised substances has, after purification by the phosphomolybdic process and combination with hydrochloric acid and platinic chloride, invariably the composition $C_5H_{13}NO$.

The stoichiometric relations of such a base, in its platinic chloride salt, would be doubly disturbed by the presence of potassium ; for not only would a portion of the platinic chloride, namely that combined with potassic chloride, be erroneously referred to neurin, but the entire amount of platinum, if estimated, as is usual by combustion, would be raised in apparent amount by the presence of the potassic chloride as an invisible impurity.

The ALBUMINOUS INGREDIENT of the brain contains considerable amounts of the usual salts, particularly calcic phosphate, of which the calcium is obtained as gypsum, when the albumin is treated with dilute sulphuric acid, and the solution of hemi-albumin is filtered from the undissolved hemi-proteïn and slowly evaporated. By combustion of the albuminous matter of the brain, all the usual salts and bases, including copper, iron, and manganese, are liberated.

The greater part of the *soluble salts* contained in the brain passes into the water extracts, while the greater part of the *insoluble salts*, such as phosphates of earths, remain with the albumin ; but a portion of both remains with the phosphorised and nitrogenised principles, and follows them into all their solutions in ether and alcohol. Only *sulphates* have not been found in these latter, though they are present in the water extracts.

It follows that all educts from the brain have to be freed from the salts or bases described before they can be subjected to quantitative chemical examination ; and an error which seems to me to pervade all analyses of the brain for inorganic ingredients hitherto is the following. When the brain is burnt in its entirety, the phosphoric acid resulting from the destruction of the phosphorised compounds expels sulphuric, hydrochloric, and carbonic acids, and comes to be considered as an inorganic ingredient, the fact of its previous combination being overlooked. When the brain is freed from matter soluble in spirit and ether, and the albumin and water extract alone are examined for inorganic ingredients, the fallacy otherwise introduced by the phosphoric acid in organic combination is no doubt avoided ; but the inorganic salts described above, as in combination with the phosphorised and nitrogenised matters are overlooked and left out of consideration.

A vast range of other organic analyses (animal and vegetable) for inorganic constituents is no doubt affected by the same fallacy, and it seems to me that the right making of such analyses is a much more complicated problem than has hitherto been believed.

VI. "On Magnetic Circuits in Dynamo- and Magneto-Electric Machines. No. 2." By Lord ELPHINSTONE and CHARLES W. VINCENT, F.R.S.E., F.C.S., F.I.C. Communicated by Professor G. G. STOKES, Sec. R.S. Received March 10, 1880.

A large amount of magnetism is retained by the soft iron cores of electro-magnets, when arranged so as to form a complete magnetic circuit: and sparks and other indications of the passage of an electric current can be obtained at the ends of the helix wires surrounding those soft iron cores, each time the masses of iron are separated and the closed magnetic circuit opened. In order to procure a spark the breaking of the circuit must be effected suddenly, either by a jerk, tilt, or sliding movement.

In the case of the 58 lb. magnet described in our former note, the current that is capable of causing a spark, although only momentary in duration, is found to be sufficient in quantity and intensity to magnetize a small electro-magnet, weighing with its coils between 5 and 6 lbs., enabling it to sustain its own weight for any indefinite time when suspended by its armature.

When the armature of the small magnet is placed at the distance of $\frac{1}{8}$ of an inch from its poles, in such a manner as to be free to move, the instant the armature of the large magnet is suddenly tilted or slid off it darts to them, the completion of the circuit of the small magnet being signalled by a smart click. The rupture of one closed magnetic circuit is thus caused to produce another closed magnetic circuit.

But when the interval between armature and magnet, whose circuit it was intended to close, exceeded $\frac{1}{4}$ of an inch, the former was not attracted with sufficient force to overcome the friction of the table upon which it was resting.

The mode of removing the armature from the large magnet appeared to be of no moment, but the time occupied by the removal had much influence upon the amount of magnetic force manifested in the smaller circuit. This was particularly the case if there were an interval, no matter how small, between the armature and the poles of the magnet round which the electric current was sent.

For example, if with an interval of $\frac{1}{16}$ of an inch between the armature and the poles of the small magnet, the armature of the large magnet was slowly slid off, the magnetization of the small magnet never rose to a sufficient intensity to draw its keeper to itself, whereas, when the sliding took place rapidly, the small armature was strongly attracted as above mentioned.

The largest amount of magnetization was bestowed upon the small electro-magnet by the interaction, when it was held upright, its poles being completely covered by a closely fitting armature. And it was also found that when thus set up in preparation for the formation of a closed magnetic circuit, the magnetization was produced by a much slower motion of the large armature than when the small magnet had its circuit partly open. When the circuit was completely closed, if the large armature were twisted off by a slow equable motion, in such a manner that both poles were uncovered at the same time, then the small magnet could be made to sustain not only its own weight (between 5 and 6 lbs.) but an additional 3 lbs. also.

During the passage of the electric current, obtained by the forcing open of the closed circuit, the fall of magnetism in the large magnet itself is checked, the direction of the magnetic polarity remaining unchanged, the current checking or opposing the fall being in the same direction as that from the battery which caused the primary magnetization. If the ends of the helix wires are not connected together this effect is not obtained.

Electric currents, though of less intensity and quantity, can be produced in the helices of electro-magnets, without altogether breaking up the closed magnetic circuits. For instance, with the 58 lb. electro-magnet, the circuit being completely closed by its armature, and the helices being connected with a galvanometer, a very slight pull applied to the armature produces a current of electricity giving a considerable deflection of the needle in the same direction as the battery current; and the stronger the pull the greater the deflection of the galvanometer needle, up to the point at which the magnet is lifted from the ground, after which no further motion of the needle is produced, unless the magnet is subjected to additional strain. Thus, hanging a 4 lb. weight upon the uplifted magnet, produced deflections in the same direction as the pull on the armature, and on removal of the weight produced reverse deflections.

Trying the same set of experiments with a very small electro-magnet, so that we might proceed to absolute rupture of the closed magnetic circuit without danger to the galvanometer, we found that the addition of successive weights to the magnet while hanging suspended by its armature, produced successive deflections of the galvanometer, the needle coming to rest at zero after each addition, as in the case of the large magnet.

When the maximum weight which the magnet was capable of sustaining was reached, and a real movement of the armature commenced, the induced current in the helix of the electro-magnet was very greatly increased by the addition of even the smallest weight.

From these experiments it may be inferred that in like manner as the passage of an electric current round a bar of iron produces elongation of the bar, so the elongation of the bar produces in its turn an electric current in the helix, which tends to strengthen the magnetization; and also that a magnet is absolutely stronger under tension than when at rest.

On the other hand, pressure on the armature, either continuous or sudden and momentary (a blow for example), causes an electric current in the helices in the opposite direction to original magnetization, or in other words, against magnetization; tending thereby to weaken the power of the magnet.

The 58 lb. magnet in closed circuit was hung by its armature, and on afterwards connecting its helices with the galvanometer no current could be detected, but on lowering it until it rested with its whole weight on the ground a current in the direction of demagnetization was produced, giving a deflection of 15°. In the same way a current in the direction of magnetization was obtained, giving a deflection of 15°, by the application of sufficient strain to lift the magnet off the ground, and this result was invariable. The degree of swing, however, depended upon the rapidity with which the magnet was either raised or lowered.

It may be remarked that whereas any very slight application of force by pulling on the armature was sufficient to cause a current in the helices giving a deflection of 5° to 10° of the galvanometer needle, a great amount of pressure is necessary to produce a similar deflection. A slight pull with the finger and thumb in the one case was equal to the pressure of a hundredweight in the other.

By the momentary removal of the armature, the closed magnetic circuit is broken, and though by its immediate restoration a new closed circuit is formed, nevertheless the tension on the molecules of iron by the magnetic stress is very greatly reduced. Under these conditions a very slight pressure upon the armature produces a great swing of the needle, whilst a pull produces scarcely any effect at all until actual movement of the armature takes place.

If the pressure on the armature is great and continuous, a point is soon reached at which a slight pressure is no longer effective.

The effects produced are somewhat different if pressure is applied unequally. For instance:—A weight of 7 lbs. placed on the armature over the north pole of the 58 lb. magnet caused a current in the helices giving a deflection of 20° at the galvanometer. The same weight on the south pole gave the same deflection in the opposite direction, Pressure with the hand produced like swings of the needle proportionate to the force used, and the amount of swing can be easily controlled, and the needle brought to rest by judicious pressure on either pole of the magnet.

If a lateral pressure be applied to one side of the armature between the poles, and the needle swings say 5°: on removal of the pressure, a current is produced in the opposite direction, and the reverse swing in place of being 5° will be 8°, and so on in proportion to the amount of force made use of.

None of the above-mentioned effects could be shown with the small magnets under pressure: and it was not found possible to produce a recognisable current without actual movement of the armatures.

Under certain circumstances the attractive force of electro-magnets in closed magnetic circuit is found to increase with lapse of time. For example :—A small U-shaped electro-magnet with limbs 6 inches long, having a core of $\frac{3}{4}$-inch iron, and helices consisting of four layers of No. 16 covered copper wire, when excited by four Bunsen cells, supported as an armature a similar U-shaped iron bar, but without a helix upon it, this latter remained firmly attached after the voltaic current had ceased, but the hanging on to it of an additional weight of 3 lb. 6 oz. instantly wrenched it away from the electro-magnet, and broke the closed magnetic circuit.

The magnet was then re-excited, the armature being fixed to the electro-magnet by being held in contact with the poles whilst an electric current, of a few seconds' duration, passed through the circulating wire. In place of immediately attempting to add any additional weight, the two iron U's were left hanging face to face, in the form of the link of a chain, for twenty-four hours, at the end of which time the weight of 3 lbs. 6 oz. was hung on and sustained. Forty-eight hours later, an additional weight of 3 lbs. 10 oz. was carefully added, making in all 7 lbs. sustained. Twelve hours afterwards 1 lb. more was added, bringing up the entire weight to 8 lbs. beyond that of the armature; this was suffered to remain for five days, when the system was taken to pieces.

On a subsequent occasion the same magnet sustained an entire weight of 10 lbs. beyond that of the U-shaped armature, the weight sustained being reached by beginning with an amount well within the sustaining power of the electro-magnet wire in closed circuit, and increasing it by small additions made with intervening intervals of time varying from twelve hours to several days.

Another and smaller U magnet was likewise experimented on; this weighed with its coils 3 lbs. 6 oz. Its armature was a strip of soft iron completely covering the poles, and having a hook in the centre, to which weights could be easily attached.

This electro-magnet was excited by the passage, for a few seconds, of the current from two one-pint bichromate cells. On breaking battery contact, the armature failed to sustain 4 lbs. The electric current was again sent round the electro-magnet, and the armature was pressed against the poles, being carefully adjusted so as to cover them completely, and at the same time to place the hook precisely in the centre, so that the pull should be fair and equal when a weight was hung upon it. By this careful manipulation, on breaking contact with the bichromate cells, the closed magnetic circuit was found capable of sustaining the 4 lb. weight.

By successive additions of 2 oz. weights, made at intervals of a few minutes, the weight hanging to the armature was raised to 5 lbs., after which the attempted addition of 2 oz. caused the disruption of the system.

The experiment was repeated under similar conditions, but with slightly extended intervals of time between the additions of the 2 oz. weights. The magnet in closed circuit was made to hold 4 lbs., 4¼ lbs., 4½ lbs., 4 lbs. 14 oz., 5 lbs. 2 oz., the time taken in all, for the successive additions, being ten minutes. The system was then left for twelve hours, when by additions of 4 oz. at intervals of a few minutes the weight sustained was increased to 6 lbs. 4 oz. Eleven hours later, this was further increased to 7 lbs. 6 oz., and two hours afterwards to 8 lbs. 2 oz.

A still smaller electro-magnet, weighing with its coils 5 oz., and having an armature consisting of a very thin slip of soft iron, when excited by one of the bichromate cells, could not be made when in closed circuit to sustain 1½ lbs. at the moment of breaking the voltaic circuit. It, however, sustained 1 lb. with ease. The latter weight was therefore suspended, and the cell wires removed after the closed magnetic circuit was completed. By successive additions of 2 oz. weights at short intervals of time (five minutes to ten minutes each), this small magnet could be made to sustain 2 lbs. 2 oz., but the addition of 1 oz. beyond this weight at once separated the armature and magnet. It was thought that a longer interval of time should, as in the former instances, enable the magnet to sustain a still greater weight. It was therefore brought into closed circuit, as before, and made to sustain 2 lbs. 2 oz. in the manner just related, and was thus left for twelve hours. Successive additions of 2 oz. were then made to the hanging weight until it reached 2 lbs. 14 oz. Twenty-four hours afterwards, 4 oz. more were added, bringing the entire weight suspended to 50 oz.

This small, soft iron magnet which, at the instant the voltaic current was withdrawn, was totally unable to sustain five times its own weight, was thus, by gradual growth of its magnetic force, enabled to hold ten times its own weight.

In the course of these experiments it was remarked that the longer the period the soft iron remained in closed magnetic circuit the more magnetically ductile did its molecules appear to become. An electro-magnet, which had been for a few days in closed circuit, could after rupture of the circuit be made to sustain weights in a fresh closed circuit at much shorter intervals of time than if it was magnetized, after being for some time with its poles uncovered. The direction of the battery current with reference to the residual magnetism of the electro-magnets appeared to be of no moment. A magnet which had been left for some time with its poles uncovered had less residual magnetism after a momentary current had passed through its helices, than another magnet which had been in active closed circuit, even if the battery current had, in the latter case, to overcome a considerable amount of residual magnetism.

We found, moreover, that soft iron magnets retain their residual magnetism longer, and are capable of acquiring increased magnetization much more rapidly after having been bearing weights (thereby keeping the iron in a state of strain), than if they have been left in their normal condition and without bearing any weight at all.

The conditions under which the closed magnetic circuit retains its force are not yet clearly established.

With the 58 lb. magnet a succession of gentle taps struck vertically with a wooden mallet upon the centre of the armature, while resting on the magnet in closed circuit, in a very few moments completely dissipated the magnetic force so far as the sustaining power of the magnet was concerned.

Removal of any portion of the weight suspended to the armature of a magnet hung up in closed circuit likewise tends to dissipate the force of the circuit. For example:—Half an hour after the removal of a weight of 10 lbs., which had been suspended to the armature of a U magnet for twenty-one days, the armature fell off on receiving a slight touch. In another experiment, a U magnet, which was capable of sustaining 7 lbs., and which had actually been suspending 4 lbs., was left for two months with the armature on only, the weight having been removed; at the end of that time a very slight shake was sufficient to cause the armature to fall off. Many other examples might be quoted to show that release from strain diminishes the magnetic force of the circuit.

In these experiments, in which the closed magnetic circuits had given way, the soft iron had been in a state of strain from which it had been released by the removal of the suspended weights. But when no weights were hung upon the armature, and the iron had never been in a state of magnetic tension, the closed magnetic circuit, so far from diminishing, increased in force. The 58 lb. magnet was excited with a voltaic current so feeble, that although the magnet could be lifted by the armature in closed circuit, yet great care was necessary that the lift should be exactly vertical; and very little force was required to slide the armature off the poles. After the lapse of a month the armature was so firmly held that the utmost exertion of manual force could not stir it by a sliding movement, and the whole magnet could be raised from the ground even if tilted as much as 15° from the perpendicular.

The magnetism of the closed circuit of the 58 lb. magnet disappears after repeated up and down movements of either one or both of its helices, provided the ends of the helix wires are connected together either singly in two separate circuits, or together in one continuous circuit. Every up or down movement of either of the helices produces currents in the wires either for or against magnetization, which currents apparently so disturb the molecules of the iron that the fixity of their original magnetic direction is lost.

In like manner as the movements of the armature, or the increased or diminished tension of the iron, produce currents of electricity in the helix wires surrounding the magnets: so the movements of the helices produce currents of electricity which may either magnetize or demagnetize the iron. With the 58 lb. magnet in closed circuit, the two ends of one of the helices being connected to the galvanometer, and the two ends of the other helix being connected with each other, the latter helix is moved towards the armature, a current is produced in the galvanometer helix which shows a fall of magnetization. On moving the same helix away from the armature, a current is produced in the direction of magnetization.

In another experiment 30 yards of No. 16 covered copper wire, with its ends connected together, and so coiled that it could be moved freely from pole to pole over the armature, was placed on one limb of the 58 lb. magnet and the closed circuit established. Both helices were then brought into continuous circuit through the galvanometer.

On movement of the coil of wire from south limb to the north limb of the magnet, a current was produced showing an increase of magnetization. On moving the coil in the opposite direction, *i.e.*, over the north limb pole, and on to the south one, the current is reversed, and is in a direction which would cause demagnetization.

It appears, therefore, that any interference with the lines of force about a magnetic circuit, means an interference with the magnetic circuit itself, and points to the possibility of building up magnetic force of magnets by the mere movement of wires in these lines of force, though the coils moved need not of necessity be connected with the helices surrounding the magnets.

VII. "Some further Observations on the Influence of Electric Light upon Vegetation." By C. WILLIAM SIEMENS, D.C.L., LL.D., F.R.S. Received March 18, 1880.

When upon the 4th of March, I presented to the Royal Society a paper on the above-named subject, I was able to show by the result of experiment the effect of radiation from the electric arc in promoting the formation of chlorophyll within the leaf-cell of plants, and in favouring vigorous and continuous growth.

I ventured to express an opinion that the ripening of fruit would be accelerated by the same agency, but proof thereof was then wanting. A continuation of my experiments has furnished interesting evidence that the electric arc is also efficacious in hastening the ripening process in strawberries, and probably in nearly every description of fruit.

Several plants of early strawberries in pots were divided as before into two groups, the one to be subjected to the action of daylight only, and the other to solar light during the day time, and to electric light during the night. Both groups of plants were placed below glass at temperatures varying from 65° to 70° F., but those that were to receive daylight only were shielded from the effect of the electric light during the night by matting.

At the commencement of the experiment the strawberry plants selected were just setting fruit, being still partly in bloom. After eight days the fruit on the plants exposed to electric light had swelled very much more than on the others, some of the berries beginning to show signs of ripening. The experiment was interrupted for two nights at this stage, but after the electric light was resumed, very rapid progress towards ripening was observable; so that with four days' continuous exposure to both day and electric light, the greater number of the berries had attained to ripeness, and presented a rich colouring, while the fruit on those plants that had been exposed to daylight only had by this time scarcely begun to show even a sign of redness.

This experiment goes to show that the electric light is very efficacious in promoting the formation of the saccharine and aromatic matter upon which the ripening and flavour of fruit depends; and if experience should confirm this result, the horticulturist will have the means of making himself practically independent of solar light for producing a high quality of fruit at all seasons of the year.

Two strawberry plants are placed before the meeting illustrative of the result here described.

Although I have shown that in employing a lamp of 1,400 candle-power a distance of 3 metres produces a maximum beneficial result on vegetation, the effect is nevertheless very marked upon plants at greater distances. This influence at a distance was illustrated in my experiments by the condition of three melon plants towards the back of the house, which throve remarkably well for a period of about a fortnight, during which the electric light was placed in front of the house, at a distance of from 5 to 6 metres from the plants. The electric light was then removed in front of the other end of the same house, thus depriving the plants in question of its influence, since which time they have continued their growth, but show a very decided falling off in size and colouring of the leaves that have since formed.

In reply to the questions that have been frequently asked regarding the cost of maintaining an experimental electric light of 1,400 candle-power, such as I have used in these experiments, I may state that the 3 horse-power Otto gas engine employed in driving the dynamo-machine, consumes nearly 900 cubic feet of gas during the night of 12 hours, or 75 cubic feet an hour (including 7 cubic feet of gas employed by the igniting burner) which, at 3*s.* 6*d.* per 1,000 cubic feet, represents a cost of a little over 3*d.* an hour, or with the carbons 5*d.* an hour. This, however, does not include superintendence or incidental expenses, the amount of which must depend upon the circumstances of each case.*

The Society then adjourned over the Easter Recess to Thursday, April 8th, at half-past four in the afternoon.

Presents, March 4, 1880.

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Geneva:—Société de Physique. Mémoires. Tome XXVI. 2^e Partie. 4to. *Genève* 1879. The Society.

London:—Anthropological Institute. Journal. Vol. IX. No. 2. 8vo. *London* 1879. The Institute.

Clinical Society. General Index to the First Twelve Vols. of the Transactions. 8vo. *London* 1880. The Society.

Corporation of London. Catalogue of the Library. Fifteenth Supplement. 8vo. *London* 1879. The Corporation.

Geological Society. Quarterly Journal. Vol. XXXVI. Part 1. 8vo. *London* 1880. The Society.

Pharmaceutical Society. Calendar. 8vo. *London* 1880. The Society.

Royal Astronomical Society. Memoirs. Vol. XLI. 4to. *London* 1879. The Society.

Royal Microscopical Society. Journal. Vol. III. No. 1. 8vo. *London* 1880. The Society.

Pesth:—Magyar Tudományos Akadémia. Almanach. 1879–1880. 8vo. *Budapest* 1879–80. Értesitö 1878. Nos. 1–7. 1879. Nos. 1-6. 8vo. *Budapest* 1878–9. Archæologiai Értesitö. 8vo. *Budapest* 1878. Évkönyek. XVI. Nos. 2–5. 4to. *Budapest* 1878–9. Közlemények Archæologiai. XII, XIII, No. 1. 4to.

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* Since sending in my paper to the Royal Society my attention has been drawn to some experiments made in 1861 by M. Hervé-Mangon, who proved that by means of the electric light, chlorophyll was developed in young plants of rye.

Transactions (*continued.*)

Budapest 1878–9; Math. és Term. XIV, XV. 8vo. *Budapest* 1877–8; Nyelvtudományi. XIV. Nos. 2, 3. XV. Nos. 1, 2. 8vo. *Budapest* 1878–9. Értekezések a nyelvt. és szépirodalom köréböl. VII, Nos. 3–10; VIII, Nos. 1–4. Ért. a társad. tudományok köréböl V. Nos. 1–8. Ért. a történelmi tud. kör. VII. Nos. 5–10. VIII. Nos. 1–8. Ért. a mathem. tud. kör. VI. Nos. 3–10. VII. Nos. 1–5. Ért. a természet-tud. Kör. VIII. Nos. 8–16; IX. Nos. 1–19. 8vo. *Budapest* 1878–9. Monumenta Hungariæ Historica. I. Diplomataria. Vol. XVI.—III. Comitialia Regni Hung. Vol. VI. Regni Transyl Vols. IV, V.—IV. Acta Extera, Vol. IV. 8vo. *Budapest* 1878–9. Anjoukori okmánytár. 8vo. *Budapest* 1878. Történelmi Tár. Vol. XXV. 8vo. *Budapest* 1878. Archivum Rákóczianum. Osztály I. Vols. VI, VII. 8vo. *Budapest* 1878–9. Literarische Berichte. 1878. Nos. 1–4. 1879. Nos. 1–4. 8vo. Monum. Archæol. Kötet III. Rész, 2. 4to. *Budapest* 1878. Nyelvemléktár. Vol. VI. Magyar-ugor Szótár, Füzet IV. Régi Magyar Könyvtár; Szabó. Bethlen Gábor Levelei. A Szörényi Bánság; Pesty, Vols. I–III. Magyar Hölgyek Levelei. A Magyar Helyesirás. 8vo. *Budapest* 1878–9. The Academy.

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Cape Town:—Royal Observatory. Results of Astronomical Observations, 1876. 8vo. *Cape Town* 1879. The Observatory.

Greenwich:—Royal Observatory. Observations, 1877. Astronomical Results, 1877. Magnetical and Meteorological Results, 1877. 4to. *London* 1879. The Observatory.

Doberck (W.) On the Colour of Revolving Double-Stars. 4to. *Kiel* 1879. The Author.

Pacini (Filippo) Del Processo Morboso del Colera Asiatico del suo stadio di Morte Apparente e della Legge Matematica da cui è regolato. 8vo. *Firenze* 1879. The Author.

Plantamour (Ph.) Des Mouvements Périodiques du Sol, &c. 8vo. *Genève* 1879. The Author.

Plateau (J.) Un mot sur l'Irradiation. 8vo. The Author.

Presents, March 11, 1880.

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Edinburgh:—Royal Scottish Society of Arts. Transactions. Vol. X. Part 3. 8vo. *Edinburgh* 1879. The Society.

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Jena:—Jenaische Gesellschaft für Medicin und Naturwissenschaft. Sitzungsberichte für 1879. 8vo. *Jena* 1879. Jenaische Zeitschrift. Band XIV. Heft 1. 8vo. *Jena* 1880. The Society.

London:—Mathematical Society. Proceedings. Nos. 151, 152. 8vo. The Society.

National Association for Promotion of Social Science. Sessional Proceedings. Vol. XIII. No. 2. 8vo. *London* 1880. The Association.

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St. Petersburg:—Physikalische Central-Observatorium. Annalen. 1878. Theil 1, 2. 4to. *St. Petersburg* 1879. The Observatory.

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Duncan (P. M.), F.R.S. On a Part of the Life-cycle of Clathrocystis Æruginosa. 8vo. *London.* [1879.] The Author.

Phillips (J. A.) On Concretionary Particles and Fragments of other Rocks contained in Granite. 8vo. *London* 1880. The Author.

Plateau (J.) Sur la Viscosité Superficielle des Liquides. 8vo. *Bruxelles* [1879]. The Author.

Preudhomme de Borre (A.) Note sur le Breyeria Borinensis. 8vo. [*Brussels*] 1879. The Author.

Presents, March 18, 1880.

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London:—Howard Association. Report 1879, and five pamphlets. 8vo. *London* 1879–80. The Association.

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Melbourne:—Zoological and Acclimatization Society. Proceedings. Vol. II. 8vo. *Melbourne* 1873. Sir Samuel Wilson.

Paris:—Association Française pour l'Avancement des Sciences. Comptes-Rendus. 1872–78. 8vo. *Paris* 1873-79. The Association.

Transactions (*continued*).

Sydney:—Linnean Society of New South Wales. Proceedings. Vol. IV. Parts 1 and 2. 8vo. *Sydney* 1879. The Society.

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Bombay:—Report on the Administration of the Meteorological Department in Western India. 1878–79. 8vo. *Bombay* 1879. The Meteorological Reporter.

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Mines and Mineral Statistics, Notes, &c., prepared in connexion with the New South Wales International Exhibition. 8vo. *Sydney* 1875. Annual Reports of the Department of Mines. 1875–6–7. 8vo. *Sydney* 1876–78. Professor Liversidge.

Sewage and Health Board. Reports. 1875–77. 4to. *Sydney* 1875–77. Professor Liversidge.

Wellington:—Statistics of the Colony of New Zealand 1878. 4to. *Wellington* 1879. The Registrar-General of New Zealand.

Zi-ka-wei:—Observatoire. La Lumière Zodiacale étudiée d'après les observations faites de 1875 à 1879. 4to. *Zi-ka-wei* 1879. The Observatory.

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Gardener's Chronicle. Select Index of Plants recorded from 1841 to 1878. Abe—Hyb. folio. The Editor.

Journal of Medicine and Dosimetric Therapeutics. Nos. 1–3. 8vo. *London* 1880. The Editor.

Cialdi (A.) Note sur les Molès à Piles et Arceaux dans les Ports à Bassin sur l'usage qu'en ont fait les Romains. 8vo. *Saint-Germain* 1879. C. W. Merrifield, F.R.S.

Gulliver (George), F.R.S. Notes of Researches in Anatomy, Physiology, Pathology, and Botany. 8vo. *Canterbury* 1880.
The Author.

Liversidge (A.) Disease in the Sugar Cane, Queensland. 12mo. *Sydney.* And a Volume containing twenty-one Pamphlets by Professor Liversidge.
The Author.

Spratt (Rear-Admiral T.), F.R.S. A Suggestion for the Improvement of the Entrance to the Mersey. 8vo. *London* 1880.
The Author.

April 8, 1880.

THE PRESIDENT, followed by Mr. WARREN DE LA RUE, in the Chair.

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

The following Papers were read:—

I. "Note on Thermal Transpiration." By O. Reynolds, F.R.S., Professor of Engineering in Owens College, Manchester. In a Letter to Professor Stokes, Sec. R.S. Communicated by Professor G. G. Stokes. Received October 25, 1879.

Owens College, 23rd October, 1879.

Dear Sir,

I have just received a copy of a paper by Professor Maxwell from the "Philosophical Transactions of the Royal Society," read April 11, 1878, "On the Stresses in Rarefied Gases." To this paper I find that there is an appendix added in May, 1879, in the course of which he refers to my investigation in the following words:—

"This phenomenon, to which Professor Reynolds has given the name of Thermal Transpiration, was discovered entirely by him. . . . It was not till after I had read Professor Reynolds's paper that I began to reconsider the surface conditions of a gas, so that what I have done is simply to extend to the surface phenomena the method which I think most suitable for treating the interior of the gas. I think that this method is, in some respects, better than that adopted by Professor Reynolds, while I admit that his method is sufficient to establish the existence of the phenomena, though not to afford an estimate of their amount."

As the abstract of my paper does not contain a sufficient account of what is in the paper to enable a reader to form a fair judgment of the relative merits of the two methods, I venture to request those interested in the subject to withhold their opinion until they have an opportunity of reading my paper. In the meantime I can only express my opinion that Professor Maxwell is mistaken in supposing that the results which are obtained from his method are more definite than those to be obtained by mine.

His method only applies to a particular case, and the equation which he has given is identical with that which I have given for this particular case.

The particular case treated by Professor Maxwell is the extreme limit—when the tube is large as compared with the distances between the molecules; he does not deal at all with the other limit—when the distances between the molecules are large as compared with the tube. Whereas I have given definite values for the coefficients in both limits, as well as indicating the manner in which the coefficients vary between these limits.

It so happens that the case in which the tube is large as compared with the molecular distances is one in which the results are too small to be experimentally appreciable, and hence Professor Maxwell's method does not explain any of the actual experimental results.

In order to explain the experimental results obtained with porous plates, Professor Maxwell has reverted to Graham's assumption that fine plates act as apertures in thin plates, while the coarse plates act like a tube, an assumption which my experiments show conclusively to be unnecessary and erroneous, the only sensible action in either case being that of tubes, and hence the phenomena of porous plates is that of transpiration and not effusion.

I remain
Yours truly,
OSBORNE REYNOLDS.

PROFESSOR STOKES, F.R.S.,
Secretary to the Royal Society.

Note by the Communicator.

In communicating the above letter to the Royal Society, in accordance with Professor Reynolds's wishes, I would beg permission to add a few remarks.

Professor Maxwell did not profess to treat more than the two extreme cases, constituting what Graham called respectively transpiration and diffusion. His statistical method applies, indeed, only to the first of these limits; but he has distinctly considered the second, following a suggestion of Sir William Thomson's. It is true that at the first limit, as Professor Reynolds remarks, the results are too small to be experimentally appreciable; but this was distinctly stated by Professor Maxwell himself, at the foot of p. 256.

As to the second limit, I must remark, in the first place, that I cannot find that Graham made any assumption that porous plates act as apertures in thin plates. The result that the time of passage varies, *cæteris paribus*, as the square root of the density in the case of fine porous plates, was obtained by pure experiment; and though he could not fail to notice the accordance of this result with that of the mere hydrodynamical passage through a small aperture, he has carefully distinguished between the two. Nor can I agree with Professor Reynolds in regarding the explanation given by Professors Thomson and Maxwell of the phenomenon of thermal transpiration or thermal effusion, whichever it be called, afforded by assimilating a fine porous plate to a thin plate pierced by apertures of ideal fineness as erroneous, even though it should be shown that such assimilation is unnecessary. Professor Maxwell did not profess to treat in his paper the intermediate cases between the two extreme limits.

Perhaps I should mention, that the foot-note at p. 281 in Professor Maxwell's paper was added as the paper passed through the press. I recollect noticing the thing as, in my capacity of Secretary, I looked over the paper before sending it to be printed off, and considering whether I should affix a date. As, however, it seemed to me to contain merely an explanation of an expression in the text, and as Maxwell, who had carefully added the dates of fresh matter in other parts, did not seem to have thought it necessary to do so in this case, I left it as it was. In a letter I received from him at the time, he informed me that he felt very ill, and was hardly fit even to go through his own paper; though a subsequent letter, in which he entered into some scientific matters, was written in his usual cheerful style. No one had, I believe, at that time any notion of the very serious nature of his illness.

March 13, 1880. G. G. Stokes.

II. "On the Sensitive State of Vacuum Discharges. Part II." By William Spottiswoode, D.C.L., LL.D., Pres. R.S., and J. Fletcher Moulton, late Fellow of Christ's College, Cambridge. Received March 11, 1880.

(Abstract.)

This paper forms a sequel to that published under the same title in the "Phil. Trans.," 1879, p. 165. It describes a continuation of the research into the nature and laws of the disruptive discharge, or elec-

tric spark. The methods of the earlier paper have been extended, and others adapted to the new circumstances have been devised, in order to carry the investigation into high vacua. In particular, independent sources of electricity have been used for affecting the discharge, whether in the sensitive or in the non-sensitive state; and the results have been confirmatory of the conclusions derived from the more limited means formerly described. Further, the effects of various tubes containing discharges in the sensitive state upon a tube containing a discharge in the non-sensitive state have been observed and compared; and the tube so used as a test has been called the standard tube, and the method of its use the standard tube method. By this means, principally, the laws of the discharge in comparatively moderate vacua have been extended to high vacua.

In the higher vacua, the phenomena of molecular streams, and the phosphorescence consequent on them, that have been studied and described by Mr. Crookes, present themselves. These derive great importance for the purposes of the present paper from the fact that in high vacua the ordinary luminous discharge becomes so feeble in appearance that it is often difficult to observe. Under these circumstances the phosphorescence, which like the ordinary luminous effects may exist either in a sensitive or in a non-sensitive state, forms the best index of what is going on within the tube. Much information as to the nature and procedure of the discharge may be derived from the mode of interference of one molecular stream with another, from the direction and character of shadows cast by these streams, and by a form of interference which has here been called that of virtual shadows.

The conditions of pressure and of electrical violence, under which phosphorescence is produced, have been carefully studied; and it has been found that, with a suitable adjustment of the discharge, the phenomena are not confined to high vacua, but can be obtained under pressures much exceeding those of ordinary vacuum tubes. The phenomena of these molecular streams have also been compared with those exhibited by the projection of finely divided solid conducting matter when heaped up over the negative terminal, with the view of ascertaining the nature of the phenomenon and its position in the discharge.

At the close of the paper the authors have discussed some of the general conclusions which they think may be fairly drawn from their present researches. First, as to the relative order of magnitude of the time-quantities entering into the discharge; *e.g.*, the times occupied by the discharge of positive or negative electricity, or of molecular streams, in leaving a terminal; the time occupied by the same elements in passing along the tube, &c. Secondly, as to the durational character of the negative as compared with the positive discharge, which appears to increase with the degree of exhaustion. Thirdly, as to the mode of formation of the positive column; and fourthly, as to the relation of the molecular streams to the discharge proper.

But for the details of these conclusions the reader must be referred to the paper itself.

April 15, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. "Description of some Remains of the Gigantic Land-lizard (*Megalania prisca*, OWEN) from Australia. Part II." By Professor OWEN, C.B., F.R.S. Received March 22, 1880.

(Abstract.)

Referring to a former Part ("Phil. Trans.," 1858, p. 43) the author gives, in the present, descriptions of subsequently received fossils of *Megalania prisca*, advancing the knowledge of that species of large extinct lizard. Characters of the dorsal, sacral, and caudal vertebræ, with those of a considerable portion of the skull, are detailed. So much of the upper jaw as is preserved shows the species to have had that part sheathed with horn, as in the tortoise. Upon the head were seven horns, three in pairs and one single; they are defined as the "supraparietal," "supratemporal" and "post-orbital" pairs; the single and symmetrical horn is "nasal."

In the comparison of this character with the known genera of lizards, the author finds the closest correspondence in the diminutive existing Australian species, *Moloch horridus*, Gray. He concludes with remarks on the probable habits and conditions of extinction of the subject of his two papers.

The cranial fossils were discovered by George Frederic Bennett, Esq., Corr. Member of the Zoological Society of London, in the bed of "King's Creek," Queensland, Australia; and were transmitted to the author by George Bennett, M.D., F.L.S.

II. "On an Electro-Magnetic Gyroscope." By M. WILFRID DE FONVIELLE. Communicated by the PRESIDENT. Received April 15, 1880.

I have the honour to submit to the Royal Society of London an apparatus which I have invented, after having witnessed an experiment by M. Dieudonné Lontin.

The original Lontin experiment consists in the rapid and continuous rotation of a magnetised steel needle, placed under the influence of the currents of a peculiar induction machine.

The object of my apparatus is to give a movement of rotation in a horizontal plane, not only to a needle, but to any moveable piece of iron which is symmetrical in reference to its axis of rotation. It is consequently composed of (1) a vertical coil, which I have made rectangular, but is susceptible of receiving any form; (2) a Ruhmkorff coil of a peculiar construction; and (3) the axis of suspension. This piece can be removed at pleasure, and another substituted, thus demonstrating the form to be indifferent, if it is placed in equilibrium. The coil may however be horizontal, and in that case the moveable piece will rotate in a vertical plane.

The apparatus has been constructed in order to work easily with one Bunsen cell of 20 centims. height, or with one or two bichromate of potassium cells.

Above the frame is placed a horseshoe-magnet, supported by a vertical axis, round which it can be placed in any particular azimuth that may be required for the experiments. This axis can be taken away, and the horseshoe-magnet can be replaced by one or two or a large number of bar-magnets, laid flat upon the frame. It is possible also to place other bar-magnets underneath the frame, in a space arranged for this purpose. It is possible also to place four magnets laterally, two on each side of the frame, or to replace them by a number of electro-magnets.

To produce a continuous movement of rotation, the magnets do not appear to be absolutely necessary, although they greatly increase the velocity, and outweigh every chance of perturbation and uncertainty.

But the motion without magnets presents peculiarities, which are of great interest, and have led to an explanation, accepted by M. Lontin, and which will be developed more fully hereafter.

Under ordinary circumstances the moveable piece does not start by itself, but requires an external impulse.

Another peculiarity of this movement is that the direction of the current can be altered without altering, in any respect, the direction of the rotation, although sometimes it diminishes or increases the velocity. But owing to the necessity of giving an impulse to the moveable piece, these experiments are rather tedious, as it is necessary to wait for some minutes before knowing with certainty whether the rotation is due to electro-magnetic reactions or to the mechanical impulse.

All the effects exhibited with magnets and moveable pieces of iron have the advantage of being produced without the help of any external force other than magnetical attraction working at a distance. This it is, I venture to think, what renders them so really effective.

When the moveable pieces are star-shaped, or composed of a number of iron pieces insulated magnetically, they are not moved by themselves, except when they occupy certain positions in relation to the position of equilibrium of a magnetic needle under the action of a continuous voltaic current flowing through the coil.

Under the same circumstances, the more arms the star possesses the more easily it revolves. An even number of arms appears to be less favourable to rotation than an uneven number.

But the difference of the phenomena exhibited when there is no magnet in operation is very striking, as there is then a definite direction of rotation, and if the moveable piece should be propelled in the wrong direction, its velocity quickly diminishes, and the piece soon rotates in the right direction. If the magnet is placed in a direction perpendicular to the frame the rotation is stopped.

A very small moveable piece has also been constructed so that it can be placed in different positions in the interior of the frame. The position of the axis within the frame involves no difference in the direction of the rotation, although the velocity can be in some degree altered, but it is not very easy to ascertain the fact, the velocity being so great that very often the motion seems to be imperceptible to the eye. These phenomena, which can be varied to any extent, as will be shown hereafter, appear to be capable of a very simple explanation, by an application of the laws of induction discovered by Faraday.

The possibility of producing the same movement by means of moveables of any form whatever, and notably of two spirals constructed of a flat wire and wound in an opposite direction, appears to demonstrate that the rotatory action is exercised individually on each molecule of iron, and that the total impulse must be regarded as the integral of the individual impulsive actions. This remarkable property appears to furnish a very simple means of completely explaining all the circumstances of these curious phenomena by means of the known laws of induction, and to dispense with having recourse to any new hypothesis. It is sufficient, in fact, to remark that the molecule of iron acts in its movement of rotation in two different ways in each of the two nearly equal currents of induction which successively traverse the spirals, but the alternate appearances of which are separated by very feeble intervals of time. In fact, during the whole continuance of the two phases of rotatory movement which the galvanometric frame brings closer together, each molecule of soft iron increases the intensity of the current which affects it, and which, independently of its real direction, the author calls *positive*, in order to fix the ideas; at the same time it diminishes that of the current which repels it, and which, for the same reason, is called *negative*. In two other phases of its movement the same molecule diminishes the intensity of the positive current, which then tends to draw it back, and increases that of the negative current, which turns it away from the frame. The actions exerted in the two phases of the movement, that is, in the total extent of the plane described by the molecules, tend then to keep up the continuous rotation, which progressively increases in speed until it reaches that which corresponds to the absolute intensity of the attractions or repulsions exercised by the currents induced by the energy of the inductive current, the value of the friction of the resistance of the arc, and other occasions.

When we bring the pole of a magnet into action, it is clear that its influence determines in each of the molecules of the moveable object a transient magnetisation which strengthens the induction currents produced in the spires in the cases in which it is concordant, and which paralyses them in the opposite case. It hence results that, in presence of a permanent magnetic centre, the movement is possible only in a direction determined by its position and its nature. The author believes that this principle applies even to the action of the earth.

When we change the position of the active pole in relation to the axis of rotation, the rotation changes its direction; but the pole of the magnet may be placed above or below, to right or left, without the rotation changing its direction. The two poles of a bar or a horseshoe-magnet combine to accelerate the movement when they are placed in the direction of the frame; but if we place the magnet in a perpendicular direction, all movement is, as a rule, rendered impossible. It is the same with near position; in proportion as we approach it to that limit of position, the rotation in general will be found to slacken. It is clear that a magnetisable body so strongly tempered as not to have the capacity of being magnetised and demagnetised to the given extent, will remain insensible to these successive dynamic reactions, and consequently immoveable, and that it is necessary to employ the softest possible iron in the construction of the moveable objects. The same phenomena, especially with the spiral, may evidently be produced if we place it about the frame. They are accompanied, especially with the full disk, by a strident sound, arising from the alternate magnetisations and demagnetisations; and this fact seems to be a new confirmation of the theories which have been advanced.

I must not omit to mention also a number of other phenomena which at least, in some respects, can be quoted in support of that opinion which my friend, M. Lontin, considers also as being valid.

If we take two spirals made of a similar iron ribbon, but curved in an opposite direction, viz., the one to the left and the other to the right, the velocity and direction of rotation are precisely the same under similar circumstances. This proves the action to be really molecular.

This remarkable experiment may be made in a very striking form. If we place a left-handed spiral to the left of the frame, and a right-handed one to the right, or *vice versâ*, at a distance sufficiently small from the frame, the influence is felt, and the rotation of the two spirals is obtained. But they rotate in the same direction. If the axis of the spirals be placed so near to the frame that a part of the solid is immersed in it, the rotation may be very rapid indeed. It seems impossible to witness that experiment without coming to the conclusion that it is not unreasonable to believe that a motive power comparable with that of other magneto-electrical machines may be obtained by taking advantage of these properties of the induction current.

This conviction receives some support from the following curious experiment.

If a spiral be placed on the top of the machine, after the removal of the horseshoe-magnet, it rotates under the influence of the two bar-magnets placed underneath. The direction of rotation is in reverse direction to the gyration of a moveable placed in the interior of the frame, but in the same as the moveable whose axis is placed outside.

A very pretty experiment can be made by placing a number of moveables in these various positions, and to see them changing their direction under the influence of active magnets, when the direction of primary current of the coil is altered.

Experiments have been tried to ascertain whether induced currents, or even interrupted currents, can work the machine. The results appear to have been affirmative, at least for the inducing current, although the phenomena are less easy to produce. It has been acknowledged also that the velocity is increased by placing the condenser in communication with the coil, and that the number of vibrations exerts also a great influence.

It is easy to show that a magnetised steel star will not move if it is magnetised up to saturation, and if incompletely magnetised it works like an ordinary star, but with a reduced velocity.

A magnetic bar placed perpendicularly to the frame and within it prevents the rotation of the moveable piece. But if an iron bar is placed in the same position, it seems to accelerate it; at all events, the motion seems to be a reversible one. Here the direction of the primary current is reversed and ceases almost to rotate or it stops entirely.

All these varied experiments appear to be in conformity with the theory already developed, but all these points, as well as many others, require more mature consideration.

Perhaps the best forms of the moveable piece are the spiral and the copper disk, surrounded by a continuous ribbon of iron.

No result was obtained with unmagnetisable substances, but the experiments were not made with a sufficient degree of accuracy to be deemed final.

No attempt has been made either to ascertain whether the rotation of the moveable pieces obtained by a motive power can produce alternate currents in the coil, which appears to be very dubious, as the motion is produced by the destruction of the symmetry which exists generally between the two sides of a galvanometric frame.

It can be very easily proved that the frame is an excellent magnetising machine, and that the magnetisation takes place in the position of equilibrium of a magnetised needle under a continuous current. If a needle is placed horizontally within the frame, the end which is sent forward becomes magnetic, so that it is very easy to alter the magnetism in any way which might be required.

The induction machine which is presented has been reproduced from one which M. Lontin had constructed for some special purposes, described in a letter inserted in "Electricité" (5th April last), and the peculiarity of which is that inducing and induced wires have a similar resistance. But I understand that the resistance of the wire going round the frame may be enlarged to four times its present value without diminishing the velocity of the rotation in any sensible manner. So the actual conditions can be largely altered without interfering with the production of the phenomena under consideration; and it may be inferred that the motive power derived from them can be greatly increased by these obvious alterations, although it is quite impossible to state whether it can be made useful to any practical purposes.

In concluding this short preliminary notice, I would venture to add that the apparatus most nearly resembling mine is Faraday's disk, with this difference, that it is made of mica and worked by a frictional electrical machine. The mechanical work produced is very small in either instrument.

It may be stated to the credit of the gyroscope that the work done by a man in turning the glass disk, is executed under the actual condition by one Bunsen element of 20 centims. high; so that in certain respects the action may be said to be wonderfully great. But it must not be forgotten that it is really Faraday's disk which has paved the way in this new field of electrical researches; and that certainly I should not have taken advantage of the experiment shown to me by M. Lontin, if I had not been acquainted with all the circumstances of the disk rotation exhibited to me by your great natural philosopher so many years ago.

III. "Report on the Exploration of the Caves of Borneo." By A. Hart Everett. "Introductory Remarks." By John Evans, D.C.L., LL.D., Treas. R.S. And "Note on the Bones Collected." By G. Busk, V.P.R.S. Received March 4, 1880.

In the year 1878 the Council of the Royal Society made a grant of £50 from the Donation Fund, towards the expenses of carrying on an investigation of the Caves of Borneo, which it was thought possible might prove to contain remains both of palæontological and anthropological interest. A similar grant was made by the British Association, and a Committee appointed; and by the aid of private subscriptions a sufficient sum was raised to secure the services of Mr. Hart Everett, whose report upon his investigations, extending over a period of nearly nine months, is now enclosed.

A preliminary report from the Committee, together with one of Mr. Everett's reports has already been submitted to the British Association at its meeting in Sheffield, and has appeared in print. It was then pointed out that although the examination of these caves had not, as was hoped, thrown any light upon the early history of man in that part of the world, yet that the evidence obtained, though negative in character, was not without value, inasmuch as the true nature of the Borneo cave deposits had now been carefully ascertained by Mr. Everett. His final report confirms the opinion already expressed. It only remains to be added that with the exception of the bones mentioned in the enclosed note by Mr. G. Busk, F.R.S., which have been placed in the Museum of the Royal College of Surgeons, the whole of the objects sent to England by Mr. Everett have been made over to the British Museum. Accompanying this is Mr. Everett's first quarterly report, together with his map and plans, so that they may if thought fit be deposited in the archives of the Society, so as to be available, if necessary, for future reference.

"Report on the Exploration of the Bornean Caves in 1878–9."
By A. Hart Everett.

1. The limestone formation.
2. The caves and their deposits.
3. The human remains.
4. Concluding remarks.

1. *The Limestone Formation.*

The caves of Sarawak are situated in a limestone formation substantially identical with that of the Malay Peninsula, and occupying a considerable area of the north-west and north-east parts of the Island of Borneo. Its westernmost extension seems to be represented by the Ahup Hill on the frontier between Sambas and Sarawak, whence it runs nearly uninterruptedly to the upper waters of the Sadong River at Semabang. It reappears in the Tatan River near Bintulu, and again comes to the surface in the Niah, Baram, and Linbang rivers, in Brunei territory, and it is known to be largely developed in northern Borneo.

Where the original structure of the rock has not been obliterated by metamorphic action, it is found to be crowded with organic remains (encrinites, &c.), but as these have never been examined by palæontologists, it is impossible to fix with any approach to exactitude the age of the formation. Its position relative to the other rocks of the island is also not well determined. It appears, however, always to underlie the great sandstone-conglomerate formation which constitutes the major part of the highlands of north-west Borneo.

The limestone hills nowhere attain to a greater elevation than 1,800 feet above the sea-level, at any rate in Sarawak, and they more commonly vary from 300 feet to 800 feet in height. In the Baram district the Mulu Mountain is said to be limestone and to rise to a height of 9,000 feet, but I am not aware that it has ever been visited by a European observer. The hills invariably spring up steeply from the low country, and the majority of them present lines of old sea-cliffs which generally face to N. and N.W., *i.e.*, towards the quarter still occupied by the waters of the sea. The rock itself is much fissured and jointed, and the hills in many instances are absolutely honeycombed with caverns.

As is usual in limestone districts the drainage of the country is largely subterranean. Owing to this fact, coupled with the heavy rainfall (the mean for the last three years was 165 inches at Kuching), the land at the base of the hills is subject to frequent flooding during the prevalence of the north-east monsoon, when the underground watercourses are of insufficient capacity to carry off the water as fast as it reaches them. As an instance of the extent to which subterraneous drainage with its consequent subterranean denudation has gone on in Sarawak, I may cite the Siniawan river, which passes beneath four distinct hills in its short course, and one of these hills—the Jambusan Hill—is pierced besides by at least three ancient river-tunnels of large size at varying levels.

2. *The Caves and their Deposits.*

The total number of the caves examined by me has been thirty-two, of which two were situated in Mount Sobis, up the Niah river, and the remainder in Upper Sarawak Proper. They comprised examples of tunnel, fissure, and ordinary ramifying caverns. Partial excavations were carried on in twelve of these caves. The deposits contained in them varied. A few afforded nothing but thick accumulations of bats' or bird-guano still in process of deposition. This deposit was examined in three instances, and proved to be perfectly barren, with the exception of a few of the bones of the bats and swifts, to which it owed its production. The commonest deposit in the caves of Upper Sarawak was found to be an exceedingly tenacious, dark yellow, homogeneous clay, which is sometimes crusted over with as much as a foot of dry mortar-like stalagmite, and sometimes is itself concreted into a kind of stony, pseudo-stalagmitic mass; but more generally it occurs in the form of simple wet clay lying immediately on the limestone floors of the caves and without any other deposit above it. It occurs both at the water-level and in caves 150 feet or more above it. Occasionally, as in some of the Bidi caves, it is mixed with sand and fine water-worn gravel. It is evidently derived from the waste of the clay shales and soft felsitic porphyries which now make up the lowlands in the vicinity of the limestone hills—worn fragments of these rocks occurring in it. I have very seldom met with organic remains in this clay, notwithstanding that, in addition to my own excavations, I have always been careful to search for bones in the *débris* left by streams running through the caves and carrying away the softer parts of the deposit. Such few remains as have presented themselves indicate that the clay is of fluviatile origin. They comprise bones and teeth of pig and porcupine, a large part of the skeleton of a Chelonian reptile, and numerous land and fresh-water shells. A prolonged search would doubtless reveal remains from time to time, but certainly not in sufficient abundance or of interest to warrant the cost of exploration.

In addition to the guano and clay, there was found in four instances a regular series of deposits (in caves Nos. V, XIII, XXI, and XXXII), of which the following note represents the section, as generalised from the excavations in caves Nos. V and XIII.

(1.) A surface layer of disturbed earth composed largely of charcoal, rotten wood, and bamboos, with fragments of modern pottery, glass beads, recent bones, quantities of fresh-water shells (chiefly the common *potamides*), and other *débris*—being the relics left by the Dyaks, who camp temporarily in the caves when they are employed in gathering the harvests of the edible birds' nests, which is done three times annually. This layer is, in some cases, a mere film, but about the entrance hall of No. XIII it was as much as a foot in thickness.

(2.) A talus of loam or clay mixed with earthy carbonate of lime, which locally forms a hard concrete, and crowded with the tests of many species of recent land shells, together with the bones, generally fragmentary, of various small mammals belonging chiefly to the order Rodentia. This talus is composed, in great measure, of large angular and subangular blocks of limestone. In cave No. V its summit is nearly 50 feet above the floor of the cave.

(3.) A stratum of river mud mingled with bat-guano, and with rounded masses of limestone and creamy crystalline stalagmite interspersed. The maximum thickness observed in the excavations was 3 feet. This stratum is crowded with the remains of bats, and also with those of larger mammals—all (as I am led to understand) of genera now extant in Borneo. The bones are almost invariably in a very broken condition, and so rounded and water-worn as to be past identification. As a sample of these bones has been examined in England, it is not necessary to speak more particularly of them here. In addition to the mammalian remains, the mud exhibits a miscellaneous assemblage of the remains of small reptiles (chiefly Chelonian), fish bones and scales, chelæ of crustaceans, land and fresh-water shells, leaves, &c., &c. In the upper level of this river mud traces of the presence of man are abundant.

(4.) The yellow clay, more or less concreted into hard pseudo-stalagmite, and containing casts of land shells, and bones and teeth of pig. In No. XIII, a narrow band of nearly pure stalagmite (about 4 inches thick) intervenes between the river mud and the yellow clay. The latter deposit rests immediately on the limestone floor of the cave. It contains a few water-worn pebbles and fine gravel, and it has been extensively denuded, prior to the introduction of the river mud above it.

The foregoing series of deposits is found, with wonderfully slight variation, at points so distant from each other as Jambusan and Niah. At both places the floors of the caves which present it are at a level of some 40 feet above the flat land at the bases of the hills. All four caves open on the face of a perpendicular cliff, so that their height above the present valleys affords a gauge of the denudation of the soft rocks in the vicinity of these hills since the introduction of the river mud.

The above are the principal kinds of deposits that are met with. Apart from the evidence as to their slight antiquity afforded by the mammalian remains, and by the fact of the presence of man in a fairly advanced stage of civilisation in the particular instances examined, it seems highly probable that the contents of all the Sarawak caves, at least to a height of many hundred feet, will prove equally recent, and for the following reason: The contents of the Sarawak caves must have been accumulated since the date of the last submergence of north-west Borneo, unless the subsidence of the land was very trifling indeed. But the submergence actually went on to a depth of 500 feet, and probably much more, as is abundantly evidenced by the indications of purely marine denudation on the inland hills; and that it was very recent in a geological sense may with fairness be deduced, I think, from the slight amount of differentiation which the present Fauna of the island has undergone since its last connexion with continental Asia, coupled with the rapid rate at which the Sarawak coast is even now advancing seaward, which argues that the tract of land now intervening between the sea and the limestone hills cannot be of much antiquity. The absence of any heavy floors of crystalline stalagmite in the caves seems to add confirmatory testimony in this direction, as does, perhaps, the absence of the large mammals of Borneo (elephant, rhinoceros, tapir, and wild ox), from the north-west districts. It may be worth remarking, that all the tribes of Land Dyaks have most circumstantial traditions current among them on the subject of a great subsidence of the land.

With regard to the rate of accumulation of the cave-deposits in Sarawak, it seems probable that it would be in excess of that generally observable in the case of other countries, for the rocks are of an extremely decomposable class, and, as I have noted above, the rainfall is prodigious.

With respect to the possible future discovery of ossiferous deposits other than those mentioned above, I think it probable that such will be found. They cannot, however, be very numerous in proportion to the number of caves. The natives have been in the habit for many years past of excavating the contents of the caves and fissures for the purpose of washing out the alluvial gold they afford. The caves examined in this way are situated at all elevations up to 100 feet. Both Malays and Chinese set a value on fossil teeth, which they preserve as charms or use for medicinal purposes ; nevertheless, they have never met with a regular ossiferous deposit in the course of their explorations. Had they done so, it would have been certain to attract their attention. Bones and teeth have, however, been found from time to time, and of these I forwarded a sample to Mr. Evans, but many of them were evidently quite recent.

In the event of other bone-beds being ultimately discovered in any part of Borneo, they will doubtless resemble generally the accumulation of fluviatile *débris* described in caves V and XIII. At present no animals habitually use caves in this island in the same way as the caves of Europe were used by the large Carnivora as retreats to which to carry their prey, so that the rich assemblage of mammalian remains which characterises the old hyæna dens of England cannot be looked for in Borneo. On the other hand, the fissures which abound like natural pitfalls over the limestone country, and which in Europe have furnished deposits of bones, are in Borneo barren or nearly so, so far as my experience has gone. The reason is to be found, I suspect, in the remarkably rugged and precipitous nature of the limestone hills, which makes them practically inaccessible to the larger mammals, and in their dense coating of jungle, the matted roots of which bridge over all the fissures to a greater or less degree, and afford a safe passage to the smaller animals.

3. *The Human Remains.*

Many of the caves present traces of the presence of man. Eleven of the caves examined by me exhibited such traces, and I had information of five others. The cave exploration has, indeed, yielded traces of man or his handiwork under three distinct sets of conditions, viz., (1) in river gravel, (2) in the river mud of the Jambusan cave, as mentioned in the preceding section, and (3) in the surface-layers of various caverns in Upper Sarawak and at Niah.

(1.) During my first exploration I discovered imbedded at the bottom of a bed of river gravel exposed in a section on the left bank of the Siniawan river, a single stone celt. It was forwarded to the late Sir C. Lyell with a note of the circumstances of its occurrence, and was pronounced by him to be of Neolithic type. It is the only existing evidence, to my knowledge, of the use of stone by man for the manufacture of industrial implements yet discovered in Borneo. At present iron seems to be universally employed even by the rudest tribes.

(2.) In cave No. XIII, scattered abundantly throughout the upper 8 inches of the river mud, there occurred water-worn fragments of a rather coarse but fairly well made pottery. It was so fragmentary and water-worn that it was impossible to distinguish of what kind of utensils it had formed a part. Associated with it were a few marine shells (*Cardium*, *Cypræa*, and others), a single fragment of stone apparently bearing marks of human workmanship, pieces of burnt bone, fresh-water shells (*Neritina* and *Potamides*) also bearing the marks of fire, the tooth of a tiger cat, with a hole bored through the base, a rude bone bead, and a few clean chips of quartz. No stone implements properly so called were observed, though carefully looked for. These remains indicate the presence of a settlement of people at some distance without the cave on the banks of the stream, which formed the river mud deposit. The quality of the pottery shows that this people had attained a fair degree of civilisation. The presence of the marine shells seems to imply that the sea coast was within easy reach of the vicinity of the Jambusan Hill. The remains generally, although of slight interest except to the local archæologist, belong to a ruder stage of art than the following.

(3.) The traces of man in the remainder of the eleven caves above referred to consist of human bones, associated, in some instances, with works of art. These remains occur always either just within or but a few yards removed from the entrances of the caves. The caves in which they lie commonly open on the faces of steep mural precipices. That at Ahup, where the largest accumulation exists, is at an elevation of not less than 100 feet above the valley. The bones have belonged to individuals of various ages, they are mostly fragmentary, and they lie scattered on the surface, or but lightly imbedded in the earth without reference to their proper anatomical relations. Their condition will be better judged from the sample sent than from any description that I could give. Occasionally fragments occur bearing the marks of fire. The works of art associated with them include broken jars, cups, cooking pots, and other utensils of earthenware. The pottery is of excellent make, and often glazed and painted. Besides the pottery, beads and armlets of a very hard dark-blue glass, pieces of iron, manufactured gold, and fragments of charcoal have been met with. Similar beads are in the possession of the Land Dyaks at this day, but they can give no account of their origin.

No tradition is extant among the natives with regard to these relics. No tribes in Borneo make habitual use of caves either as domiciles, or as places of sepulture, or for any other purpose. The character of the earthenware, however, and the use of iron and gold point to a very modern date indeed for the people who left these signs of their presence and hence the subject, though curious to a local geologist, does not call for any detailed remarks here. It is very possible that the remains date no farther back than the Hindu-Javanese occupation of Borneo, when this part of the island with Pontianak and Banjar were tributary to Majapahit, or they may be of Chinese origin—in either case quite recent.

4. *Concluding Remarks.*

The general result of the exploration may be summed up as follows: —The existence of ossiferous caves in Borneo has been proved, and at the same time the existence of man in the island with the Fauna, whose remains are entombed in these caves. But, both from the recent nature of this fauna, and from the fact that the race of men whose remains are associated with it had already reached an advanced stage of civilisation, the discovery has in no way aided the solution of those problems for the unravelling of which it was originally promoted. No light has been thrown on the origin of the human race—the history of the development of the fauna characterising the Indo-Malayan subregion has not been advanced—nor, virtually, has any evidence been obtained towards showing what races of men inhabited Borneo previously to the immigration of the various tribes of Malayan stock which now people the island. Furthermore, the presumption that the north-west portion of Borneo has too recently emerged above the waters of the sea to render it probable that future discoveries will be made of cave deposits of greatly higher antiquity than those already examined, has been strengthened. Under these circumstances it seems advisable that cavern research in north-west Borneo should now be left to private enterprise, and that no further expense should be hazarded, at any rate, until the higher parts of the island in the north-east may be conveniently examined.

A. HART EVERETT.

JOHN EVANS, ESQ.
Sarawak, August 6th, 1879.

Notes on the foregoing Report.

Page 311. "*Lines of old sea cliffs.*"—I inclose a rough sketch of the Sibaiyat range of limestone hills, situated up the right-hand branch of the Sarawak River, which will assist in giving an idea of the appearance of the cliffs referred to. In the Stäat Mountain similar cliffs rise sheer up from the plain to a height of 1,200 feet. I also inclose a sketch of that part of the Jambusan Hill known as Gunong Bak, in which are found caves V, XIII, and XXI. This sketch was made about three years after the jungle had been removed by a bush-fire, and it will serve to show how the Sarawak limestone has been worn and scarred by denudational agencies.

Page 313. "*Submerged to a depth of* 500 *feet.*"—I infer that the last subsidence of north-west Borneo reached a depth of not less than 500 feet from the fact that the limestone hills between the upper part of the Sarawak River and the Samarahan exhibit traces of marine denudation equally with the hills situated nearer to the coast, although their bases are probably not less than 400 feet higher above the sea-level. Pebbles of cinnabar ore have been met with on the summit of the Busan Hills. The nearest deposit of cinnabar is that at Tagora, a peak rising nearly 800 feet above the sea-level at the base of the Boñgoh Mountain, about eight miles to the southward. It can hardly admit of doubt that these pebbles were carried to the spot in which they occurred when the Busan Hills were submerged beneath the sea, and, as the hills vary in height from 400 to 500 feet, we have, in this instance, almost demonstrative evidence of subsidence to the depth which I have indicated as a probable minimum.

Page 313. "*Indications of purely marine denudation.*"—Every limestone hill is surrounded by a great assemblage of reefs, rocks, and sea-stacks, which often extend from side to side of the smaller valleys. Where the superficial alluvium has been removed, it is seen that these rocks are, almost invariably, integral portions of a smoothly-worn and hollowed floor of limestone. They decrease in number as the distance from the hill is increased; but, in the immediate vicinity, if the jungle be cleared, the land may be observed to be literally studded with masses of limestone, all fantastically worn, and varying from the size of small boulders to that of craggy stacks, 30 or even 50 feet high. Sometimes two reefs will run out parallel from the hill, and form a miniature cove, with a small cave at its inland extremity. The most striking form presented by the rocks are those of the "tabular" and "mushroom" types. Their bases being protected from the honeycombing action of the rain, still present surfaces smoothly polished by the even wash of sea-waves. The exact counterpart of these rocks and of these inland cliffs may be seen in the Philippine Archipelago on the present shores of the islands lying to the northward of Surigao. Such peculiar assemblages of rocks cannot be referred to the action of streams varying their course, for the rocks surround every hill, large and small, and besides, the action of the streams in the limestone district of Sarawak is rather in the direction of cutting one definite channel in the solid rock and keeping to it. Still less could the heavy tropical rains produce such results by their long-continued operation over a rock-surface of unequal hardness. Were there no other argument against such a supposition, the presence of the "mushroom" rocks would be fatal to it.

Page 314. "*Sarawak Coast .... advancing seaward.*"—The shore line of north-west Borneo (Sarawak) appears to be gaining on the sea steadily as a whole. Whether the land is stationary and the gain is due solely to the amount of sediment poured into the sea by an extensive river system, draining a country composed of rocks peculiarly liable to rapid degradation by denudational agencies and exposed, at the same time, to a rainfall equalled by that of few countries on the face of the globe, or whether, in addition to the shoaling of the sea by the introduction of fluviatile *débris*, the land is at present undergoing a slow elevatory movement, I do not feel prepared to decide. Of the mere fact of the recent increase of the land there is abundant evidence. The coast between Lunder and Samarahan, and again, between Kalakat and Igan, is a flat belt of alluvial soil, but just raised above the level of the highest tides, and traversed in every direction by broad tidal channels. The belt extends inland from ten to thirty miles. Cape Sirik is its most prominent point, and, although it is composed of soft alluvium, and is exposed to the fury of the north-east monsoon, blowing down the whole expanse of the China Sea, this cape extends itself so rapidly seawards that the subject is one of common remark among the natives in its vicinity. The Paloh Malanans have farmed close up to the point for many years past, and they state the addition to the land annually to average three fathoms. One of the elder men pointed out a distance of nearly two miles, as showing the increase within his memory. Numerous facts could be adduced pointing in the same direction.

Page 314. "*No animals habitually use caves.*"—Wild pig are said by the natives to retire into caves to die. This may explain why their remains are not uncommon in such situations. I have seen traces of a bear in a cave, but as a rule none of the larger animals enter the caverns. The latter, however, are not without a varied Fauna of their own. Besides the infinite hosts of swifts (*Collocalia*) and bats of many species which throng their recesses, owls, and occasionally hawks, are met with. Several kinds of snakes, lizards (*Varanidæ* and *Geckotidæ*), fish, and Crustacea also occur, as well as spiders, crickets, and myriapoda (*Julidæ*, *Geophilus? Polydesmus?*) The recent guano often swarms with a slender yellow *Bulimus*.

Page 316. "*No tradition is extant.*"—It has been suggested that these superficial human remains are the remains of the Chinese who perished in the insurrection in Sarawak (1857). Apart from the decayed condition of the bones, this idea is inadmissible for many reasons. It is sufficient to mention the general identity of the remains at Niah in Brunei territory (where there is no reason to suppose any Chinese were ever located) with those of Ahup in Sarawak.

Page 316. "*No tribe makes habitual use of caves.*"—A very wild tribe of Punans, called by the natives Rock Punans, who inhabit the great Tibang Mountain at the source of the Rejang River, are popularly reported to live in caves, being so uncivilised as not yet to have learnt to construct artificial shelters. The head of the Rejang has never been visited by a European, so that this report is probably incorrect. Mr. Hugh B. Low, whose knowledge of the tribes of north-west Borneo is unequalled, writes in answer to my inquiries as follows:—"I do not know of any tribe that buries its dead in caves. Tana Nipa, of Tatan, was buried in a cave, but this was to secure him from his Dyak neighbours. The Orang Kaya Sahgieng expressed a dying wish that he might be buried in Lubang Danan in Ba Koiat, but it was only in order that he might gain an additional claim to the cave in question, the ownership of which was disputed. The 'tailed men' between Mandai and Melani are *said* to live in caves."

"Note on the Collection of Bones from Caves in Borneo, referred to in Mr. Everett's Report on the 'Exploration of the Bornean Caves in 1878–9.'" By GEORGE BUSK, F.R.S., V.P. Anth. Inst.

With the exception of portions of the lower jaw of a small pig, and two or three detached teeth of the same animal, and some fragments of pottery, the collection is composed entirely of human remains.

The bones are all more or less fragmentary and vary very much in condition, some appearing as if they had lain on the surface of the ground, exposed to the weather, whilst others are partially encrusted with a friable, argillaceo-calcareous stalagmitic deposit, admitting of very easy removal. None of the bones, though some are dry and fragile, appear to be of any antiquity, and none adhere to the tongue.

The remains are those of at least five individuals, differing a good deal in age and probably of both sexes, but this is not certain. They include—

1. Eleven or twelve portions of the skull, amongst which are four more or less perfect temporals, of which three belong to the right side. These bones are all distinguished by the large size of the mastoid process; in one only does any portion of the zygoma remain, which is of slender conformation. The only other specimens belonging to the cranium are—1. The face, with a large part of the forehead and the orbits complete. This fragment is remarkable for the great comparative width across the malar region, which amounts to about 5 inches, whilst the vertical length of the face from the fronto-nasal suture to the alveolar border is scarcely $2\frac{1}{4}$ inches. The orbits have a transverse diameter of $1''\cdot5$, and a vertical of $1''\cdot25$, giving an orbital index of ·83. The nose measures $1''\cdot8 \times 1''\cdot1$, affording a nasal index of ·61. The frontal overhangs the nasals very much, and the frontal sinuses are well developed, but the orbital border is not thickened. The alveolar arch is almost perfectly semicircular and very wide. The bone is further remarkable for the great apparent depth of the sphenoidal part of the temporal fossa, owing to the sudden bulging of the squamosal. The specimen on the whole presents an exaggerated Malay aspect.

2. Another and the most considerable of the cranial specimens consists of the greater portion of a calvaria. The entire face is wanting below the frontal border of the orbits; as is also nearly the whole of the right side of the skull. The calvaria is well formed and evenly arched; the forehead upright and rounded. In the vertical view (*norma verticalis*) the outline forms a regular broad oval. The sutures are all open and for the most part deeply serrated. The chief points to be noticed besides the above are (*a*) the enormous size of the mastoid process, in a skull otherwise it may be said of delicate conformation; and (*b*) the extraordinary condition of the foramen magnum, the border of which is so much thickened and elevated, as at first sight to convey the impression that the atlas was anchylosed to the occipital.

The bone in the surrounding part of the surface is extremely thin and apparently atrophied, but there is otherwise no sign of disease.

From its imperfect condition this calvaria affords no distinctive characteristics, but in one respect it agrees with the facial specimen above described, viz., in the remarkable bulging of the anterior part of the squamosal where it joins the alarsphenoid.

The longitudinal diameter of this calvaria is 7″—its width 5·25, and height 5·7, the circumference being 20 inches.

The other bones of the skeleton are represented by—

1. A clavicle of small size and delicate make, probably that of a female.

2. Two or three fragments of the humerus, in one of which the medullary cavity is filled with root fibres. And in its posterior aspect near the lower end there are three or four transverse cuts of slight depth, and done as it would seem from the chipped appearance by chopping with a sharp metallic instrument. There is also a deeper incision on the external condyloid ridge immediately above the condyle.

3. An entire sacrum and a portion of the left os innominatum probably of the same individual.

4. A fragment of the right os innominatum belonging to another individual.

5. Of bones belonging to the lower extremity, the collection includes portions of four thigh bones, one with the lower epiphysis naturally detached. The tibia is represented by three specimens, none of which present anything worthy of remark. The only bone belonging to the foot is a first metatarsal of small size.

From the above it will be seen that these bones present nothing of especial interest; and with respect to the race to which they may have belonged, the information they afford is very meagre. On this point all that can be said is that they may well have belonged to the Malay type, but there is also no apparent reason why they should not have been of Chinese origin. What tends to afford some support to this supposition is the marked fulness or bulging of the squamosal in the sphenoidal fossa, to which I have called attention, and which, upon examination of the collection of crania in the Royal College of Surgeons, I find is presented by several among the Chinese crania in a more marked degree than in the other races to which my attention was directed.

April 22, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read :—

I. "Effects of Electric Currents on the Surfaces of Mutual Contact of Aqueous Solutions." By G. GORE, LL.D., F.R.S. Received March 17, 1880.

(Preliminary Statement.)

In the year 1859 I made the following experiments, for the purpose of ascertaining whether visible movements, similar to those obtained by passing an electric current through mercury and an aqueous solution, could be obtained by passing a current through the surface of mutual contact of two aqueous liquids alone :—"1st. A definite layer of oil of vitriol was placed beneath a layer of distilled water weakly acidulated with sulphuric acid, and the terminal wires of a voltaic battery immersed in the upper liquid ; no visible movements occurred at the boundary line of the two liquids.

"2nd. A dense solution of cyanide of potassium was placed in a small glass beaker, a few particles of charcoal sifted upon its surface, and a layer of aqueous ammonia, half an inch deep, carefully poured upon it. A vertical diaphragm of thin sheet gutta-percha was then fixed so as completely to divide the upper liquid into two equal parts ; the vessel was placed in a strong light, and two horizontal platinum wire electrodes, from sixty-six freshly charged Smee's cells, were immersed one-eighth of an inch deep in the liquid ammonia on each side of the diaphragm. A copious current of electricity circulated, but no movement of the liquids at their mutual boundary line could be detected." (See "Proc. Roy. Soc.," vol. x, 1860, p. 235, par. 9.)

Recently, also, I have made similar experiments, but in a much more searching manner, in order to ascertain whether an electric current, passing between two aqueous liquids, affects their diffusion into each other. The essential difference in the form of these experiments from that of the above-mentioned ones was to concentrate the action of the current upon a very much smaller surface of contact of the liquids, and thus render any visible effect upon their diffusion more manifest.

After making several forms of apparatus, in order to obviate certain difficulties of manipulation which arose and were fatal to success, I found that, when an electric current was passed between the surfaces of mutual contact of certain aqueous solutions of different specific gravities, the boundary line of contact of the two liquids became indefinite at the surface where the current passed from the lighter into the heavier solution, and became sharply defined where the current left the heavier liquid and re-entered the lighter one; and that on reversing the direction of the current several times in succession after suitable intervals of time, these effects were reversed with each such change. Also, in various cases in which the contiguous boundary layers of the two liquids had become mixed, the liquids separated, and the line of separation of the two solutions became, by the influence of the electric current, as perfect as that between strata of oil and water lying upon each other. In rarer cases two such distinct lines of stratification appeared. Other new phenomena were also observed.

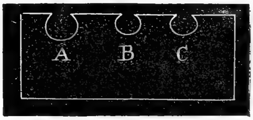
As I have sought, without success, for any record of previous discovery of essentially similar effects, and as it is evident that those I have observed belong to a large class of similar phenomena, I beg leave to take the earliest opportunity of submitting this brief statement to the Royal Society.

II. "On the Critical State of Gases." By WILLIAM RAMSAY, Ph.D., Professor of Chemistry in University College, Bristol. Communicated by Professor MILLS, F.R.S. Received February 26, 1880.

In a paper read before the Chemical Society, in May, 1879, I gave an account of a method of determining what is termed by Kopp the "specific volumes" of liquids; that was shown to be the volume of liquid at its boiling-point, at ordinary atmospheric pressure, obtainable from 22,326 volumes of its gas, supposed to exist at 0°. Being desirous of extending these researches, with the view of ascertaining such relations at higher temperatures, since April, 1879, I have made numerous experiments, the results of, and deductions from which I hope to publish before long. The temperatures observed vary from the boiling-points of the liquids examined, to about 50° above their critical points; and in course of these experiments I have noticed some curious facts, which may not be unworthy of the attention of the Society.

It is well known that at temperatures above that which produces what is termed by Dr. Andrews the "critical point" of a liquid, the substance is supposed to exist in a peculiar condition, and Dr. Andrews purposely abstained from speculating on the nature of the matter, whether it be liquid or gaseous, or in an intermediate condition, to which no name has been given. As my observations bear directly on this point, it may be advisable first to describe the experiments I have made, and then to draw the deductions which appear to follow from them.

A piece of barometer tubing, about 3 inches long, was sealed at one end and drawn into a capillary tube at the other; after being filled with methyl formate, it was exhausted, and after two-thirds of the ether had evaporated, was sealed. By this means all air was removed from the tube, which contained merely the ether and its vapour. This tube was placed in a hole, drilled in a large block of copper. The accompanying wood-cut shows its cross section. The block was placed in an inclined position, so as to slope the barometer tube to an angle of about 30°.

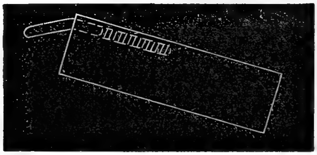


A, Hole for barometer-tube. B and C, Holes for thermometers.

1. On applying heat the temperature of the copper gradually rose to 221°·5 (corr.); during the rise the meniscus of the liquid gradually became less curved, and at the above-mentioned temperature disappeared. On cooling to 218° a mist was seen at the point where the meniscus had disappeared, and the meniscus shortly afterwards became again visible.

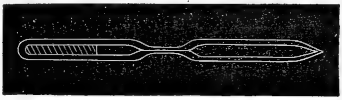
2. Two similar tubes were prepared, one containing less and the other more of the same ether; the point at which the meniscus disappeared in the former was 228°, and in the latter 215°.

3. A piece of barometer tubing was bent into the shape—



and heated. At 250° the liquid had so expanded as to fill the whole tube, although the temperature of the portion outside the heater could not have much exceeded 100°, for it could be freely handled. The liquid appeared in violent motion, and on cooling, before a meniscus was observed, the lower portion of the tube became filled with mist.

As soon as a meniscus was visible, the liquid boiled so violently as to give the appearance of a denser mist.



4. A tube of the shape shown was filled to the mark with methyl formate and heated in an inclined position, the portion containing the liquid being the lower. The liquid, as usual, expanded, and at the moment when the meniscus disappeared it nearly filled the lower half. The Bunsen's burner below the copper heater was then withdrawn, and on cooling, the liquid all condensed in the *lower* half.

5. The last experiment was varied by tilting the heater after the meniscus of the liquid had disappeared, so that that half which had contained the liquid was uppermost. On cooling, the liquid condensed in the *upper* half of the tube.

6. The experiment was again varied by keeping the tube at a temperature a few degrees above the point where the meniscus vanished, for half an hour. On cooling, an almost equal quantity had condensed in each division of the tube. (During Experiments 4, 5, and 6 great care must be taken to keep the heater from draughts of cold air, otherwise unequal cooling results and distillation takes place.)

7. It was noticed that that half of the tube containing liquid, after the meniscus had vanished, appeared *full*, while the other half of the tube seemed to be *empty*. The refractive indices of the fluid contained in the tubes were therefore different. The portion of the tube containing liquid was shown to be a more powerful cylindrical lens than the empty portion, for on focussing a spot behind the tube with a microscope, the focus was shorter when the portion which had contained liquid was placed between the microscope and the spot than when the portion appearing empty was interposed.

8. From experiments on the expansion of liquids above their boiling-points, of which numerical details shall be given on a future occasion, it appears probable that the specific gravity of the hot liquid, at the temperature at which the meniscus vanishes, is identical with that of the compressed gas evolved from the liquid. This has also been noticed by Ansdell in two cases, viz., hydrogen chloride and acetylene.

9. I am inclined to believe, from observations on the expansions of liquids at high temperatures, although they were not expressly directed to elucidate this point, that liquids above the temperatures at which their menisci vanish are not uniformly compressible. But as further experiments are necessary, I mention this opinion with some hesitation.

From these observations I would draw the following inferences :—

When a liquid is heated under pressure it expands, and at the same time evolves vapour. The vapour gains in specific gravity, while the specific gravity of the liquid is rapidly diminishing. *The critical point is that point at which the liquid, owing to expansion, and the gas, owing to compression, acquire the same specific gravity, and consequently mix with one another.* From the first experiment it is seen that, on cooling, the liquid contracts more rapidly than the gas, and consequently separates as a mist through the whole of the tube, and, from its gravity, separates at the lower half. The second experiment shows that when the tube contains a small amount of liquid the space left for gas is larger, and consequently more vapour must be given off by the liquid before enough gas can be compressed till it acquires the same specific gravity as the liquid; the temperature at which the meniscus disappears is consequently higher. If the space left for gas be smaller, the opposite is the case. From the third experiment it is seen that it is possible to raise the critical point of a liquid, provided a portion be continuously cooled. Probably the average temperature of the two portions of the tube is about 220°. The fourth, fifth, sixth, and seventh experiments demonstrate that by suitable means it is possible to prevent, or rather to retard, the mixing of gas and liquid. They then retain their several refractive indices. If, however, time be allowed for diffusion through the capillary tube, the whole becomes homogeneous, and the refractive indices of the fluids contained in either portion of the tube are then identical.

I shall not refer to Experiment 8 until I have made a more thorough investigation of the subject; I venture to state, however, that it looks probable that as long as gas is being compressed, pressure rises gradually with decrease of volume, whereas, even above their critical points, liquids are comparatively incompressible. I am inclined to think that carbonic anhydride, examined by Dr. Andrews, is abnormal in this respect, but of this I am by no means certain.

In conclusion, let me refer to a paper communicated to the Society by Messrs. Hannay and Hogarth last October, entitled "On the Solubility of Solids in Gases." Should the views of the subject suggested by the above experiments be correct, it follows that these gentlemen have observed nothing unusual, but merely the ordinary phenomenon of solubility of a solid in a hot liquid. This view is borne out by their own experiments. They found that on reducing pressure, that is, on allowing the liquid to change to gas, the solid precipitated; and also on heating the tube containing the solution locally, that is, by converting the liquid near the heated point into gas, precipitation took place. I have taken the liberty of repeating one of their experiments with a slight modification.

A tube shaped like that used in Experiment 4, after a small fragment of potassium iodide had been placed in the lower compartment, was filled with nearly anhydrous alcohol: and after rather more than two-thirds of the alcohol had been evaporated under reduced pressure, the tube was sealed. The lower portion of the tube contained a strong alcoholic solution of potassium iodide, besides a small piece of undissolved salt. The upper portion of the tube was free from alcohol, but its walls were encrusted with a thin crystalline film of potassium iodide. The tube was heated in a sloping position, the liquid being in the lower half. After the meniscus had disappeared, the iodide in the lower half of the tube dissolved, while the film in the upper half, even in its thinnest portions, remained unchanged. On cooling, very sparkling crystals deposited in the lower half of the tube, but no glittering crystals in the upper half.

By repeated distillation the iodide in the upper portion of the tube was washed down into the lower half, and when dry the sides of the upper tube were quite bright and clean. The tube was again heated in the same position to 20° above the temperature at which the meniscus had disappeared. On cooling the sparkling crystals again appeared in the lower tube, but not a trace in the upper tube. To eliminate all possibility of mistake the experiment was repeated five times with the same result, and finally the alcohol was distilled into the upper tube; it was then broken off, and its contents carefully tested for iodine with sodium hypochlorite and starch-paste. There was not the faintest blue colour, and it is therefore certain that potassium iodide is absolutely insoluble in alcohol vapour.

It would be interesting to speculate on the condition of a fluid, of which two portions possess the same specific gravity, but refract light differently. Such speculations would, however, be premature in our present knowledge of the subject.

In conclusion, I may remark, that Messrs. Hannay and Hogarth found that the absorption spectrum of coloured salts remains unaltered, even when the liquid in which they are dissolved loses its meniscus. Surely no clearer proof is needed to show that the solids are not present as gases, but are simply solutions in a liquid medium.

(Received March 20, 1880.)

Since the foregoing was written, Professor Stokes has been so kind as to suggest in a letter to Dr. Mills, some possible causes of error, which may be briefly catalogued as follows:—

1. The tubes may have contained air.

That this cannot have been the case is, I think, ensured by the method by which they were filled. The tube was *completely* filled with liquid, and the liquid made to evaporate out under a vacuum of 25 inches till its volume had become reduced to the required amount. While the reduced pressure was sustained, the tube was sealed at the base of its capillary neck. It is improbable that the liquids experimented on dissolve air, for when heated, no air-bubbles can be seen to escape.

2. The liquids may not have been quite pure; they may not have been absolutely free from water.

The methyl formate used in the experiments presents no special advantage but that of its meniscus disappearing at a comparatively low temperature. It was procured from Kahlbaum, and previous to use had been distilled over metallic sodium. Its boiling-point was absolutely constant. Still, as treatment with sodium might have led to the formation of some other compound of nearly the same boiling-point, it was redistilled, in accordance with a suggestion of Professor Stokes, and the first and last portions of the distillate were selected for experiment. Two tubes were filled to as nearly the same height as could be judged by eye, one with the first portion of the distillate and one with the last. Both were heated at the same time in the copper heater, and were exactly similarly placed with regard to the thermometer. The temperature of the heater having been raised till the meniscus had disappeared in both tubes, the source of heat was removed and the heater was allowed to cool. The tube containing the first fraction gave the first symptoms of a cloud, or mist, at 218·4 (corr.), and that containing the last fraction at 218·1 (corr.). The very slight difference is doubtless owing to the two tubes not containing exactly the same amount of liquid.

To eliminate any source of error dependent on the use of methyl formate, two other substances were employed, viz., carbon disulphide, CS_2, and carbon tetrachloride, CCl_4. The former of these liquids was rectified five times over sodium, and then boiled at 48·7 (corr.). The latter was rectified four times over phosphoric anhydride, and boiled constantly at 77·5 (corr.).

They yielded the following results:—

	Tube more than half full.	Tube less than half full.
CS_2	282·7	286·4
CCl_4	283·3	288·4

These readings are given for the first appearance of a cloud in the tube on cooling, and differ from the temperature at which the meniscus disappears by being about half a degree lower. They also do not represent extreme instances, for in the first cases the liquids do not entirely fill the tube, and in the second about half an inch of liquids remains in the tubes before it becomes impossible to distinguish liquid from gas.

3. Owing to too rapid rise of temperature, the tubes and the heater may not have been at the same temperature.

The numbers given are not the result of one reading, but the mean of four or five. The copper block, after being raised to the temperature at which the meniscus of the liquid disappeared, was allowed to cool till a cloud became visible, shortly afterwards followed by a reappearance of the meniscus. The temperature was again raised, and so on, readings being taken each time. They never differed by more than half a degree. The tubes, therefore, must have possessed the temperature of the heater.

The experiments described in a former part of this paper, relating to the difference of refraction shown by a liquid above its so-called critical point, and the gas evolved from it, were repeated with carbon tetrachloride and carbon disulphide, and held good in both cases. The phenomena observed differed in no particular from those already described.

In conclusion a few remarks on the liquefaction of the so-called permanent gases may not be deemed out of place. If the deductions from the above experiments be correct, it follows that that form of matter which we call gas may be converted into liquid by pressure alone; but the meniscus will never become visible, for the process of change is a gradual one. To render the meniscus visible, it is necessary to take advantage of the fact that liquids under such circumstances have a much greater coefficient of expansion by heat, and conversely a much greater coefficient of contraction on withdrawal of heat, than gases. It, therefore, becomes necessary to lower the temperature until the liquid by contraction acquires a specific gravity greater than that of its gas, and then, and not till then, does the phenomenon of a meniscus become observable.

III. "Revision of the Atomic Weight of Aluminium." By J. W. Mallet, F.R.S. Received March 13, 1880.

(Abstract.)

After remarking that there is probably no one of the so-called chemical elements, equally abundant in nature with aluminium, and occurring in as numerous compounds, with regard to the atomic weight of which our knowledge has long rested upon so slender a foundation of accurate experiment, the author gives a brief statement of prior determinations, noticing in succession the researches of Berzelius, Sir Humphry Davy, Thomson, Mather, himself, Dumas, Charles Tissier, and Terreil, and quotes the numbers given in several of the more recent chemical handbooks, reports, &c.

He then proceeds to describe a redetermination of this atomic

weight, which occupied a large part of his leisure time during the last three years. In this investigation he spared no pains to attain as precise a result as possible, and aimed especially at the discovery, and as far as possible removal, of sources of error connected with the methods employed. The following general principles were kept in view:—

1st. That each process should be as simple as possible, and should involve as little as possible of known liability to error.

2nd. That different and independent processes should be resorted to as the means of checking each other's results, even though it may fairly be assumed that one is more advantageous than another.

3rd. That each process should be carried out with quantities of material differing considerably from each other in successive experiments.

4th. That only such other atomic weights should be involved as may be counted among those already known with the nearest approach to accuracy.

The most scrupulous care was taken in the purification and examination of all the reagents used, and, as far as possible, vessels of platinum or of hard porcelain were substituted for those of glass.

In the author's first series of experiments, the alumina was determined which resulted from the ignition of a known weight of ammonium alum.

In the second series, a solution was made of a known weight of bromide of aluminium, and was very exactly precipitated by a solution of nitrate of silver, and the weight of bromide of aluminium compared with that of the silver required for forming the solution of nitrate.

In the third series, bromide of aluminium was used for the preparation of pure aluminium, by reduction by sodium; a carefully weighed portion of the pure aluminium was dissolved in a solution of caustic soda, and the hydrogen given off was estimated by volume; or else burnt, and the weight of resulting water determined.

In the following epitome of the results, A, B, C denote groups of experiments under each series, the experiments of each group differing only in the quantity of material operated on, while the different groups differ in the particular preparation used, or in some feature of the method. Thus in Series I, A was made with alum dried by exposure to air for two hours; B with alum dried by exposure for twenty-four hours. In Series II, A, B, C were made with successive portions of a distillate of bromide of aluminium. In Series III, A was made by estimating the hydrogen by volume, B by estimating by weight the water resulting from its combustion. Only the number of experiments in each group, the mean result from the group, and the probable error of the mean, are here given.

Series I.

	A.	B.
No.	5	5
Mean	27·040	27·096
P. e.	±·0073	±·0054

Series II.

	A.	B.	C.
No.	3	5	3
Mean	27·034	27·023	27·018
P. e.	±·0049	±·0052	±·0069

Series III.

	A.	B.
No.	6	3
Mean	27·005	26·990
P. e.	±·0033	±·0046

In view of the gradual loss of water which crystallised ammonium alum has been shown to undergo on exposure to the air, the author considers that of these various sets of experiments, Series I, B, is entitled to least confidence. He is inclined to attach most weight to Series III, A, since the method used was very simple in principle, the determination of one of the two quantities concerned was rendered very exact by the great volume occupied by the hydrogen, the comparison was made directly with the standard element in our system of atomic weights, and not through the intervention of any other substance whose atomic weight must be assumed, and the agreement of the results among themselves is particularly good.

The general mean from all the thirty experiments, if all be included, is Al=27·032, with a probable error for this mean of ±·0045. If Series I, B, be excluded, the mean of all the remaining twenty-five experiments is Al=27·019, with a probable error of ±·0030.

The general result adds, the author hopes, aluminium to the, unfortunately still limited, list of those elementary substances whose atomic weights have been determined within the limits of precision attainable with our present means of experiment.

This result also adds one to the cases already on record of the numbers representing carefully determined atomic weights approaching closely to integers, and leads the author to say a word on the reconsideration of "Prout's Law." Taking the following 18 elements as the only ones of which the atomic weights may be fairly considered as determined, with reference to hydrogen, with the greatest attainable precision, or a near approach thereto, namely oxygen, nitrogen, chlorine, bromine, iodine, sulphur, potassium, sodium, lithium, silver, thallium, aluminium, carbon, phosphorus, barium, calcium, magnesium, and lead, and making a reasonable allowance for the errors of the determinations, he calculates the probability that nine of those numbers should lie, as they are found to do, within 0·1 of integers, supposing the value of the true numbers to be determined by chance, and finds it only as 1 to 235·2. The exact figure for the chance will of course depend upon the limit of error taken; but the above example seems sufficient to show that not only is Prout's law not as yet absolutely overturned, but that a heavy and apparently increasing weight of probability in its favour, or in favour of some modification of it, exists, and demands consideration.

IV. "On the Height of the Aurora Borealis." By Warren De La Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Received April 22, 1880.

Our experiments on the electric discharge, which have been already published in the "Phil. Trans." and the "Proceedings of the Royal Society," enable us to state with some degree of probability the height of the Aurora Borealis, when its display is of maximum brilliancy, and also the height at which this phenomenon could not occur on account of the great tenuity of the atmosphere.

In Part III of our electric researches, "Phil. Trans.," p. 159, vol. 171, we have shown that the least resistance to the discharge in hydrogen is at a pressure of 0·642 millim., 845 **M**; after this degree of exhaustion has been reached, a further reduction of pressure rapidly increases the resistance. When the exhaustion has reached 0·002 millim., 3 **M**, the discharge only just passes with a potential of 11,000 chloride of silver cells (11,330 volts); at the highest exhaust we have been able to obtain (and which we believe has not been surpassed), namely, 0·000055 millim., 0·066 **M**, not only did 11,000 cells fail to produce a discharge, but even a 1-inch spark from an induction coil could not do so.

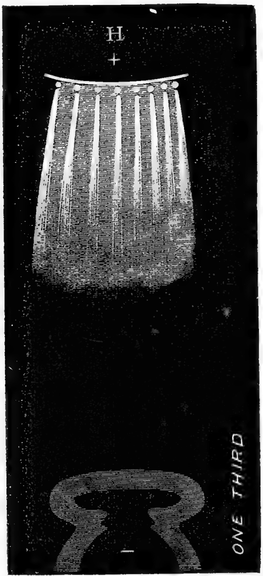
Although we have not experimentally determined the pressure of least resistance for air, we have ascertained that while the discharge occurs in hydrogen at atmospheric pressure between disks 0·22 inch distant, they require to be approached to 0·13 inch to allow the discharge to take place in air. We may therefore assume that the pressure of least resistance for air is

$$\frac{0{\cdot}642\times 13}{22}=0{\cdot}379 \text{ millim., } 498{\cdot}6 \ \mathbf{M}.$$

At a height of 37·67 miles above the sea level, the atmosphere would have this pressure (neglecting change of temperature), and therefore the display at this elevation would be of maximum brilliancy and would be visible at a distance of 585 miles.

The greatest exhaust that we have produced, 0·000055 millim., 0·066 **M**, corresponds to a height of 81·47 miles, and as 11,000 cells failed to produce a discharge in hydrogen at this low pressure, it may be assumed that at this height the discharge would be considerably less brilliant, especially in air, than that at 37·67 miles, the height of maximum brilliancy.

At a height of 124·15 miles the pressure would be only 0·00000001 millim., 0·00001 **M**, and it is scarcely probable that an electric discharge would occur with any potential conceivable at such a height.



The colour of the discharge varies greatly with the tenuity of air or other gas with the same potential. Thus in air at a pressure of 62 millim., 81579 **M**, the discharge has the carmine tint which is so frequently observed in the display of the Aurora; this corresponds to an altitude 12·4 miles, and would be visible at a distance 336 miles. At a pressure of 1·5 millims., 1974 **M**, corresponding to a height of 30·86 miles, the discharge becomes salmon coloured, having completely lost the carmine tint. At a pressure of 0·8 millim., corresponding to 33·96 miles, the tint of the discharge is of a paler salmon colour, and as the exhaust is carried further it becomes a pale milky white. The roseate and salmon coloured tints are always in the vicinity of the positive source of the electric current, the positive luminosity fades away gradually, and frequently becomes almost invisible at some distance from its source; as, for instance, in the hydrogen discharge at a pressure of 2·3 millims., 3027 **M**, shown in the accompanying figure, H, which resembles in some respects the phenomena of the Aurora. The discharge at the negative terminal in air is always of a violet hue, and this tint in the Aurora indicates a proximity to the negative source.

The following table, with the exception of pressure 0·00000001 millim., exhibits deductions from actual observations.

Pressure mm.	Pressure **M**.	Height miles.	Visible at miles.	Remarks.
0·00000001	0·00001	124·15	1061	No discharge could occur.
0·000055	0·066	81·47	860	Pale and faint.
0·379	499·0	37·67	585	Maximum brilliancy.
0·800	1053·0	33·96	555	Pale salmon.
1·000	1316·0	32·87	546	Salmon coloured.
1·500	1974·0	30·86	529	,, ,,
3·000	3947·0	27·42	499	Carmine.
20·660	27184·0	17·86	403	,,
62·000	81579·0	12·42	336	,,
118·700	156184·0	11·58	324	Full red.

It is conceivable that the Aurora may occur at times at an altitude of a few thousand feet.

April 29, 1880.

THE TREASURER in the Chair.

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

The following Papers were read:—

I. "Note on the Spectrum of Carbon." By J. NORMAN LOCKYER, F.R.S. Received April 8, 1880,

In the year 1878 [*] I communicated to the Royal Society a paper in which the conclusion was drawn that the vapour of carbon was present in the solar atmosphere.

This conclusion was founded upon the reversal in the solar spectrum of a set of flutings in the ultra-violet.[†] The conclusion that these flutings were due to the vapour of carbon, and not to any compound of carbon, was founded upon experiments similar to those employed in the researches of Attfield and Watts, who showed that the other almost exactly similar sets of flutings in the visible part of the spectrum were seen when several different compounds of carbon were exposed to the action of heat and electricity. In my photographs the ultra-violet flutings appeared under conditions in which carbon was the only constant, and it seemed therefore reasonable to assume that the flutings were due to carbon itself, and not to any compound of carbon.

Professors Liveing and Dewar have recently on several occasions called this result in question. Professor Dewar, in a paper received by the Royal Society on January 8, 1880, writes as follows:—

"The almost impossible problem of eliminating hydrogen from masses of carbon, such as can be employed in experiments of this kind, prove conclusively that the inference drawn by Mr. Lockyer as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, *seeing that undoubtedly hydrogen was present in the carbon*[‡] used as the poles."

Subsequently, in a paper received by the Royal Society on February 2, Messrs. Liveing and Dewar wrote as follows:—

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[*] "Proc. Roy. Soc.," vol. 27, p. 308.

[†] The approximate wave-length of the brightest member on the least refrangible edge is 3881·0.

[‡] The italics are mine.—J. N. L.

"Mr. Lockyer ('Proc. Roy. Soc.,' vol. 27, p. 308) has recently* obtained a photograph of the arc in chlorine, which shows the series of fluted bands in the ultra-violet, on the strength of which he throws over the conclusion of Ångström and Thalèn, and draws inferences as to the existence of carbon vapour above the chromosphere in the coronal atmosphere of the sun, which, if true, would be contrary to all we know of the properties of carbon. We cannot help thinking that *these bands were due to the presence of a small quantity of nitrogen.*" †

It will be seen that on January 8 Mr. Dewar alone attributed the flutings to hydrocarbon, while on February 2 Mr. Dewar, associated with Mr. Liveing, attributed them to a nitrocarbon.

In the latter paper Messrs. Liveing and Dewar published some experiments on the spectra of various carbon compounds, and from their observations they have drawn the conclusion that the set of flutings which I have shown to be reversed in the solar spectrum is really due to cyanogen, and that certain other sets of flutings shown by Attfield and Watts to be due to carbon are really due to hydrocarbon.

As Messrs. Liveing and Dewar do not controvert the very definite conclusions arrived at by Attfield, Morren, Watts, and others, I can only presume that they have taken for granted that the experimental work performed by these men was tainted by the presence of impurities, and that it was impossible to avoid them. I have therefore thought it desirable to go over the ground again, modifying the experimental method so as to demonstrate the absence of impurities. Indeed, I have started upon a somewhat lengthy research, and have taken a large number of photographs under various conditions, so that to reduce them all will be a work of time. I beg permission, therefore, in the meantime, to submit to the notice of the Society an experiment with a tube containing CCl_4, which, I think, establishes the conclusions arrived at by prior investigators. And I may add that it is the more important to settle the question, as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views of a kind which appear to me calculated to mislead, and which I consider to have long been shown to be erroneous.

As my results, to which I now draw attention, simply endorse former work, it is important to state that former work somewhat at length and in the words of the authors.

The prior work of Attfield (in 1862) will be gathered from the following extract from his paper ("Phil. Trans.," vol. 152, p. 221 *et seq.*):—

"On recently reading Swan's paper by the light that Professors Bunsen and Kirchhoff have thrown on the subject, I came to the conclusion that these bands must be due to incandescent carbon vapour;

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* That is, in 1878.—J. N. L.

† The italics are mine.—J. N. L.

that, if so, they must be absent from flames in which carbon is absent, and present in flames in which carbon is present; that they must be observable equally in the flames of the oxide, sulphide, and nitride as in that of the hydride of carbon; and, finally, that they must be present whether the incandescence be produced by the chemical force, as in burning jets of the gases in the open air, or by the electric force, as when hermetically sealed tubes of the gases are exposed to the discharge of a powerful induction coil. . . .

"To establish the absolute identity of the hydro- and nitro-carbon spectra, excluding of course the lines due to nitrogen, they were simultaneously brought into the field of the spectroscope: one occupying the upper, and the other the lower half of the field.

"This was readily effected after fixing the small prism, usually supplied with spectroscopes, over half of the narrow slit at the further end of the object-tube of the instrument. The light from the oxyhydrocarbon flame was now directed up the axis of the tube by reflection from the little prism, while that from the oxynitrocarbon flame passed directly through the uncovered half of the slit. A glance through the eye-tube was sufficient to show that the characteristic lines of the hydrocarbon spectrum were perfectly continued in the nitrocarbon spectrum. A similar arrangement of apparatus, in which the hydrocarbon light was replaced by that of pure nitrogen, showed that the remaining lines of the nitrocarbon spectrum were identical with those of the nitrogen spectrum. In this last experiment the source of the pure nitrogen light was the electric discharge through the rarefied gas.

"The above experiment certainly seemed to go far towards proving the spectrum in question to be that of the element carbon. Nevertheless, the ignition of the gases having been effected in air, it was conceivable that hydrogen, nitrogen, or oxygen had influenced the phenomena. To eliminate this possible source of error, the experiments were repeated out of contact with air. A thin glass tube 1 inch in diameter and 3 inches long, with platinum wires fused into its sides, and its ends prolonged by glass quills having a capillary bore, was filled with pure dry cyanogen, and the greater portion of this gas then removed by a good air-pump. Another tube was similarly prepared with olefiant gas. The platinum wires in these tubes were then so connected with each other that the electric discharge from a powerful induction-coil could pass through both at the same time. On now observing the spectra of these two lights in the simultaneous manner previously described, the characteristic lines of the hydrocarbon spectrum were found to be rigidly continued in that of the nitrocarbon. Moreover, by the same method of simultaneous observation, the spectrum of each of these electric flames, as they may be termed, was compared with the corresponding chemical flames, that is, with the oxyhydrocarbon and oxynitrocarbon jets of gas burning in air. The characteristic lines were present in every case. Lastly, by similar inter-observation a few other lines in the electric spectrum of the hydrocarbon were proved to be due to the presence of hydrogen, and several others in the electric spectrum of the nitrocarbon to be caused by the presence of nitrogen. . . . .

"The spectrum under investigation having then been obtained in one case when only carbon and hydrogen were present, and in another when all elements but carbon and nitrogen were absent, furnishes, to my mind, sufficient evidence that the spectrum is that of carbon.

"But an interesting confirmation of the conclusion just stated is found in the fact that the same spectrum is obtained when no other elements but carbon and oxygen are present, and also when carbon and sulphur are the only elements under examination. And first with regard to carbon and oxygen. Carbonic oxide burned in air gives a flame possessing a continuous spectrum. A mixture of carbonic oxide and oxygen burned from a platinum-tipped safety-jet also gives a more or less continuous spectrum, but the light of the spectrum has a tendency to group itself in ill-defined ridges. Carbonic oxide, however, ignited by the electric discharge in a semi-vacuous tube, gives a bright sharp spectrum. This spectrum was proved, by the simultaneous method of observation, to be that of carbon plus the spectrum of oxygen. With regard to carbon and sulphur almost the same remarks may be made. Bisulphide of carbon vapour burns in air with a bluish flame. Its spectrum is continuous. Mixed with oxygen and burned at the safety-jet, its flame still gives a continuous spectrum, though more distinctly furrowed than in the case of carbonic oxide; but when ignited by the electric current its spectrum is well defined, and is that of carbon plus the sulphur. That is to say, it is the spectrum of carbon plus the spectrum that is obtained from vapour of sulphur when ignited by the electric discharge in an otherwise vacuous tube.

"Having thus demonstrated that dissimilar compounds containing carbon emit, when sufficiently ignited, similar rays of light, I come to the conclusion that those rays are characteristic of ignited carbon vapour, and that the phenomenon they give rise to on being refracted by a prism is the spectrum of carbon."

The only reference to this admirable work, in which vacuum tubes and the electric discharge were largely employed, which I can find in Messrs. Dewar and Liveing's paper is the following:—"The spectrum of hydrocarbon *burning in air*[*] has been . . . described . . . . by Attfield."

In 1865 Morren wrote:[†]—

"A la réception de cet intéressant et substantiel Mémoire, j'avoue

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[*] The italics are mine.—J. N. L.

[†] "Annales de Chemie et de Physique," 4 série, tome iv, pp. 309, 312.

que je ne regardai pas d'abord comme fondée l'assertion de M. Attfield. . . . .

"Je me suis donc mis au travail avec la pensée préconçue de combattre l'assertion émise par le savant Anglais; mais pas du tout, il résulte au contraire des expériences auxquelles je me suis livré que M. Attfield a raison, et que c'est bien la vapeur du carbone qui donne la spectre indiqué plus haut. . . . .

"Si on fait brûler le cyanogène au moyen du chalumeau à deux courants, en faisant arriver au centre de la flamme du cyanogène un courant d'oxygène très-pur (cette condition est indispensable) on voit se produre un des plus beaux effets de combustion possible, et cette expérience est certainement une des plus magnifiques qu'on puisse réaliser sur la combustion des gaz. Il se produit, au milieu de la flamme *rosé-violâtre* du cyanogène, une boule d'un blanc vert éboulissant qui rappelle la lumière électrique produite par le courant de la pile entre deux charbons de cornue. Si le spectroscope est dirigé sur cette brillante lumière, on aperçoit, avec une splendeur merveilleuse, le même spectre de la partie bleue des flammes hydrocarburées. Ainsi donc c'est du charbon seul, mais à l'état de vapeur, qui forme cette boule brillante qui plus loin, par son union avec l'oxygène, va passer a l'état d'acid carbonique. Du reste ce spectre n'est pas seul; avec lui on voit, mais très-effacé, le spectre spécial du cyanogène, et celui-ci tend de plus en plus à disparaître à mesure que l'oxygène arrive avec plus d'abondance et brûle de mieux en mieux le cyanogène. Quant au spectre de l'azote, on ne l'aperçoit pas dans cette vive lumière. Le magnifique éclat de ce beau spectre, le plus beau qu'il m'ait été donné de voir, permet de bien comprendre l'aspect creusé et ombré avec une teinte croissante qu'on remarque dans les parties qui n'ont pas de raies brillantes, et même entre ces raies."

Four years later Dr. Watts took up the question, and in 1869 his work was thus summarised by himself: *—

"This spectrum (that consisting of the flutings in question) may be obtained from the flame of any hydrocarbon, though in many cases, owing to the faintness of the spectrum, only some of the groups can be recognised. In the flame of an ordinary Bunsen burner δ and ϵ are easily seen, γ and f are much fainter, and the red group cannot be detected.

"This spectrum is proved to be that of carbon, inasmuch as it can be obtained alike from compounds of carbon with *hydrogen*, with *nitrogen*, with *oxygen*, with *sulphur*, and with *chlorine*. I have obtained it, namely, from each of the following compounds:—Olefiant gas, cyanogen, carbonic oxide, naphthalin, carbonic disulphide, carbonic tetrachloride, amylic alcohol, and marsh-gas."

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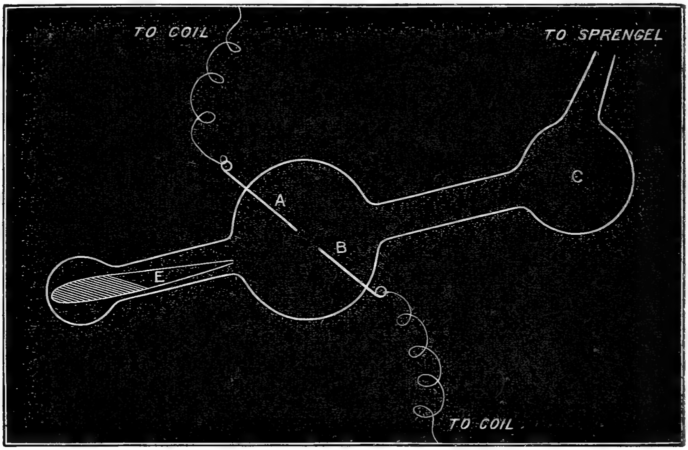
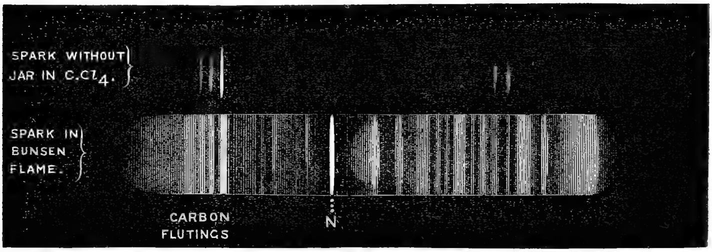
* "Phil. Mag.," October, 1869.

I now come to my own experiments.

The tube to which I have already referred is shown in the diagram: A and B are platinum wires for passing the spark inside the tube; E is a small tube into which carbon tetrachloride was introduced; it was drawn out to a long narrow orifice to prevent the rapid evaporation of the liquid during the exhaustion of the tube. The tube was bent upwards and a bulb blown at C in order that the spark might be examined with the tube end-on, as it is found that after the spark has passed for some time a deposit is formed on the sides of the bulb immediately surrounding the platinums, thus obstructing the light. After a vacuum had been obtained the tube was allowed to remain on the Sprengel for a long time, in order that the last traces of air might be expelled by the slow evaporation of the liquid.





The carbon tetrachloride was prepared by Dr. Hodgkinson, who very kindly supplied me with sufficient for my experiments.

On passing the spark without the jar in this tube, the spectrum observed consists of those sets of flutings which, according to Messrs.

Liveing and Dewar, are due to hydrocarbon, and the set of flutings which is reversed in the sun, and ascribed by Messrs. Liveing and Dewar to cyanogen, also appear in a photograph of the violet end of the spectrum. On connecting a Leyden jar with the coil and then passing the spark the flutings almost entirely vanish and the line spectra of chlorine and carbon take the place of the flutings without either a line of hydrogen or a line of nitrogen being visible.

As a long experience has taught me that these tubes often leak slightly at the platinums after they are detached from the pump, I took the occasion afforded by a visit of Dr. Schuster to my laboratory while the experiments were being made to get my observations confirmed. He has been good enough to write me the following letter, and to allow me to give it here:

"*March* 21.

"MY DEAR LOCKYER,—The following is an account of the experiment which I saw performed in your laboratory on Monday, March 15:—

"A tube containing carbon tetrachloride was attached to the Sprengel pump. As exhaustion proceeded the air was gradually displaced by the vapour of the tetrachloride. The electrodes were a few millimetres apart. If the spark was taken without a condenser in the vapour the well-known carbon bands first discovered by Swan in the spectrum of a candle were seen with great brilliancy; I also saw the blue band which you said was identical in position with one of the blue bands seen in the flame of cyanogen or in the spectrum of the electric arc. When the condenser and air-break were introduced this spectrum gave way to a line spectrum in which I could recognise the lines of chlorine. *The lines of nitrogen were absent, not a trace of the principal double line in the green being seen.* The hydrogen line $H\alpha(C)$ was faintly visible when I first observed the spectrum, but it got gradually weaker and *finally disappeared altogether. When this line was no longer visible the condenser was taken out of circuit again, and the same carbon bands were seen as before.* These bands, therefore, show themselves with great brilliancy when a strong and powerful spark does not reveal the presence either of hydrogen or nitrogen.

"(Signed) ARTHUR SCHUSTER."

This result, which entirely endorses the work of Attfield and Watts, has been controlled by many other experiments. I have also repeated Morren's experiment and confirmed it, and I have also found that the undoubted spectrum of cyanogen is visible neither in the electric arc nor in the surrounding flame.

I hope to show in the complete paper that the various difficulties which have always been acknowledged to attend observations of this substance may in all probability be due to the fact that the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum, and that the tension of the current used now brings one set of flutings into prominence, and now another.

I have so often taken occasion to refer with admiration to the work of Ångström and Thalèn that I shall not be misunderstood when I say that their conclusions, to which such prominence is given, and on which such great stress is laid, by Messrs. Liveing and Dewar, rests more upon a theory which has been shown to be false since the labours commenced and analogy than upon experiment.

Their work, undertaken at a time when the existence of so-called "double spectra" was not established upon the firm basis that it has now, and when there was no idea that the spectrum recorded for us the results of successive dissociations, gave the benefit of the doubt in favour of flutings being due to compounds, and it was thought less improbable that cyanogen or acetylene should have two spectra than that carbon or hydrogen should possess them.

These views were accepted by myself until years of work and experience had shown them to be untenable, and later researches have thrown doubt upon the view that the fluted spectra of aluminium and magnesium are entirely due to the oxides of those metals instead of to the metals themselves—and this is the very basis of the analogy which Ångström and Thalèn employed.

From what I have shown it will be clear that the consequences drawn in the following paragraphs* by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands are entirely invalid.

"The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green bands of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn-out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connexion the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon as it now appears, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum.

"The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connexion with conclusions to

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* Paper read February 2, 1880.

which our experiments point. Provided we admit that materials of the comet contain ready formed hydrocarbons, and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum or of metallic lines. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature, and if nitrogen were also present that we should have the cyanogen spectrum as well. Either, then, the first supposition is the true one, not disproving the presence of nitrogen, or else the atmosphere which the comet meets is hydrogen only, and contains no nitrogen."

II. "On the Diurnal Variation in the Amount of Carbon Dioxide in the Air." By GEORGE FREDERICK ARMSTRONG, M.A., F.G.S., C.E., Professor of Engineering in the Yorkshire College, Leeds. Communicated by Professor THORPE, F.R.S. Received April 12, 1880.

Although a large share of attention has been given to the elucidation of the causes which influence the amount of carbonic acid present in the atmosphere during the day, no systematic observations with reference to the relative quantities present in the air of the land during the day and the night appear to have been undertaken since the well-known experiments of the younger De Saussure at Chambeisy,* upwards of 50 years ago (1826–30), and a similar set by Boussingault at Paris,† a few years later, until M. Truchot‡ took up the question in 1873. But the results thus obtained cannot be said to be altogether satisfactory.

On the other hand, the question as to the existence of a diurnal variation in the amount of carbonic acid in sea-air may be said to have been fully worked out and set at rest by the careful and extensive set of observations made some years ago by Dr. Thorpe§ on the air of the Irish Sea and Atlantic Ocean. His results went to show that no appreciable diurnal difference exists.

As regards a diurnal variation in the case of land-air, the results of De Saussure are somewhat contradictory; and latterly, what he himself considered as "one of the most remarkable results" in the whole range of his inquiries—the discovery of a distinct diurnal variation in the amount of carbonic acid—has been regarded as but very imperfectly established.

De Saussure published his first set of results which were deduced

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* "Ann. de Chim. et de Phys.," vols. xxxviii (1828) and xliv (1830).

† "Ann. de Chim. et de Phys.," [3], 10 (1844).

‡ "Compt. Rend.," 77, 675.

§ "Journ. Chem. Soc.," [12], v. 189.

from experiments extending over the previous four or five years, in 1828. Among these were nine day and an equal number of night observations. The latter showed an excess of 0·726 vol. of CO_2 in 10,000 vols. of air over those of the day, and it was upon this somewhat limited basis that he was led to affirm the existence of a diurnal variation. His exact figures are, for the day, 5·037 vols., and for the night, 5·764 vols. CO_2 in 10,000 of air.

In 1830 he published a further set of results, among which were some experiments not made with the primary object of establishing this variation, but for the purpose of finding the difference between the amounts of carbonic acid in the air of a town (Geneva) and of the open country.* There were in all 13 such experiments extending over parts of two years (1827-28), eight of which had reference to the day and five to the night. Upon examining these a diametrically opposite result to his former one is obtained. The mean of those made in the country gives, for the day, 4·45 vols., and for the night, 4·02 vols. CO_2 in 10,000 of air; thus showing an excess of 0·43 vol. in favour of the day.

The experimental evidence as to the existence of a diurnal variation in the amount of atmospheric carbon dioxide was therefore inconclusive, and I accordingly undertook, at Dr. Thorpe's suggestion and with his collaboration, to carry out a series of observations with direct reference to the question at Grasmere in Westmoreland, during the summer and autumn of 1879.

Up to this time the state of our knowledge concerning a diurnal variation in the amount of atmospheric carbon dioxide may be said to be comprised in the following statement.

Vols. of CO_2 in 10,000 of Air.

	Day.	Night.	Excess.	
			Day.	Night.
Land-air.				
De Saussure:				
1828	5·037	5·764	..	0·727
1830	4·56	4·81	..	0·250
,,	4·45	4·02	0·43	—
P. Truchot:				
1873 { away from vegetation	3·14	3·78	..	0·64
1873 { near vegetation	4·15	6·49	..	2·34
Sea-air.				
T. E. Thorpe	3·011	2·993	0·028	—

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* "Ann. de Chim. et de Phys.," vol. xliv (1830).

The method of estimation which I employed consisted in absorbing the carbonic acid from a known volume of air by means of baryta water of known strength, the amount of carbonic acid so absorbed being determined by titrating the alkaline fluid with a standard acid solution, 1 cub. centim. of which was approximately equal to 1 mgrm. of CO_2. This method is generally known as Pettenkofer's, but it is worth noting that it had been used by Dalton as far back as 1802.* It is particularly well adapted to the end I had in view, for it is not merely the most expeditious of the processes hitherto devised, but it allows of the observations being made under strictly comparative conditions.

The observations to which this communication refers were made in a garden at Grasmere situated at the foot of a hill some 1,300 feet or 1,400 feet high, and which lies only a few feet above the level of the adjacent lake, which is 208 feet above that of the sea.

They were commenced on the 21st July and continued to the 25th October; the whole summer and autumn being of an exceptionally wet and sunless character. It was originally intended to pursue the observations "*de die in diem*" until a sufficient number were obtained to furnish a trustworthy average for comparison; but the weather proved such as to render this, if not impracticable, at least undesirable owing to the difficulty, especially at midnight, of filling the jars with air in the open without at the same time admitting a few drops of rain also, even although the greatest care was used in the operation.

Whenever, therefore, any such danger owing to the state of the weather seemed imminent, and that there was therefore a chance of the error so occasioned creeping into the experiments, the observation for the particular time was abandoned. In some instances by slightly varying the times chosen for the observations (noon and midnight) it was found possible to include some that otherwise must, for the foregoing reason, have been omitted. And, with but few exceptions, the slight differences as to time observable in the accompanying tables are to be attributed to this cause.

The place chosen for collecting the air for examination was a nook on the lawn upwards of 100 feet from the house and almost entirely surrounded by trees, shrubs, and flowers; the plants being chiefly the following, namely: pine, yew, holly, laurel, hawthorn, mountain ash, rhododendron, geranium, rose, phlox, and azalea.

The air was taken at from four to five feet from the ground and always, with one or two exceptions noted in the tables, at the same place. On two or three occasions it was taken near a conservatory, by well lighting the interior of which it was possible to dispense with the lamp otherwise employed when filling the jars at night; and thus

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* Dr. Angus Smith, "Air and Rain," p. 448 *et seq.*

it was possible to ascertain whether any errors were being introduced thereby.

The jars used for collecting the air were the ordinary spice jars of the confectioners, but of a somewhat unusually large size and with mouths sufficiently wide to readily admit the hand for cleansing and drying. The latter was effected by means of clean linen cloths specially kept for the purpose. The mouths of the jars were easily and tightly closed with Jennings' tin and india-rubber capsules.

The jars employed were four in number, and were distinguished as A, B, C and D. Their available capacities were as follow :—

A=14,260 cub. centims. B=14,845 cub. centims.
C=14,905 cub. centims. D=13,385 cub. centims.

The quantities of air experimented upon are thus very considerable; indeed the jars are by far the largest hitherto employed in similar investigations since De Saussure used a globe of 34 litres; while they are nearly double the capacity of those used by Dr. Thorpe in his Atlantic and Irish Sea experiments.

The increased quantity of air thus taken for examination should, it will be seen, tend to enhance the value and accuracy of the results, since the possibility of detecting any differences that may exist in the amounts of carbonic acid present in the air during the day and at night is thereby greatly increased.

The mode of filling the jars, the interiors of which were previously carefully dried and polished, was partly by blowing the air into them with a large pair of ordinary kitchen bellows and partly by violently tossing them to and fro; but a test experiment showed that the latter means was alone sufficient for the purpose.

Baryta water, the strength of which was frequently tested, was used to absorb the carbonic acid, and 75 cub. centims. were introduced into each jar. This quantity was chosen to allow of a duplicate titration of 25 cub. centims. in each experiment.

After being filled with air and the baryta water added, the jars were well shaken and rolled on their sides so as to cover their internal surfaces with the baryta solution, and they were then allowed to stand not less than four hours. Several test experiments showed that this time was sufficient to effect the maximum absorption of the carbonic acid so attainable.

Two jars at least were filled at each observation, and wherever the results from one jar only are noted in the tables the omission is due to some accident either in the filling, or at a subsequent stage of the experiment.

The exposure of the baryta water being completed, the contents of each jar were quickly decanted into a stoppered bottle and allowed to stand until the liquid was perfectly clear. And this was found to be a point of importance, inasmuch as the precision with which the titration point can be fixed is largely dependent upon there being no carbonate in suspension.

The acid used in titration was a standard solution of sulphuric as used by Dalton, 1 cub. centim. of which was equal to 1 mgrm. of carbonic acid. This was substituted for the oxalic acid as recommended by Pettenkofer owing to its greater stability.

The point of neutrality was ascertained by means of a dilute alcoholic solution of pure aurin; a definitely measured quantity being employed in each titration.

The results thus obtained and set forth in the following tables, fully establish the existence of a diurnal variation in the quantity of carbonic acid, the night average exceeding that of the day by 0·34 cub. centim. in 10,000 of air; the quantities respectively being very nearly in the ratio of 90 to 100. These figures are, as will be seen, based upon 53 day and 62 night experiments; there being 27 complete day and 29 complete night observations.

The details of the experiments contained in the accompaying tables are arranged in the following order:—

Column (1). Number of the observation.
,, (2). Date of the observation.
,, (3). Hour at which the observation was made.
,, (4). Barometric reading.
,, (5). } Thermometric reading: dry and wet bulb.
,, (6). }
,, (7). State and direction of wind, when observable.
,, (8). }
,, (9). } Jars employed and results obtained from each.
,, (10). }
,, (11). }
,, (12). Special remarks.
,, (13). General meteorology at the time of observation.
,, (14). General meteorology of the preceding day or night, if important.

On looking over the day experiments, the averages, it will be seen, are low as compared with those of the older experimenters, the final average being only 2·96 vols. of carbonic acid in 10,000 vols. of air. But it is extremely interesting to compare this result with those obtained by Schulze at Rostock* during the years 1868-71. His figures are, as averages:—

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* "Watts' Chem. Dict.," Suppl. III, 113.

TABLE I.—Day Observations.

No. of observation.	Date.	Hour.	Bar. Inches.	Temperature.		Wind.	Vols. CO_2 in 10,000 of air.				Remarks.	Meteorology.	Meteorology of previous night.
							Jars.						
				Dry Bulb.	Wet Bulb.		A	B	C	D			
1	July 21	4.30 P.M.	29·28	60·0	56·7	... ...	2·991	...	...	...	... ...	Cloudy after much rain.	
2	,, 24	12.30 ,,	29·79	64·0	58·0	... ...	2·780	...	...	...	... ...	Bright sunshine; drizzle and mist in early morning.	
3	,, 27	,,	29·75	61·0	55·5	... ...	...	...	3·260	3·020	... ...	Fair, but overcast and inclined to rain.	Overcast after fine evening.
4	,, 30	12.15 ,,	29·68	67·0	59·0	... ...	2·980	...	...	3·230	... ...	Sunshine; some clouds.	Overcast after showery day; sheet lightning.
5	Aug. 1	10.10 ,,	29·58	58·5	54·7	... ...	2·990	2·974	3·019	3·110	B and D were titrated after 7¼ hours, and A and C after 24½ hours' exposure.	Sunshine; some clouds.	Few clouds after fine day; moon and starlight.
6	,, 9	12.15 ,,	29·68	62·0	58·0	N. by W.	...	2·874	3·072	...	... ...	Cloud and sunshine.	Cloudy, after fine day.
7	,, 13	Noon.	29·58	60·5	60·0	S. by W.; light air	3·100	...	3·010	...	... ...	Overcast after heavy rain and electrical disturbance.	Starlight; moderate dew after very sultry day.
8	,, 14	12.15 P.M.	29·85	64·5	60·2	Light air	...	2·710	...	2·920	... ...	Fine.	
9	,, 18	12.20 ,,	29·38	61·0	57·0	N. by E.	...	2·900	2·930	...	... ...	Fine break in showery morning.	
10	,, 19	12.10 ,,	29·54	61·0	58·0	S. by W.; light air	3·080	...	...	2·900	... ...	Overcast: tendency to rain.	Heavy rain.
11	,, 20	12.20 ,,	29·32	61·0	59·0	Light breeze ...	...	2·810	2·860	...	... ...	Overcast; tendency to rain.	Rain.
12	,, 22	Noon.	29·24	57·0	53·0	W. by S.	...	2·840	2·900	...	... ...	Showers preceded by heavy rain.	
13	,, 23	12.20 P.M.	29·55	61·0	56·0	S.W.; light breeze	...	2·840	2·915	...	Air taken near conservatory.	Fine; sky three parts obscured.	Overcast; few breaks preceded by much rain.
14	,, 24	12.35 ,,	29·74	64·0	58·0	W. by S. ,,	...	3·060	3·010	...	... ...	Fine; some clouds; heavy mist during early morning.	Starlight; cloudless; considerable dew.

15	„ 25	Noon.	29·31	60·5	54·0	S.W.; strong breeze; freshened to half a gale.	...	2·950	2·990	...	... ...	Overcast; some sun; rain followed by hail-storm began; large meniscus-shaped hail-stones.	Overcast; rain beginning after fine day.
16	„ 29	1 P.M.	29·31	54·5	54·5	W.; light breeze, with occasional gusts.	...	2·891	3·020	...	... ...	Rain, preceded by three days' heavy and incessant rain.	
17	„ 31	Noon.	29·76	56·0	52·0	W. by N.; light breeze.	...	3·004	3·001	...	... ...	Sunshine and cloud; unsettled; showery morning.	
18	Sept. 7	12.10 P.M.	29·22	64·5	62·0	S.; light air ...	...	3·040	2·950	...	... ...	Overcast; dull and tendency to rain.	
19	„ 8	Noon.	29·28	58·0	53·0	W. by S.; fresh breeze.	...	2·830	2·980	...	... ...	Overcast; some gleams of sunshine, but generally cloudy.	Tempestuous and rainy.
20	„ 10	„	29·72	58·0	54·0	W. by N.; light breeze.	...	2·960	2·861	...	... ...	Sunshine and cloud after very fine morning.	Fine; preceding day unsettled.
21	„ 12	11.40 P.M.	29·26	58·0	54·0	S.W.; ditto ...	...	2·970	2·980	...	... ...	Sunshine and cloud after fine morning.	
22	„ 13	Noon.	29·46	57·5	52·0	W. by S.; light air	...	...	2·980	...	... ...	Fine and bright; some clouds.	Starlight, preceded by rain; some cloud.
23	„ 24	12.10 P.M.	29·28	54·5	49·0	S.W. „	...	2·910	3·030	...	... ...	Sunshine and cloud; tendency to overcast.	
24	„ 29	Noon.	29·90	55·0	51·0	W. „	...	2·915	2·910	...	... ...	Sunshine and much cloud; very bright early morning.	Almost cloudless; heavy rain during preceding afternoon; moon near full.
25	Oct. 11	„	30·32	53·0	52·0	S. by E. to S., ditto	...	2·920	2·910	...	... ...	Fine, but overcast and dull; haze and mist on hills.	
26	„ 18	12.35 P.M.	29·70	48·5	45·0	W. by N.; light breeze.	...	2·880	2·980	...	... ...	Sunshine and much cloud after fine and brilliant morning.	
27	„ 25	12.30 „	29·48	48·0	46·0	W. by S.; ditto ...	...	2·940	3·010	...	... ...	Showery after bright early morning; some sun.	
							17·921	58·218	65·578	15·180	=156·897.		

$$\text{Day average} = \frac{156 \cdot 897}{53} = 2 \cdot 9603 \text{ vols. carbonic acid in } 10{,}000 \text{ vols. of air.}$$

Table II.—Night Observations.

No. of observation.	Date.	Hour.	Bar. Inches.	Temperature.		Wind.	Vols. CO_2 in 10,000 of air.				Remarks.	Meteorology.	Meteorology of previous day.
				Dry Bulb.	Wet Bulb.		Jars.						
							A	B	C	D			
1	July 26	12 A.M.	29·76	55·0	54·0	... ...	...	...	3·163	...	... ...	Overcast; occasional drizzle.	
2	,, 30	,, ,,	29·66	60·5	60·0	... ...	...	3·761	3·835	...	... ...	Overcast; some sheet lightning.	Showery.
3	,, 31	12.10 ,,	29·70	51·0	51·0	... ...	...	3·611	3·829	...	... ...	Few clouds; moon and starlight.	Fine.
4	Aug. 2	12.15 ,,	29·88	49·0	49·0	... ...	...	3·600	...	3·410	... ...	Some clouds; moon and starlight.	Fine.
5	,, 8	11.15 P.M.	29·60	50·5	47·0	... ...	3·352	3·371	3·130	3·218	... ...	Cloudy	Fine.
6	,, 10	12.10 A.M.	29·75	44·0	44·0	... ...	...	3·604	3·601	3·495	... ...	Cloudless; starlight; heavy dew.	Fine and bright.
7	,, 11	,, ,,	29·80	58·0	55·0	Calm...	3·590	...	...	3·420	... ...	Overcast; no dew.	
8	,, 12	12.5 ,,	29·76	59·5	59·0	Light air	3·560	4·080	3·836	3·650	... ...	Clear; starlight; moderate dew.	Very sultry.
9	,, 15	,, ,,	29·86	48·0	48·0	,,	...	3·450	3·770	3·270	... ...	Fine; starlight; heavy dew.	Fine and hot.
10	,, 21	,, ,,	29·42	57·0	56·0	Light breeze ...	...	3·070	...	...	... ...	Overcast; tendency to rain.	Fine but cloudy.
11	,, 23	,, ,,	29·42	56·0	54·5	S. W., ditto... ...	...	2·950	2·980	...	Air taken near conservatory.	Overcast, with few breaks.	Much rain.
12	,, 24	,, ,,	29·65	45·0	45·0	Light air	3·320	3·340	3·370	...	Air in A taken near conservatory. A and B filled with bellows and by throwing. C by throwing only.	Cloudless; starlight; considerable dew.	Fine with considerable cloud.
13	,, 25	,, ,,	29·51	56·5	54·0	Light breeze ...	...	3·110	2·990	...	... ...	Overcast; beginning to rain.	Fine.
14	,, 30	,, ,,	29·56	46·0	45·0	... ...	...	3·190	2·970	...	... ...	Moonlight; few clouds	Drenching rain.
15	Sept. 1	,, ,,	30·06	44·0	44·0	W.; light air ...	...	3·140	2·900	...	... ...	Moonlight; much cloud; few drops of rain.	Fine and bright afternoon.
16	,, 2	,, ,,	30·14	55·0	51·0	W.; fresh breeze	...	2·920	2·990	...	... ...	Overcast; cloudy ...	Fine and bright.

17	,,	5	,, ,,	29·92	50·0	49·0	Light air	...	3·600	3·650	...	... ...	Overcast; moon just visible through haze; moderate dew.	Fine and brilliant.
18	,,	8	,, ,,	29·22	53·0	52·0	S.; fresh breeze ...	...	2·910	2·910	...	... ...	Overcast with few breaks.	Much and heavy rain.
19	,,	10	,, ,,	29·52	54·0	52·0	Light air	...	3·086	3·013	...	... ...	Cloudy; few stars visible.	Unsettled; wind north.
20	,,	11	,, ,,	29·72	54·0	52·0	W.; light air ...	...	3·100	3·090	...	... ...	Overcast; masses of heavy black cloud with few breaks; tendency to rain.	Morning bright; afternoon dull.
21	,,	13	,, ,,	29·32	51·5	51·0	W.; ditto	...	...	3·489	...	... ...	Some clouds; starlight	Afternoon cloudy, followed by rain.
22	,,	14	,, ,,	29·52	44·0	41·0	Light air	...	3·180	3·180	...	... ...	Fine; few small clouds; starlight; considerable dew.	Fine and bright; sky at sunset intensely red.
23	,,	20	,, ,,	29·92	44·0	43·0	,, ...	...	3·120	3·360	...	... ...	Much cloud; starlight; slight dew.	Dull and warm.
24	,,	25	,, ,,	29·52	41·0	41·0	,, ...	...	3·590	3·681	...	... ...	Overcast; few stars visible; distant thunder with occasional flashes of lightning; beginning to rain.	Sunshine and cloud; tendency to overcast.
25	,,	27	,, ,,	30·06	38·0	38·0	W by N.; ditto ...	...	3·200	3·150	...	... ...	Almost cloudless; moon and starlight; considerable dew.	Rain during morning; afternoon fine.
26	,,	29	,, ,,	29·80	45·0	43·0	S.W.; light breeze	...	2·980	2·860	...	... ...	Almost cloudless; moon (near full) and starlight; moderate dew	Drenching rain during morning; cleared towards evening.
27	,,	30	,, ,,	30·00	37·5	37·0	W. by N.; light air	...	3·190	3·153	...	... ...	Cloudless; moon and starlight; considerable dew.	Fine and bright; some cloud.
28	Oct.	12	,, ,,	30·33	49·0	48 0	Calm	...	3·190	3·044	...	... ...	Overcast but fair; very dark; slight dew.	Overcast and dull, but fair; much haze and mist on the hills.
29	,,	24	11. 50 P.M.	29·40	44·0	43·0	W. by S.; slight breeze.	...	3·100	2·925	...	... ...	Overcast and showery; some moonlight.	Dull and showery afternoon.
								13·822	85·443	84·869	20·463	=204·597		

$$\text{Night average} = \frac{204{\cdot}597}{62} = 3{\cdot}2999 \text{ vols. of carbonic acid in 10,000 vols. of air.}$$

Night average	3·30 vols. CO_2 in 10,000 of air.		
Day ,, ...	2·96	,,	,,
Excess of night over day	0·34	,,	,,

	Vols. of CO_2 in 10,000 vols. of air.
1868 (October to December) inclusive......	2·8943
1869 (January to December) inclusive......	2·8668
1870 (January to December) inclusive......	2·9052
1871 (January to July) inclusive..........	3·0126

These give a final average on the four years of $\frac{11\cdot6789}{4} = 2\cdot92$ vols. CO_2 as against 2·96 vols. CO_2 in 10,000 of air obtained in the present instance; a difference, that is, of only 0·04 vol. CO_2 in 10,000 of air. This close correspondence between the results is important on account of the method of determination pursued being essentially the same in each case, with the exception that Schulze employed jars of only 4 litres capacity as compared with 14 litres in the present case.

But what is chiefly interesting in this comparison arises from the fact of these low averages obtained by Schulze having been attributed to the contiguity of Rostock, where the experiments were made, to the sea. They approximated so closely to Dr. Thorpe's Atlantic results* for the day, namely, 3·011 vols. CO_2 in 10,000 of air, that their lowness was generally regarded as being due to the influence of sea-air, and this solution has been the more forcibly urged also owing to the fact that, in using a modern method of determination, an average of 347 similar experiments made at Duhme† by Fittbogen and Hasselbarth, has been found to give as much as 3·34 vols. CO_2, and 17 other determinations made by Henneberg at Weend, near Göttingen,‡ 3·2 vols. CO_2 in 10,000 vols. of air, while Truchot has obtained considerably higher figures still.

A similar explanation might with equal plausibility be applied to the low results more recently obtained by Reiset§ in the open country near Dieppe. For, although he employed the more perfectly absorbing method of the aspirator and Woulfe's bottles, and for the remainder followed Pettenkofer's plan of determination, yet, as a mean of 92 experiments made between September, 1872, and August, 1873, his average is only 2·942 vols. CO_2 in 10,000 of air. This result, it will be seen, agrees in a still more remarkable manner with that arrived at in the Grasmere experiments, the difference between the two averages being only 0·018 vol. CO_2 in 10,000 of air. But the sea-air explanation can scarcely be so confidently relied upon in the latter instance as in some of the others. Rostock and the place at which the air was collected near Dieppe, are both close to the sea, the one on the *embouchure* of the Nebel, and the other not more than 5 miles from the coast. Grasmere,

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* "Journ. Chem. Soc.," [2], v, 189.

† "Watts' Chem. Dict.," Suppl. III, 132.

‡ Ibid.

§ "Compt. Rend.," 88, 1007–1011.

on the other hand, is fully 20 miles distant from the sea along the most direct line that can be drawn. From this, then we may infer that there may possibly be other causes which tend to reduce the amount of carbonic acid present, supposing, that is, that the results in question do materially differ from the normal state; an assumption which, as regards the open country, has not yet been clearly established.

The most distinct climatological peculiarity of Grasmere may be said to be its rainfall, which, during such a year as last when the observations in question were made, might have risen to 120 or 130 inches, and it is situated, moreover, within 6 or 7 miles of a point on the Sty-Head Pass which has the largest rainfall in the British Isles, an average, that is, of 175, or in bad years such as last, of 200 inches per annum, while the general average for Great Britain is under 30 inches. A glance at the temperature columns in the tables will show how frequently the point of saturation was practically reached during the observations.

It is hardly possible, however, that the diminished quantity of the carbonic acid is due to the solution of this gas in rain-water. It is well known that the amount of any gas absorbed by water is a function of the pressure which it itself exerts; and it may be readily shown that the actual pressure of the carbonic acid in the atmosphere is far too small to exert any sensible influence upon its amount in the air. If this explanation, therefore, of a diminished quantity of carbonic acid is no longer available, may we not possibly turn to the operation of such an exceedingly active vegetation as is found in some parts of the English Lake District as a more feasible cause of any observed reduction in its amount? For we are not without evidence, as some further experiments shortly to be described as well as those of M. Reiset, near Dieppe, appear to indicate, that this may after all be the chief disturbing agent.

But to return to the point of chief importance in connexion with the experiments at Grasmere, the unquestionable evidence, that is, they afford of an appreciable diurnal variation in the amount of atmospheric carbonic acid.

The only results with which, in reference to this point, it is worth while to compare them, are those of M. Truchot made at Clermont Ferrand, Auvergne, in 1873. With these, although they agree as to the main fact of the existence of a diurnal difference, they nevertheless disagree considerably as to its amount.

M. Truchot, for instance, as shown by the table (ante, p. 344), has detected a diurnal variation in air taken in the neighbourhood of vegetation and in sunshine—in a position that is presumably identical with that in which the Grasmere observations were made—of as much as 2·34 vols. CO_2 in 10,000 of air, while in a position even unsurrounded by vegetation the diurnal difference is as high as 0·64 vol. CO_2 in 10,000 of air. These results compared with that obtained at Grasmere—a diurnal variation, namely, of 0·34 vol. CO_2 in 10,000 of air—seem to point to some cause of variance unconnected with either climate or local conditions.

And now that the existence of a diurnal variation may be said to be fully established, the question as to upon what the difference observed depends still remains to be answered. That in the main it is due to plant action seems more than probable. And in order to test the accepted theory that plants absorb carbonic acid during the day and exhale it at night—to establish which must clearly be a first step in the evidence in proof of a connexion between vegetable activity and a diurnal variation in the quantity of atmospheric carbon dioxide—a short series of experiments, ten in all, was made at Grasmere concurrently with those an account of which has already been given.

But before entering into a description of these it will be useful to refer to the somewhat recent experiments of M. Reiset* already mentioned, which were made in the neighbourhood of Dieppe in 1873, inasmuch as they directly bear upon the point in question. He, using Pettenkofer's method with aspiration, found that, in air taken in the daytime over a field of red clover in the month of June, there were only 2·898 vols. CO_2 in 10,000 of air present as compared with 2·915 vols. in air taken simultaneously in the open; and that in the air over a barley field in July, there were only 2·829 vols. as compared with 2·933 vols. in that of the open; or a reduction in the air taken over the crops, in one case of 0·017 vol. of carbonic acid, and in the other of 0·104 vol. in 10,000 of air as compared with that taken in the open. Nevertheless, in a leafy coppice he obtained what at first sight appeared to be somewhat contradictory results. In this position the air gave a larger quantity of carbonic acid than that simultaneously taken in the open. That taken in the coppice was found to contain 2·997 vols. in 10,000 of air, while that of the open only yielded 2·902 vols. in the same quantity of air. The explanation of this seeming anomaly may possibly be found in the fact of a diminished quantity of light existing within the coppice.

For the purpose of the supplementary experiments to which reference has been made, a fine and healthy young geranium plant was selected, due regard being had to the extent of its leaf-surface. This was suspended day and night alternately in a jar of air sufficiently large to contain it, the mouth being closed as already described by one of Jennings' capsules.

The times chosen for the experiments were from 7 A.M. to 7 P.M. for the day, and from 7 P.M. to 7 A.M. for the night. When the plant

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* "Compt. Rend.," 88, 1007–1011.

was removed from the jar, baryta water was introduced and well shaken, and after standing a sufficient time to effect the absorption of the carbonic acid was titrated in the usual manner.

The results obtained were highly interesting, but it would not at present be prudent to speak of them except in general terms. It will suffice to say that the experiments showed that if the plant did not actually absorb carbonic acid during the day, it exhaled none; while at night large quantities were so got rid of—thus fully substantiating the generally accepted view of the matter in point.

As a mean of seven night experiments made between 23rd August and 29th September, there were 12·18 vols. CO_2 in 10,000 of air found in the jar. The largest quantity present was 14·9 vols., and the least 9·13 vols. in 10,000 vols. of air. The former was at an early period of the experiments, the latter towards their close when the plant had lost a few of its leaves and was beginning to show a diminished activity generally.

Summarising the results contained in this communication, it may be stated—

(1.) That the normal amount of carbonic acid present in the air of the land is distinctly less than that usually stated, and that it does not exceed 3·5 vols. in 10,000 of air.

(2.) That plants absorb carbonic acid during the day and exhale it at night, and that vegetation, therefore, affects the quantity of carbonic acid present in the air, decreasing it by day and increasing it at night.

(3.) That from this cause there is, during that part of the year when vegetation is active, at least 10 per cent. more carbonic acid present in the air of the open country at night than during the day.

III. "Measurement of the Actinism of the Sun's Rays and of Daylight." By Dr. R. ANGUS SMITH, F.R.S.

(Preliminary Notice.)

When examining the air of towns and the effect of smoke and fogs, I have often wished for a very simple chemical method of measuring the total light absorbed by these gases, vapours, and floating solids. I do not undervalue the work of others, but I think I have obtained a process promising good results with great simplicity, although I dare say it introduces its own class of difficulties.

1. The fundamental fact is that when iodide of potassium in solution is treated with nitric acid, so small in quantity as to cause no change of colour in dull diffused light, a change takes place when the same

mixture is brought into clear light; iodine is set free and the solution becomes yellow.

2. The amount of iodine freed can be titrated with great exactness by the use of hyposulphite, as is well known.

In these two facts lies the whole process, the first is the new part, the second makes the first quantitative, and its use is of course part of the novelty.

3. It is known that strong acid liberates iodine. Weak acid does so after a long time, but the process is hastened by light.

4. Heat even to the boiling point does not act so well as light (experiments being made in sealed tubes to prevent loss of iodine, and with a considerable volume of air).

5. Heat assists the action of light.

6. A solution may be exposed day after day so as to give the accumulated effect of sunlight, in a measurable condition at the end of the time.

7. The solution of iodide of potassium as hitherto obtained is subject to change. An old solution, that is, one nearly a month old, was found more sensitive than a new one in all cases tried.

8. The result of No. 7 is, that a certain allowance may require to be made for this, in those cases where the periods of observation with one solution are long.

9. The amount of allowance to be made for temperature is not made out. It is not certain that any is required in the cases when weak acid is used. The weather has not allowed any combined action of great light and heat, but with heat and light in the rays from an electric light with a parabolic reflector, the action was very rapid.

10. Specimens of experiments (prospective at first). It was found convenient to use a solution of 2 grms. of iodide of potassium, afterwards changed to 1 grm., in 100 of water, and to use half of this for an experiment, *i.e.*, 50 cub. centims. of the solution, which may be called A.

A nitric acid solution having an acidity equal to 1 per cent. of sulphuric anhydride was made; this may be called B. Only very small portions of B were added to A.

The following experiments were made in wide loosely corked and half filled test tubes, and here are early trials:—

13th February.

50 c.c.	of A with	0·2 c.c.	of B ;	no action in three hours.	
50	,,	0·4	,,	action ;	not looked at before the end of 3 hours.
50	,,	0·8	,,	decided action ;	

No colour in a shaded part of the room in 420 hours in liquids of the same strength.

49 c.c. of A with 1·0 c.c. of B; colour after 40 minutes; } Sunshine and
48·8 „ 1·2 „ „ 35 „ } cloud
48·5 „ 1·5 „ „ 30 „ } alternately.
48·5 „ 1·5 „ in the diffused light of the laboratory;
no colour after 4 hours.

Here we have action in sunlight in proportion to the acid. The acid gives delicacy. The light commences the chemical action.

Examples in which the decomposition was measured by a solution of hyposulphite of sodium, which may be called solution C=0·1 grm. per litre of iodine (or as convenient). I shall extract experiments made with B solution 0·8 cub. centim., because it is an intermediate one (·2, ·4, ·8, 1·6, and 3·2 have hitherto been the favourites).

1880.		B sol.		Measure by C solution (hyposulphite).
Mar. 3	Sunshine and cloud alternately	0·8	After 2½ hours	8·1. First colour in 20′
„ 4	Sunshine	0·8	..	First colour in 30′
„ 5	Dull all day	0·8	„ 4 „	0·9
„ 8	Sunshine	0·8	„ 2½ „	7·5. Colour in 20′
„ 9	A little sunshine	0·8	„ 2½ „	4·8
„ 10	Foggy, with a gleam of sunshine	0·8	„ 6 „	1·5
„ 11	Bright	0·8	„ 2½ „	7·2
„ 12	Dull and wet........	0·8	„ 3 „	0·6
„ 13	Dark and dull	0·8	„ 2½ „	Faint trace
„ 15	Changeable	0·8	„ 2½ „	1·8
„ 16	Changeable	0·8	„ 2½ „	1·6
„ 18	Sun through haze....	0·8	„ 2½ „	5·8
„ 19	Bright	0·8	„ 2½ „	11·5
„ 20	Fog till 11.30	0·8	„ 2½ „	3·2
April 1	Sun and showers	0·8	„ 2½ „	1·6

Example of one day's observations showing the measured amounts after 2½ and 7 hours. C solution gives the proportion of light effect. The light varied from hour to hour.

March 18th. Sunny.

B sol.		C sol. required after 2½ hours.		C sol. required after 7 hours.
0·2 c.c.	......	1·25 c.c.	......	5·2 c.c.
0·4 „	......	4·1 „	......	11·1 „
0·8 „	......	5·8 „	......	17·5 „
1·6 „	......	9·8 „	......	27·0 „
3·2 „	......	12·8 „	......	33·3 „
6·4 „	......	17·8 „	......	34·8 „

The rate of increase of decomposition requires to be learned, also the most convenient solutions of B for every light and perhaps temperature.

Effect of temperature. Tubes exposed to daylight during a dull day.

B solution.	C solution required.	
0·2	0·15	
0·4	0·90	Temperature = 12°·8 C.
0·8	1·00	
1·0	3·20	
1·6	6·00	

These were in duplicate to begin with, but finding after an hour and a half that almost no action had taken place, one-half were put into the dark. At the end of the day the result was :—

B solution.	C solution required.	
0·2	0·0	
0·4	0·0	
0·8	0·1	Temperature = 11°·1 C.
1·0	0·1	
1·6	0·2	

It is seen that with an almost equal temperature the action is very decided in light on a dull day, but scarcely measurable in darkness.

To examine further the effect of heat the following were tried :—

Temp. 12°·8 C. in light.		Temp. 25° C. in darkness.	
B sol.	C sol. required.	B. sol.	C sol. required.
0·4	0·4	0·4	0
0·8	1·3	0·8	0
1·0	1·9	1·0	0
1·6	5·2	1·6	?

It was suspected that not only did the iodide solution change by keeping, but the nitric acid also, weak as it was. Indeed I had once a mode projected of measuring light by the decomposition of nitric acid or nitrates, but I did not expect this to take place in such weak solutions.

To avoid this change, sulphuric acid was tried with the following result. This trial serves also as a test for the effect of light separate from heat.

(1) 2½ hours' exposure to not very bright clouds; (2) dark:

(1) Temp. 12° C. in light.		(2) Temp. 20° C. in dark.	
Sulphuric acid used, same acidity.	C sol. required.	Sulphuric acid.	C sol. required.
0·4	0·5	0·4	0
0·8	3·9	0·8	0
1·6	4·9	1·6	0
3·2	6·1	3·2	0

11. There seems, therefore, no reason to doubt that this is a true

photometric process, with special capacities to be developed in time. I may add that I did obtain better results at the window of my house than at the laboratory at the same time, the latter being nearer the centre of the town; thus the process has done the duty it was intended for, although only once tried for this special purpose. I am looking to it as an agent specially for the examination of climate, but of course it may have many uses. This process does not aim at delicacy, but at accumulation of effect. I have not spoken of a standard; the results are only comparative, but the process may be made to supply its own standard.

12. Since writing the above it appears that by using sulphuric acid some of the fears at first entertained may be avoided, as is shown by the following extract:

B sol.	C sol. required after $2\frac{1}{2}$ hours' exposure of A to light.	C sol. required after 50 hours' exposure of A to darkness.
0·2	7·6	0·3
0·5	15·1	0·6
1·0	23·4	0·6
2·0	30·4	0·7
4·0	43·6	0·7
6·0	53·8	1·3

The temperature of the solutions exposed to light =13° C., kept in darkness =22° C. The iodine volatilized by heat was found to be so little that it might be neglected here.

The strength of solutions and the kind of acid to be used may vary. Similar results may be got by using bromide of potassium, but it is less delicate. The surface exposed and other questions require attention.

IV. "On some Elementary Principles in Animal Mechanics. No. X (concluding the series). Further illustrations of the 'Law of Fatigue.'" By the Rev. SAMUEL HAUGHTON, M.D. (Dubl.), D.C.L. (Oxon.), F.R.S., Fellow of Trinity College, Dublin. Received April 19, 1880.

The following experiments were made upon Dr. Alexander Macalister, at intervals from November, 1877, to May, 1879, and consisted in observing the lengths of time during which the extended horizontal arms, with supinated hands holding equal weights, could be held out:—

The following results were obtained, in which w denotes the weight held in the hand in pounds; and t, the time, in seconds, of holding out before fatigue stopped the experiment:

I.

$w=9·75$ lbs.

No.	t.	No.	t.
1	64·0	6	64·0
2	62·0	7	61·0
3	65·0	8	57·0
4	64·0	9	76·0
5	64·0	10	68·0

Mean=64·5 seconds ± 1·1 (mean probable error).

II.

$w=7·75$ lbs.

No.	t.	No.	t.
1	85·0	6	76·0
2	85·0	7	81·0
3	73·0	8	90·0
4	78·0	9	80·0
5	76·0	10	85·0

Mean=80·9 seconds ± 1·1 (mean probable error).

III.

$w=6·87$ lbs.

No.	t.	No.	t.
1	95·0	6	90·0
2	94·0	7	99·0
3	95·0	8	93·0
4	100·0	9	91·0
5	108·0	10	90·0

Mean=95·5 seconds ± 1·2 (mean probable error).

IV.

$w=5·87$ lbs.

No.	t.	No.	t.
1	110·0	6	110·0
2	114·0	7	123·0
3	117·0	8	115·0
4	117·0	9	108·0
5	124·0	10	106·0

Mean=114·4 seconds ± 1·3 (mean probable error).

V.

$w=4{\cdot}25$ lbs.

No.	t.	No.	t.
1	133·0	6	140·0
2	140·0	7	150·0
3	155·0	8	158·0
4	137·0	9	120·0
5	135·0	10	122·0

Mean=139·0 seconds ± 2·7 (mean probable error).

VI.

$w=3{\cdot}19$ lbs.

No.	t.	No.	t.
1	164·0	6	155·0
2	169·0	7	137·0
3	145·0	8	160·0
4	170·0	9	160·0
5	170·0	10	155·0

Mean=158·5 seconds ± 2·3 (mean probable error).

VII.

$w=2{\cdot}56$ lbs.

No.	t.	No.	t.
1	188·0	6	200·0
2	198·0	7	177·0
3	182·0	8	188·0
4	182·0	9	195·0
5	183·0	10	180·0

Mean=187·3 seconds ± 1·7 (mean probable error).

VIII.

$w=0{\cdot}00$ lbs.

No.	t.	No.	t.
1	450·0	6	350·0
2	453·0	7	544·0
3	448·0	8	510·0
4	485·0	9	565·0
5	462·0	10	544·0

Mean=481·1 seconds ± 13·4 (mean probable error).

If we convert the mean probable errors, in seconds, of the foregoing experiments into mean probable errors in percentages, we obtain the following results:—

w.		Mean probable error.
9·75 lbs.	....	±1·71 per cent.
7·75 ,,	....	±1·36 ,,
6·87 ,,	....	±1·26 ,,
5·87 ,,	....	±1·13 ,,
4·25 ,,	....	±1·94 ,,
3·19 ,,	....	±1·48 ,,
2·56 ,,	....	±0·91 ,,
0·00 ,,	....	±2·78 ,,

This table shows the degree of credit attaching to the several groups of observations, and proves (what I did not anticipate) that the fatigue produced by small weights is as definite as that produced by large weights.

The interpretation of these observations by means of the Law of Fatigue is thus found.

When the arm is extended horizontally, the muscles of the shoulder joint keep it in position; and, at the same time, the scapula and its muscles and surroundings are raised by the reaction; so that the arm and scapula equilibrate by means of equal and opposite couples acting round the centre of the glenoid cavity. If, therefore, w denote the weight held in the hand, and α denote the weight, which, if held in the hand, would represent the sum of the couples of the arm and scapula and surroundings about the centre of the glenoid; then it is plain that the total *work* done by the muscles before fatigue stops the experiment must be proportional to $(w+\alpha)t$, where t is the whole time of the experiment. The *rate of work* is evidently proportional to

$$w+\alpha$$

Therefore, by the *Law of Fatigue* we have

$$(w+\alpha)^2 t = \mathrm{A} \quad \ldots \text{(1)}$$

in which α, A are unknown constants to be determined by experiment.

From the foregoing experiments, we have the eight following equations:—

$$\text{(2).}$$

$$(\alpha+9{\cdot}75)^2 \times 64{\cdot}5 = \mathrm{A}.$$
$$(\alpha+7{\cdot}75)^2 \times 80{\cdot}9 = \mathrm{A}.$$
$$(\alpha+6{\cdot}87)^2 \times 95{\cdot}5 = \mathrm{A}.$$
$$(\alpha+5{\cdot}87)^2 \times 114{\cdot}4 = \mathrm{A}.$$
$$(\alpha+4{\cdot}25)^2 \times 139{\cdot}0 = \mathrm{A}.$$
$$(\alpha+3{\cdot}19)^2 \times 158{\cdot}5 = \mathrm{A}.$$
$$(\alpha+2{\cdot}56)^2 \times 187{\cdot}3 = \mathrm{A}.$$
$$(\alpha+0{\cdot}00)^2 \times 481{\cdot}1 = \mathrm{A}.$$

From the discussion of these equations I find the following results, as in Note IX :—

α.	$\dfrac{\delta\sqrt{A}}{\Sigma(\sqrt{A})}$.
5·0	1·66 per cent.
5·5	1·49 ,,
5·6	1·40 ,,
5·7	1·39 ,,
5·8	1·41 ,,
5·9	1·50 ,,
6·0	1·60 ,,
7·0	2·31 ,,

From this, I find, by interpolation, the most probable values of α and $\sqrt{A}$ to be

$$\alpha = 5{\cdot}68 \text{ lbs.}$$

$$\sqrt{A} = 120{\cdot}31 \text{ ,,}$$

$$A = 14{,}475 \text{ ,,}$$

$$\frac{\delta\sqrt{A}}{\Sigma(\sqrt{A})} = 1{\cdot}31 \text{ per cent.}$$

From these values I obtain the following comparison of theory with observation :—

Weight.	Time of Fatigue.		Difference.	
	Calculated.	Observed.	Seconds.	Per cent.
lbs.	Secs.	Secs.		
9·75	64·5	60·8	+ 3.7	+ 5·73
7·75	80·9	80·2	+ 0·7	+ 0·87
6·87	95·5	91·9	+ 3·6	+ 3·76
5·87	114·4	108·5	+ 5·9	+ 5·41
4·25	139·0	146·8	− 7·8	− 5·31
3·19	158·5	184·0	−25·5	−13·80
2·56	187·3	213·2	−25·9	−12·10
0·00	481·1	448·7	+32·4	+ 7·21

The maximum useful work will take place when wt is a maximum, or $wt = \dfrac{Aw}{(w+\alpha)^2}$ which will be a maximum when $w=\alpha$, and the *maximum useful work* will be, if τ be the time corresponding to $w = \alpha = \dfrac{A}{4\alpha}$, $wt = \alpha\tau$, and $\tau = 112{\cdot}16$ seconds.

It appears to be highly probable that, in all cases, the *most useful work* will be done when the resistance to be overcome is equal to the weight of the limb itself, estimated as applied at the same distance from the centre of motion as the resistance or weight.

I attempted, in the following manner, to check the value found for α, the equivalent of the arm and shoulder, estimated at the middle of the hand.

Dr. Macalister, closing his eyes, had his arm suspended to a balance by a string looped round the middle of the hand, and the weight of the suspended arm, allowed to hang "dead," was measured by weights placed in the opposite scale.

After many trials, I found the weight to be 5 lbs. with a possible error of 2 ozs., or $\pm 0{\cdot}125$ either way.

To find the effect of the shoulder, I measured the horizontal distance of the centre of gravity of his scapula from the centre of his glenoid cavity, and found it to be $3\frac{1}{2}$ inches, while the distance of the middle of his hand from the same centre was found to be 22 inches.

The weights lifted with the scapula were thus estimated:—

Muscles.*	ozs.
Supraspinatus	1·85
Infraspinatus	3·51
Teres major	3·30
Teres minor	1·19
Subscapularis	3·88
Deltoideus (half)	5·90
Trapezius (one-third)	2·40
Serratus magnus (two-thirds)	3·92
	25·95 ozs.

To this must be added about one-third more, to represent fat, fascia, tendons, and skin, or 8·65 ozs., making altogether 34·60 ozs.

As these weights were all obtained from dead subjects, we must add somewhat more than one half,† to bring the weights up to the standard of the living subject, which makes altogether over 51·90 ozs.

Adding to this the weight of the average fresh scapula, 4·5 ozs., we have, finally for the total weight applied at the centre of gravity of the scapula, 56·40 ozs., which, when reduced to the corresponding weight held in the palm of the hand, gives $56{\cdot}4 \times \frac{3{\cdot}5}{22} = 8{\cdot}97$ ozs. $= 0{\cdot}56$ lb.

Adding this result to the weight of Dr. Macalister's arm, estimated

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* "Principles of Animal Mechanics," p. 406.

† "Principles of Animal Mechanics," p. 45.

at 22 inches from the centre of the shoulder joint, we obtain for the value of α,

$$\alpha = 5{\cdot}56 \text{ lbs.} \pm 0{\cdot}125 \text{ (probable error).}$$

This result agrees remarkably well with the result $\alpha = 5{\cdot}68$ lbs., obtained by calculations founded on the Law of Fatigue, from the long series of observations detailed in this note.

V. "On the Absence of Potassium in Protagon prepared by Dr. Gamgee." By H. E. Roscoe, LL.D., F.R.S. Received April 29, 1880.

Protagon is stated by Gamgee to contain no potassium. Thudichum, on the other hand, asserts that this body contains from 0·7 to 1·6 per cent. of this metal.

In order to ascertain the quantity of potassium contained in protagon prepared by Gamgee, 5 grams of this material was completely burnt, leaving a residue of 0·177 gram, consisting chiefly of metaphosphoric acid. On precipitating the potassium in the usual way, and on decomposing the platinochloride, a residue of 0·003 gram of platinum was obtained; this corresponds to a percentage of 0·0236 of potassium on the protagon employed.

Spectroscopic experiments, made upon solutions of the chloride and phosphate of potassium, and upon a solution of the latter salt with the addition of the relative quantity of phosphoric acid present in the ash, according to the above analysis, convinced me of the correctness of the conclusions contained in my letter printed on page 113, vol. 30, of the "Proceedings of the Royal Society."

The Society adjourned over Ascension Day to Thursday, May 13.

Presents, April 8, 1880.

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May 13, 1880.

THE 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:—

Allbutt, Thomas Clifford, M.A., M.D., F.L.S.
Attfield, Prof. John, Ph.D., F.C.S.
Blanford, Henry Francis, F.G.S.
Dallinger, Rev. William Henry.
Dyer, William Turner Thiselton, M.A., F.L.S.
Godwin-Austen, Lieut.-Col. Henry Haversham.
Graves, The Right Rev. Charles, D.D., Bishop of Limerick.
Hughes, Prof. David Edward.
Jeffery, Henry M., M.A.
M'Coy, Prof. Frederick, F.G.S.
Moulton, J. Fletcher, M.A.
Niven, Prof. Charles, M.A., F.R.A.S.
Rae, John, LL.D.
Reynolds, Prof. J. Emerson, M.D.
Tilden, William A., D.Sc.

The following Papers were read:—

I. "Notice of further Experimental Researches on the Time-relations of the Excitatory Process in the Ventricle of the Heart of the Frog." By J. BURDON-SANDERSON, M.D., LL.D., F.R.S., Jodrell Professor of Physiology, University College, and F. J. M. PAGE, B.Sc., F.C.S. Received March 31, 1880.

In a previous paper ("Proceedings," vol. xxvii, p. 410) we communicated experimental results relating to the phenomena which present themselves in the ventricle of the heart of the frog, when mechanically or electrically excited. Our present purpose is to report what has been done during the past winter towards the further elucidation of the subject.

By the term "excitatory state" we mean, as in our former communication, nothing more than the condition which is produced in any excitable structure (whether in plants or animals) by excitation. So far as it has been as yet investigated, this state is characterised (1) by the appearance of electromotive properties in the excited part, which did not exist before excitation, and cease to exist as soon as its effect is over; (2) by diminished excitability (showing itself in the suspension or diminution of the power which the structure before possessed of responding to a second excitation); (3) by the fact that it is propagated from the part first excited to contiguous parts at a rate which is different in different structures, and in the same structure at different temperatures. Hitherto the relations of these three facts have been chiefly studied in the excitable tissues of the higher animals. Our investigations lead us to conclude that they are equally characteristic of the excitatory state in plants. We attach great importance to them, as being the only outward and visible signs by which the hidden process of excitation constantly reveals itself. For those more obvious changes which, in the contractile structures, follow excitation, are wanting in those which are merely excitable, and thereby lose their value as characteristics.

It was from the first apparent that the physiological relations of the three kinds of phenomena which admitted of investigation, viz., those of electrical change, of diminished excitability, and of propagation, could only be learnt by the use of exact methods for the determination of the order of occurrence and duration, *i.e.*, of the *time-relations* of each. For this purpose the means were then wanting; the first step was to contrive and construct the instrument, of which the description has just been given. The rheotome was completed in April, 1879, since which time the observations now communicated have been made.

Shortly after the communication of our last experiments, Professor Engelmann made a valuable addition to the researches* previously published by him on the electrical phenomena of the excitatory process in the ventricle of the heart of the frog. The most important of his results and conclusions are as follows:—(1) In 47 out of 78 preparations, investigated with the aid of Bernstein's rheotome, it was found that, when the surface of the separated ventricle ("ventricle-apex preparation") was excited by the passage of a single induction shock between platinum electrodes in contact with its surface and not more than a millimeter apart, the surface being led off, as in our experiments, by two contacts at unequal distances from the seat of excitation, the nearest of the two led-off surfaces (2 millims. distant), became first negative, then positive to the other (5 millims. distant). Thus the variation assumed the character of a "*Doppelschwankung*," *i.e.*, it consisted of two phases of opposite signs. In the remaining 31 instances, all that was observed was that the nearer contact became at once negative, and that its negativity gradually subsided. (2) In 33 experiments in which the same method was employed, the means of the deflections of the galvanometer, corresponding to the first four-tenths of a second after

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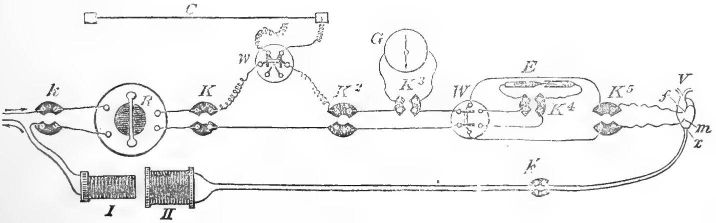
* "Ueber das electrische Verhalten des thätigen Herzens," "Pflüger's Archiv," vol. xvii, p. 68.

excitation, were recorded graphically in a curve of which the abscissæ express the times, the ordinates the deflections. This curve shows that, in the 33 preparations, the first phase began 0″·06 after excitation, and rapidly attained its maximum; that the reversal of sign took place at 0″·26, the contacts becoming finally equipotential at 0″·5. In 10 similar experiments, in which the nearest contact was only 1 millim. from the seat of excitation, the interval of time between the excitation and the beginning of the variation was much shorter. (3) The phenomenon of the "Doppelschwankung" is understood by Engelmann to mean that "each part of the muscular substance of the ventricle becomes, for a moment during excitation, negatively electromotive (*vorübergehend negativ electromotorisch wirksam*), and that this negativity is propagated from the seat of excitation in all directions." The rate of propagation was estimated as probably 50 millims. per second.

It is to be noticed that the researches of Engelmann refer exclusively to the first half-second after excitation, and that he has taken no account of the phenomena which occur later; and, further, that his observations are, as regards this first period, in the main in accordance with the statements contained in our former paper. His interpretation of them is however different, for phenomena to which we apply the term initial are regarded by him as constituting the whole process.

1. *Time-relations of the Excitatory Variation in the Uninjured Heart.*—By the term excitatory variation we mean the succession of electrical changes which manifest themselves galvanometrically as the result of excitation, when the surface of the ventricle is led off at two points. The method of observing these changes is as follows:—The preparation rests on the carefully lacquered surface of the upper end of a brass box of cylindrical form, the axis of which is vertical. Through this box water flows at 10° C. Of the two contacts by which the ventricle is led off, one (*m*) is close to the apex, the other (*f*) near the base, on the ventral aspect. The two platinum wire electrodes, by which the induction shocks pass for excitation, are insulated up to their tips, which are 1 millim. apart. They are in contact with the surface of the ventricle at the apex. The arrangement of the galvano-

FIG. 1.



metric circuit is shown in the woodcut. When the keys K, K^2, K^3, and K^5 are open, and the switch *w* is thrown over to the left, as represented, and the rheotome closed, any current coming from *m* to *f* passes through the compensator (C), the rheotome (R), and the galvanometer (G).

Before making each observation, K is closed, and the slider of the compensator moved until the needle of the galvanometer is at zero: this having been done, K is opened and the rheotome set in motion, when it is found that its revolutions are without effect on the galvanometer.

The binding screws connected with the outer pools of the rheotome are connected with the key *k*, from one of the blocks of which a wire passes through the primary coil of the induction apparatus, and then through a second apparatus for the purpose of short-circuiting the closing induction currents, to one pole of a Grove's battery of two cells, the other wire leading to the opposite pole. The terminals of the secondary coil of the induction apparatus are connected with the exciting electrodes in such a way that the rotations of the rheotome, each of which makes and breaks the primary current of the induction apparatus, are without effect on the preparation, excepting when the key k' is opened for the purpose of making an observation.

The rate of revolution of the rheotome which is best adapted for the purpose is $\frac{1}{3}$ revolution per second. The period to be investigated usually lasts two seconds after each excitation. This period is conveniently divided into 20 periods of 0″·1 each. The "closing time" of the rheotome, *i.e.*, the time during which it closes the galvanometric circuit each revolution, is 0″·1.

As the purpose of the observation is to investigate the condition of the led-off surfaces during each successive period, twenty observations must be made, for each of which the rheotome must be set, so that its closing time coincides with one of these 0″·1 periods. This is effected with the aid of a table, one column of which shows the periods, the other the reading of the rheotome scale which corresponds to it. As each observation requires three-quarters of a minute to make and record it, and must be repeated to ensure accuracy, the investigation of a variation cannot be completed in less than a quarter of an hour. The following example will serve to illustrate the form which the results assume:—

Table I.

Time after excitation, at which galvanometric circuit was opened, the period of closure being 0″·1......	0″·1	0″·2	0″·3	0″·4	0″·5	0″·6	0″·7	0″·8	0″·9	1″·0	1″·1	1″·2	1″·3	1″·4	1″·5	1″·6	1″·7	1″·8	1″·9	2″·0
Deflections	−42	−3	0	0	0	0	0	0	0	0	0	0	0	0	0	+3	+30	+12	+1	0

If it is desired to investigate more minutely a part of the variation, as for example, the changes which take place in the first four-tenths of a second, a table is used in which the rheotome readings for twenty periods ending respectively at 0″·02, 0″·04, 0″·06 and so on, after excitation, are recorded. The following table exemplifies the kind of result so obtained, the period of closure being 0″·1.

Table II.

Time after excitation of opening of galvanometer circuit...............	0″·02	0″·04	0″·06	0″·08	0″·10	0″·12	0″·14	0″·16	0″·18	0″·20	0″·22	0″·24	0″·26	0″·28	0″·30 &c.
Deflections	−0	−1	−12	−31	−42	−46	−40	−32	−12	−7	−4	0	0	0	0 &c.

These readings show that in the particular preparation investigated (which may be taken as a representative one) the state of negativity of the contact *m* did not last more than 0″·06, and culminated before a tenth of a second had elapsed after excitation.

Whenever the surface of the ventricle is led off in the way above described, *i.e.*, by contacts of which one is close to the seat of excitation, the other remote from it, the same results are obtained. We are therefore justified, first, in repeating the statement contained in our last paper, viz., that the electrical effect of excitation manifests itself in two phases, an initial and a terminal one, which have opposite signs; and, secondly, in adding to our previous statement, that the initial is separated from the terminal phase, by a relatively prolonged state of equipotentiality of the two contacts. If we compare this result with that of Professor Engelmann, observed in a similar way, it will be at once seen that although the interpretation may be different, our observations are in accordance with his, so far as they relate to the same period. The whole of the phenomena recorded by him belong to the beginning of the 1st second. The beginning of the period of equipotentiality is regarded by him as the end of the excitatory effect. To us the absence of galvanometric effect during this period, which we designate the isoelectrical interval, is merely the expression of the fact that both contacts are in the same degree of excitation.

2. *Time-relations of the Variation in the partially warmed Ventricle.*—The proof that the period of equipotentiality is one of balanced activities, not of quiescence, is readily obtained by subjecting the two led-off surfaces, *i.e.*, the base and apex of the heart, to different temperatures. For this purpose the cylindrical support on which the preparation rests is divided into two chambers through which water flows at different temperatures by a vertical non-conducting septum. If, then, the ventricle is placed in such a position that its apex rests

on the cooler surface, and a series of rheotome observations are made during the 2nd second, it is observed that the deflections of the terminal phase are diminished. In the contrary case they are increased and begin at an earlier period; so that, at a time at which the surfaces *m* and *f* were before isoelectrical, the surface *m* is now positive to *f*. To produce these effects a difference between the temperatures of the chambers of only 3° is sufficient. That which exists between the led-off surfaces is obviously much less.

If, in the manner described in our previous paper, the surface of the ventricle is temporarily warmed by the passage for a second or two of a voltaic current through a platinum wire fixed in the neighbourhood of one of the contacts—say the contact *m*—similar but much more considerable effects are produced, which, when observed with the aid of the rheotome, exhibit the characters shown in the table.

Table III.

Time after excitation, at which the galvanometer circuit was closed.	0″·2	0″·4	0″·6	0″·8	1″·0	1″·2	1″·4	1″·6	1″·8	2″·0
Preliminary observation before warming	−30	0	0	0	0	+ 5	+23	+20	+5	0
Immediately after warming	−24	+17	+30	+71	+95	+99	+63	+ 9	+5	0
12 seconds later	−25	+5	+ 2	+ 9	+25	+55	+52	+ 6	+2	0
24 seconds later	−29	0	0	0	+ 6	+14	+50	+ 9	+2	0
36 seconds later	...	0	0	0	+ 2	+ 7	+34	+14	+5	0
48 seconds later	...	0	0	0	0	+ 5	+24	+16	0	0

The table represents, first, the results of a series of preliminary observations, showing the electrical state of the surfaces during ten successive periods after excitation when their temperatures were equal; and, secondly, ten series of observations relating to the same ten periods, showing the condition of the same surfaces at various intervals of time after warming. We learn from it (1) that in the unexcited state the revolution of the rheotome, *i.e.*, the closure of the galvanometer circuit, is without effect on the needle, whether the temperatures of the two surfaces are equal or not; (2) that in the excited state the needle remains unaffected during the isoelectrical interval only in case the temperatures are equal. If they are unequal the warmer of the two tends to become positive to the cooler, the electrical difference between them increasing and diminishing with the difference of temperature. It is to be noticed that this effect manifests itself first at the end of the isoelectrical interval, over which it gradually extends as the difference of temperature increases.

3. *Time-relations of the Variation in the Injured Ventricle.*—In our former paper we gave an account of the influence of slight injuries of

the surface in modifying the phenomena of the excitatory variation. The rheotome has enabled us to add very considerably to our knowledge of these modifications. The effect of all injuries, whether chemical or mechanical, in so far resemble each other, that in all cases the injured surface tends during the whole of the excitatory period to become positive to the uninjured. The degree in which this effect manifests itself varies in the same direction with the degree of injury. If the injury is very slight, such for example as is produced by touching the surface with a camel-hair pencil, which has been dipped in 10 per cent. solution of salt, and then drained with filter-paper, the change which follows resembles that of slight warming. The end phase begins earlier, and its deflections are increased. If it is produced by approaching the platinum loop a little too near, the effect is similar, but begins earlier and lasts longer, so that the isoelectrical interval is much reduced in duration. If the led-off surface (*m*) is completely destroyed, as by touching it instantaneously with the hot wire after having removed the electrode, and then renewing the contact, it is found that the whole isoelectrical interval is, as it were, filled up. large positive* deflections being obtained with the rheotome throughout the whole excitatory period, with the exception of the first tenth. This is well seen in the following table, in which we have given the means of the deflections recorded when the galvanometric circuit was closed for ten successive periods of two-tenths of a second. By comparing these numbers with those given in Table III, it will be seen that whereas, in the partially warmed ventricle, the temporary effect produced culminates early in the 2nd second, the more lasting effect produced in the injured ventricle reaches its maximum about the middle of the 1st second.

Table IV.

Time after excitation of opening of galvanometer circuit, the period of closure being 0″·18	0″·2	0″·4	0″·6	0″·8	1″·0	1″·2	1″·4	1″·6	1″·8	2″·0
Deflection	+58·6	+85·3	+82·3	+75·0	+68·3	+62·3	+50·0	+31·8	+21·3	+4·1

4. *The Period of Diminished Excitability.*—Since the researches of M. Marey,† in 1876, it has been known that the excitability of the ventricle is diminished during the period of systole, Marey showed that if the ventricle is excited by an induction shock of just sufficient strength to evoke a response, the excitation is followed by a period of

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* Whenever the words "positive" or "negative" are used absolutely, the state of *m* in relation to *f* is meant.

† Marey, "Des Excitations Artificielles du Cœur." "Physiol. Expérimentale," II, 1876, p. 85.

diminished excitability, during which it will not respond to a shock of the same intensity. This he termed the "refractory period;" he further found that if the induction shocks were strengthened, a second response could be obtained after a shorter interval, but that in this case the interval between excitation and response was increased.

For the purpose of obtaining information as to the time-relations of these phenomena, we adopt the following method:—For one of the wires leading from the battery to the primary coil of the du Bois induction apparatus, two are substituted, of which one passes through the central, the other through the peripheral pools of the rheotome. The primary circuit is thereby closed, and immediately afterwards opened twice in each revolution; the intervals between the two excitations can be varied at will. The closing shocks being cut off in the way before mentioned, the rheotome is set so that the two opening shocks follow each other at an interval of two seconds. If the ventricle is at 10°C., and the distance of the secondary from the primary coil such that the excitation is just sufficiently strong to produce a contraction, it is usually followed by a second. If the interval between the two excitations is shortened by the tenth of a second, the second excitation is fruitless. If now the temperature of the preparation is increased by a single degree, a response occurs which in its turn is abolished by taking another tenth off the time interval, and so on. Proceeding in this way we obtain the clearest possible demonstration of two facts, namely, (1) that the duration of the period of diminished excitability agrees pretty closely with that of electrical activity, and (2) that it is similarly affected by changes of temperature.

5. *Rate of Propagation of the Excitatory Wave.*—It has already been stated that the rate of propagation of the "Reizwelle," *i.e.*, of the excitatory electrical disturbance, was estimated by Engelmann to be 50 millims. per second. His observations, indeed, give a less rate than this, but for good reasons, which he has stated, he thinks that they require correction. We have been able by the following method to show that the estimate is considerably below the mark. The heart is led off as usual at the base (f) and near the apex (m), and excited at the apex (x), and care is taken that the contacts x, m, and f are in the same straight line. A third non-polarisable electrode is in contact with the surface at m' between m and f, and 4 millims. from m.* The time after excitation, at which the first effect, *i.e.*, the first negative deflection is observed, is determined at m and m', by rheotome observations, in which the galvanometer circuit passes through m and m' alternately (for which purpose a switch is used).

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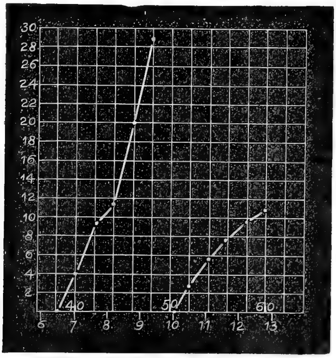
* These measurements could not have been carried out had we not possessed a few gigantic specimens of *R. esculenta*, from the Platten See in Hungary. These we owed to the kindness of Professor Goltz.

Two parallel series of results are obtained, which when set off as ordinates on the same time-axis exhibit the characters shown in the figure. Our results show that in a fresh preparation at 12°, the excitatory wave is propagated at a rate of about 130 millims. per second. It will be noticed that the rapidity with which the electromotive force of the initial phase attains its maximum, renders it possible to make this observation with great exactitude.

FIG. 2.



The figures above the line denote degrees of the scale of the rheotome; those below, 100ths of a second measured from the moment of excitation; those in the vertical column degrees of the scale of the galvanometer. The ascending line to the left shows the deflections observed when the galvanometer circuit was opened at 38, 40, 42, and so on of the scale of the rheotome, the preparation being led off at *m* and *f*. The similar line to the right shows the corresponding deflections at 50, 52, and so on, when *m′* was substituted for *m* and the excitation wave had 4 millims. further to go. The records represent the means of two observations taken as soon as possible after each other. It is reckoned that in these instances the 4 millims. were accomplished in the time corresponding to 11 divisions of the rheotome scale, *i.e.*, in 0·032 second.

The facts which have been stated in the preceding paragraphs are consistent with the following theories:—

1. Every excited part is negative to every unexcited part, so long as the state of excitation lasts. 2. The local duration (*örtliche Dauer*, Engelmann) of the excitatory state, *i.e.*, the time it lasts in each structural element, is measured by the time-interval between the beginning of the initial and the beginning of the terminal phase of the variation.

As regards the second of these propositions, we are in disagreement with Professor Engelmann, who attributes to the excitatory process in the ventricle of the frog's heart a duration of only two-tenths of a second. We conclude, from our own observations, that its duration is nearly two seconds at the temperature at which most of our experiments were made, namely, at 10° or 12° C. This conclusion is, indeed, the only one which the facts admit of, for, inasmuch as the total duration of the variation is over two seconds, and the time occupied in the propagation of the excitatory wave from apex to base is one-twentieth of a second at most, it is clear that it must last, at the part furthest from the seat of excitation, as long as $1''\cdot9$.

When both contacts are at the same temperature, and in all other respects under the same conditions, the local duration of the excitatory state is the same at both, consequently, it begins and ends earlier at *m* than at *f*, the initial and terminal differences expressing themselves in the initial and terminal phases of the normal variation.

When one contact is warmer than the other, the local duration of the excitatory state is less in the warmed than in the unwarmed surface. Consequently the terminal interval, *i.e.*, the time during which the excitatory state at *f* overlaps that at *m*, is longer. The terminal phase of the variation begins earlier, and its deflections are larger.

If the surface at *m* is slightly injured, the local duration at the injured surface is diminished in the same way as when its temperature is increased. The effects, indeed, are indistinguishable; but if the injury is of such intensity as to destroy the surface, its most prominent effect is to diminish its electromotive activity. Consequently, the activity of the contact *f* is unbalanced, and expresses itself in the large deflections which are exemplified in Table III.

In general, it is observed that in the injured ventricle the total duration of the variation is less than in the same preparation before injury. We are not as yet in a position to offer an explanation of this fact.

APPENDIX.

Condition of the Surface of the Ventricle in the Resting Heart.—It has already been repeatedly stated that the surface of the ventricle, so long as it is absolutely uninjured and at the same temperature in all parts, is isoelectrical. Any inequality as regards the mechanical, chemical, or thermal conditions to which the surface is exposed, is accompanied by corresponding electrical differences, which are of the following nature :—

If a platinum wire, warmed by the passage along it of a voltaic current, is brought into the neighbourhood of a led-off surface, the effect invariably produced is to render that surface positive to the other. If the warming is continued for a few seconds only, the effect promptly subsides. If it is continued, the needle also returns to zero, but continues to move to the negative side. If, as soon as this second effect has been produced, the wire is withdrawn, it also subsides, but very slowly. The nearer the wire, and the longer it remains hot, the greater the electrical difference produced, and the longer the time required for its subsidence. Corresponding results are obtained by touching the surface, in the manner before described, with a camel-hair pencil, containing solution of common salt. The immediate result is to render the surface so acted upon positive to the other surface. Here, as before, the effect rapidly subsides. If now, the application is repeated or a stronger solution is used, the first effect is the same, but it is followed by a slow deflection of the needle in the opposite direction.

It is important to notice that, in both modes of acting on the surface of the ventricle, the primary effect (that of increased "positivity" of the warmed or slightly injured surface) is, when measured in electromotive force, inconsiderable. After warming, we have never observed a greater difference than 0·003 volt. When the solution of salt is used the effect may amount to as much as 0·001 volt. The subsequent negative effect varies according to the extent and degree of the injury, and may, as stated in our former paper, amount to from two to three hundredths of a volt.

Another question relating to the physiological condition of the resting heart is that of the influence of temperature on its excitability. We have estimated the excitability of the ventricle by measuring, at different temperatures, galvanometrically, the strength of the weakest induction shock by which a response could be evoked, and have arrived at the general result that, when the temperature is raised from 10° C. to 30° C., the excitability increases by one-third or one-sixtieth for each degree. The suggestion offers itself that the two new facts which have been stated, viz., the slight positivity of the warmed surface and the increased excitability are correlated, but it is of no value, excepting as an indication for further experiments.

The full account of the experiments, of which the results are shortly communicated above, will be published in the "Journal of Physiology."

II. "On a New Rheotome." By J. BURDON-SANDERSON, M.D., LL.D., F.R.S., Jodrell Professor of Physiology, University College. Received May 5, 1880.

I beg leave to submit to the Royal Society the following description of an instrument intended for the purpose of investigating the successive phases of the electrical change which takes place in the excitable parts of plants and animals in consequence of excitation. Two instruments intended for a similar purpose are already in use. One of them, known as the differential or repeating rheotome, was devised by Professor Bernstein, and described by him in his work "On the Excitation Process in the Nervous and Muscular Systems," published in 1871.* The other is called by its contriver, Professor Hermann, the Fall Rheotome, and is described in a paper published in "Pflüger's Archiv" in 1877. I have given a short account of it, which will be found in the "Journal of Physiology" (vol. i, p. 196).

In each of these instruments, as also in the instrument now to be described, two independent circuits, one of which may be called the exciting circuit, the other the galvanoscopic, are successively closed and immediately afterwards opened by a mechanism of such a nature that the *time-interval* between the two closures and (as regards the galvanoscopic circuit) the *duration* of the period of closure may be varied according to the purpose of the observation, and accurately measured. In the fall rheotome each of the times required is determined by the measurement of the distance which a falling weight passes through in the interval between the first and second event, as, *e.g.*, between the closure of the exciting and galvanoscopic circuits and between the closure and opening of the latter. In Bernstein's rheotome time is measured in terms of the rotation-time of a wheel, which is driven by an electro-magnetic motor.

Without making any comparison between the instrument now to be described and its predecessors, or venturing to claim for it any superiority, I think I am justified in bringing it under the notice of the Royal Society, on the ground that in very numerous experiments it has been found well adapted for its purpose, and that the value of the results obtained by means of it could not be so well estimated by the reader without a knowledge of the construction and mode of action of the instrument used.

Description of the Instrument.

From a circular iron plate, supported on levelling screws, spring two strong brass pillars, each 6 centims. in height, on the summits of which rests a horizontal bar. In the middle of the bar is a screw, which ends below in a steel point. A similar steel point rises from the centre of the iron plate. On these centres works a vertical axis about 5 centims. in length. The steel point on which the lower end of the axis works is surrounded by a circular plate of vulcanite about ·5 centim. thick. In this plate are cut eight pools, of which the forms are shown in the diagram (fig. 1). They are intended to contain mercury and are traversed by platinum wires, of which the arrangement is such that the pools, *a*, *a*, *a*, *a*, and *b*, *b*, *b*, *b*, are severally connected with each other. The two rings of wire are insulated and are connected the one with the binding-screw, G, the other to the binding-screw, G′.

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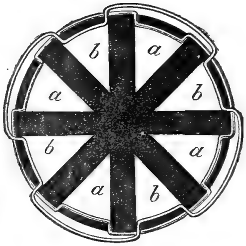
* "Untersuchungen über den Erregungsvorgang im Nerven- und Muskelsysteme," von J. Bernstein. Heidelberg, 1871.

The fixed circular plate above described is surrounded by a moveable annular plate of the same material and thickness, which also has eight excavations or pools, all of which can be brought into communication

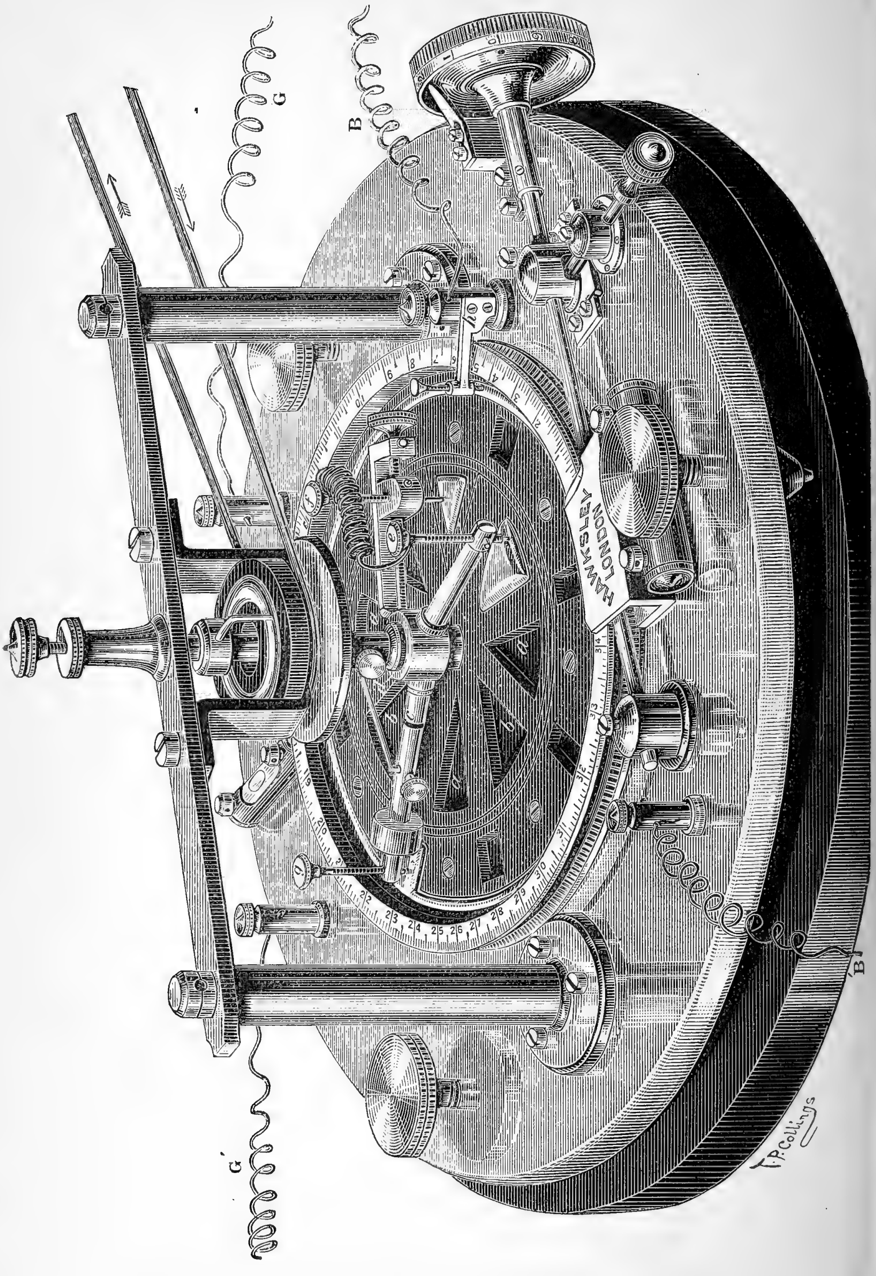
Fig. 1.



by means of an annular trough of mercury by which they are surrounded. To the outer edge of the annular plate is fixed a graduated circle of brass, which revolves with it. Against this circle works a tangent screw, each turn of which corresponds to a single division of the graduation; this screw is furnished with a divided milled head and can be thrown out of gear when necessary by the auxiliary screw, in a manner which will be readily understood from the drawing. The vertical axis carries at its upper end a pulley, by means of which it is connected with a motor, and can be made to revolve at any desired rate. Above the pulley is a platinum wire, the end of which is bent downwards so as to dip into an annular pool of mercury, which is in metallic connexion with the horizontal bar, and thereby with the iron plate and the binding screw B′. The vertical axis also carries three arms, of which one is longer than the other two; the longer arm carries at its end the vertical screw *g*; this screw is prolonged at its lower end by an amalgamated gold wire, the end of which dips into the mercury contained in the external pool, as the axis rotates. This arm is insulated at *c*, but is connected with the axis, and thereby through the annular pool above described with the binding screw B′. As the external pools are connected through the arm *h* with the binding screw B, B and B′ are in communication whenever the screw *g* is in contact with the mercury in any of the pools. Each pool is furnished with a vulcanite stop, by which it is thrown out of communication with the external pool. In the drawing all are thus out of communication excepting one.

The two shorter arms are in metallic connexion with each other, but are insulated from the axis by a collar of vulcanite *d*. One of them carries a screw *e*, which like *g* ends in a gold wire, of which the tip is amalgamated; the other carries a similar screw *f*, and is so constructed that the screw *f* can be moved radially (*i.e.*, in a vertical

FIG. 2



plane which contains the axis of rotation) to any required distance from the axis. The two arms meet each other at an angle of 45°.

From the form and arrangement of the internal pools (see fig. 1) it is evident that whenever the screw *e* is in contact with any of the pools *b*, *b*, *b*, *b*, the screw *f* must be in contact with the pools *a*, *a*, *a*, *a* (see fig. 2), and further inasmuch as *f* is nearer the axis than *e*, the times at which *e* makes and breaks contact with the pool *b* must respectively precede and follow those at which contact with the pool *a* is made and broken by *f*. Consequently the time during which both are in contact (*i.e.*, during which the circuit G G′ is closed) is determined solely by the duration of the immersion of *f*, and can be varied by altering its distance from the centre.

When it is desired to use the rheotome for investigating the effects of series of excitations recurring at short intervals (tetanus of muscle or nerve), all the pools are equally filled with mercury.

Their equality must be tested by interpolating each set of pools in the circuit of a Grove's cell, which also includes a recording chronograph; if the chronographic record shows that the durations of the contacts are not perfectly equal, the error must be corrected by adding or subtracting mercury. For investigating the effects of single excitations, only one of the external and one couple of the internal pools are used.

The drawing (fig. 2) shows the arrangement of the apparatus as used in the investigations made by Mr. Page and myself of the electrical phenomena of the ventricle of the heart of the frog. The instrument is set so that the external contact *g* is broken at a moment which immediately precedes the immersion of *f*: consequently the galvanometer circuit *G G′* is open at the moment of excitation, but closed immediately after, remaining closed so long as *f* is immersed. By means of the tangent screw, the interval between the opening of the exciting circuit *B B′*, and the closing of the galvanometer circuit *G G′*, can be varied at will.

III. "On the Chemical Composition of Aleurone-Grains." By S. H. Vines, M.A., D.Sc., Fellow of Christ's College, Cambridge. Communicated by Michael Foster, M.D., Prælector of Physiology in Trinity College, Cambridge. Received April 1, 1880.

In a former communication ("Proc. Roy. Soc.," vol. 28, p. 218) I gave an account of an investigation of the aleurone-grains of the blue lupin. The following is an abstract of the results arrived at by the investigation of the grains of some other seeds by the same methods:—

II. *The Aleurone-Grains of the Peony* (Pæonia officinalis).

a. Microscopical Observations.—In accordance with the researches of Pfeffer ("Jahrb. f. wiss. Bot.," viii, 1872), the grains were found to be readily soluble in distilled water, the only proteid substance remaining in the cells being the matrix, which, like that of the lupin, is insoluble in dilute acids and alkalies. They also dissolve in 10 per cent. and in saturated NaCl solutions. Their solubility is unaffected by previous treatment with either ether or alcohol.

b. Chemical Observations.—The watery extract of the seeds (which has only a very faint acid reaction if a relatively large quantity of water is used) becomes turbid on boiling, and it gives all the reactions of a fluid holding proteid substance in solution. If it be boiled, evaporated to small bulk, and then filtered into alcohol, a dense precipitate is formed. The substance precipitated readily dissolves in distilled water, and the solution gives the reactions characteristic of a solution of peptones. The substance does not dialyse. It is probable, therefore, that it is identical with the soluble proteid detected in the seeds of the lupin, which I provisionally termed Hemialbumose.

This substance is readily precipitated from its watery solution by the addition of a small quantity of acetic acid. The precipitate differs, however, from the original substance in that it is only slightly soluble in distilled water: it is more soluble in solutions of alkaline carbonates, and it dissolves completely in dilute solutions of the alkalies. In this respect it resembles acid-albumin (syntonin).

If the seeds, after extraction with water until the filtrate gives only a very faint xanthoproteic reaction, be treated with 10 per cent. NaCl solution, a fluid is obtained which gives a dense precipitate on boiling, a precipitate on dilution with water, and on saturation with NaCl.

Further, if the seeds be extracted with saturated NaCl or $MgSO_4$ solution, a fluid is obtained which gives a slight turbidity on boiling, but no perceptible turbidity on dilution.

From these observations, it appears that the aleurone-grains of the peony consist of one proteid soluble in water (hemialbumose), and of another, insoluble in distilled water, but soluble in 10 per cent. NaCl solution, and precipitable from its solution by boiling or by saturation. This latter body corresponds in these respects with the substance found in the seeds of the lupin, and termed vegetable myosin.

It seems probable that the grains of the peony contain no substance (or, at most, a very small quantity of it) analogous to the vegetable vitellin, which occurs in the grains of the lupin, and which is soluble in saturated NaCl solution.

III. *The Aleurone-Grains of the Castor Oil Plant* (Ricinus communis).

a. Microscopical Observations.—When mounted in alcohol, the grains are seen to be ovoid bodies which present no indication of a complex structure; occasionally a rounded mass may be distinguished at the more pointed end.

On the addition of water, the whole grain increases in size and becomes clearer, in consequence, apparently, of the solution of a part of its substance. It is then seen to consist of the rounded mass before mentioned, the globoid, and of a large crystalline body, the crystalloid, which are imbedded in a more or less spherical mass of proteid ground-substance.

The action of water is not affected by previous treatment of the grains with either alcohol or ether. The following results show that the solvent action of NaCl and $MgSO_4$ solutions is profoundly modified by such treatment.

1. Grains treated with ether:—

a. The ground-substance dissolves readily in 10 per cent. NaCl solution, and partially in saturated NaCl or $MgSO_4$ solution.

β. The crystalloid dissolves very slowly in 10 per cent. NaCl solution, but it is quite insoluble in saturated NaCl or $MgSO_4$ solution.

γ. The whole grain dissolves readily in 20 per cent. NaCl or $MgSO_4$ solution.

2. Grains treated with alcohol (or with alcohol and ether):—

a. The ground-substance dissolves readily in 10 per cent. NaCl solution, and partially in saturated NaCl or $MgSO_4$ solution.

β. The crystalloid dissolves slowly in 10 per cent. NaCl solution, but entirely and at once in saturated NaCl or $MgSO_4$ solution. If the grains, after treatment with alcohol, be washed with water, the crystalloids lose their solubility in saturated NaCl or $MgSO_4$ solution, but regain it on further treatment with alcohol.

γ. Both the ground-substance and the crystalloid dissolve very readily in 20 per cent. NaCl and $MgSO_4$ solutions; the crystalloid dissolves more readily after treatment with alcohol than it does after treatment with ether only.

b. Chemical Observations.—If crushed seeds be treated with water, after the oil has been extracted from them by alcohol or ether, a fluid is obtained which gives a precipitate on boiling. The filtrate, after evaporation to small bulk, gives a precipitate on being poured into alcohol. The substance precipitated is readily soluble in distilled water; the solution gives the reactions characteristic of a fluid holding peptone in solution. The substance does not dialyse. In these particulars it resembles the substances found in the seeds of the lupin and of the peony, and it may therefore be also termed hemialbumose. It is probably this substance which is seen to be dissolved when the grains are treated with water under the microscope.

1. Seeds treated with ether:—

a. When extracted with 10 per cent. NaCl solution, a fluid is obtained which gives a precipitate on boiling, on dilution with water, and on saturation with NaCl; the supernatant saturated NaCl solution also gives a precipitate on boiling.

β. When extracted with saturated NaCl solution, a fluid is obtained which gives a precipitate on boiling and on dilution.

2. Seeds treated with alcohol (or with ether and alcohol):—

a. Extracted with 10 per cent. NaCl solution, a fluid is obtained which gives a precipitate on boiling, on dilution, and on saturation; the supernatant saturated fluid gives a precipitate on boiling and on dilution.

β. Extracted with saturated NaCl solution, a fluid is obtained which gives a precipitate on boiling and on dilution.

From these observations it appears that the ground-substance consists of hemialbumose and of two globulins, of which one is soluble and the other insoluble in saturated solution of NaCl or $MgSO_4$. The crystalloid consists of a globulin, which is soluble in 10 and 20 per cent. NaCl solutions, but insoluble in saturated NaCl solution; after treatment with alcohol it becomes soluble in saturated NaCl and $MgSO_4$ solutions.

Additional Remarks on the Aleurone-Grains of the Blue Lupin.

After the foregoing results had been obtained, it became necessary to complete the investigation of the grains of the lupin by ascertaining the action upon them of saturated NaCl and $MgSO_4$ solutions.

I found that the grains dissolved readily in both these fluids, and that their solubility was not affected by treatment with alcohol. The solution, in quantity, gives a precipitate on boiling and on dilution.

General Remarks.

The three kinds of aleurone-grains which have been investigated represent three degrees of complexity of composition. In the peony, the whole grain dissolves readily in water; in the lupin, the grain dissolves only partly in water, the residue being readily soluble in 10 per cent. NaCl solution; in Ricinus there is a morphological as well as a chemical differentiation; the ground-substance dissolves partly in water and partly in 10 per cent. NaCl solution, and in this respect it resembles the entire grain of the lupin, whereas the crystalloid dissolves very slowly in 10 per cent. NaCl solution.

It is by no means easy to give a satisfactory explanation of the entire solubility of the grains of the peony in distilled water, and of those of the lupin in saturated NaCl or $MgSO_4$ solution. In the grains of the peony the relative proportion of globulin to hemialbumose is apparently small, and it may perhaps be assumed that the neutral salts in the cells suffice to bring the globulin into temporary solution, so as to cause the entire disintegration of the grains. As regards the lupin, it is clear that the globulin of which the grains principally consist is a vitellin, and possibly the formation of a precipitate which takes place during the saturation of the 10 per cent. NaCl solution may be attributed to a conversion of a part at least of the vitellin into a myosin.

In the course of my observations it became evident that alcohol, contrary to the generally received opinion, does not render the vegetable globulins insoluble in solutions of neutral salts.* All the above-mentioned reactions are given by aleurone-grains which have been in alcohol for a very considerable time. The grains of Ricinus, for example, after having been in absolute alcohol for more than a year, give the same reactions as fresh ones. This is true of the globulins, not only whilst they exist in the form of aleurone-grains, but after their extraction; thus, the precipitate obtained by diluting a 10 per cent. NaCl extract of Ricinus seeds was quite soluble in 10 per cent. NaCl solution after having been for a month in alcohol.

The action of alcohol upon the crystalloids of Ricinus is remarkable in that it renders them readily soluble in saturated NaCl or $MgSO_4$ solution. That they are otherwise quite insoluble in these fluids is proved by the fact that I have kept quantities of crystalloids in excess of the saturated solutions for months; if some of these be treated for a few minutes with alcohol, they can be seen under the microscope to dissolve at once in these solutions. It is of interest to note that if, after treatment with alcohol, the crystalloids be washed with water, they lose their solubility in these saturated salt solutions, and that they only regain it after being again treated with alcohol. Alcohol appears, in this case, to convert a substance which is insoluble in saturated salt solutions into one which is soluble, that is, a myosin substance into a vitellin substance; and, from the foregoing facts, it seems that this conversion is closely connected with the removal of water from the substance.

These observations led me to investigate the crystalloids of a variety of plants with these reagents, and by the kindness of Dr. A. F. W. Schimper, of Strassburg, who has closely studied the subject ("Unters. üb. die Proteinkrystalloide," 1879), I was enabled to experiment upon the crystalloids which Professor Drechsel has succeeded in producing artificially from the proteids in the seeds of Bertholletia (Brazil nut) and of the pumpkin (Cucurbita) ("Journal f. Prakt. Chem.," 1879).

The results may be briefly stated as follows:—

A. Crystalloids insoluble in NaCl or $MgSO_4$ solution; *Musa Hillii*, *Musa Ensete*.

B. Partially soluble in NaCl or $MgSO_4$ solution; *Sparganium ramosum*.

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* Radlkofer ("Ueb. Krystalle proteinartiger Körper," 1859) has already pointed out that alcohol does not cause the coagulation of the crystalloids of Ricinus, of Bertholletia, and of *Sparganium ramosum*.

C. Crystalloids entirely soluble in NaCl or $MgSO_4$ solution:—

a. Readily soluble in both 10 per cent. and saturated solutions, Bertholletia, pumpkin (Cucurbita), artificial crystals.

b. Slowly soluble in 10 per cent. solutions, more readily soluble in 20 per cent. solutions, soluble in saturated solutions only after treatment with alcohol, *Ricinus communis*, *Viola elatior*, *Linum usitatissimum.*

The crystalloids of *Musa Hillii* and of *Musa Ensete* (for this material also I am indebted to Dr. Schimper) are remarkable for their insolubility. They swell-up slightly, but do not dissolve in solutions of neutral salts of various degrees of concentration and in 1 per cent. Na_2CO_3 solution. They swell-up considerably, but do not dissolve in dilute HCl. They swell-up and dissolve partially in dilute KHO. They therefore, probably consist of some relatively insoluble albuminate.

When treated with 10 or 20 per cent. solution of NaCl or $MgSO_4$, the crystalloids of *Sparganium ramosum* swell-up considerably, and the central portion dissolves, leaving the external portion of the crystalloid as a thick-walled vesicle; on the addition of water, a granular precipitate is thrown down inside the vesicle. The process of solution is the same when the crystalloids are treated with 1 per cent. Na_2CO_3 solution, but dilution with water does not, in this case, produce a precipitate inside the vesicle. The central portion of the crystalloid dissolves also when they are treated with saturated NaCl solution, but the swelling-up is less considerable. The vesicle dissolves readily in dilute KHO, and in dilute (·4 per cent.) HCl. These crystalloids, therefore, consist of two substances, a vitellin, forming the central mass, and an albuminate, forming the outer portion. This albuminate may be probably regarded as having been produced by an alteration of the globulin of which the whole crystalloid doubtless consisted originally.

It appears that no definite relation exists between the crystalline form and the solubility of the crystalloids in solutions of neutral salts. According to Schimper (*loc. cit.*) all the crystalloids which I have examined belong to two systems, the regular and the hexagonal.

1. Hexagonal rhombohedra: crystalloids of Musa, Sparganium, Bertholletia, and the artificial crystals obtained from Bertholletia.

2. Regular tetrahedra: crystalloids of Ricinus, Viola, Linum, Cucurbita, and the artificial crystals obtained from Cucurbita.

In all cases I found the aleurone-grain to be invested by a peripheral layer (*Hüllmembran*), which is apparently insoluble, and which, as Pfeffer suggests, contributes to form the proteid network or matrix which remains in the cells after the more soluble portions of the grains have been dissolved out.

Whenever saturated solutions of NaCl were used, or when saturation with NaCl was necessary, for the purpose of separating myosin from vitellin, I verified the results by using $MgSO_4$, in accordance with the researches of Hammarsten ("Ueb. das Paraglobulin," "Pflüger's Archiv," 1878).

In conclusion, I would add a few remarks to those which I made in my former communication with reference to the relation between the globulins and the hemialbumose existing in seeds and the various bodies, such as conglutin, legumin, &c., which Ritthausen has extracted from them. I therein expressed my concurrence with Weyl's opinion that these caseins are the products of the alteration of the globulins effected by the alkaline solutions used in extracting them. I still maintain this view, but I think now that it is only a partial explanation. My observations, more particularly those on the peony, make it clear that a considerable proportion of these caseins is hemialbumose precipitated by the dilute acetic acid which is used in Ritthausen's method for throwing down the caseins from the alkaline extracts.

IV. "Some Observations upon the Hydrolytic Ferments of the Pancreas and Small Intestine." By HORACE T. BROWN, F.I.C., F.C.S., and JOHN HERON, F.C.S. Communicated by Dr. W. ROBERTS, F.R.S. Received April 15, 1880.

We were requested a few months ago by Dr. W. Roberts to verify a statement, recently made by Musculus and De Méring, that *maltose* is a product of the action of an aqueous extract of pancreas upon starch-paste. During the prosecution of the inquiry, and while following up certain lines of experiment which suggested themselves from time to time, we have, besides fully confirming the results of the above-mentioned observers, ascertained certain facts which we believe are of some physiological importance in elucidating the still very obscure processes of animal digestion and nutrition.

I. *Hydrolytic Action of the Pancreas.*

The first observation upon the amylolytic action of the pancreatic secretion appears to have been made by Bouchardat and Sandras* in the year 1845. The general functions of the gland were more fully studied in 1856 by Claude Bernard,† and a few years later by Cohnheim ("Virchow's Archiv," 28, 241, 1863). Danilewski‡ in 1862, and Hüfner ("Journ. f. Prakt. Chem." [2], 5, 1872, 396), ten years later,

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* "Des Fonctions du Pancréas, et de son Influence dans la Digestion des Féculents." "Compt. Rend.," 20, 1085.

† "Mémoire sur le Pancréas." 1856. "Leçons de Physiologie Expérimentale." Paris, 1856.

‡ "Ueber specifisch wirkende Körper des Natürlichen und Künstlichen Pancreatischen Saftes." "Virchow's Archiv," 1852, 25, 279.

isolated a soluble amylolytic ferment from the pancreas, the former observer by acidifying the aqueous infusion with phosphoric acid and precipitating with lime; the latter by the glycerine method first described by Wittich.

Hüfner found that the isolated body, which was still doubtless impure, contained 14·95 per cent. of nitrogen, but a less quantity of carbon and a greater quantity of oxygen than an ordinary albuminoid.

Until recently it has always been taken for granted that the fermentable sugar produced by the action of pancreas upon starch-paste is dextrose. Nasse, however, in 1878 ("Pflüger's Archiv f. Physiologie," 14, 473), attempted to show that the fermentable and cupric-oxide reducing body, obtained by the action of saliva upon starch, is a specific sugar which he calls *ptyalose*, and that the action of pancreatic juice appears to result in the production of the same body. Nasse did not isolate this hypothetical sugar, nor even approximately determine its specific rotatory power, but relied mainly upon its non-reduction of Barfoed's solution, and its doubling in reducing power on boiling with dilute acid, as evidence of its non-identity with dextrose.

In the early part of last year Musculus and De Méring published an important memoir ("Bull. Soc. Chim.," 31, 105), upon the action of diastase, saliva, and pancreatic secretion upon starch and glycogen.

They concluded from their experiments that the fermentable sugar produced in all these reactions is a mixture of maltose and dextrose. We have shown in a recent communication to the Chemical Society ("Journ. Chem. Soc.," 1879, 1, 648; "Liebig's Ann.," 199, 245), that *dextrose* can certainly not be included amongst the products of the action of malt-diastase upon starch. The experiments we are about to describe will however afford ample proof of the correctness of that portion of the above statement of Musculus and De Méring referring to the action of pancreatic extract.

Our experiments were made either with the aqueous infusion of the gland, or with the actual tissue itself in a finely divided state.

The first method is by far the most convenient, and is the one which we generally adopted when studying the action of the pancreas, but it will be seen that certain tissues can produce under some circumstances hydrolytic effects which their aqueous infusions are incapable of exercising.

With a clear aqueous infusion of the gland the course of experiment did not differ materially from that which we employed when investigating the action of malt-diastase upon starch (*loc. cit.*). As a rule the infusion had no power of reducing cupric-oxide, hence it was not necessary to apply any correction to the quantity of cupric-oxide reduced by the transformation products.

In cases where the transformation was effected by the actual tissue itself, this was previously dried very rapidly in a current of air at

35°C., a process which could easily be carried out without decomposition taking place. The dried tissue was used with a given volume of the experimental liquid containing a known quantity of the carbohydrate. The volume of the liquid was kept constant during the experiment, and the small increase in solid matter due to hydration was taken into consideration. Far more accurate results are obtained by this method than by attempting to estimate the correction for the total solids by digesting a given weight of the tissue in a given volume of water. In the latter case the solvent action of the water upon the tissue always differed somewhat from the solvent action of the solution of the carbohydrate employed.

When a clear infusion of pancreas, obtained by acting for several hours upon one part of the finely divided pancreas of the pig with five parts of cold water, is added to starch-paste at 40° C., complete limpidity is produced with extreme rapidity, and in from ten to fifteen minutes iodine ceases to produce any reaction for soluble starch or erythrodextrin. Careful observations at different periods of the reaction showed unmistakeably that the transformation in its first stages does not differ in the least from transformations of starch-paste made at 60° C. with aqueous malt-extract, which has not been previously heated to a higher temperature. When the specific rotatory power of the transformation products has reached $[\alpha]_{j3\cdot86}$, 162°·5, the cupric-oxide reducing power is found to be κ 49—50. When about 3 cub. centims. of the pancreatic infusion have been used for every grain of starch, the time occupied in fully bringing about this change is from forty to fifty minutes. The liquid now contains *maltose*, and an achroodextrin having all the properties of our *achroodextrin* ζ.

The maltose was proved to be identical with that obtained by the action of malt-extract by isolating it in its characteristic crystalline form, and by determining its specific rotatory and cupric-oxide reducing powers.

The composition of the matter in solution at this point is

Maltose........	80·8
Dextrin........	19·2
	100·0

which is that required by our No. 8 equation.

$$\underbrace{10C_{12}H_{20}O_{10}}_{\text{Soluble starch.}} + 8H_2O = \underbrace{8C_{12}H_{22}O_{11}}_{\text{Maltose.}} + \underbrace{C_{24}H_{40}O_{20}}_{\text{Achroodextrin } \zeta.}$$

On continuing to digest the liquid at 40° C., the values of $[\alpha]_j$ and κ undergo a little alteration, which is, however, very slow when compared with the previously rapid degradation of the starch-molecule.

The following is an example of such an experiment:—

90 cub. centims. of a liquid containing 3·5 grms. of starch-products, already reduced to the above point, were digested at 45° C. for twelve hours, with the further addition of 10 cub. centims. of fresh pancreatic extract.

At the end of that time, the liquid having remained perfectly free from organic life, the starch-products yielded on analysis the following corrected numbers:—

$[\alpha]_{j3\cdot86}$	$128^{\circ}\cdot7$
$\kappa_{3\cdot86}$	$66^{\circ}\cdot8$

corresponding very closely with the composition

Maltose........	55·5
Dextrose	32·7
Dextrin	11·8
	100·0

The united evidence afforded by many such experiments as the above proves conclusively that the *prolonged* action of a pancreatic infusion yields results differing materially from those obtained under similar conditions with malt-extract. With malt-extract the hydrolytic effect upon the starch-products, after No. 8 equation is reached, is confined entirely to the conversion of the achroodextrin into maltose, the latter body being the final product of the reaction. On the other hand, an aqueous infusion of pancreas, whilst converting achroodextrin into maltose, sensibly hydrolyses the maltose to dextrose.

In order to put this important point of difference between the action of malt and pancreatic extract beyond all doubt, a solution of pure maltose was digested with an infusion of pancreas, in just the same manner as maltose had, on many previous occasions, been treated with malt-extract.

100 cub. centims. of a solution of maltose, containing 5·813 grms. of substance, were mixed with 20 cub. centims. of a clear aqueous infusion of pancreas, the infusion being added in successive portions of 5 cub. centims. each. Temperature of digestion 45° C.

In four hours the optical activity of the solution had fallen from $[\alpha]_{j3\cdot86}$, $150^{\circ}\cdot0$, to $[\alpha]_{3\cdot86}$, $148^{\circ}\cdot7$; and after sixteen hours to $[\alpha]_{3\cdot86}$, $135^{\circ}\cdot1$; the value of κ at the end of this time having risen from 61 to 67·3. The composition deduced from these latter numbers is

Maltose........	83·8
Dextrose	16·2
	100·0

This experiment, repeated many times, always gave the same results, thus proving that the amylolytic agent of the pancreas is capable, as stated by Musculus and De Méring, of slowly converting maltose into dextrose, a property which is not shared by malt-diastase. Both malt-diastase and pancreatic-diastase have, however, the property of hydrolysing the lowest achroodextrin of the series, a fact which we have proved by submitting to their action the isolated and purified achroodextrin ζ.

A series of experiments was now made with the object of ascertaining if the pancreas contains any ferment capable of inverting cane-sugar. It will be remembered that a cold aqueous infusion of malt possesses this property to a limited extent. The first experiments were made by digesting solutions of cane-sugar at 40° C., with 25 per cent. of an aqueous extract of pancreas (5 of water to 1 of pancreas). As long as the solutions remained free from bacteria we never observed even the slightest inversion of the cane-sugar, the specific rotatory power of the solutions remaining constant throughout the experiment. If the digestion was carried on for a length of time sufficient to develop organic life, and a sensible evolution of gas, a marked inversion of the sugar set in, a result which is not however attributable to any soluble ferment derived from the pancreas.

From some observations made at a later period of the inquiry it seemed possible that the gland-tissue itself might have an effect which was not shared by its aqueous infusion. An experiment was consequently made in which the finely divided pancreas itself was digested with the cane-sugar solution. Here again, however, as in the former case, absolutely no inversion took place.

J. Béchamp, in his experiments upon the action of various tissues upon starch-paste and cane-sugar, states that the pancreas has a very slight invertive action upon cane-sugar ("Les Microzymas," p. 68). In an examination of the details of his experiments, however, it is clear that he only obtained evidence of invertive action after the gland had been in contact with a sugar solution for several days, and in no cases without the previous appearance of bacterial life, to which doubtless the effects are attributable; the invertive action of some of these organisms being almost as well marked as that of the Saccharomyces.

II. *The Hydrolytic Action of the Small Intestine.*

Claude Bernard first called attention to any distinct hydrolytic action of the small intestine. He found that a solution of cane-sugar, inclosed within a portion of the intestine ligatured above and below, or placed in contact with an infusion of its mucous membrane, speedily acquired the property of reducing a cupric solution.

Bernard found this property common to the small intestine of the dog, pig, rabbit, rat, and various other animals, and looked upon the invertive action as one of the most important functions of the *succus entericus.*

The observations made from time to time upon the *amylolytic* action of the secretions of the small intestine are somewhat conflicting. Thiry ("Wien Sitzungsber.," 50, 77), in 1864, by isolating a portion of the small intestine of a living animal, obtained an albuminous secretion which he found had no action upon starch, and with this observation Funke agrees. On the other hand, Masloff ("Unters. Physiolog. Inst. Heidelberg" [2], 1879, p. 290), Frerichs, and Busch ("Virchow's Archiv," xiv, p. 140) state that the small intestine has the power of transforming starch.

When commencing to investigate the hydrolytic action of the small intestine of the pig, we made use of an aqueous infusion of the tissue, made by acting upon one part of the well-washed and finely-minced intestine with five parts of water for from ten to fifteen hours.

Such an aqueous extract was prepared from three different portions of the intestine.

(1.) A portion of the *duodenum* immediately below the glands of Brunner.

(2.) The *agminated Peyer's glands* (Peyer's patches) cut from the jejunum.

(3.) Portions of the *jejunum* and *ileum* not containing any of the agminated Peyer's glands.

20 cub. centims. of the clear filtered infusion made with each of the above portions of intestine were added to 100 cub. centims. of a cane-sugar solution containing 4·557 grms. of sugar per 100 cub. centims. On digesting for three hours at 40° C., and allowing subsequently to stand in the cold for twenty-four hours, scarcely a trace of inversion was found to have taken place.

The action of equal quantities of the various infusions upon starch-paste was scarcely more marked; about 3 grms. of starch, in the form of starch-paste, being employed in each case, at a temperature of 40—45° C. After digestion for sixty minutes, none of the samples of starch-paste showed any signs of limpidity. After sixteen hours, No. 1 was found limpid, but contained only soluble starch; No. 2 was perfectly limpid, and contained a little erythrodextrin; whilst No. 3 was absolutely unacted upon, the gelatinisation being still as perfect as at the outset of the experiment.

The pig, from which was derived the intestine used in the above experiments, had been killed after fasting for thirty-six hours. It occurred to us that the absence of any well-marked amylolytic action might be due to this fact, and that a different result would probably be obtained by infusing an intestine in which the various glands had been more recently active. In order to test this, an animal was killed about two hours after administering a liberal allowance of barleymeal. In this case the aqueous extract of the small intestine possessed a somewhat greater action upon starch than in the previous experiment, but the transforming power was still very feeble, more than two hours being required for the production of limpidity in the starch-paste containing the most active of the three portions of the intestine, which was in this, as in the former case, the region of the jejunum and ileum containing the Peyer's patches.

It is possible, as was first shown by Berthelot, to obtain a clear aqueous infusion of ordinary yeast, which is capable of exercising a very decided invertive action upon cane-sugar. The action of this aqueous infusion is, however, feeble when compared with the inversion produced by actual contact of the yeast-cells themselves.

Reasoning from this fact, it seemed to us probable that, in the case of the intestine, far more pronounced hydrolytic results might be expected from acting with the tissue itself, than from merely using its aqueous infusion. This was found to be the case.

In the following experiments the different portions of the small intestine, after thorough and prolonged washing, were dried rapidly in a current of air at 35° C., and were divided into very fine shreds, which were immersed directly in the solutions under examination. The intestine was taken from a pig of eight months old, killed during the period of digestion.

The solution of cane-sugar employed contained 3·020 grms. of sugar per 100 cub. centims.

To every 100 cub. centims. of the solution were added 5 grms. of the finely divided dry intestine. Temperature of digestion, 40° C.

All the determinations were made by the optical method.

Comparative Action of the different portions of the Small Intestine of the Pig upon Cane-Sugar.

Portion of the small intestine.	Percentage of cane-sugar inverted.			
	After 1½ hrs. at 40°.	After 3½ hrs. at 40°.	After 16 hrs. in the cold.	After further digestion for 5 hrs. at 45°.
(1) Duodenum immediately below the pylorus, containing Brunner's glands	No action.	No action.	No action.	13·0
(2) Duodenum below the glands of Brunner	No action.	No action.	10·9	13·0
(3) Jejunum, not including any of Peyer's patches	..	14·0	19·5	25·1
(4) Ileum	..	14·0	19·5	25·1
(5) Agminated glands of Peyer (Peyer's patches), cut from the jejunum......	9·2	18·4	24·6	26·7

Action of various portions of the Small Intestine upon Starch.

The action of the *tissue* of the small intestine upon starch-paste, as upon cane-sugar, is decidedly more energetic than that of its aqueous infusion. Limpidity of the starch-paste is, however, not rapidly brought about, and when produced, the resulting soluble starch is very stable, and resists any sensible hydrolysation for a considerable time.

Strictly comparative experiments upon the amylolytic power of the various parts of the intestine were made in the following manner:—

30 grms. of potato-starch were gelatinised with 1000 cub. centims. of water, and 1 cub. centim. of malt-extract was added to the resulting starch-paste after cooling to 60° C. The moment limpidity was produced, the further action of this trace of malt-extract was arrested by boiling. The liquid, filtered perfectly bright on cooling, and containing, besides soluble starch, only a trace of erythrodextrin and maltose, was analysed; its specific gravity, optical activity, and cupric-oxide reducing power being determined. The solution was divided into portions measuring 100 cub. centims., into each of which were immersed 5 grms. of the dried and finely divided intestine. The various experimental liquids were digested under exactly similar conditions, in the water-bath at 40° C.

The portions of the intestine taken were as follows, the animal from which they were derived being a young pig, killed during active digestion of starchy food.

(1.) A portion of the duodenum taken immediately below the pylorus, and containing numerous Brunner's glands, which were very apparent in this case owing to active digestion being in progress at the time of the animal's death.

(2.) Lower portion of the duodenum not containing any Brunner's glands.

(3.) Agminated Peyer's glands taken from the jejunum.

(4.) Portions of the jejunum not containing any Peyer's patches.

(5.) Portions of the ileum, taken at the distance of a few inches from the ileo-cæcal valve.

The solutions gave the following iodine reactions, the dilute iodine solution being slowly added in each case up to an excess; thus ensuring the detection of any erythrodextrin.

After digestion for fifteen minutes—all deep blue—no production of erythrodextrin.

After thirty minutes—a very slight production of erythrodextrin in all.

After forty-five minutes—all gave a violet reaction. From the larger amount of iodine solution requisite to produce a permanent coloration in No. 3, it was evident that the hydrolytic action was proceeding more rapidly in this than in any of the other solutions.

After three and a-half hours—Nos. 1 and 2—deep violet reaction. No. 3 contained only a trace of unconverted soluble starch, and no erythrodextrin.

Nos. 4 and 5, violet reaction, but much lighter in tint than Nos. 1 and 2.

After the digestion for three and a-half hours at 40°, and lying in the cold for sixteen hours longer, the various liquids were fully analysed. The corrected results are here given:—

Portion of the intestine.	3½ hours at 40°.	16 hours in the cold.	
	$[\alpha]_{j3\cdot86}$.	$[\alpha]_{j3\cdot86}$.	$\kappa_{3\cdot86}$.
(1) Duodenum, with Brunner's glands....	179°·8	149°·3	41·7
(2) Lower part of duodenum............	163°·4	140°·7	47·8
(3) Peyer's patches....................	148°·3	122°·3	63·3
(4) Jejunum..........................	159°·7	133°·0	53·0
(5) Ileum............................	157°·2	134°·9	50·6

Upon calculating the composition of the transformation products from the above numbers, a very remarkable fact was brought to light. *Maltose* was present in only one case, that of No. 3, the whole of the cupric-oxide reducing body consisting in the other cases of *dextrose*. The percentage composition of the products is here given.

			$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$
(1)	Dextrose........	41·7				
	Soluble starch and dextrin.......	58·3	150°·3	41·7	149°·3	41·7
		100·0	Calculated.		Found.	

			$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$
(2)	Dextrose........	47·8				
	Soluble starch and dextrin.......	52·2	140°·7	47·8	140°·7	47·8
		100·0	Calculated.		Found.	

			$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$
(3)	Maltose.........	16·6				
	Dextrose........	53·1	122°·0	63·3	122°·3	63·3
	Dextrin.........	30·3				
			Calculated.		Found.	
		100·0				

(4)	Dextrose	53·0	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$
	Soluble starch and dextrin	47·0	132°·6	53·0	133°·0	53·0
		100·0	Calculated.		Found.	

(5)	Dextrose	50·6	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$	$[\alpha]_{j3\cdot86}$	$\kappa_{3\cdot86}$
	Soluble starch and dextrin	49·4	136°·3	50·6	134°·9	50·6
		100·0	Calculated.		Found.	

The question now arises—has the dextrose obtained in these experiments passed previously through the stage of maltose, or has it been derived more directly from the starch?

Direct evidence, as well as that furnished by analogy, points strongly to the former proposition being the correct one. We have seen, in four out of five of the experiments, that the soluble starch resisted very persistently the hydrolytic action of the ferment. Where this resistance to transformation was least strongly marked, as in the experiment with the Peyer's glands, a little maltose was found.

The conclusion seemed a fair one that we had here to deal with a remarkable hydrolysing agent, differing, in its relative action upon starch and starch-products, from that of any other known ferment of its class; an agent which is in fact capable of hydrolysing *maltose* with greater ease than *soluble starch.*

On further experiment this surmise proved correct.

All portions of the small intestine exert at 40° *a rapid hydrolytic action upon maltose; an action, however, which varies in intensity in different parts of the intestine, and is far more energetic than that of a similar portion of the intestine upon starch-paste, soluble starch, the higher dextrins, and even cane-sugar itself.*

After thoroughly washing the small intestine of a pig, the portions of the jejunum containing the agminated Peyer's glands were cut out, rapidly dried at 35°, and cut into fine shreds, 5 grms. of which were immersed in 100 cub. centims. of a solution containing 3·107 grms. of pure maltose.

The mixture was digested at 40° for sixteen hours, and at the end of that time it was found that *the maltose had been entirely converted into a dextrose* which possessed the same specific rotatory power and cupric-oxide reducing power as ordinary dextro-glucose, with which it was doubtless identical.

All portions of the small intestine exert, *cœteris paribus*, a much more rapid and complete action upon maltose than upon cane-sugar, and the hydrolytic effect of the agminated Peyer's glands upon either of these carbohydrates is far greater than that of any other portion of the small intestine, either for equal weights or for equal areas of the tissue.

We have satisfied ourselves of the truth of these statements by numerous experiments.

The following results exhibit the relative action upon maltose and cane-sugar of (1) the agminated Peyer's glands of the jejunum, and (2) of the adjoining portions of the jejunum containing, besides the glands of Lieberkühn, only solitary Peyer's glands. The experiments were conducted under exactly similar conditions, 5 grms. of the dried and finely divided intestine acting in each case upon 3 grms. of the carbohydrate dissolved in 100 cub. centims. of water.

1.—Action of agminated Peyer's Glands of the Jejunum upon Cane-Sugar and Maltose.

	Percentage of carbohydrate hydrolysed.			
	1½ hours at 40°.	3½ hours at 40°.	After 16 hours in the cold.	5 hours more at 45°.
Cane-sugar ..	9·3	18·4	24·6	26·7
Maltose.....	15·4	33·9	52·2	74·3

2.—Action of the Jejunum, without agminated Peyer's Glands, upon Cane-Sugar and Maltose.

	Percentage of carbohydrate hydrolysed.			
	1½ hours at 40°.	3½ hours at 40°.	After 16 hours in the cold.	5 hours more at 45°.
Cane-sugar ..	10·9	13·6	21·7	24·4
Maltose.....	4·2	26·6	38·6	57·9

These experiments, in conjunction with those upon cane-sugar, described at p. 399, prove that the activity of the small intestine upon saccharose is slow and incomplete, when compared with its power of converting maltose into dextrose; and also that, while the conversion of maltose into dextrose under the action of the intestine ferment is as continuous and uninterrupted a process as is its conversion by dilute sulphuric acid, the invertive action upon cane-sugar is decidedly limited, the action being either arrested or proceeding with extreme slowness, when 25 per cent. of the total quantity of cane-sugar has been inverted. The reason for this limited invertive action is by no means clear, and the subject requires further investigation.

Claude Bernard, who first demonstrated the existence in the small intestine of a soluble ferment capable of inverting cane-sugar, considered that in this property resided one of the most important functions of the *succus entericus.* By injecting a solution of cane-sugar into the veins and cellular tissue of animals he demonstrated that this carbohydrate, after traversing the system, is eliminated weight for weight in the urine, without undergoing any modification or assimilation. In order that cane-sugar shall be assimilated by the animal or vegetable economy, it must first be *inverted.* The seat of the invertive action is in the small intestine itself.

If this function of the small intestine has the importance attributed to it by Bernard, it is in the highest degree probable that the relatively far more active maltose-hydrating ferment, coexisting with the invertive ferment, must possess some considerable physiological value.

It must be remembered that, under natural conditions, the amount of cane-sugar which an animal is called upon at any given time to assimilate is very small when compared with the amount of products derived directly from starch.

We cannot consider, under these circumstances, that so well-marked and striking a function of the small intestine as that of converting maltose into dextrose can be useless to the animal economy. The most probable explanation is that maltose is incapable of assimilation in its unaltered state, but has first to be broken down to the smaller moleculed dextrose, just as cane-sugar, prior to assimilation, is converted into the chemically less complex dextrose and levulose. This is rendered the more probable from the known similarity of composition of maltose and cane-sugar, both bodies belonging to the class of sugars represented by the formula $C_{12}H_{22}O_{11}$.

Whether maltose is capable, under any circumstances, of being directly assimilated is a question, the solution of which we must leave in the hands of experimental physiologists. Probably a series of carefully conducted injection experiments, similar to those made by Bernard with cane-sugar, would yield the desired information. It is true that the estimation of maltose in urine would be attended with greater difficulties than the estimation of cane-sugar, but these difficulties are by no means insurmountable.

It will be remembered that the action of artificial pancreatic juice upon gelatinised starch is very rapid, the transformation products in a short time containing 80 per cent. of maltose, which is but very slowly and partially converted into dextrose by a continuance of the reaction. The active agent of the small intestine, on the other hand, while exerting but little action upon gelatinised or soluble starch, converts with great readiness maltose into dextrose. Thus we see that in the transition from colloidal starch to highly diffusible dextrose, the hydrolytic actions of the pancreas and small intestine *are mutually dependent and complementary to each other*, neither one set of actions alone being sufficient.

The small intestine does not contain a very active *amylolytic* ferment, because it is seldom or never called upon to act upon unaltered starch, the first portion of the work being completed by the pancreatic secretion. Brücke ("Wien. Sitzungsber.," 65 (3), 126), when experimenting upon dogs fed with amylaceous food, found that the soluble starch and erythrodextrin which were produced in the stomach at once disappeared on passing the pylorus, under the rapid action of the pancreatic juice.

We have now to consider more fully the part played by the three different sets of glands of the small intestine in bringing about the hydrolytic effects which we have described. These glands are known as—(1) the glands of Brunner; (2) the glands or follicles of Lieberkühn; and (3) the glands of Peyer.

Brunner's glands occur only in the duodenum. They are most numerous immediately below the pylorus, and resemble closely in structure the salivary glands, or minute portions of the pancreas. When a portion of the duodenum containing these glands is macerated in water, the liquid becomes extremely viscous, owing to the extraction of the special glandular secretion. This effect is best observed by taking the duodenum of an animal which has been killed during digestion. The viscid secretion, resembling in appearance submaxillary or sublingual saliva, has but a very slight amylolytic action. The portion of the duodenum containing the glands has only a very slight invertive action upon cane-sugar, but a somewhat more decided hydrolytic action upon maltose.

The glands or follicles of Lieberkühn consist of tubular depressions in the mucous membrane of the intestine, and are generally supposed to secrete the *succus entericus*, to which, however, the Brunner's glands must also largely contribute. Since Lieberkühn's follicles are pretty evenly distributed throughout the whole of the small intestine, and the hydrolytic effect of equal areas of the intestine varies very much in different parts, it is evident that these glands play no very important part either in the inversion of cane-sugar or in the still more active hydration of maltose to dextrose.

We believe that the variable hydrolytic action of the different regions of the small intestine is mainly if not entirely due to the relative frequency of the glands of Peyer. The solitary Peyer's glands occur most scantily in the upper portion of the duodenum, and here the hydrolytic effect is by far the least. As the solitary glands increase in number the action of the intestine becomes more strongly marked, and finally the regions of the jejunum and ileum containing the agminated glands, or Peyer's patches, are the portions of the intestine which exert the most pronounced hydrolytic effect upon maltose, cane-sugar, and starch. These glands consist of small ovoid masses of adenoid tissue, embedded in the mucous membrane, and enclosing vast numbers of leucocytes. The glands are in intimate relation both with the vascular and lymphatic systems, each follicle being penetrated by blood-vessels, and surrounded by lymph sinuses which are in connexion with the lacteals of the villi.

The function of the Peyer's glands is by no means established. The opinion held by physiologists up to a few years ago that they discharge, by occasional rupture an intermittent secretion into the intestine, appears now to be abandoned in favour of the view that they are instrumental in absorbing material from the blood and chyle, which they elaborate and again transmit, in a modified form, in part to the portal blood, and in part to the lacteal system.

The property which the glands undoubtedly possess of hydrolysing maltose to dextrose, and of so rendering the starch products of the pancreatic digestion more fitted for nutrition, is probably one of these special functions of elaboration.

Our work on these matters has been necessarily restricted to the chemical side of the subject, and we must now, lest we should incur the charge of overstepping our *métier*, leave further observations in the hands of physiologists.

The following are the main points which we consider have been established by our experiments:—

(1.) The action of artificial pancreatic juice upon starch-paste or soluble starch at 40° C. is, in the earliest stages of the reaction, similar to that of unheated malt-extract acting at 60° C. and under, the composition of the starch-products becoming comparatively stationary when 80·8 per cent. of maltose has been produced.

(2.) Both malt-diastase and pancreatic-diastase are capable of hydrolysing the lowest achroodextrin to maltose.

(3.) Pancreatic-diastase is capable, by long continued action at 40°, of slowly but sensibly converting maltose into dextrose, a change which malt-diastase is incapable of effecting even under the most favourable circumstances.

(4.) Neither artificial pancreatic juice, nor the tissue of the gland itself contains any ferment which is capable of inverting cane-sugar.

(5.) The small intestine is capable of hydrolysing maltose, of inverting cane-sugar, and of acting feebly as an amylolytic ferment.

(6.) The action of the tissue of the small intestine in bringing about these changes is far greater than that of its mere aqueous infusion, and differs materially in different regions of the intestine.

(7.) The variability of the hydrolytic action of different portions of the small intestine is independent of the relative frequency either of the glands of Lieberkühn, or of those of Brunner, but appears to be correlative with the distribution of Peyer's glands.

(8.) In the transition from colloidal starch to readily diffusible and easily assimilated dextrose, the actions of the pancreas and of the Peyer's glands are mutually dependent and complementary.

The pancreas readily breaks down the starch to maltose, but is capable only of a very slow conversion of the resulting maltose to dextrose. The Peyer's glands on the other hand, whilst almost powerless upon starch itself, take up the work at a point where the pancreatic juice almost ceases to act, and so complete the conversion of starch into dextrose.

V. "On the Ova of the *Echidna hystrix.*" By Professor OWEN, C.B., F.R.S. Received April 26, 1880.

(Abstract.)

The present communication carries forward the subject of monotrematous generation to a stage beyond those detailed in vols. 124 (1834), p. 555, and 155 (1865), p. 671, of the "Philosophical Transactions," and relates to the discovery of ova in the right uterus of a female *Echidna hystrix*, killed 14th September, 1879, and in the left uterus of one killed 30th August, 1879, at Towoomba, Queensland. These, with other specimens, killed between 30th August and 10th October, were transmitted to the author by Geo. Fred. Bennett, Esq., Corresponding Member of Zoological Society of London, and were received in February, 1880.

In the largest ovum the first fission of the germ mass, corresponding to that described by Martin Barry and Bischoff in the rabbit's ovum, had commenced but was not completed.

In other respects the ova of *Echidna* closely correspond with those of the *Ornithorhynchus* described and figured in the volume above cited, p. 555, Plate XXV, figs. 3–7. No further progress in embryonal development was detected; but the fission-stage strengthens the conclusion, previously arrived at, of the viviparity of the Monotremata.

The functional equality of both uteri in the genus *Echidna* corresponds with the equal development of the right with left female organs, in which it differs anatomically from the *Ornithorhynchus.*

The Society adjourned over the Whitsuntide Recess to Thursday, May 27th.

May 27, 1880.

THE PRESIDENT in the Chair.

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

The following Papers were read:—

I. "On some Thermal Effects of Electric Currents." By WILLIAM HENRY PREECE, General Post Office. Communicated by Professor STOKES, Sec. R.S. Received April 28, 1880.

I have been engaged for some time past in experimenting on the thermal effects of electric currents, but the final results of those experiments are not sufficiently ripe at present to justify my bringing them before the Royal Society. I have, however, obtained one result which I believe to be sufficiently novel to justify a short preliminary note.

The most striking facts elicited by these experiments are:

1. The extreme rapidity with which thin wires acquire and lose their increased temperature.

2. The excessive sensibility to linear expansion which fine wires of high resistance evince.

Now as the rate of heating, and therefore of expansion and contraction, varies very nearly directly as the increment or decrement of the currents, when these variations are very small, it occurred to me that if a long wire of small diameter and high resistance were attached to a sounding board, or to the centre of a disk (such as one of those used for telephones and phonographs) and it formed part of a circuit conveying telephonic currents, sonorous vibrations ought to be reproduced.

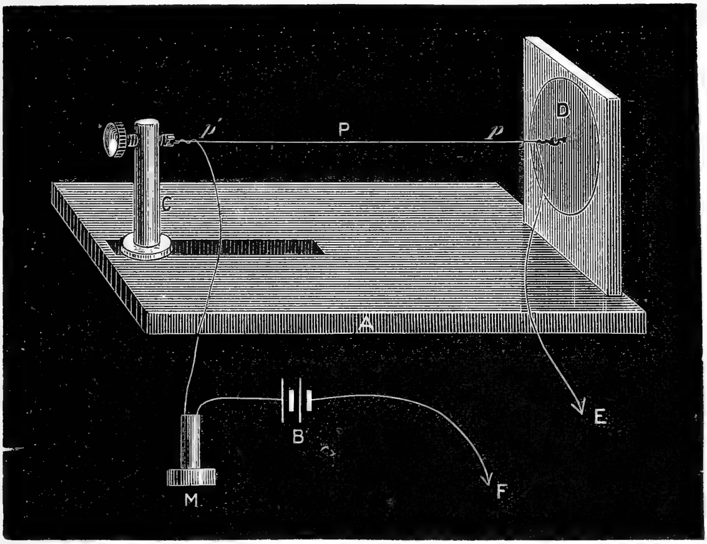
The sketch shows the arrangement of the apparatus used for the experiment.

A was a stout base of mahogany, on which a brass support C was attached so that it could slide and be fixed at any distance from D.

D was at first a disk of thin paper, and then of thin iron.

P was the wire experimented upon whose loose ends were connected to terminals on the wooden base, so as to be inserted in the circuit containing a microphone transmitter M and a battery B of six bichromate of potash cells in another room out of hearing.

A platinum wire of 0·003 inch diameter and six inches long from p to p' was first used, and the sonorous effects were most marked and encouraging, when the microphone transmitter M was spoken into. The articulation, though muffled, was clear, and words could easily be heard.



1. Experiments were first made to determine the length which gave the loudest sound and the clearest articulation, and, after repeated trials with every variation of length from one inch to six feet, it was found that a wire six inches long gave the maximum effect.

2. Experiments were then made to determine the diameter of the wire that gave the best effect, and after repeated trials with every gauge drawn from 0·0005 inch to 0·005 inch, it was found that wire of the diameter 0·001 inch gave the best effect.

3. Experiments were then tried with wires six inches in length and 0·001 inch diameter of different materials, viz., gold, iron, aluminium, silver, copper, palladium, and platinum, and they came out in the following order of merit:—

Platinum..............	Very clear.
Aluminium............	Very variable.
Palladium	Clear.
Iron..................	Clear.
Copper	Faint.
Silver	Faint.
Gold	Very poor.

4. The effect of mechanical strain was tried. It was found not to vary the effect. When once the requisite tension, which varied with

each metal, was obtained, further tightening up did not vary the clearness or loudness of articulation.

Gold would scarcely bear the tension required to reproduce sonorous vibrations, hence its low position.

5. Very thin carbon pencil, ·0625 inch diameter, was tried under compression and under tension, but no effect whatever was experienced unless a bad joint was made, when at once a faint microphonic effect was apparent.

6. No sibilant sounds whatever could be reproduced.

7. That the effect was due to heating and cooling was shown by the fact that it was possible to increase the current to such a strength as to render the temperature of the wire sensible to the touch, and then to make its elongation and contraction by low sounds evident to the eye.

It therefore appears from these experiments that wires conveying those currents of electricity which are required for telephonic purposes expand and contract as they are heated and cooled, and as the variations in the strength of the current are small compared with the strength of the current itself, the expansion and contraction vary in the same ratio as the condensation and rarefaction of the air particles conveying the sonorous vibrations which produced these vibrations.

The mechanical changes, or molecular vibrations in the wire, due directly or indirectly to telephonic currents, which result in the reproduction of sound, bear a close analogy to the mechanical changes due to the direct transmission of sound, but with this important difference, that while the vibrations due to sound are progressive along the wire, and their velocity is low and easily measured, those due to thermal effects are practically instantaneous, and therefore affect simultaneously the whole length of the wire.

(Received May 27, 1880.)

Note.—De la Rive, in 1843 (*vide* "Electricity," vol. i, p. 304), observed that an iron wire emitted sounds when rapid discontinuous currents were passed through it, but he attributed the effect to magnetism, for he failed to obtain the same effect in non-magnetic wires like platinum or silver.

Graham Bell found, in 1874, that a simple helix without an iron core emitted sounds, and (in 1876) that very distinct sounds proceed from straight pieces of iron, steel, retort carbon, and plumbago when conveying currents.

Professor Hughes showed that his microphone was reversible, that is, that it could receive as well as transmit sonorous vibrations.

Mr. Weisendanger ("Telegraphic Journal," Oct. 1, 1878) reproduced sounds on a microphonic receiver, which he called a thermophone, and attributed the effect to its true cause, viz., the expansion of bodies under the influence of heat, which, in fact, is the explanation of all microphone receivers.

Ader reproduced speech by the vibrations of a wire conveying currents of electricity, but he found that only magnetic metals were effective, and therefore, like De la Rive, he attributed the result to magnetic agencies. (*Vide* Count du Moncel, "Telegraphic Journal," March 1, 1879.)

These and many other sonorous effects of currents on wires may be really due to such heat effects as I have described.

II. "A Preliminary Account of the Reduction of Observations on Strained Material, Leyden Jars, and Voltameters." By JOHN PERRY and W. E. AYRTON. Communicated by Professor G. G. STOKES, Sec. R.S. Received April 17, 1880.

It has been shown by Dr. Hopkinson that, if two Leyden jars be made of the same glass, but of different thicknesses:—

1st. If they be charged with the same difference of potential for equal times, discharged for equal times, and then insulated, that the residual charge will, after equal times, have in both cases the same difference of potential.

2nd. That residual charge is proportional to exciting charge. These propositions may be included in one law—the superposition of simultaneous forces is applicable to the phenomena of residual charge. All the investigations in Dr. Hopkinson's paper in the "Transactions of the Royal Society," vol. 167, part 2, serve to prove this law, and, so far, they support the theory of residual charge, which we owe to the late Professor Clerk Maxwell. We should, therefore, be inclined to think that one of the best methods of investigating the relation between the relative powers of different glasses to possess residual charge would be simply to charge jars made of these glasses for the same great length of time, discharge for the same short intervals, and insulate, measuring in each case the time increase of soaking out of the residual charge. Whatever the thickness of the glass or the amount of the original charge, we know that the same glass will always give the same proportion of residual charge at the same times from insulation. Any change of the state of the glass caused by heat would be shown as a change on the curve of increasing residual charge. There seems to be no doubt that this method would give what may be called a measure of the specific power of producing residual charge phenomena of the glass experimented on.

In this way, since 1875, we have obtained a considerable number of curves for residual charge in condensers of different dielectrics at different temperatures, and so we may say that we have measured the power of producing residual charge phenomena in the substances experimented upon in different states, and this power, which we call our "residual-charge-function," ought, according to Dr. Hopkinson, to be constant for the same substance at the same temperature. Thus, if v_t is the residual charge at the time t after insulation, and if V was the original long continued charge, $v_t \div \mathrm{V}$ is our residual-charge-function for the time t, which we may designate by F_t. Of course, it is evident that if F'_t is our function for a different dielectric, then $\mathrm{F}_t \div \mathrm{F}'_t$ is not necessarily the same as $\mathrm{F}_{t'} \div \mathrm{F}'_{t'}$; in fact, the residual-charge-function is only completely specified when a table is given of its values at different times after insulation. We have used the same name and symbol in the experiments we have been simultaneously making during the last five years on strained wires and beams, and on voltameters and magnets, and of course there is always the difficulty experienced by Dr. Hopkinson with his more complicated function "ψt," which renders it necessary to give a table of values instead of only one value of the function. Thus, for two particular kinds of glass, Dr. Hopkinson finds—

$$\frac{\psi(5)-\mathrm{B}}{\psi'(5)-\mathrm{B}}=3{\cdot}5,$$

whereas

$$\frac{\psi(60)-\mathrm{B}}{\psi'(60)-\mathrm{B}}=6,$$

showing how with the same two glasses the ratio is affected by varying the time from 5 to 60. Again, also in his investigation of the influence of temperature on residual charge in the same glass, he finds that at 175° F. the values of $\psi(1)-13$ and $\psi(5)-13$ are respectively 0·38 and 0·034, with a ratio one to the other of 11; whereas the values of these functions at 108° F. are 0·155 and 0·05 respectively with a ratio one to the other of 3·1. This method of measurement is subject, therefore, to exactly the same objections as the easier method of which we have spoken; in fact, we are not aware that hitherto any method has shown itself to be better than actually giving the curves of rise of potential after insulation, or the curves of change of current during charge and discharge of a condenser. This is the method which we have found the easiest for the discussion of such phenomena, and we may direct attention to such curves for voltameters in a paper communicated by us to the Society of Telegraph Engineers, and which appeared in their Journal, vol. v, 1877, Nos. 15, 16, p. 391. Curve EFGG′ there shown, in fig. 4, p. 396, is especially interesting, as we see from it that if a voltameter with dilute sulphuric acid be charged with one Minotto's cell for a certain time, then discharged for a certain time, by removing the battery and connecting together the platinum plates of the voltameter through a resistance, and finally the platinum plates be insulated, that the difference of potentials between the plates gradually rises, presenting phenomena exactly like the soaking out of the residual charge in a Leyden jar that has been insulated after being discharged for a short time.

The unsatisfactory nature of the ordinary experiments with condensers has led us to try if we could find the actual constants in Professor Clerk Maxwell's differential equation—

$$\frac{d^n v}{dt^n}+a\frac{d^{n-1}v}{dt^{n-1}}+\ .\ .\ .\ .\ +mv=a'u+b'\frac{du}{dt}+\ .\ .\ .\ .\ +m'\frac{d^{n-1}u}{dt^{n-1}}\ .\ \ .\ \ \ (1),$$

where v is the difference of potentials of the two surfaces of the condenser, and u the current flowing into or from the condenser at the time t, for it is quite evident that if his theory be correct these constants completely determine the residual charge phenomena of the substance. The difficulty in obtaining these constants has involved us in a very large amount of labour, and we have only yet reduced a small number of the curves we have experimentally obtained during the last five years; in addition we have found that many of our experiments will have to be repeated with somewhat different conditions for the investigation to be regarded as complete. But some of the results already obtained from this reduction are of interest. Thus, in one set of observations, we have tried to determine the constants from curves of the soaking out of the residual charge of a Leyden jar discharged for a very short time, having been previously kept charged for so long a time in a room of nearly constant temperature, that no loss was observed during two or three days, that is, the jar may be regarded as possessing almost infinite insulation.

The Leyden jar employed for these experiments was that of a Thomson's quadrant electrometer, and the measure of its charge was determined from the deflection produced in the needle of the electrometer itself when the electrodes of a Latimer Clark's constant cell attached to the electrometer quadrants were reversed. In reality three such cells in succession were used in order to detect any irregularity in the cells themselves. The Leyden jar was first rather highly charged, and then left with the replenisher untouched for some days until the loss of charge from day to day became imperceptible. The readings obtained with each of the three Clark's cells were then accurately taken, when one of them being left on the electrometer electrodes the Leyden jar was discharged for a very short time and then insulated; the soaking out of the residual charge was now measured by taking time-readings of the increase of deflection produced by the constant cell. To allow for alteration of zero of the electrometer arising from alterations of its charge frequent reversals of the cell were taken. After some minutes the right and left readings due to the reversal of the cell could be observed on the ordinary scale placed two metres away from the electrometer; but at the commencement of the insulation after discharge, the soaking out was far too rapid to allow of numbers on the scale being read, consequently the following device was employed to allow of our obtaining the early part of the curve with accuracy. In the slit of the lamp were two cross-wires, an image of which by means of the mirror in the electrometer and of an auxiliary lens was formed on a cylinder 13 centims. in diameter and 100 centims. in length, revolved fairly rapidly by clockwork around a horizontal axis. The comparatively rapid motion of the image of the cross-wires was then accurately followed by an observer, who with a pencil dotted its position from to time on a sheet of paper wrapped round the revolving cylinder. On unwrapping the paper the dots made by the pencil enabled the curve of rise of potential to be drawn accurately from the beginning in each case.

In these experiments as the jar is insulated, u, $\frac{du}{dt}$, &c., equal 0, and the solution of the differential equation (1) is

$$v = \mathrm{A} + \mathrm{B}\epsilon^{-\beta t} + \mathrm{C}\epsilon^{-\gamma t} + \quad . \quad . \quad . \quad . \quad \quad . \quad (2),$$

where β, γ, &c., are such that $x+\beta$, $x+\gamma$, &c., are factors of the expression

$$x^n + ax^{n-1} + ax^{n-2} + \quad . \quad . \quad . \quad . \quad + mx.$$

Hence if we determine β, γ, &c., we determine the constants of the differential equation (1). Our method of reduction was as follows:—

If is less than γ, γ than δ, and so on in equation (2), then, after certain intervals of time, certain of the exponents became unimportant; and if the time is sufficiently great, equation (2) reduces itself to

$$v = \mathrm{A} + \mathrm{B}\epsilon^{-\beta t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

If a curve is obtained experimentally connecting v and t, it is easy to determine A, B, and β. Thus from the observations given in the first two columns of the following table, corresponding with one of our several experiments with the Leyden jar (v being very slightly corrected by a curve), $\frac{dv}{dt}$ was determined for the various times.

Then the values of v and $\frac{dv}{dt}$ for the same times being used as co-ordinates of points on a sheet of squared paper, we found that for the last 140 hours of our experiment the points lay, with very considerable accuracy, in a straight line; and hence during this time we may assume equation (3) to correctly represent the law of the soaking out of the residual charge. These plotted points enabled A to be determined. It is evident, also, that the values of log $(v-\mathrm{A})$ and t when plotted as co-ordinates of points give a straight line and enable B and β to be determined. We found that equation (3) for the particular experiment in question became

$$v'=474-177{\cdot}34\epsilon^{-0{\cdot}0000637t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

and giving values to t we have calculated column 3 in the following table. Proceeding in precisely the same way with the remainders $(v-v')$ given in column 4, we obtain the other coefficients and exponents in the equation (2).

Soaking out of the Residual Charge in the Leyden Jar.
June 16, 1876.

Time in minutes.	Observed value of v.	v' calculated from (4).	Difference $(v-v')$.
0	0	296·7	296·7
0·5	82	296·7	214·7
1	102	296·8	194·8
2	120	296·9	176·9
4	138	297·1	159·1
7	155	297·4	142·4
10	168	297·8	129·8
20	190	298·8	108·8
40	213	301·1	88·1
80	236	305·4	69·4
150	264	312·8	48·8
250	296	322·8	26·8
350	324	332·1	8·1
500	345	345·0	0
700	362	360·1	−1·9
1,000	398·4	380·2	1·8
1,500	404·5	405·8	1·3
2,000	423·9	424·4	0·5
2,500	437·9	437·9	0
3,000	447·6	447·7	0·1
4,000	459·0	460·1	1·1
5,000	465·4	466·6	1·2
6,000	469·5	470·1	0·6
7,000	471·8	471·9	0·1
8,000	472·9	472·6	−0·3
9,000	473·6	473·7	0·1

We have stated above that we slightly corrected, by means of a curve, the values of v before putting them in column 2. It would make the present paper too long to describe in full the slightly wavy nature of the actual curves connecting v and t obtained in our experiments. That the variations are mainly due to slight fluctuations in the temperature of the room is seen from our finding a diurnal period in the waves. Indeed, we have found that small changes of temperature very materially affect all the phenomena with which we deal in this paper. But there is something more in these small waves than can be explained by changes of temperature, and possibly the solution (2) of the differential equation may have some coefficients which are sine and cosine functions of the time. A study of the constants on Professor Maxwell's theory ought to tell whether

$$x^n + ax^{n-1} + \;.\;.\;.\;.\; + mx = 0$$

has any unreal roots.

In the same way as that above described, we have obtained the exponentials of

$$u = \mathrm{A} + \mathrm{B}\epsilon^{-bt} + \mathrm{C}\epsilon^{-ct} + \quad .\;.\;.\;.\;.\;.\;. \quad (5),$$

the solution of equation (1) when v is kept constant, from actual observations of the current flowing into a condenser from a battery with a constant electromotive force. The curve for u, just like the curve described above, possesses a slight waviness, and when this is neglected its simple logarithmic nature subsequently to a certain period of time is very striking.

In former papers read before this and other Societies we have given our reasons for believing that the phenomena of polarisation in voltameters, under the action of an electromotive force insufficient to produce disruptive discharge in the liquid, that is rapid and visible decomposition, are of exactly the same nature as the phenomena exhibited by condensers having glass or other solid dielectrics. And we think that if our analogies fail for greater electromotive forces it is because rupture of a solid dielectric introduces instantaneously entirely different conditions. An examination of the following reductions, which we give as a sample of the observations of current flowing into and out of a voltameter containing ordinary water and charged with one Minotto's cell (which has an electromotive force less than will produce visible decomposition), will show that there is a striking adherence to the logarithmic law, and that we here add another analogy to those we have already given between condensers with liquid and with solid dielectrics.

In the following table t is the time, in minutes, after applying one Minotto's cell, u is the current flowing through the voltameter, the unit of u being 0·371 microfarad per second; u' is the current calculated from the formula

$$u' = 180{\cdot}1 + 426{\cdot}6\epsilon^{-0{\cdot}01773t} \quad .\;.\;.\;.\;.\;.\;. \quad (6),$$

which we have obtained from the general solution (5) of the differential equation by a method explained towards the commencement of this paper.

Charging of a Voltameter, consisting of Platinum Plates dipping in ordinary Water, with one Minotto's Cell.

February 14, 1878.

Time in minutes.	Observed value of u.	u' calculated from (6).	Difference $(u-u')$.
4	690	587	103
10	618	537	81
20	526	479	47
30	455	431	14
40	398	390	8
50	355	356	−1
60	322	327	−5
70	297	303	−6
80	279	283	−4
90	265	267	−2
100	252	253	−1
110	240	241	−1
120	230	231	−1
130	223	223	0
140	216	216	0
150	211	210	1
160	205	205	0
170	201	201	0
180	198	198	0
190	195	195	0

After the deflection had diminished from 690 to 195 in rather more than three hours, the Minotto's cell was suddenly removed, and the voltameter discharged through the galvanometer, the terminals of which at the same moment were reversed, so that the deflections for discharge should be on the same side as those for charging, in order to somewhat diminish the swinging of the needle.

The general differential equation (1) when applied to the discharge curve thus obtained by experiment, leads to the solution—

$$u'=11{\cdot}4+158{\cdot}5\epsilon^{-0{\cdot}0579t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

from which u' given in the third column of the following table has been calculated.

Discharging of the Voltameter.

February 14, 1878.

Time in minutes.	Observed value of u.	u' calculated from (7).	Difference $(u-u')$.
6	244·0	123·4	120·6
10	156·8	100·2	56·6
15	91·4	77·9	13·5
20	62·0	61·2	0·8
30	37·8	39·3	−1·5
40	26·6	27·0	−0·4
50	20·6	20·2	0·4
60	16·4	16·3	0·1
70	14·0	14·1	−0·1
80	13·0	12·9	0·1

The reason why the exponentials in the equation connecting currents and time when charging differ from those connecting currents and time when discharging the voltameter, is as follows:—If r is the resistance of the battery, galvanometer, connexions, &c., but excluding the resistance of the voltameter itself, then—

$$u=\frac{V-v}{r},$$

where V is the electromotive force of the battery, and v that of the voltameter, at any time t during the charging—

$$\therefore \frac{dv}{dt}=-r\frac{du}{dt},$$

$$\frac{d^2v}{dt^2}=-r\frac{d^2u}{dt^2},$$

which when substituted in the general differential equation, enable the integration to be effected. But during the discharge after the removal of the battery, if r' is the resistance of the galvanometer, connexions, &c., but excluding that of the voltameter itself, then—

$$-u=\frac{v}{r'},$$

$$\therefore \frac{dv}{dt}=-r'\frac{du}{dt},$$

&c.

Consequently the constants in the differential equation, and therefore the exponentials in the equations of our curves, must be different.

Guided by this, we have arranged in our later experiments that r

and r' should be both very small, so that the exponentials in the equations both of the charge and discharge curves should be the same.

When a beam or wire is suddenly bent or twisted, the forces producing the strain being maintained constant, it is known that the strain increases with the time, and if the forces suddenly cease to act, that the strain does not altogether disappear at once, but diminishes gradually. This strain w, we have found can, like differences of potential and current, be most easily expressed as the sum of certain exponentials of the time. There are other analogies with condensers. Rapid tremors given to a strained body, our experiments show, cause it to attain its ultimate state more rapidly; one example of this is the more rapid soaking out of the residual charge in a Leyden jar, as noticed by Dr. Hopkinson; our curves connecting w and t for strained beams, like those connecting v and t in condensers, or u and t in voltameters, we find to be of a slightly wavy nature, and to be all similarly affected by change of temperature. Like several other experimenters, we have found that the strain in a body at any time depends not only on the forces acting upon the body at that time, but also on all the forces which have acted on the body during all previous time; so that, for instance, if a beam fixed at the ends has been loaded negatively for some time, if this load be removed and a positive load applied and soon taken off, the beam may be found to have a negative deflection; or again, if a current be sent for some time through a voltameter in one direction, and then reversed for some time, and finally the battery removed, the discharge from the voltameter, through a galvanometer, will, in certain cases, first be found opposite to the latter charging current, but subsequently it will become nought, and then become the opposite of the first charging current, this experiment being best seen with electrolytes having a certain amount of rigidity. Similar phenomena are also evidenced with iron under magnetisation, and Dr. Hopkinson's experiments on Leyden jars also lead to similar results.

We have consequently, while investigating the results we have experimentally obtained during the last few years with strained substances, been led to construct the following theory, which is analogous with that of the late Professor Clerk Maxwell for Leyden jars.

A perfect fluid is such that the only stress which can exist at any small interface which may be drawn in the fluid is a stress normal to the interface, and hence if the fluid is contained between two parallel planes which have a motion in their planes relatively to one another there is no force required to cause this motion, or to maintain it, in the case of a perfect fluid. If the fluid is a gas or a vapour, we can calculate the force which would be required to maintain a given relative velocity of the plates, and our students have obtained experimentally some materials (not yet published) for the calculation of such forces when the fluid is water. If the fluid is tar, or a mixture of tar and pitch, in any proportion, or even if it is the more solid substance pitch or sealing-wax, we find that although the viscosity is almost infinitely greater than in the case of water, still the phenomena exhibited are exactly the same. And just as experiments on water enable us to examine best some phenomena exhibited by viscous substances, so experiments on sealing-wax enable us to make the best examination of other phenomena.

When external forces suddenly act on a viscous fluid (and we know that all substances, whether solid, liquid, or gaseous, come under the head of "viscous fluids,"), tending to cause strains in the material, we can calculate in the well-known way the strains and stresses. If the forces do not alter, and if the shape of the body varies very little with time, the stresses do not alter, but the strains increase according to a law—

$$X=r\frac{ds}{dt},$$

where X is the stress at any interface, s the corresponding strain, t the time, and r a constant. Of this strain, if the part which was suddenly produced, and which is suddenly removable, is f, then—

$$X=kf,$$

k being the coefficient usually given in books to connect the corresponding stress and strain.

During the increase of strain, mechanical energy is being converted into heat, through the agency of internal friction or viscosity at the rate—

$$X\frac{ds}{dt}$$

per unit time.

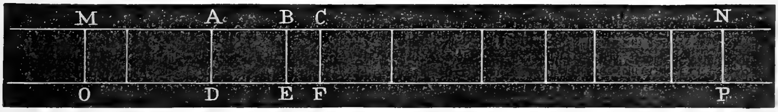
If the external forces are not kept constant, the stresses X alter, and also the strains; the second relation given above remains the same, but the first becomes—

$$X=r\frac{d}{dt}(s-f).$$

Now the first and last relations constitute our hypothesis; we have arrived at it by analogy, and not yet by experiment. We know that for the motions of pendulums, &c., in air, this law holds, and as the velocity or $\frac{ds}{dt}$ is less and less, the law is found to be more and more true. For the motion of steamships it is usual to assume the law—

$$X=r\left(\frac{ds}{dt}\right)^n,$$

where n varies from 2 to 2·28, but as motions in water become slower and slower, n becomes more and more nearly equal to unity. In the following investigation, we assume that the rates of deformation of the materials, after the first instant, are so slow, that n always equals unity. Our hypothesis is exactly analogous with Ohm's law in electricity; and just as Ohm's law has only been proved for metallic conductors of electricity, so our hypothesis is only known to be true for gases and liquids. All the analogies which we have observed cannot be discussed in this short paper, but they are such that we are led to believe that if our hypothesis proves to be untrue for materials subjected to small strains, then Ohm's law will prove untrue for currents of electricity in bad conductors. It will be seen that it leads to the conclusion that just as there are no perfect insulators of electricity, so no material, however rigid it may appear to be to us, can for an infinite time resist the effect of even small forces tending to change its form.



Let MNOP be a large prism of unit square section, formed of blocks of different materials of lengths, a_1, a_2, &c. Let them be subjected to shear stress by the action of tangential force v distributed over the surface MN, and an equal and opposite tangential force distributed over the parallel plane OP. Let the compound prism be so long that we need not speak of the terminal couple which is required to produce equilibrium. Let f_1, f_2, &c., be the strains existing in the blocks which would be instantaneously destroyed if the stress disappeared, so that if the shear stresses in the blocks are respectively X_1, X_2, &c., then—

$$X_1 = k_1 f_1 \quad \ldots \ldots \quad (8),$$
$$X_2 = k_2 f_2, \text{ \&c.},$$

where k_1, k_2, &c., are the moduli of elasticity of the separate blocks. The distributed external force acting on either of the upper or lower sides of the block being of course $a_1 X_1$.

If the velocity of OP with regard to MN is u, then u is the rate at which each block is gaining strain, because the motion of OP with regard to MN is the measure of the common strain which exists in all the blocks. Consequently, by our hypothesis for any block, we have—

$$X_1 = r_1\left(u - \frac{df_1}{dt}\right) \quad \ldots \ldots \quad (9),$$

where r_1 is a constant for the material of which the particular block is composed. Hence, combining (8) and (9) we have—

$$u=\frac{1}{r_1}X_1+\frac{1}{k_1}\frac{dX_1}{dt},$$

from which by denoting in the usual way an inverse operation

$$X_1=\left(\frac{1}{r_1}+\frac{1}{k_1}\frac{d}{dt}\right)^{-1}u.$$

The total distributed force v is of course equal to the sum of all the forces a_1X_1, a_2X_2, &c., so that

$$v=\left\{a_1\left(\frac{1}{r_1}+\frac{1}{k_1}\frac{d}{dt}\right)^{-1}+a_2\left(\frac{1}{r_2}+\frac{1}{k_2}\frac{d}{dt}\right)^{-1}+\ \ldots\ldots\ \&c.\right\}u,$$

an equation between the force distributed over either of the parallel planes, and the rate of motion of the planes relatively to one another.

If any number of blocks are of the same material, or if, although not of the same material, the ratio of r to k is the same in them, then it is evident that for all such blocks there will only be one inverse term of the above equation. Hence if there are n different materials having different ratios of r to k, we have the general equation

$$\frac{d^nv}{dt^n}+b_1\frac{d^{n-1}v}{dt^{n-1}}+\ \ldots\ +b_{n-1}\frac{dv}{dt}+b_nv$$
$$=c_nu+c_{n-1}\frac{du}{dt}+\ \ldots\ +c_2\frac{d^{n-2}u}{dt^{n-2}}+c_1\frac{d^{n-1}u}{dt^{n-1}}\quad\ldots\quad(10).*$$

In the preceding we have followed Professor Clerk Maxwell's method of investigating the charge of a condenser, and the currents flowing into it, the method of reasoning from our hypothesis regarding the action of a viscous block being the same as his reasoning from Ohm's law. From the form of the equation it is evident that the order

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* If we put $m_1=\frac{r_1}{k_1}$, &c., and let

s^r represent the sum of products of m_1, m_2, &c., r at a time,

$s^r{}_p$ " " " r at a time, but only such as involve m_p,

then the differential equation is evidently as follows :—

$$v+s'\frac{dv}{dt}+s^2\frac{d^2v}{dt^2}+\ \ldots\ldots\ +s^n\frac{d^nv}{dt^n}=\sigma u+\left(s'\sigma-\sum_{p=1}^{p=n}a_pr_ps_p{}'\right)\frac{du}{dt}$$

$$+\left(s^2\sigma-\sum_{p=1}^{p=n}a_pr_ps_p{}^2\right)\frac{d^2u}{dt^2}+\ \ldots\ldots\ +\left(s^{n-1}\sigma-\sum_{p=1}^{p=n}a_pr_ps_p^{n-1}\right)\frac{d^{n-1}u}{dt^{n-1}},$$

where $\sigma=a_1r_1+a_2r_2+\ \ldots\ldots\ +a_nr_n$,

Now if the body consists of only two substances, then we have

$$v+\left(\frac{r_1}{k_1}+\frac{r_2}{k_2}\right)\frac{dv}{dt}+\frac{r_1r_2}{k_1k_2}\frac{d^2v}{dt^2}=(a_1r_1+a_2r_2)u+\left\{r_1r_2\left(\frac{a_2}{k_1}+\frac{a_1}{k_2}\right)\right\}\frac{du}{dt}$$

of the materials is indifferent, so that if there are several blocks of the same substance, they may be subdivided, or united without altering the phenomena. An investigation of the cases in which the materials are arranged otherwise than in rectangular blocks of unit depth, would lead to similar results, and if we may assume our hypothesis to be true for other kinds of strain than shear, then the above equation may be proved to be true for any strain of any heterogeneous body strained in any way.

The integration of the general differential equation (10), where v is constant, is, putting $\frac{dw}{dt}$ for u,

$$w = \mathrm{A} + \mathrm{B}t + \mathrm{C}\epsilon^{-\gamma t} + \mathrm{D}\epsilon^{-\delta t} + \ldots$$

where γ, δ, &c., are roots of the equation

$$x^n + \frac{c_2}{c_1}x^{n-1} + \ldots + \frac{c_n}{c_1}x - \frac{b_n v}{c_1} = 0,$$

and when v is maintained at zero during recovery after removal of the load

$$w = \mathrm{A}_1 + \mathrm{C}_1\epsilon^{-\gamma t} + \mathrm{D}_1\epsilon^{-\delta t} + \ldots$$

Of course if complete tables of the values of u and v, and their differential coefficients, could be given for all times during any experiment, it would be best to use the general differential equation in determining the constants. The method we have adopted is essentially the same as that we have employed in the analogous cases for Leyden jars, voltameters, &c. The wavy nature of some of the curves we have experimentally obtained connecting strain and time in strained beams, indicates that perhaps the equation given above may have imaginary roots, and that possibly there may be terms in our solutions of the shape

$$\mathrm{C} \cos bt\, \epsilon^{-\beta t}.$$

Much, however, of the waviness we have observed, we have traced to variations of temperature, as it generally has a periodic time equal to 24 hours.

Our theory for strained beams and twisted wires we have tested to about the same extent as Professor Clerk Maxwell's theory for Leyden

where v is the load on the beam or couple twisting the wire, and u is $\frac{dw}{dt}$ if w is the deflection of the beam or twist of the wire.

If v is constant then the equation becomes

$$\frac{d^2w}{dt^2} + g_1\frac{dw}{dt} + g_2 = 0,$$

g_1 and g_2 being certain functions of a, k, and r, and the solution of this is

$$w = \mathrm{A} + \frac{g_2}{g_1}t - \mathrm{B}\epsilon^{-g_1 t}.$$

jars has been tested, and we find the same kind of confirmation. For example, the following is one of the many experiments we tried :—

September, 1877.

A circular glass rod 128 centims. long, 0·832 centim. in diameter, rested on two knife edges 117 centims. apart, and was loaded with a weight of 400 grms. hung at its centre. The deflections of the rod were observed by a ray of light coming from a rigidly fixed lamp, falling on a mirror rotated by the bending of the rod, and reflected on to a rigidly fixed vertical scale. To prevent errors arising from slight alterations of the position of the flame, it was always focussed so that the image of two cross-wires fixed in the lamp slit and formed by a fixed lens always occupied the same position, when not reflected by the mirror attached to the deflected rod, and it was the position of this image, after being reflected by this mirror, which was observed on the vertical scale. After the beam had been loaded for 145·55 hours, observations of the increasing deflection being taken all the time, the load was removed. For several hours after the load had been removed, the actual observations themselves needed very little correction by drawing a curve, afterwards the slight waviness of the true curve due to changes of temperature had to be eliminated. In the several tables of observations of loaded and relieved beams (each experiment lasting two or three weeks), we have given the temperature of the room at the time of making the observation.

By the method of reduction adopted, we find that during the recovery of the rod from t equal 20, to t equal 240 (1=1·25 hours) the curve is almost purely logarithmic, and we find by calculation that

$$w_1 = 10{\cdot}6 + 224{\cdot}79\epsilon^{-0{\cdot}01296t},$$

and the column of values of $(w-w_1)$, in the following table shows that this expression satisfies the observations, very accurately for a very long period, namely, 220 on our scale of time, or 275 hours. Taking these numbers, we have calculated w_2, the next term in the solution of the differential equation, and find that

$$w_2 = 138{\cdot}7\epsilon^{-0{\cdot}3125t},$$

so that the last column of the table shows the inaccuracy of the assumption—

$$w = 10{\cdot}6 + 224{\cdot}79\epsilon^{-0{\cdot}01296t} + 138{\cdot}7\epsilon^{-0{\cdot}3125t},$$

an inaccuracy which is of very little importance from

$$t = 2{\cdot}4 \text{ to } t = 240,$$

and even this inaccuracy we have found is to some extent due to the fact that the temperature of the room was steadily falling at that time from 26°C. to 21° C.

Recovery of the Glass Beam after being loaded for 145·55 hours with 400 grms.

Time (1=1·25 hours) reckoned from removal of load.	Observed value of *w*.	$w-w_1$.	$w-w_1-w_2$.
0 load on	6475·0		
load off	....		
0·005	600		
0·4	411·3	177·0	154·4
1·4	340·8	109·4	19·9
2·4	300·6	71·9	6·5
3·4	275·6	49·9	1·9
4·4	278·5	35·6	0·5
5·4	246·3	26·1	0·4
6·4	237·1	19·7	0·9
8·0	224·6	11·4	0·0
10·0	214·2	6·2	0·1
12·0	206·9	3·9	0·6
14·0	200·6	2·5	0·8
16	195·0	1·7	0·8
18	189·8	1·2	0·7
20	184·5	0·4	0·1
30	162·8	−0·2	−0·2
40	145·3	0·8	0·8
50	131·0	2·8	2·8
60	116·3	2·4	2·4
70	103·2	1·9	1·9
80	91·5	0·2	0·2
90	81·2	0·6	0·6
100	72·3	0·2	0·2
110	64·7	0·1	0·1
120	58·0	0·0	0·0
130	52·5	0·2	0·2
140	47·7	0·5	0·5
150	43·3	0·5	0·5
160	39·3	0·4	0·4
170	35·8	0·4	0·4
180	32·7	0·3	0·3
190	29·8	0·0	0·0
200	27·3	−0·1	−0·1
210	25·1	−0·3	−0·3
220	23·2	−0·4	−0·4
230	21·6	−0·4	−0·4
240	20·0	−0·4	−0·4

There cannot be much doubt of the fact, that electric induction through dielectrics, and magnetisation of iron, are really phenomena of stress and strain, and it is probable that the correctness of Ohm's law when applied to dielectrics is dependent on the theory of continued strain, which we have just given, and which we are attempting to prove. In addition to experiments such as we have described, we have been making others to find whether beams which are kept in a state of strain rise in temperature through internal friction, which is of course analogous with electric resistance, and in addition to see whether a soft steel magnet acted upon by a constant current also rises in temperature; but hitherto these heating effects, if they exist, have been too small for our instruments to detect. In speaking of beams we refer especially to glass beams; a beam of sealing-wax would probably under the same circumstances perceptibly rise in temperature.

As a further example of the reductions which we are describing in this paper, we give a table of observed and calculated values of the diminishing deflection of a glass fibre, which had been twisted for some days, and then released. The fibre was 85 centims. long, of a mean diameter of 0·034 centim. suspended vertically, so that its upper end could not turn in the support, and carrying a weight of 211·6 grms. Attached to this weight was a vane dipping into water to diminish the rapidity of the swings.

The untwisting of the vertical glass wire was observed by the reflection of a ray of light, coming from a fixed lamp, by a mirror rigidly attached to the wire at its lower extremity. The spot of light, or rather the image of the cross-wires in the lamp slit, moved over a horizontal semicircular scale graduated in millimetres, and one metre in radius, with the mirror turning at its centre. An arrangement similar to that employed in our deflected beam experiments was used to obviate errors arising from difference of position of the flame of the lamp on different days.

The mirror and the weight having been kept twisted through a right angle for about a week, the deflecting couple was suddenly removed, and time readings of the diminishing deflections taken. As at first the deflection diminished with considerable rapidity, and as in addition the spot of light described a series of oscillations about the varying mean position, the system of registration we employed was as follows:—The position of the spot of light on the scale at the end of every swing was observed as accurately as possible, and the time recorded by simultaneously pressing down the key of a "break circuit chronograph." From these successive time observations of the swings about a varying mean position, the true curve was drawn in accordance with a method mathematically developed by us.*

The reduction of this curve by the method previously explained leads to the equation

$$w_1 = 72{\cdot}8 + 35{\cdot}55\epsilon^{-0{\cdot}0569t},$$

where w_1 is the deflection, and t the time from the moment of removing the deflecting couple, the unit of t being 20 seconds. Several of the earlier observations have been omitted in the following table, as it is not until after 40 seconds that the above equation represents accurately the torsional strain. From this time, however, until the end of the experiment, the equation is very accurate.

Recovery of Glass Wire previously subjected to Torsional Strain.

November 16, 1876.

Time from moment of removing deflecting couple.	Observed deflection w.	w_1 calculated from formula.	Difference $(w-w_1)$.
0·7	143·5	107·0	36·5
1·0	131·5	106·4	25·1
1·5	118·4	105·4	13·0
2	110·4	104·5	5·9
3	102·9	102·8	0·1
4	100·4	101·1	0·3
5	98·7	99·5	−0·8
6	97·2	98·1	−0·9
7	96·0	96·7	−0·7
8	95·0	95·4	−0·4
9	94·0	94·1	−0·1
10	92·9	92·9	0·0
11	91·9	91·8	0·1
12	90·8	90·8	0·0
13	89·9	89·8	0·1
14	88·8	88·8	0·0
15	88·0	88·0	0·0
16	87·1	87·1	0·0
17	86·3	86·3	0·0
18	85·6	85·6	−0·1
19	84·9	84·9	0·0
20	84·3	84·2	0·1
21	83·6	83·6	0·0
22	82·9	83·0	−0·1
23	82·2	82·4	−0·2
24	81·9	81·9	0·0

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* "Journal Soc. of Telegraph Engineers," vol. v, 1877, p. 391.

As a further example of the reduction by our method of the results of observation, we give five tables of experiments made with a special form of voltameter devised so as to enable experiments to be made with a gas-freed liquid, or a liquid more or less saturated with any particular gas, and with the platinum plates previously free from gas, or made to occlude any special gas. The voltameter itself was something like the upper part of a Geissler's mercury pump; that is, by lowering a column of mercury the electrolyte could be lowered from the platinum plates, leaving them in a vacuous space. The electrolyte of course commenced to boil, and to rapidly free itself from gas. By a valve this gas was allowed to escape, on again raising the mercury column, and the liquid above it. This operation being repeated a number of times, the liquid being left for a considerable period under the vacuous space towards the end of the operation, a large portion of the gas in the liquid could be removed. In order to remove the gas occluded in the platinums they were not, as is usual in voltameters, made in the form of plates, but each was a platinum spiral, the two ends of which protruded to the outside of the voltameter. By means of a small Grove's battery, each of these spirals could be made red hot in the vacuous space produced when the electrolyte was lowered, and thus a voltameter was obtained with gas-freed platinums in a gas-freed liquid, or by subsequently bubbling through a stream of a particular gas, with platinums in a liquid, with more or less at will of that gas occluded.

As these platinum spirals presented but a small surface, and as they were necessarily some little distance apart, the currents charging and discharging such a voltameter, when only one Minotto's cell was employed, were rather small, so that it was necessary to employ a fairly delicate reflecting galvanometer. From the first elongation of the spot of light on charging or discharging the voltameter, we could calculate the time integral of the current in the first half swing of the needle, and the subsequent time readings of the diminishing deflection were ascertained by an experienced observer, dotting with a pencil on the horizontal cylinder revolving uniformly (referred to at the commencement of the paper) the limits of the excursions of the spot of light in the successive swings about the varying mean position, the locus of which was subsequently drawn in by our method already referred to.

In the following table u is the observed current flowing into the voltameter to charge it at the time t, from the moment of applying one Minotto's cell; the voltameter contained distilled water well freed from gas, and the platinum spirals were kept red hot for some time in the vacuous space, before immersion in the liquid. The solution of the general differential equation leads to

$$u'=86{\cdot}2+85{\cdot}7\epsilon^{-0{\cdot}06245t},$$

which from the following table, it will be seen accurately represents the charging current from t equal 7 to t equal 25. After the current had flowed into the voltameter for 1 hour and $7\frac{1}{3}$ minutes, the cell was removed, and the voltameter discharged through the galvanometer, the connexions of which were simultaneously reversed, so that the discharge deflection might be on the same side as the charge deflection, and therefore less swinging about of the needle produced at the moment of discharge. In the second of the following tables u is the discharge current at a time t from the commencement of the discharge, and u' is calculated from the equation

$$u'=30{\cdot}90\epsilon^{-0{\cdot}0553t},$$

which accurately represents the discharge current from t equal 5 to t equal 25. The two exponents in the solutions of the differential equation for the current flowing in during charge, and the current flowing out during discharge, are

$$-0{\cdot}06245t \text{ and } -0{\cdot}0553t,$$

which, considering that the resistance of the galvanometer, battery, and connexions could not be neglected in comparison with that of the voltameter itself, are remarkably nearly equal to one another, being both—$0{\cdot}06t$ to two places of decimals.

When it was desired to measure the time integral of the discharge current during the first half swing of the galvanometer needle, rather than the deflections, a short time after the commencement of the discharge the galvanometer was short circuited for a sufficient length of time before the Minotto's cell was removed for the needle to come to rest at zero.

Charging with one Minotto's Cell of a Voltameter containing Distilled Water well freed from Gas, and the Platinum Spirals heated red hot *in vacuo*.

August, 1876.

Time in minutes.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
1·5	200	164·2	35·8
2	187·4	161·9	25·5
3	173·7	157·3	16·4
4	163·9	153·1	10·8
5	156·0	148·9	7·1
6	149·3	145·1	4·2
7	143·5	141·6	1·9
8	138·8	138·2	0·6
9	134·9	136·0	−1·1

Time in minutes.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
10	131·7	132·1	−0·4
11	129·0	129·3	−0·3
12	126·5	126·7	−0·2
13	124·0	124·3	−0·3
14	121·7	121·9	−0·2
15	119·7	119·8	−0·1
16	117·8	117·8	0·0
17	115·9	115·9	0·0
18	114·0	114·0	0·0
19	112·4	112·4	−0·1
20	110·7	110·8	−0·1
21	109·2	109·3	−0·1
22	107·8	107·9	−0·1
23	106·5	106·6	−0·1
24	105·3	105·3	0·0
25	104·2	104·2	0·0

Discharge of the same Voltameter after being charged with one Minotto's Cell for 1 hour and $7\frac{1}{3}$ minutes.

Time in minutes from the moment of discharging.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
0 before discharge	286·0		
0·5 after discharge	79·5	30·1	49·4
1	60·8	29·2	31·6
2	41·0	27·7	13·3
3	32·8	26·2	6·6
4	28·0	24·8	3·2
5	24·7	23·4	1·3
6	22·5	22·2	0·3
7	20·5	21·0	−0·5
8	19·4	19·9	−0·5
9	18·2	18·8	−0·6
10	17·0	17·8	−0·8
11	16·5	16·8	−0·3
12	15·9	15·9	0·0
13	15·1	15·1	0·0
14	14·3	14·3	0·0
15	13·5	13·5	0·0
16	12·8	12·8	0·0
17	12·1	12·1	0·0
18	11·4	11·4	0·0

Time in minutes from the moment of discharging.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
19	10·8	10·8	0·0
20	10·2	10·2	0·0
21	9·7	9·7	0·0
22	9·2	9·2	0·0
23	8·7	8·7	0·0
24	8·2	8·2	0·0
25	7·8	7·8	0·0

If the platinums after absorbing gas were merely kept for a long time in the vacuous space above the distilled water, but not *heated* to assist the driving out of the gas, then after raising the water to surround the wires, the charge current as observed on applying the Minotto's cell is given under u in the following table; while u' is calculated from

$$u'=96·3+79·43\epsilon^{-0·64t},$$

which is the solution of the differential equation, and which represents the charging phenomena almost from the moment of applying the Minotto's cell.

Charging; Gas-freed Distilled Water, Platinum Spirals not heated.

August, 1876.

Time in minutes.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
0·5	165·0	153·9	11·1
1	138·5	138·1	0·4
2	118·0	118·3	−0·3
3	107·5	107·9	−0·4
4	102·4	102·4	0·0
5	99·5	99·5	0·0
6	98·0	98·0	0·0
7	97·2	97·2	0·0
8	96·7	96·7	0·0
9	96·5	96·5	0·0

When the distilled water and the platinum spirals were throughly aërated, the charging current measured experimentally is shown under u in the following table; while u_1 is calculated from

$$u'=40·25+101·25\epsilon^{-0·087t}.$$

Charging with one Minotto's Cell, Distilled Water and Platinum Spirals thoroughly aërated.

August, 1876.

Time in minutes.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
1·5	144·0	129·3	14·7
2	134·4	125·5	8·9
3	120·9	118·3	2·6
4	111·7	111·8	−0·1
5	105·0	105·8	−0·8
6	99·7	100·3	−0·6
7	95·3	95·3	0
8	90·7	90·7	0
9	86·5	86·5	0
10	82·6	82·6	0
11	79·1	79·1	0
12	75·8	75·8	0
13	72·8	72·8	0
14	70·1	70·1	0
15	67·6	67·6	0
16	65·3	65·3	0
17	63·2	63·2	0
18	61·3	61·3	0
19	59·1	59·1	0
20	57·9	57·9	0
21	55·4	55·4	0
22	55·1	55·1	0
23	53·8	53·8	0
24	52·7	52·7	0
25	51·7	51·7	0

When the distilled water and the platinum spirals were thoroughly freed from air, and subsequently saturated with hydrogen, the charging current measured experimentally is given under u in the following table; and u' is calculated from

$$u'=120+38{\cdot}02\epsilon^{-0{\cdot}125t}.$$

Charging with one Minotto's Cell, Distilled Water, and Platinum Spirals, thoroughly freed from Air and then saturated with Hydrogen.

August, 1876.

Time in minutes.	Observed current u.	u' calculated from the formula.	Difference $(u-u')$.
1·35	196·0	152·1	43·9
2	170·5	149·6	20·9
3	155·8	146·1	9·7
4	148·0	143·0	5·0
5	142·5	140·3	2·2
6	138·5	137·9	0·6
7	135·5	135·8	−0·3
8	133·5	134·0	−0·5
9	132·2	132·3	−0·1
10	130·9	130·9	0
11	129·6	129·6	0
12	128·5	128·5	0
13	127·5	127·5	0
14	126·6	126·6	0
15	125·8	125·8	0
16	125·1	125·1	0
17	124·5	124·5	0
18	120·0	124·0	0
19	123·5	123·5	0
20	123·1	123·1	0

The equations, therefore, which we have obtained, as solutions of the general differential equation, to represent the phenomena of *charging* our voltameter with one Minotto's cell, when the distilled water and the platinum spirals were in different states as regards the gas absorbed are:—

I. Distilled water freed from gas, platinum spirals left for some time in the vacuum above the water, but not heated—

$$u'=96{\cdot}3+79{\cdot}43\epsilon^{-0{\cdot}64t}.$$

II. Distilled water freed from gas, spirals made red hot *in vacuo*—

$$u'=86{\cdot}2+85{\cdot}7\epsilon^{-0{\cdot}06245t}.$$

III. Distilled water, and platinum spirals thoroughly aërated—

$$u'=40{\cdot}25+101{\cdot}25\epsilon^{-0{\cdot}087t}.$$

IV. Distilled water, and platinum spirals thoroughly freed from air, and then saturated with hydrogen—

$$u'=120-38{\cdot}02\epsilon^{-0{\cdot}125t}.$$

These equations, as has been seen, give with considerable accuracy the actual value of the current flowing into the voltameter at any moment during the greater period of the charging in the different cases. The difference between the curves of charge in the different cases is partly due to the different state of the distilled water and the platinums, and partly to the level of the mercury on which the water rested being slightly different in the different cases. To remove the possibility of this latter cause of variation in the curves, the construction of the voltameter was altered, so that the level of the mercury under the water was always so far away from the bottom of the platinum spirals, that any slight alteration in the level did not at all affect the curve of charging, or discharging. We refrain, however, in this preliminary note from giving any of the reductions of the curves we have experimentally obtained with this improved form of voltameter.

We may remark that the methods usually taken for measuring the resistance of liquids are quite misleading if there is any analogy between voltameters and condensers with solid dielectrics. For instance, on our assumption, the constant terms in the above expressions for u, measuring, as they do, the current after a great time has elapsed, really represent the ratio of the electromotive force of the battery to the total resistance of the voltameter and connexions, and therefore their reciprocals are measures of the several resistances. We need hardly mention that in ordinary experiments the current is measured for as short a time as possible, "to avoid polarisation, as it is said." For small values of the time the current flowing into a voltameter, or liquid condenser, is very great, and in our opinion may be regarded as nearly infinitely great; for the first rapid rush of electricity is due to charging the voltameter as a condenser is charged; and if this rush is not quite instantaneous, it is because the electromotive force charging the voltameter does not immediately reach its constant value. But just as the potential of the end of a cable suddenly attached to a battery will acquire its maximum value more and more rapidly as the resistance of the battery and connexions is made less and less, so by making the resistance of our single element used to charge the voltameter less and less, and by measuring the time integral of the current flowing in for shorter and shorter periods of time, that is, by using galvanometer needles of more and more quickness of vibration, a better and better approximation to the *static capacity* of a voltameter is arrived at. And we found that when we had reduced the resistance of our single cell to 0·04 of an ohm, by using in this single cell a zinc plate having 15 square feet in area, the more and more carefully we made the experiments, the more and more nearly did the first discharge of a voltameter, which is always less than the first charge, become equal to it. If the charge and discharge in voltameters do not prove to be as nearly equal as in glass Leyden jars, it is because the true conduction current is so much greater proportionally in water than in glass. In certain experiments, which we hope to have the honour of describing fully in a future paper, we have been able to make an approximation to the time integral of this *conduction current* during the small times of first charge and discharge, and by subtracting it from the measured charge, or adding it to the measured discharge, we have obtained what we may reasonably call the true induction charge in a voltameter. We mention this now since, although years ago Mr. Cromwell Varley referred to the electrostatic capacity of voltameters, the investigation of this instantaneous charge has been neglected, because this charge is small compared with the residual one, just as the important investigation of the residual charge in Leyden jars was for a long time not carried out, because the residual charge was small compared with the instantaneous charge.

For assistance rendered in the carrying out of the series of experiments, the results of a few only of which are given in this preliminary account, we have to thank the following of our students:—Messrs. Fujioka, Igarashi, Iida, Inoguchi, Kasai, Kawaguchi, Mita, Nobechi, Oshima.

III. "On the Structure and Development of the Skull in the Batrachia. Part III." By W. K. Parker, F.R.S. Received April 29, 1880.

(Abstract.)

Some of the work brought forward in this paper was in hand before the first part was in print. That initial piece of work dealt only with the formation of the skull in the common frog, but it was followed by another which appeared in the "Philosophical Transactions" in 1876, which treated of the skulls of the *common* and of the "aglossal" toads.

Of the latter types only two kinds are known, viz., the nailed toad of the Cape (*Dactylethra*), and the monstrous toad of Surinam (*Pipa*). All the bulk of the Batrachia are included in the sub-group "Opisthoglossa," these have a tongue, and in most cases it is free *behind* and not in front; the "Proteroglossal" Batrachia are very few in number, and the character itself (as Dr. Günther informs me) is not well pronounced.

I have now worked out the skull, in one or more stages, in about a *tithe* of the known species, and in my second paper in both of the aberrant ("aglossal") types; in them this was done in various stages.

I am not aware that there is any "order" of any "class" in the Vertebrata where so large a percentage of species has been, or indeed,

need be, worked out, either in the skull or in any other part of their organisation.

That which calls for it here is the great and unlooked-for polymorphism of the species; I may explain this by saying that the skull, in really important modifications, differs more in the species of some of the genera than it does in the orders of some of the classes. As an instance, it would be no easy thing to find a malacopterous fish differing from an acanthopterous type, in deep-seated essential matters, so much as the common toad does from the other native species, viz., the *Natterjack;* and the common frog has only about half as many cranial elements as the bull-frog of North America.

If the metamorphosis of a single species be worked out exhaustively, it gives a range of structural characters which rises up from a larval creature on the level of the lamprey, to a reptilian form, not far below the Chelonia, and evidently related (obliquely, not genetically) to that "order."

Moreover, whilst the "opisthoglossa" have larvæ with suctorial mouths, and a *quasi-petromyzine* structure altogether, the larvæ of the "aglossa" need only to be arrested as larvæ, and to acquire a dense bony armature to be very close counterparts of the most *bizarre* forms of the ganoids of the "old red sandstone," such as *Pterichthys* and *Coccosteus.*

The Batrachia show some remarkable things in their metamorphosis, both as to the *size* their larvæ obtain, and the *time* during which metamorphosis is taking place.

In the bull-frog (*Rana pipiens*) the larvæ attain the length of about 5 inches, and take two or three years for their transformation; they may be hindered in this, and be made to take twice that time. In these the larvæ bear a moderate relation, as to size, to the adult form, which may be 7 inches long, although tail-less.

But in a frog from the neotropical region (*Pséudis*) scarcely larger than our native form, the tadpole attains the length of nearly a foot, the tail acquiring a breadth of 4 inches.

As zoologists well know, it is easy to procure *tadpoles* of this species, but very hard to get an adult. I am of opinion that the adult condition is not attained until after many years; and it suggests itself to me that this species may be the not remote descendant of a type which did not finish its *anural* metamorphosis.

On the other hand, some of the neotropical forms have very small tadpoles. *Bufo chilensis*, a large toad, has them about half the size of those of our common native Batrachia, and the newly-metamorphosed individuals are no larger than a house-fly.

But in *Pipa* the small larvæ are thoroughly metamorphosed in the maternal dorsal pouches, and at first only do they show a trace (and only a trace) of branchial tufts.

These tadpoles, which never see the light as such, have wide mouths (not suctorial), and so also have the tadpoles of the other waif of the sub-order "Aglossa," viz., *Dactylethra.* In that kind, however, the larvæ become large, and are a long while undergoing their transformations, which take place in the water, according to rule.

In the skull of the adults much variation is evidently due to the different size to which the species attains; some, as the bull-frog, are as large as the common Greek tortoise, others grow scarcely larger than a bluebottle fly. As a rule, these small kinds show two kinds of modification—they are apt to retain certain larval characters, and they are apt to acquire generalized characters, such as do not normally appear in this group, which is very remarkable for the fewness of the parts or elements composing the adult skull.

Some of the large forms, as *Rana pipiens*, have many investing bones in their skull, such as must be looked for again in archaic and extinct types, whilst others, as *Ceratophrys* and *Calyptocephalus*, have a cranial armature that is dense, extended, and almost "ganoid;" this kind of skull, however, is found in middle-sized types also, as in *Pelobates* and *Nototrema.*

In the terminal suctorial mouth of the larva of the Opisthoglossa, the mandibular pier, and its free "ramus" are carried to the front of the head. After transformation, in the larger kinds, the gape is carried behind the head, as in the crocodile; it can be guessed how much modification such a change as this will necessitate.

But it is evident that a low suctorial fish, such as the *tadpole* is, must have altogether a totally different kind of skull and skeleton to that of an active, noisy, intelligent, more or less terrestrial reptile, such as the frog becomes.

This necessarily great change involves some very curious and instructive anachronisms, so to speak, in the appearance of various parts and organs.

A low suctorial fish would have no fenestra ovalis nor stapes, and in the tadpole it is some time before these appear.

The low (urodelous) Amphibia have, in most cases, the upper hyoid element suppressed, sometimes it is present, serving as a rudimentary "*columella auris.*"

In most Batrachia this part does not appear until after transformation, and in some kinds not at all. This part especially shows how the individual is gradually changed, and makes it clear why so many variations should occur in genera and even species.

I have arranged the forms, whose skulls are worked out zoologically, taking in the results of my earlier observations, already published in the "Philosophical Transactions."

These now amount to *seventy-five;* four of these have already been treated of, and of the new types four kinds are only larval; nevertheless, I am able, after many years of labour, to give an account of the adult skull in seventy kinds, and of the larval—in one or several stages—in a good number of the species.

The illustrations are, for the most part, put in a similar manner in all the plates; and, as to *size*, I have not made the skulls of the dwarf kinds much less than those of the large types.

I am satisfied that these figures and descriptions will be found useful, not merely for comparison of the types of larval and adult Batrachian skulls one with another, but I believe that they will throw light upon, and in turn be illuminated by, all that we can learn of the structure and development of the skull in the other types of Vertebrata.

With regard to the geographical distribution of the Batrachia, there are many things of importance which I have rather hinted at than expressed in this paper.

There is a sort of *facies* or character about the allied types of any great geographical region which makes me satisfied that certain external characters repeat themselves again and again in different parts of the world.

Thus, the types of frogs that have dilated toes are evidently more nearly related to those with pointed toes of the same region than they are even to the broad-toed types of some distant region.

I should be inclined to derive the narrow-backed tree frogs of Australia from the sharp-toed frogs of the same region; the same with those of India, and the same with those of the nearctic and neotropical territories.

The true frogs ("Ranidæ") of India have many things in common, as also have the true frogs of North America; the same may be said of the sub-typical frogs, or "Cystignathidæ."

On the whole, the European and Indian territories yield the highest kinds; Australia and South America the lowest and most generalized.

IV. "On the Relation of the Urea to the Total Nitrogen of the Urine in Disease. No. I." By W. J. RUSSELL, Ph.D., F.R.S., and SAMUEL WEST, M.B. Oxon. Received May 6, 1880.

In the valuable series of papers upon the excretion of urea, communicated by Professor Parkes to the Royal Society, he showed that in health 90 per cent. of the nitrogen in the urine, was eliminated in the form of urea. It seemed to us of considerable interest and importance to ascertain whether in disease this statement still held good, or whether, as indeed seemed probable, under altered conditions, nitrogen might not be excreted in some other form. With the view of determining this point, the following experiments were undertaken.

The cases upon which the observations were made fall into two groups—the first, a series taken at random from the hospital, the patients suffering from various diseases, and being under various conditions as regarded diet, muscular exertion, &c. In the second series, the patients were healthy, and placed under conditions as far as possible constant, the amount of diet being fixed, and the patients at absolute rest.

The quantitative estimations were made in the following way. The nitrogen in the urea was determined by the process which we have previously described,* in which the urea is decomposed by sodic hypobromite, and the volume of nitrogen evolved is measured. At first, the total nitrogen was determined by the soda-lime process, but finding that in some cases this method was untrustworthy, we abandoned it, and in all the following experiments the total nitrogen was estimated by burning the residue with oxide of copper, and measuring over water the volume of nitrogen obtained. This volume was of course afterwards corrected for temperature and pressure.

In most cases, 5 cub. centims. of the urine was used. This was very slightly acidulated by adding a small crystal of pure oxalic acid, and evaporated as rapidly as possible in a small basin on a water-bath, nearly to dryness. The residue was thoroughly incorporated with oxide of copper, introduced into a combustion tube, and the basin thoroughly cleaned with fresh portions of oxide.

In order to ascertain whether any loss of ammonia occurred during the evaporation, control experiments were made by adding, without previous evaporation, a second sample of 5 cub. centims. of the urine to oxide of copper already in a combustion tube, and proceeding with the combustion slowly, but in the ordinary way. The same amount of nitrogen was obtained by both processes.

Although the determination of the nitrogen was the special object of our research, we thought it well in each case to complete the analysis by determining the amount of solid matter in solution, and from it approximately the amount of organic and inorganic matter, as well as the amount of chlorine, and the specific gravity.

To determine the solid matter in solution, 5 cub. centims. of the filtered urine was evaporated to dryness in a small light platinum dish, and heated to 110—120°, till the weight remained constant. The residue varied very much in appearance, sometimes it was hard and granular, at others remaining in a sticky condition.

The residue was heated to dull redness over a lamp, and what remained we have called inorganic residue. It was found in some cases impossible to completely burn away all carbonaceous matter

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* "Journal of Chemical Society," 1874.

without raising the temperature to such a point that chloride of sodium would be volatilised. The weight, however, of carbon remaining in these cases was exceedingly small. The amount of chlorine in the urine was ascertained by dissolving this residue in water, and adding a standard solution of silver nitrate.

Obviously, these latter determinations are not of the same degree of accuracy as are those of the nitrogen. Still they strikingly point out the variations in the composition of these urines, and give important comparable results.

The first series (Table I), consisting of twenty-three observations, falls into several small groups. The relation of the urea-nitrogen to the total nitrogen was, in all cases, calculated out in percentage amounts (the total nitrogen being taken as 100), and the mean of each group of observations given.

The first group (Table II) consists of six cases of pneumonia, and in these the urea-nitrogen represents 90 per cent. of the total nitrogen.*

The second, of two cases of jaundice, with two determinations in each. The mean of the first giving 85·7 per cent.; of the second, 90·2 per cent.

The third, of two cases of albuminuria, in which the mean is 86 per cent. In these observations, the albumen was previously precipitated and removed. In a third case the determination was made without previously removing the albumen. In this latter, the percentage was 63·6.

The fourth group consists of a collection of cases of various kinds. One of pyæmia, one of typhoid fever, rheumatic fever, acute dysentery, pleurisy, hepatic abscess, and leucocythæmia, two of erysipelas, and two of diabetes, making eleven in all. The mean percentage of them all is 93·8.

The lowest percentage in this first series is found in the cases of albuminuria, and of jaundice, a fact of interest as bearing upon the place of production of urea.

The second series consists of eighteen determinations made upon three cases, in which the diet was fixed, and the patient in a condition of absolute rest (cf. Table III).

These give a mean of 90·1 per cent. The mean of all the cases in the two series is 89·3, or, if the cases of albuminuria and jaundice be excluded, 91·3 per cent., and this agrees almost exactly with the results of Professor Parkes' experiments, in which the mean arrived at is 91 per cent.

If a calculation be made from the tables given in books of the amounts of the urinary constituents in a normal urine, the urea is found to represent about 96 per cent. of the total nitrogen, or if allowance

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* It is worthy of note that in none of these cases is the percentage of urea high; a fact to which attention has been drawn in a previous paper. "Med.-Chir. Soc. Trans.," vol. lviii.

be made for the nitrogen contained in the pigments and extractives, which are usually not specified, the real percentage would be somewhat lower.

We may, therefore, assume that the urea-nitrogen may be taken as the measure of the total nitrogen, and that this may be approximately determined by adding 10 per cent. to the amount of urea-nitrogen.

This is, however, only true if the mean of a large number of observations be taken, for there is no fixed relation between, on the one hand, the amount of the urine and the amount of the solids in it, or on the other, between the amount of the various solids *inter se*. These all vary independently in great measure of one another, and subject to no law which is at present determined, and stranger still, the relation of the urea-nitrogen to the total nitrogen, even in the same urine on closely consecutive days, is not constant, as the following table shows:—

I.	II.	III.
97·5	83·0	87·6
100·0	95·1	84·2
91·5	86·6	87·5
98·8	92·4	100·0
91·5		87·9
82·3		87·0
		82·0

These figures are taken from the tables (Table III) given in the second series of cases, and the variations are not due to altered conditions, for the patients were all at absolute rest, and upon a constant diet. If, then, such variations occur when the conditions are constant, they will certainly be not less where the conditions vary.*

Such variations render it necessary to be specially cautious in drawing conclusions from the results of a few experiments, and may explain the great difference in the observations of different investigators.

Besides the nitrogen, we determined, as we have already stated, the solid residue, the ash, and the chlorine. Of the chlorine, little can be said. Its variations are inexplicable, for although they in part depend upon the amount of chlorine ingested in the food, they seem subject to other influences which we do not at present comprehend.

The same may be said of the solid residue, and of the ash, and we can do little more than point to the great and irregular variations in the amount of each, which the tables show.

The result then of our observations is to prove that the chemistry of the urine remains essentially the same in disease as in health, and that the generalisation of Professor Parkes is true in either case. The urea

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* Cf. a similar series of observations by Dr. Byrne Power, "Dubl. Journ. of Med. Sc.," February, 1875.

Table I.—Giving Analyses of the Urine of Various Cases.

	Pneumonia.							Jaundice.						Albuminuria.				Erysipelas.		Diabetes, same case.		Leucocythæmia.	Pleurisy.	Acute Dysentery.	Hepatic Abscess.	Rheumatic Fever with Pericarditis.	Typhoid Fever.	Pyæmia.	Mean.
	1.		2.	3.	4.	5.	Mean.	1.		Mean.	2.		Mean.	Alb. preciptted.		Mean.	Alb. burnt.	1.	2.										
Total nitrogen	1·41	1·66	1·67	1·75	1·01	1·52	1·50	2·22	2·12	2·17	0·95	0·66	0·805	0·34	0·59	0·465	0·55	1·22	1·23	0·97	1·32	0·828	1·38	1·84	1·90	2·33	1·674	1·19	1·44
Nitrogen as urea	1·00	1·61	1·33	1·71	0·96	1·51	1·35	1·88	1·72	1·86	0·80	0·63	0·726	0·30	0·50	0·40	0·35	1·16	1·07	0·90	1·23	0·824	1·26	1·66	1·81	2·12	1·671	1·17	1·35
Solid residue............	4·40	5·68	...	...	2·86	2·83	3·94	4·5	5·28	4·89	2·46	1·79	4·25	1·59	2·02	1·80	2·85	4·48	3·69	4·51	5·33	2·65	4·50	...	...	5·64	4·24	3·02	4·23
Ash	0·478	0·464	...	...	0·308	...	...	...	...	...	...	...	...	0·72	0·80	0·76	0·78	0·544	1·12	1·7	1·7	1·86	0·584	...	...	0·83	1·16	0·80	1·14
C, H, & O (difference)	1·512	3·556	...	...	1·84	...	...	...	...	...	...	...	...	0·53	0·63	0·58	1·52	2·62	1·34	1·84	2·31	...	2·53	...	...	2·48	1·41	1·01	1·94
Chlorine..................	0·056	0·022	...	...	0·014	...	0·031	0·00	0·00	0·00	0·00	0·093	0·031	0·216	0·348	0·282	0·214	0·22	0·256	0·618	0·618	0·106	0·584	...	...	0·022	0·142	0·014	0·287
Amount of urine	...	...	...	...	...	...	...	...	...	...	600	1200	...	...	...	...	...	...	...	...	...	1300							
Specific gravity	...	...	...	...	...	...	...	1030	1022	...	1011	1010	...	...	...	...	...	...	1020	...	...	1020							

N.B.—The percentage is in all cases low.

N.B.—The chlorine is completely absent in three out of the four observations.

N.B.—The observations are all in the same case.

Table III.—Giving Analyses of the Urine of the three cases of Aneurism.

	Case I.							Case II.					Case III.							
	July 25.	July 27.	July 30.	August 7.	August 9.	August 10.	Mean.	December 5.	December 13.	December 21.	January 3.	Mean.	March 22.	March 25.	March 27.	April 3.	April 4.	April 8.	April 16.	Mean.
Total nitrogen	2·02	1·95	1·88	1·78	1·84	1·92	1·83	2·29	1·86	2·01	0·88	1·76	1·86	2·09	1·92	1·21	1·49	1·32	1·72	1·66
Nitrogen as urea	1·96	1·95	1·72	1·76	1·68	1·58	1·73	1·90	1·77	1·74	0·81	1·55	1·63	1·76	1·68	1·21	1·31	1·16	1·41	1·45
Solid residue	5·11	4·90	5·63	5·22	5·32	4·84	5·17	5·44	5·82	6·58	3·00	5·21	5·212	5·44	5·58	4·374	4·74	4·93	6·38	5·26
Ash	1·06	1·13	0·91	1·21	1·22	1·20	1·12	0·95	1·27	1·73	1·04	1·25	1·62	1·28	1·68	1·49	1·94	1·38	1·99	1·63
C, H, and O (difference)	2·03	1·81	2·84	2·23	2·26	1·72	2·22	2·20	2·69	2·84	1·08	2·2	1·73	2·07	1·98	1·674	1·31	2·23	2·57	1·974
Chlorine	0·106	0·148	0·092	0·170	0·241	0·229	0·164	0·085	0·312	0·582	0·419	0·35	0·337	0·227	0·533	0·505	0·561	0·376	0·688	0·461
Amount of urine (cub. centims.)	570	400	530	420	400	500	470	600	420	510	550	490	430	330	380	620	320	360	250	384
Specific gravity	1030	1030	1026	1030	1028	1027	1028	1030	1033	1032	1020	1030	1033	1032	1032	1024	1030	1024	1030	1029

may therefore be safely regarded as the measure of the total nitrogen, and as forming 90 per cent. of it.

Note.—An exception to this, of course, is found in the rare cases of acute yellow atrophy of the liver, but even in these cases it is doubtful whether the replacement of the urea by leucin and tyrosin is a constant phenomenon. In one case,* which clinically presented the features of acute atrophy of the liver, and in which, *post mortem*, the liver was found in the condition of acute fatty atrophy, the urea was still normally formed, leucin and tyrosin being absent.

Table II.—Showing the Percentage of Urea-Nitrogen in the different Groups of Cases.

Pneumonia (6 cases)	90·0
Jaundice (Case 1)	85·7
,, (Case 2)	90·2
Albuminuria (2 cases)	86·0
Collected cases	93·8
Dieted cases	90·1
Mean of all	89·3

Mean, excluding the jaundice and albuminuria cases, 91·3.

V. "On the Amount of Nitrogen excreted in the Urine by Man at Rest. No. II." By Samuel West, M.B. Oxon., and W. J. Russell, Ph.D., F.R.S. Received May 6, 1880.

Two methods of investigation have been adopted in inquiries of this kind. The first by stopping the ingestion of nitrogen, either by absolute deprivation of food, *i.e.*, by starvation, or by the giving of non-nitrogenous food. The second by reducing the ordinary diet to the lowest possible limit compatible with health. To the first objection may be made that the experiments are violent, and cannot be long maintained, and that they subject the body to most abnormal conditions. Such violence to the ordinary chemistry of the body produces its evidence in the excretions. The nitrogen in the urine under these circumstances varying even more irregularly and widely than under normal conditions, and the short possible duration of the experiments rendering the conclusions drawn, from the small number of observations, unsatisfactory.

From all these objections the second course of investigation is free, no violence is done to the body, and the observations may be prolonged

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* "Pathol. Soc. Trans.," 1880.

ad libitum. And here two courses are open, both of which lead in the same direction, though starting from opposite sides. The one which consists in taking, as Playfair and Edward Smith have done, the minimum diets upon which life has been sustained, and estimating in them the amount of nitrogen present. The other in reducing the diet to the lowest possible limit until physiological equilibrium is established, and then estimating in the urine the amount of nitrogen excreted. It is this latter course which we have followed.

The three cases we have examined were patients suffering from the same affection, viz., aneurism, a disease which produces mechanical rather than constitutional symptoms, and in these cases, so long as the treatment was carried on, produced no symptoms at all, so that for all practical purposes the patients may be regarded as healthy men. They were all placed under the conditions of the most absolute rest, not being allowed to sit up in bed, or even indeed to feed themselves. Their diet was then reduced till it was found that the health was suffering, and then increased until a condition was reached, which may be called one of "clinical equilibrium," when the health so far as could be determined clinically was perfect.

The condition of clinical equilibrium being reached, the amount of nitrogen in the food was determined by direct analysis.

In two of the cases the diet consisted of ten ounces of solids and ten ounces of liquids, distributed as follows:—

Meat	4 ozs.	Water..........	4 ozs.
Bread..........	3 ,,	Wine	3 ,,
Potatoes........	2 ,,	Tea.............	3 ,,
Butter..........	1 ,,		
	10 ozs.		10 ozs.

By calculation from Parkes' tables, this should yield 6·3 grms. of nitrogen. Analysis gave a somewhat higher number: in the first determination 7·07, and in the second 6·95. Cf. Tables IV and V.

In the third case the diet was 8 ozs. of solids and 8 ozs. of liquids, distributed in the same proportion. This by calculation from the preceding analysis should give about 5·6 grms. of nitrogen.

The analysis of the urine was made in the way described in the preceding paper. The tables give the result of 17 determinations.

Comparing now the amount of nitrogen ingested in the food, with the amount obtained from the urine, we find (Table V):—

	Nitrogen ingested.		Nitrogen in urine.
Case I	7·0	....	8·6
,, II	7·0	....	8·64
,, III............	5·6	....	6·4

In all the cases the amount in the urine is in slight excess of that in the food, so that we may fairly regard all the nitrogen here obtained as representing tissue waste, for there was no surplus in the food to increase the amount in the urine.

We obtain as the mean of these three cases $\frac{23\cdot64}{3}=7\cdot87$, or approximately 8 grms., which we therefore are justified in regarding as the minimum amount of nitrogen a healthy adult man excretes per diem. This is equivalent to 17 grms., or 260 grains of urea.

The weights of the patients could not of course be determined, but the first two weighed it is supposed about 140 lbs., the third about 130 lbs., at the time of the observations, which was in each case after the dieting had lasted already three or four weeks, the total durations of the treatment being about ten weeks.

It is interesting to compare with these observations the results obtained by the other methods of the investigation above referred to.

*I. Ranke repeated upon man the experiments which Bischoff and Voit had conducted upon the dog, and among them are two series of observations which illustrate the subject at present under consideration.

In the first no food at all was given, and the patients were kept at rest.

In one case 8 grms. of nitrogen were passed, in a second 10, and in a third 8·6. In a fourth case the amount was as low as 6†. And in another series of observations upon himself, Ranke found the amount passed in two starvation days to be 8 and 8·6 grms.

Nicholson made three estimations in the case of starving prisoners, and found as the mean of three days 8·6 grms‡.

Many other observers have noticed the rapid fall in the amount of nitrogen excreted during starvation.

(From 28 grms. to 14. "Moos. Henle Zeitschr.," vii, 291),
„ 30 „ 14. Schneller, "Schmidt's Jahrbuch," 1856, 10, p. 10.
„ 33 „ 14. Brattler, "Beitrag zur Urologie," München, 1856, p. 6.)

But the short duration of these experiments makes it probable that the minimum was not reached.

We have then 8 grms. as the mean of the only reliable determination at our command of the nitrogen excreted in the urine during starvation.

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* Kohlenstoff und Stickstoff, Auscheidung der ruhenden Menschen, "Archiv f. Anat. u. Phys.," 1862.

† "Physiol. der Menschen," Ranke, p. 509.

‡ Brit. Med., Journ., 1870, p. 70.

II. Upon a strictly non-nitrogenous diet the observations are not numerous:—

Ranke found 8 grms.,[*]
and Von Franque 7·5 grms.

III. Professor Playfair attacked the question from another side, by collecting from various sources the minimum diets upon which man could live, and to which he gave the name of subsistence diets, and by calculation the amount of nitrogen contained in them. This method gave him as a mean 9·2 grms., but his patients were none of them at absolute rest, but were performing during the day a certain amount of work.[†] Edward Smith in the same way by calculation from the diets of the spinners during the cotton famine found a somewhat larger amount of nitrogen (12 grms.),[‡] which agrees with the amount of nitrogen contained in Playfair's second class of small diets, but in all these cases the effect of muscular exertion is not eliminated.

IV. Unruh gives a series of three observations upon hospital patients kept at rest, and placed upon a restricted diet. Unfortunately he gives little description of the diet, except that it was fever diet, and consisted of beef-tea or broth, with an egg or two.

In the first, a case of cancerous obstruction, the amount of nitrogen was 8 grms. (17·5 urea). But this case is not altogether satisfactory from the amount of wasting accompanying this disease.

The other two were cases of syphilis placed upon fever diet, and kept at rest for the sake of the experiments; the first passed 8·6 grms. (18·6 urea), the second 7·5 grms. (16·2 urea).

The mean of these three cases is 8 grms. (17·5 urea).

The general results of the various series of observations may be roughly tabulated thus:—

I. Starvation. 8 grms.
II. Non-nitrogenous food. 8 grms.
III. Subsistence diet. 9 grms.
IV. Insufficient diet. 8 grms.
V. Clinical equilibrium. 8 grms.

A remarkable coincidence, considering the variety of the methods employed, and the different conditions under which the determinations were made.

We may therefore conclude that the minimum amount of nitrogen excreted by a healthy adult man is on the average 8 grms. in the 24 hours, this being equivalent to 17·5 grms., or to 260 grains of urea.

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[*] Ranke (*ut supra*).

[†] "On the Food of Man in relation to his useful Work." Edinb., 1865.

[‡] "Influence of Food," 1860.

Table IV.—Analysis of Food of Aneurism Patients.

	I.	II.
Solid food.		
Total weight	322·85	282·20
Water (loss at 115° C.)	188·43	157·90
Ash	0·94	3·50
C, H, and O (difference)	126·48	113·91
Nitrogen	7·00	6·89
	322·85	282·20
Liquids.		
Total weight	296·28	243·20
Water	288·92	237·10
Ash	0·32	0·23
C, H, and O (difference)	6·97	5·81
Nitrogen	0·07	0·06
	296·28	243·20
Total food (solids and liquid).		
Total weight	619·13	525·40
Water	477·35	395·00
Ash	1·26	3·73
C, H, and O (difference)	133·45	119·72
Nitrogen	7·07	6·95
	619·13	525·40

Table V.—Comparison of the Food, and of the Urine of the Aneurism Patients.

Ingesta (solid and liquid).

	Analysis I.	II.
Water	477·35	395·00
Ash	1·26	3·73
C, H, and O (difference)	133·45	119·72
Nitrogen	7·07	6·95
Total	619·13	525·40

Diet (as given).

Solids.

Meat	4 ozs., containing of nitrogen*			76 grains.
Bread..........	3 ,,	,,	,,	16·5 ,,
Potatoes	2 ,,	,,	,,	2·0 ,,
Butter	1 ,,	,,	,,	0·2 ,,
	10 ozs.			94·7 grains. or 6·3 grms.

Liquids.

Water	4 ozs.
Wine (port)	3 ,,
Tea............	3 ,,
	10 ozs.

Urine (the mean of each case).

	I.		II.		III.
Volume of urine	470 c.c.	....	490 c.c.	....	384 c.c.
Ash	5·264	....	6·125	....	6·26
C, H, and O (difference)	10·434	....	10·78	....	7·58
Nitrogen	8·601	....	8·624	....	6·374
Percentage of nitrogen as urea.	94·53		88·06		87·35

Urine percentage averages upon fixed diet.

	I.		II.		III.
Total nitrogen........	1·83	....	1·76	....	1·66
N as urea	1·73	....	1·55	....	1·45
Solid residue	5·17	....	5·21	....	5·26
Ash	1·12	....	1·25	....	1·63
C, H, and O (difference)	2·22	....	2·2	....	1·974
Amount of urine	470 c.c.		490 c.c.		384 c.c.
Specific gravity	1028 ,,		1030 ,,		1029 ,,

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* By calculation from tables given in Parkes' "Hygiene."

VI. "On the Artificial Formation of the Diamond." By J. B. Hannay, F.R.S.E., F.C.S. Communicated by Professor G. G. Stokes, LL.D., D.C.L., Sec. R.S. Received April 15, 1880.

In a preliminary notice, which the Royal Society has done me the honour of publishing in the "Proceedings," I gave a very short sketch of the work I have done which led me to a reaction whereby hard crystalline carbon has been produced. I have now the honour of laying a detailed account of the methods and results before the Society. As far back as September, 1879, I was searching for a solvent for the alkali metals, and tried experiments with many liquids and gases, but invariably found that when the solvent reached the permanently gaseous state chemical action ensued. This was the case even with hydrocarbons, the metal combining with the hydrogen and setting free the carbon. Paraffin spirit, boiling at 75°, was first used in experimenting, and the spirit contained a considerable amount of olefines; but even these unsaturated hydrocarbons seemed to be split up in like manner. The experiments were conducted in thick tubes from 1 to 1·5 millims. internal, and 10 to 15 external, diameter, and made of hard glass. In some cases when the carbon was set free, a considerable proportion of the hydrogen seemed to have combined with the higher olefines and paraffins, rendering them gaseous, most of the gas, however, being formed by the reaction discovered by Thorpe and Young, where a high paraffin splits up into lower olefines and paraffins. This reaction, I would remark in passing, is not always the only one to take place, as a mixture of lower paraffins and olefines on being heated under very high pressure sometimes yield a small quantity of a higher or even nearly solid paraffin, as well as gaseous products. An attempt was made to obtain solid paraffin in quantity by this method, but only traces were obtained white enough for use.

The alkali metal which decomposes the hydrocarbon retains a quantity of pure hydrogen, which may be seen by exhausting it by the Sprêngel pump. A piece of sodium was exhausted in the molten state for five hours by the Sprengel pump, and when no more hydrogen had been evolved for an hour, a piece was placed in a tube with paraffin spirit and heated for two hours, and when a considerable quantity of carbon was deposited, as much of it was removed as could be conveniently obtained and again exhausted, when 32 times its volume of hydrogen was extracted from it. This was repeated several times, and quantities of hydrogen, varying from 17 to 25 times the volume of the sodium, obtained. The carbon deposited on the tube is of a hard scaly nature, and when the sodium is slowly oxidized and dissolved in water, some very hard scales of carbon are often obtained. This was then the reaction on which my work was built. As potassium is a metal of stronger affinities, I thought that an examination of its action on paraffin would yield somewhat better results, but in this I was disappointed. Sometimes its action was very great, but it seemed to combine with some of the substance in the tube, and formed black compounds, having no hard carbon amongst them. Some of the experiments did yield a little, but on the whole it was not so good as sodium. Lithium was next tried and yielded results which were much more hopeful. When the same paraffin spirit was used the lithium floated on it, but by melting the metal in the bottom of the tube it adhered to the sides, and did not float to the top unless any portion became detached. The appearance shown by lithium is sometimes very beautiful, a small globule of the metal when it has liquefied showing, before it begins to act energetically upon the hydrocarbon, a very beautiful play of colours. After this has gone on for a little a more rapid reaction sets in as the temperature rises, and carbon is deposited somewhat more plentifully. Sometimes a piece of the metal will float until it becomes coated with carbon and it then sinks. The carbon so obtained is harder than when sodium is employed, and will often scratch glass easily. I thought that if I could by increased temperature dissolve the nascent carbon in the metal I might obtain diamond; but after very many trials I did not succeed in doing so. Many of my scientific friends who had witnessed my experiments thought that a gaseous solvent for carbon might be found, and as this was the direction of my general work, I abandoned these metallic experiments and went into a more direct examination of the question of gaseous solution. As by far the greater number of substances with which we are acquainted are found by the intervention of water and are only soluble in water, I thought that an examination of the solvent effects of water under different circumstances would be likely to yield important results. I found that hard glass tubes resisted the action of water after it had passed its critical point for a time sufficiently long to enable an observer to see whether any great change had taken place in the solution contained in the tube. By these means it was found that many chlorides, sulphates, and nitrates were as soluble in water-gas as in water, if not more so, and that at least no sudden change of solubility was observable at the critical point. By such a process, however, only qualitative results could be obtained, and a series of experiments was tried, using various volumes of water with a fixed proportion of silica in iron tubes, and the solvent power of the water estimated in a more accurate manner. An account of this work will, I hope, be laid before the Society shortly; in fact, as soon as I have the necessary time for completing the work and writing it up. The general result obtained from these experiments was that the solvent power of water was found to be determined by two conditions: 1st. Temperature or molecular *vis viva;* and 2nd. Closeness of the molecules on pressure, which seems to give penetrative power. From these observations it will be seen that if a body has any solvent action on another and does not act upon it chemically, such solvent action may be indefinitely increased by indefinitely increasing the temperature and pressure of the solvent. In nature, the temperature has been at one time higher than we can obtain artificially, and the pressure obtained by a depth of 200 miles from the surface is greater than can be supported by any of the materials from which we can form vessels. It will thus be seen that, whereas in nature almost unlimited solvent power could be obtained, we are not as yet able to reproduce these conditions artificially. Could pressure alone increase solvent power then much might be done, but pressure only acts by keeping the molecules close together when they have great *vis viva*, and this latter is only obtained by high temperature.

As glass tubes were quite out of the question when a red heat and very high pressure were required, iron tubes were resorted to, and a series of attempts made to dissolve carbon by various gaseous solvents. The difficulty of closing iron tubes as compared with glass tubes caused me to try various methods, which I shall describe here. Tubes were made of strong hydraulic tubing, 20″ long, 1″ thick, and ½″ bore. These were fitted with a plug, screwed with a strong screw fitting very well. There was placed in the tube some powdered charcoal from which all the inorganic matter had been removed by immersion in hydrochloric and hydrofluoric acids and washing with water, and then sufficient paraffin spirit to fill the tube two-thirds of its volume. The plug was screwed in with a lute composed of silicate of soda and manganese dioxide, but after heating the tube in a reverberatory furnace for four hours it was found to be impossible to remove the plug, so the end had to be bored out. There was neither liquid nor gas in the tube, the luting having leaked. Another tube similarly filled was fitted with a plug with a copper washer, the end of the tube, plug, and washer being polished, but this also leaked and no result was arrived at. Baryta, clay, asbestos, and other substances, wet with silicate of soda, were all tried with the same result—leakage. A silver washer kept comparatively tight, but only on one occasion. It was thus seen that screw-closing would give no reliable results, so another method was tried. A ball of iron, fitting the tube tightly, was placed in it after the materials had been introduced. The end of the tube was then narrowed by compression between rollers and turned smooth inside. The iron ball was then drawn up by a wire attached and luted by silicate of soda and fine manganese dioxide. It was expected that the pressure would only serve to make the closing more secure, but, on heating, the iron yielded and the ball was driven out with a loud explosion. After trying several other methods of closing—outside screwing and filling the mouth with molten metal on the top of a clay plug being amongst them—I came to the conclusion that nothing would suffice but welding up the open end. This has been, when carried out efficiently, invariably successful, and in all my later experiments I have used it alone. It requires great skill on the part of the workman, and it is only one man in a hundred who can perform the operation with invariable success. The furnace used in these experiments was a reverberatory one, 6 feet long (internal measurement) and 2 feet broad; fire-place, 15 inches; bridge, 9 inches; hearth, 4 feet. The roof sloped down towards the flue and the spent gases had exit at the level of the hearth, thus carrying the flame down as it receded from the fire, in order to have the hearth of one temperature. The walls were 13 inches thick, and the roof formed of 4-inch fire-clay covers.

Three tubes, $20'' \times 1'' \times \frac{1}{2}''$ bore, were filled as follows:—

No. I. 3 grms. sodium, $\frac{3}{4}$ full paraffin spirit.
,, II. ,, ,, $\frac{2}{3}$,, ,,
,, III. ,, ,, $\frac{3}{5}$,, ,,

On heating them in the reverberatory furnace, No. I exploded before a visible red-heat had been obtained, so the temperature was not allowed to rise any higher, and Nos. II and III allowed to lie for four hours and then slowly cooled. On being bored open next day, No. II contained a little scaly carbon, but No. III contained almost none, and nearly all its liquid had been converted into gas, which rushed out on boring it open. It was noticed by the workmen that the inside of the tube was harder to bore than the outside, and I thought, as I found out afterwards rightly, that the iron had been carbonised and converted into steel. It seemed, then, that the free carbon had been taken up by the iron. The same two tubes were welded up again, rather more than half-full of liquid, and slowly heated, but before they came to as high a temperature as they had been subjected to formerly, they exploded together. Two more were filled, welded, and heated, but again they burst simultaneously. It appeared, then, that those tubes were too weak, so two were made $20'' \times 2'' \times \frac{1}{2}''$ bore. On trying to weld them when two-thirds full, the liquid got hot, and gave off enough vapour to carbonise the white-hot plug and made a bad weld, so that they had to be kept cool by trickling water upon them immediately below the hot part. Several tubes were lost in closing before the workman became deft enough at closing them. Two were at length obtained well closed, and were heated, but again, before a red-heat was reached, they exploded simultaneously, smashing the roof of the furnace. It seemed on examining the tubes that one had gone off first, and struck the other such a blow as caused it to burst, as one had a mark near the middle as from a blow with a hammer, and was bent a little. It became plain, then, that two should not be heated together, at least where they might strike each other on explosion. Then, again, as the iron took up the carbon set free, I considered that the reaction might be favoured by adding some carbon to the liquid in the form of lamp-black, so that the liquid would be kept always saturated. A tube, $20'' \times 2'' \times \frac{1}{2}''$ bore was filled, as before, to about three-fourths of its volume, and about half a gram of lamp-black added. This was heated to just below a red-heat for six hours and allowed to cool slowly. On being cut open there was a considerable yield of scaly carbon, and the sodium, on being dissolved, left a few pretty hard scales, along with ferric oxide, spongy iron, &c. This was encouraging, and another tube was filled in the same way, but it burst on heating. An experiment was then tried with paraffin spirit and lamp-black alone, only about 2 grms. of lamp-black being added to the tube, three parts filled with hydrocarbon. This experiment went successfully, and on opening the tube after the outrush of gas it was found that nearly all the lamp-black had been absorbed by the iron. This showed that my conjecture was right about the disappearance of the carbon. Two divisions were then built upon the furnace-hearth, so as to divide it into three spaces, and three tubes of the above dimensions, and filled as follows, were put in them:—

No. I. 3 grms. naphtha, $\frac{1}{2}$ grm. lamp-black, $\frac{1}{2}$ full paraffin spirit.
" II. " " " " $\frac{2}{3}$ " "
" III. " " " " $\frac{3}{4}$ " "

On heating, No. II burst with a loud explosion, but did not harm Nos. I and III, but on opening these next day they were found to have leaked, so that there may have been no pressure inside them at the time. Other two, two-thirds filled with liquid and solids as above, also burst; but as I was absent I do not know at what temperature they were.

It seemed plain that the tubes were much too frail, and although they were made from "Lowmoor" iron, well hammered, and the tube bored out of the solid, they invariably burst lengthwise, showing a reedy structure. I determined then to try tubes on the coil principle; so two were constructed out of the toughest bar iron, made solid, and bored out afterwards. The dimensions were $20'' \times 2\frac{3}{4}'' \times \frac{1}{2}''$, and they were heated to a just visible red-heat, and contained 3 grms. sodium, $\frac{1}{2}$ grm. lamp-black, and two-thirds full of paraffin spirit. The heat was kept up eight hours, and the tubes allowed to cool in the furnace. Both kept tight, and yielded some hard scaly carbon, but nothing else. One was tried with lithium, and a better yield of carbon obtained, and it was also harder. Two more tubes of the same dimensions, with lithium, burst, and so were lost. It now became evident that much stronger tubes were necessary for this reaction, or that some other reaction would require to be found.

I would here mention a fact which, although not directly connected with our subject, is of great interest to students of chemical physics. When introducing the alkali metal into the tube it was often necessary to push it down with a rod, and in one or two cases when I had mislaid the iron rod for the purpose I used a glass rod, of which the end had not been rounded by fusion. In consequence, some small particles of glass became detached, and being lead glass they were heated to such a temperature that they were softened, and in some cases completely melted by the heat. Whenever this had taken place the piece of glass had cavities in it, and these cavities were partially filled with liquid or compressed gas, generally with a portion of each. In the same piece of glass the bubbles were of different size, and often filled to different heights with liquid, just as we find liquid carbonic acid and water filling in different proportions the cavities in the same quartz crystal. How these cavities in the glass are formed, and how the liquid gets into them, I cannot at present determine, but even little spheres of glass purposely introduced likewise developed cavities containing liquid on being fused under pressure. As this is a part of the subject I am investigating, the results being easily produced, I shall reserve its full discussion until I have examined it from various points of view, and varying the substances employed. I think it right to mention it here, however, as I have so little leisure and so many interesting discoveries just touched upon, and each one more tempting as a field of labour than the other, that it may be some time before I can have full data on the subject.

The iron used in making the tubes is what is known as "Lowmoor" iron, a very pure and strong quality, and a portion removed from the interior of a tube which has been used gave, on analysis, 2·17 per cent. of carbon, showing to what an extent carbonisation had gone on.

Having obtained results from this process of a kind which showed that diamond was unlikely to be formed by its agency, I reverted to the original idea of solution of carbon in a gaseous menstruum, and from some experiments I had been carrying on with the view of finding some commercial use for "bone oil," I concluded that the distillate from bone oil containing the nitrogenous bases would be most likely to yield such a solvent. Bone oil, the nitrogenous distillate obtained in the manufacture of bone char, and for a plentiful supply of which I am indebted to Messrs. John Poynter and Sons, of Glasgow, was distilled, and the portion boiling between 115° and 150° was taken and rectified over solid caustic potash, and latterly over sodium. When satisfied that it was free from moisture, oxygen, and sulphur, a tube, $2\frac{3}{4}'' \times 20'' \times \frac{1}{2}''$ bore, was three parts filled, and some charcoal powder added, and the whole welded up solid. I found that the nitrogenous liquid was even worse to work with than the hydrocarbon, as on coming into contact with the hot iron it burnt it away at once, and as the tube was of great diameter it was extremely difficult to keep the lower part cool. For welding it had to be arranged so that it was standing in a tub of ice, and the top projecting through the bottom of the forge, and heated until it was at a welding heat, with as little delay as possible. When a tube was obtained welded up solid it was heated to a dull red-heat for 14 hours, and allowed to cool; on opening the tube there was a very great out-rush of gas, and the carbon was to a certain extent dissolved, and some minute portions of it very hard. Still, under the microscope it presented little difference in appearance from the wood charcoal employed, some of the features, however, being obliterated, and it had a bright appearance. Another tube of the same dimensions and contents was closed up in the same manner, but after eight hours' heating it burst with a loud explosion. I had noticed that a tube which had been once used and been partially carbonised would not stand a second heating, and for this reason I had no belief in the power of cast-iron or steel to withstand the great pressure at a red-heat. Nevertheless, as many of my friends had urged upon me to try these materials, I had a cast-iron tube made, $3\frac{3}{4}'' \times 24'' \times \frac{3}{4}''$ bore, and filled two-thirds of its volume with bone oil distillate and carbon, and then welded up. We succeeded after a little trouble in making a good weld, and the tube was then slowly raised to a dull red-heat in the furnace. It had not been heated for more than an hour when it exploded with a great noise, and knocked down the back and one of the ends of the furnace, leaving the whole structure a wreck. The tube had broken into small fragments, and was quite unlike the malleable iron tubes which generally tore up. Thinking that it was perhaps a bad casting, I tried another, but it leaked all over, and emptied itself before the temperature was nearly up. A third tube of the same material burst like the first, but as I had built up the furnace with large blast-furnace blocks, it was not blown down. Cast-iron being inadmissible, experiments were then made with steel. I had several tubes made of this material by the best firms in the kingdom—made by the three methods, Bessemer, Siemens, and the crucible method—but they had the same faults as cast-iron, although to a less degree. The difficulty in making a good weld in cast-iron and steel tubes makes their employment in such experiments as these a matter of inconvenience. Out of five tubes made of steel, some of which were made of the very toughest material manufactured by Messrs. Cammell and Co., only one held in the substance completely. Three burst in the furnace, and one had leaked by its porosity. The top of the furnace, by the continued shocks of explosions, fell in at the bursting of the last of the steel tubes. The continued strain on the nerves, watching the temperature of the furnace, and in a state of tension in case of an explosion, induces a nervous state which is extremely weakening, and when the explosion occurs it sometimes shakes one so severely that sickness supervenes.

It appeared that as the bone oil had so hardened the carbon, if it acted upon nascent carbon it might harden it so much as to produce diamond. An experiment was accordingly tried in which bone oil distillate and paraffin spirit were mixed, so that when an alkali metal was made to act upon it the decomposition of the hydrocarbon might yield carbon which would be crystallized by the action of the nitrogenous liquid. The proportions used were 90 per cent. bone oil, and 10 per cent. paraffin spirit, with lithium as the metal. The tube used was a coil-tube of Lowmoor iron, 4 inches in external, $\frac{3}{4}$-inch internal diameter, and 24 inches long. Three grms. of lithium was employed, and the tube filled three parts of its length with the mixed liquids, a little lamp-black added, and welded up. It was heated 14 hours to a dull red-heat, and then bored open. A very high pressure was found to exist inside the tube, and as the material was at the other end of the tube it was removed by a long punch. It was found to be a mixture of carbon and lithium compounds, with some cyanides. Some of the carbon was very hard, but could be crushed by agate, and would not scratch it; but there was mixed with it (as with many of the other experiments) a few grains of silica. This was, perhaps, introduced in the welding, as a little silica had to be employed here, and a little may have been added with the lamp-black, which was never quite free from sand or other accidental impurity. The silica particles were easily removed by placing the substance on the stage of the microscope, and examining by polarized light, when all the particles which showed rotary power were removed.

The results obtained by the use of the two liquids being so much more satisfactory, further experiments were undertaken in this direction, and a series of four tubes filled with varying proportions. The tubes had the dimensions $20'' \times 3\frac{3}{4}'' \times \frac{1}{2}''$ bore, and had 3 grms. of lithium introduced into each and then filled as follows:—

No. I.	80	per cent.	bone oil,	20	per cent.	paraffin spirit	5 grms. of lamp-black added to each.
,, II.	40	,,	,,	60	,,	,,	
,, III.	20	,,	,,	80	,,	,,	
,, IV.	10	,,	,,	90	,,	,,	

These were heated separately in the furnace, with the result that Nos. I and III burst, and Nos. II and IV withstood the pressure. No. II on being bored open gave off a great volume of gas, but on removing the contents nothing but lithium compounds and soft carbon was obtained. No. IV also evolved much gas, but the solid matter was very hard and contained some hard particles of carbon, but no diamond. It seemed therefore probable that some such proportion of ingredients might yield successful results, and another series of five, ranging from 30 per cent. to 10 per cent. of bone oil distillate, was prepared, but not one of them gave any results. One by one the tubes exploded, and the furnace had to be reconstructed at the fourth experiment. I thought I should either have to abandon the attempt or begin experiments of a very expensive nature, using large tubes and a large furnace, as 20-inch tubes of a greater diameter than 4 inches could not be closed when three parts filled—at least by welding. As some of them, however, seemed to stand, I determined to make some further trials with the apparatus I had at my disposal; so another tube, $20'' \times 4'' \times \frac{1}{2}''$ bore was filled, using 4 grms. of lithium and a mixture of bone oil, carefully rectified, 90 per cent., and paraffin spirit 10 per cent., using these proportions because I had never had any results with a high percentage of bone oil, the tubes so filled having burst. The tube was closed with great difficulty, being three-parts full of liquid, and then heated to a visible red-heat for fourteen hours, and allowed to cool slowly. On opening the tube a great volume of gas was given off, and only a little liquid remained. In the end of the tube which had been the upper end in the furnace, the tube lying obliquely, there was a hard smooth mass adhering to the sides of the tube, and entirely covering the bottom. As I had never obtained all the solids in one piece before, I wished to examine it, and so had the other end of the tube cut off, exposing the hard mass. It was quite black, and was removed with a chisel, and as it appeared to be composed principally of iron and lithium it was laid aside for analysis. I was pulverising it in a mortar, when I felt that some parts of the material were extremely hard—not resisting a blow, but hard otherwise. On looking closer, I saw that these were mostly transparent pieces imbedded in the hard matrix, and on triturating them I obtained some free from the black matter. They turned out to be crystalline carbon, exactly like diamond. I shall describe further on the analyses, &c., but will here go on with the account of my further experiments. Two tubes were filled in the same manner as the last, but one burst on heating, and the other had leaked so that there was no reaction. Two more tubes were prepared, but were spoiled on welding, and on cutting off the carbonised portion the remainder was too short to work. After much trouble three tubes were obtained, well closed, in which the three alkali metals were inclosed with liquid containing 20 per cent. bone oil and 80 per cent. paraffin. All three stood, and, on opening, only the potassium one had leaked to any extent. The results were not good, however, the sodium tube containing only soft scaly carbon, and the other two very little better. The reaction did not seem to have proceeded in the same manner in the lithium tube as before, as the mass was soft and friable. Still, lithium seemed to yield the best results, so it was adhered to in the further experiments. A list of disasters now awaited me. Eight tubes failed through bursting and leaking, and one of the explosions, when two were being heated together, destroyed a part of the furnace and injured one of my workmen. Besides this, two tubes were spoiled in welding. However, I had four experiments after this, all withstanding the pressure, and in one of these, with 10 per cent. bone oil and 90 per cent. paraffin spirit, a small quantity of diamond was found. The contents of this tube were different from the other successful one, being much looser and not in the same hard mass as the first. In another series of six experiments two were at first thought to have been successful, but I afterwards found that one of them was not so, the transparent matter being siliceous, but insoluble in cold hydrofluoric acid, although it dissolved on boiling. The uncertainty and great expense involved in using these forged coils of iron with tubes bored out of the solid induced me to again try steel, and Messrs. Cammell and Co. having prepared some tubes for me, I tried them, but with the same results—they exploded into fragments at a red-heat. And herein they are much more dangerous than coiled tubes, because the latter seldom fly into fragments, but just tear open a little. A further unforeseen danger in using steel tubes was discovered. One which had stood the heating very well was being bored, and when the inner skin was cut so that the gas rushed out, the whole exploded, endangering the life of the workman who was boring, but as he was standing at the end of the tube and the pieces flew laterally he was not hurt. I have performed over eighty experiments, and have only obtained three results of a successful nature. The identification of the crystalline pieces as carbon was easy enough, but I have been anxious to find whether they are pure carbon or a compound with some other element, and to that end the following experiments were conducted.

A portion of the substance from the first successful experiment was weighed out after it had been freed from all foreign matter adhering to it, and placed in a very small platinum boat made of a strip of thin foil, the ends of which were wrapped round two stout platinum wires which were sealed into a wide glass tube. The carbon particles were transferred to this boat after being weighed, and the tube connected by india-rubber stoppers with an oxygen gasometer on the one side and a series of potash bulbs on the other. The oxygen was dried over solid caustic potash before entering the tube, and again after leaving the potash bulbs. The carbon (14 mgrms.) having been weighed out, the potash bulbs were weighed, and a current of oxygen passed through the apparatus, and the platinum wires connected with a battery strong enough to heat the foil to a bright red-heat. After a few minutes the oxygen was stopped and the bulbs weighed, when it was found that they had gained 1 mgrm. On repeating this operation no gain was found, the moisture having been entirely driven off by the first treatment. The carbon was now placed in the boat and a slow current of oxygen started, then the bulbs connected and the current made to pass through the platinum until all the diamond had been burnt, when the current was stopped and the oxygen allowed to pass for fifteen minutes more, when the bulbs were detached and weighed. They were then reconnected and the gas passed for other ten minutes to find whether all the carbonic acid had been expelled and reweighed. They weighed 0·2 mgrm. less than before. The numbers were as follow:—

Potash bulbs before combustion......	43·8308	
,, ,, after ,, ...	43·8776	
		·0468
Drying tube before combustion......	26·4294	
,, ,, after ,, ...	26·4328	
		·0034
		·0502

This gives a composition of 97·85 per cent. of carbon, which is a pretty fair approximation to pure carbon. However, to determine whether or not this was not the case, some further experiments were tried. A small quantity of the carbon was placed on the platinum boat and burnt in oxygen without any of the gas being allowed to pass out of the apparatus, and the mixed gases so obtained transferred to a eudiometer, and the carbonic acid and oxygen absorbed. It was then found that a residue amounting to about 3 per cent. by volume of the carbonic acid was left unabsorbed by alkaline pyrogallate solution. This proved to be nitrogen. A blank experiment was done, but it gave only a minute bubble of nitrogen. Another experiment was performed with the following results:—

Total volume..........	183·7°		
After absorption of CO_2	148·5°	$CO_2 =$	35·2
After ,, ,, O	1·1°	O	147·4
		N	1·1

This plainly shows that nitrogen was present from some cause or another, and as every precaution was taken in transferring the gas from one vessel to another, and as the blank experiment showed nothing, I am inclined to believe that the carbon, or at least some portions of it, contained nitrogen chemically combined. The numbers above given are degrees on the eudiometer tube, and are not more than one-third of a cubic centimetre each. Their exact value was of no consequence in the experiment, and the tube was only calibrated by comparing one part with another, and not with an absolute measure.

From the fact that no diamond was found when nitrogen compounds were absent, and from the fact that the mixed product (for only a portion of the 14 mgrms. was clear diamond) contains nitrogen, I am inclined to believe that it is by the decomposition of a nitrogenous body, and not the hydrocarbon, that the diamond is formed in this reaction. The experiments are, however, too few, and the evidence too vague, to draw any conclusions, as there are even very few negative experiments from which anything can be learned, most of the results being lost by explosion. I intend, when my other work—which I laid aside for the diamond experiments—is finished, to begin a series of experiments on the decompositions of carbon compounds by metals, to find whether a more easily controlled reaction may not be discovered.

VII. "Further Note on the Spectrum of Carbon." By J. NORMAN LOCKYER, F.R.S. Received May 11, 1880.

The preliminary discussion of a considerable number of photographs of the spectra of various carbon compounds has brought to light a relationship which I think may be worthy of notice in the Proceedings; it was noticed orally in connexion with the paper read before the Society on April 29th.

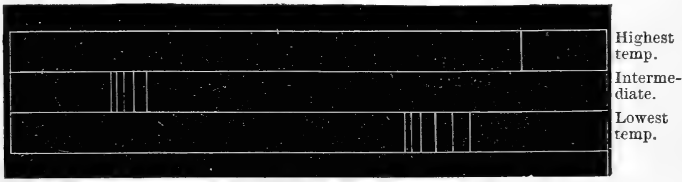
A comparison of the photographs of the various carbon compounds observed under various conditions enabled us to isolate the lines in the blue and ultra-violet portions of the spectrum (wave-lengths 4300–3800).

In this manner the constant lines seen in the photographs of the spectra of CCl_4, $C_{10}H_8$, CN, CHI_3, CS_2, CO_2, CO, &c., have been mapped, and the coincident lines and flutings thus marked.

The phenomena thus seen with more or less constancy are a blue line, with a wave-length of 4266; a set of blue flutings, extending from 4215 to 4151; and another set of ultra-violet flutings, which extend from 3885 to 3843 (all approximate numbers).

FIG. 1.

Action of three different temperatures on a hypothetical substance, assuming three stages of complete dissociation.

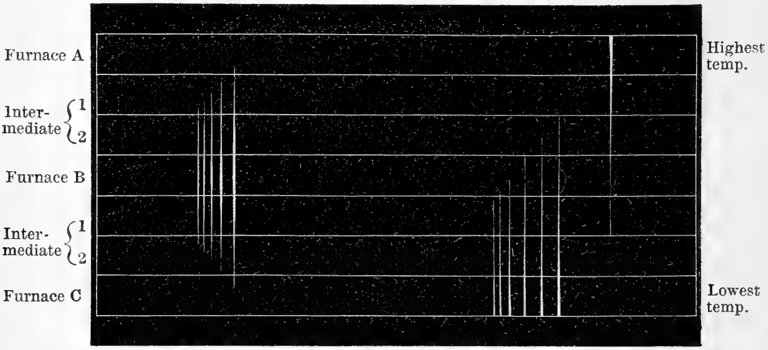


In a photograph of the spectrum of the electric arc (with a weak battery) from carbon poles in an atmosphere of chlorine, the blue flutings alone are visible, while, when the spark is similarly photographed, the ultra-violet flutings and the blue line (4266) are also visible, while the blue flutings become fainter.

From this we may assume, in accordance with the working hypothesis of a series of different temperature furnaces, as set forth in the paper of December, 1878 (see fig. 1), that the different flutings and the line correspond to different temperature spectra, the blue flutings to the lowest and the blue line to the highest temperature, while the ultra-violet flutings occupy an intermediate position.

Fig. 2.

Spectra of the hypothetical substance in intermediate furnaces, assuming that the vapours are not completely dissociated.



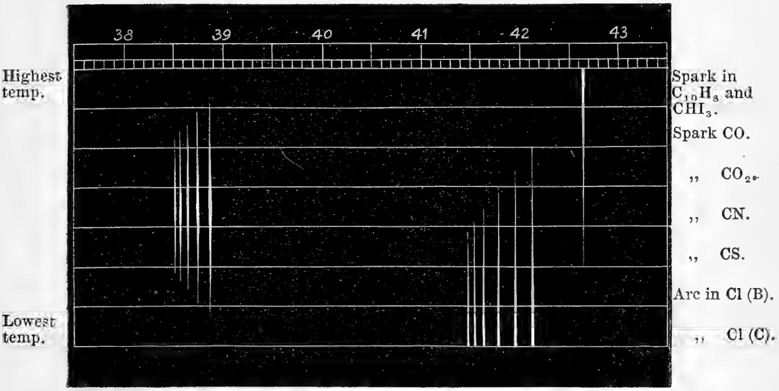
According to this working hypothesis, there should be a series of heat-levels forming a perfect gradation between the spectrum which contains the blue line alone and that which contains the blue flutings alone (fig. 2).

In comparing the spectra of carbon under different conditions, I find this to be true. *The blue line never appears in conjunction with the blue flutings, unless the ultra-violet flutings are also present.* In other words, the highest and the lowest hypothetical temperature spectra are never visible together without the spectrum of the intermediate hypothetical temperature.

But this is not all. By placing the spectra of the substances at different heat-levels, so to speak, I am enabled to construct a diagram which not only indicates the mere presence or absence of the lines and their relative intensities, but shows that there exists a perfect gradation between the spectrum which contains the line alone and that which contains the blue flutings alone (fig. 3). I would point out that there is nothing theoretical in this diagram. All the heat-levels depicted are copied from photographs of carbon under the conditions indicated, and theory has merely enabled me to arrange them *in order*.

FIG. 3.

The photographed spectra of some carbon compounds.



This map I submit, therefore, bears out the hypothesis of differences of temperature indicated above, for it is seen that, while the blue line gradually thins out, the ultra-violet flutings appear first and grow in intensity. As these increase, the blue flutings become visible, and further as the latter augments and the line disappears, the ultra-violet flutings gradually die out altogether.

It is philosophical to infer from these observations not only that the line and flutings in question are produced by carbon, but that the blue line (4266), since it is visible at the highest temperature, corresponds to the most simple molecular grouping we have reached in the experiments, and the flutings to others more complex.

Presents, May 13, 1880.

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June 3, 1880.

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, Dr. Allen Thomson and Sir James Paget 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.

Allbutt, Thomas Clifford, M.A., M.D., F.L.S.
Attfield, Prof. John, Ph.D., F.C.S.
Blanford, Henry Francis, F.G.S.
Dallinger, Rev. William Henry.
Dyer, William Turner Thiselton, M.A., F.L.S.
Godwin-Austen, Lieut.-Col. Henry Haversham.
Graves, The Right Rev. Charles, D.D., Bishop of Limerick.
Hughes, Prof. David Edward.
Jeffery, Henry M., M.A.
M'Coy, Prof. Frederick, F.G.S.
Moulton, J. Fletcher, M.A.
Niven, Prof. Charles, M.A., F.R.A.S.
Rae, John, LL.D.
Reynolds, Prof. J. Emerson, M.D.
Tilden, William A., D.Sc.

Thanks were given to the Scrutators.

June 10, 1880.

THE PRESIDENT in the Chair.

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

Dr. John Attfield, Dr. Clifford Allbutt, Rev. William Henry Dallinger, Mr. William Turner Thiselton Dyer, Lieut.-Col. Henry Haversham Godwin-Austen, Prof. David Edward Hughes, Mr. Henry M. Jeffery, Mr. J. Fletcher Moulton, and Dr. John Rae were admitted into the Society.

The following Papers were read:—

I. "On a Fourth State of Matter," in a letter to the Secretary. By W. CROOKES, F.R.S. Received May 17, 1880.

7, Kensington Park Gardens, London, W.,
April 29, 1880.

DEAR PROFESSOR STOKES,

In introducing the discussion on Mr. Spottiswoode and Mr. Moulton's paper on the "Sensitive State of Vacuum Discharges," at the meeting of the Royal Society on April 15th, Dr. De la Rue, who occupied the chair, good-naturedly challenged me to substantiate my statement that there is such a thing as a fourth or ultra-gaseous state of matter.

I had no time then to enter fully into the subject; nor was I prepared, on the spur of the moment, to marshal all the facts and reasons which have led me to this conclusion. But as I find that many other scientific men besides Dr. De la Rue are in doubt as to whether matter has been shown to exist in a state beyond that of gas, I will now endeavour to substantiate my position.

I will commence by explaining what seems to me to be the constitution of matter in its three states of solid, liquid, and gas.

I. First as to Solids:—These are composed of discontinuous molecules, separated from each other by a space which is relatively large—possibly enormous—in comparison with the diameter of the central nucleus we call *molecule.* These molecules, themselves built up of *atoms*, are governed by certain forces. Two of these forces I will here refer to—attraction and motion. Attraction when exerted at sensible distances is known as *gravitation*, but when the distances are molecular it is called *adhesion* and *cohesion.* Attraction appears to be independent of absolute temperature; it increases as the distance between the molecules diminishes; and were there no other counteracting force the result would be a mass of molecules in actual contact, with no molecular movement whatever—a state of things beyond our conception—a state, too, which would probably result in the creation of something that, according to our present views, would not be *matter.*

This force of cohesion is counterbalanced by the movements of the individual molecules themselves, movements varying directly with the temperature, increasing and diminishing in amplitude as the temperature rises and falls. The molecules in solids do not travel from one part to another, but possess adhesion and retain fixity of position about their centres of oscillation. Matter, as we know it, has so high an absolute temperature that the movements of the molecules are large in comparison with their diameter, for the mass must be able to bear a reduction of temperature of nearly 300° C. before the amplitude of the molecular excursions would vanish.

The state of solidity therefore—the state which we are in the habit of considering *par excellence* as that of *matter*—is merely the effect on our senses of the motion of the discrete molecules among themselves.

Solids exist of all consistences, from the hardest metal, the most elastic crystal, down to thinnest jelly. A perfect solid would have no viscosity, *i.e.*, when rendered discontinuous or divided by the forcible passage of a harder solid, it would not close up behind and again become continuous.

In solid bodies the cohesion varies according to some unknown factor which we call chemical constitution; hence each kind of solid matter requires raising to a different temperature before the oscillating molecules lose their fixed position with reference to one another. At this point, varying in different bodies through a very wide range of temperature, the solid becomes liquid.

II. In liquids the force of cohesion is very much reduced, and the adhesion or the fixity of position of the centres of oscillation of the molecules is destroyed. When artificially heated, the inter-molecular movements increase in proportion as the temperature rises, until at last cohesion is broken down, and the molecules fly off into space with enormous velocities.

Liquids possess the property of viscosity—that is to say, they offer a certain opposition to the passage of solid bodies; at the same time they cannot permanently resist such opposition, however slight, if continuously applied. Liquids vary in consistency from the hard, brittle, apparently solid pitch, to the lightest and most ethereal liquid capable of existing at any particular temperature.

The state of liquidity, therefore, is due to inter-molecular motions of a larger and more tumultuous character than those which characterise the solid state.

III. In gases the molecules fly about in every conceivable direction, with constant collisions and enormous and constantly varying velocities, and their mean free path is sufficiently great to release them from the force of adhesion. Being free to move, the molecules exert pressure in all directions, and were it not for gravitation they would fly off into space. The gaseous state remains so long as the collisions continue to be almost infinite in number, and of inconceivable irregularity. The state of gaseity, therefore, is pre-eminently a state dependent on collisions. A given space contains millions of millions of molecules in rapid movement in all directions, each molecule having millions of encounters in a second. In such a case, the length of the mean free path of the molecules is exceedingly small compared with the dimensions of the containing vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed.

What, then, are these molecules? Take a single lone molecule in space. Is it solid, liquid, or gas? Solid it cannot be, because the idea of solidity involves certain properties which are absent in the isolated molecule. In fact, an isolated molecule is an inconceivable entity, whether we try, like Newton, to visualize it as a little hard spherical body, or with Boscovich and Faraday, to regard it as a centre of force, or accept Sir William Thomson's vortex atom. But if the individual molecule is not solid, *à fortiori* it cannot be regarded as a liquid or gas, for these states are even more due to inter-molecular collisions than is the solid state. The individual molecules, therefore, must be classed by themselves in a distinct state or category.

The same reasoning applies to two or to any number of contiguous molecules, provided their motion is arrested or controlled, so that no collisions occur between them; and even supposing this aggregation of isolated non-colliding molecules to be bodily transferred from one part of space to another, that kind of movement would not thereby cause this molecular collocation to assume the properties of gas; a molecular wind may still be supposed to consist of isolated molecules, in the same way as the discharge from a mitrailleuse consists of isolated bullets.

Matter in the fourth state is the ultimate result of gaseous expansion. By great rarefaction the free path of the molecules is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the containing vessel, the properties which constitute gaseity are reduced to a minimum, and the matter then becomes exalted to an ultra-gaseous state.

But the same condition of things will be produced if by any means we can take a portion of gas, and by some extraneous force infuse order into the apparently disorderly jostling of the molecules in every direction, by coercing them into a methodical rectilinear movement. This I have shown to be the case in the phenomena which cause the movements of the radiometer, and I have rendered such motion visible in my later researches on the negative discharge in vacuum tubes. In the one case the heated lamp-black and in the other the electrically excited negative pole supplies the *force majeure* which entirely or partially changes into a rectilinear motion the irregular vibration in all directions; and according to the extent to which this onward movement has replaced the irregular motions which constitute the essence of the gaseous condition, to that extent do I consider that the molecules have assumed the condition of radiant matter.

Between the third and the fourth states there is no sharp line of demarcation, any more than there is between the solid and liquid states, or the liquid and gaseous states; they each merge insensibly one into the other. In the fourth state properties of matter which exist even in the third state are shown *directly*, whereas in the state of gas they are only shown *indirectly*, by viscosity and so forth.

The ordinary laws of gases are a simplification of the effects arising from the properties of matter in the fourth state; such a simplification is only permissible when the mean length of path is small compared with the dimensions of the vessel. For simplicity's sake we make abstraction of the individual molecules, and feign to our imagination *continuous* matter of which the fundamental properties—such as pressure varying as the density, and so forth—are ascertained by experiment. A gas is nothing more than an assemblage of molecules contemplated from a simplified point of view. When we deal with phenomena in which we are obliged to contemplate the molecules individually, we must not speak of the assemblage as *gas*.

These considerations lead to another and curious speculation. The molecule—intangible, invisible, and hard to be conceived—is the only true *matter*, and that which we call matter is nothing more than the effect upon our senses of the movements of molecules, or, as John Stuart Mill expresses it, "a permanent possibility of sensation." The space covered by the motion of molecules has no more right to be called matter than the air traversed by a rifle bullet can be called lead. From this point of view, then, matter is but a mode of motion; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* retaining the properties of inertia and weight would remain, *matter*, as we know it, would cease to exist.

Believe me,
Dear Professor STOKES,
Very sincerely yours,
WILLIAM CROOKES.

II. "On *Bacterium fœtidum:* an Organism associated with Profuse Sweating from the Soles of the Feet." By George Thin, M.D. Communicated by Professor Huxley, Sec. R.S. Received May 12, 1880.*

[Plate 6.]

The feet of certain individuals are characterised by a peculiar powerful and fœtid odour. This odour, although it is usually believed to be connected with the sweat from the feet, is really connected with the moisture that soaks the soles of the stockings and the inside of the boots. The moisture, which comes from the skin of the soles, especially from that of the heels, has no offensive smell whilst it is exuding, but it rapidly acquires the characteristic odour when taken up by the stocking.

The fluid is not pure sweat, but is an admixture of sweat with serous exudation from the blood. This admixture occurs in persons whose feet sweat profusely, and who, from much standing or walking, acquire an erythematous or eczematous condition of the skin of the soles, the local erythema or eczema being favoured by the softening and macerating effect of the sweat on the epidermis. That the fluid is not sweat is shown by its reaction. In the case which furnished me with an opportunity for investigation the reaction with litmus paper applied to the wet heel was very faintly alkaline, the fluid in the stockings and on the inner surface of the sole of the boot being more decidedly, but still faintly, alkaline. At the same time the sweat on other parts of the body was acid.

When a small portion of the sole of the wet stocking was teased out in water, the drop of water was found to be swarming with minute spherical bodies, many of them being in pairs, fairly equal in size and refracting light uniformly. Clusters or colonies of them were lying on hollow parts of the cotton fibres. These bodies I shall in this paper call micrococci, implying by that term no more than that they were spherical, that they were found singly and in pairs, and that they were capable of development. No rod-shaped bacteria were found in the drop.

Having made more than one examination, and finding the micrococci always present, I inoculated with them a drop of pure vitreous humour, and placed it over a cell prepared in the usual manner. The cells were kept at a temperature of from 96° to 98° F., and within twenty-four hours appearances were observed which showed that the micrococci (or spores) were developing after the fashion in which the spores of the *Bacillus anthracis* are known to develop. In order to

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* This paper records the results of one of a series of investigations on the parasites that infest the human skin, the expenses connected with which have been defrayed by the Scientific Grants Committee of the British Medical Association.

study more conveniently the different forms assumed by the organism when under cultivation, it was grown for successive generations in pure vitreous humour, contained in pure test glasses, and kept at 96° to 98° F.

The glasses, fitted with glass caps, and covered by a larger protecting glass, were placed on small glass squares and purified by being kept at a temperature over 300° F. for about two hours. The flasks for the vitreous humour were protected by carbolised cotton wool and similarly heated. The vitreous humour was obtained by collection from ox and sheep's eyes. It was squeezed through fine muslin, and introduced with proper precautions to a pure flask, which was then placed for half an hour in boiling water. All the apparatus used and the method of experimenting were modelled after those described by Mr. Lister in a paper on the lactic fermentation in the twenty-ninth volume of the "Transactions of the Pathological Society of London." It was ascertained that milk, vitreous humour, and turnip infusion, so prepared and protected by the carbolised cotton caps, kept free from organisms and retained their natural appearance and taste for periods extending over six weeks.

A second generation of the organism, which for convenience I may call *Bacterium fœtidum*, was obtained by placing a small piece of the wet stocking in one of the test-glasses, charged with pure vitreous humour. This and all the succeeding generations were cultivated at a temperature which varied between 94° and 98° F. The successive generations were obtained by inoculating pure vitreous humour, with requisite precautions.

In twenty-four hours the surface of the vitreous humour was always found covered with a delicate scum, which in forty-eight hours was compact and tolerably resistant.

In the scum of one day's growth and in the fluid below it organisms were found as cocci, single and in pairs, in transition stages towards rod formation, as single and jointed rods, and as elongated single rods. Many of the rods were actively motile.

The compact scum of two days' growth was sufficiently resistant to be removed in an unbroken sheet. When disturbed by the needle it fell to the bottom of the glass. It was found to contain all the forms found in the twenty-four hours' growth, and in addition long unbroken rods in transition stages towards the formation of chains of spores.

Spores were also found lying beside the empty and partially empty sheaths from which they had been discharged. Groups of single spores and pairs, identical in size and appearance with those which had come to maturity in the sheaths, were found mixed up with rods in all phases of development.

As no individual coccus or rod was kept under continuous observation, the process of growth can only be inferred by comparing the various forms with each other, and thus tracing the successive stages in the development.

Examples of these different stages were drawn (Plate 6), and with the assistance of the figures I shall endeavour to describe what I believe to be the order of succession.

The first stage is undoubtedly the production of a pair from a single coccus. The individuals of the pair were sometimes found so closely associated that there was no independent movement in each member, in some a distinct movement of each could be observed, whilst in others the union was so loose that there was a perceptible distance between them, and they oscillated round each other, connected undoubtedly by a band of union which with the microscope I employed was not distinctly visible.

The next stage I take to be that in which the whole body is wedge-shaped, the round brightly refractive coccus being found in the thick end of the wedge. Another phase which is probably the successor of the preceding one, is the appearance of a canoe-shaped figure with the bright coccus in the centre. In one of these canoe-shaped bodies Mr. Knowsley Thornton and myself observed two of the bright refractive bodies in active oscillation in the centre of the canoe; one and then two being successively visible according to their relative position. The canoe-shaped envelope itself was motionless (the preparation was a permanent one sealed in diluted Goadby's solution), and Mr. Thornton was able to draw it with the camera. (See fig. 2, *a*, *b*.)

Other appearances connected with the early stage of development, and probably following the wedge and canoe-shaped figures, show the organism developed into a staff-shaped body, containing two elements of very different refractive power. The coccus element is still distinct and is brightly refractive, the other element is very slightly refractive and is seen as a dull shade, with however perfectly distinct outlines.

The latter element it may be convenient to term protoplasm, using the word merely to express the idea of an element which is distinct from the coccus and the sheath. The relative positions of the coccus or cocci and the protoplasm are various.

The coccus may be at one end of the rod, two cocci may be in the centre close together with a prolongation of protoplasm on either side, or a central rod of protoplasm may have a coccus at either end. (Fig. 2.)

In the next stage we have the formation of the rods characteristic of bacteria. The distinction between the coccus and the protoplasm becomes lost, although transitions are found in which faint differences of refraction still betray the two elements. At this stage, in the double flail-shaped rods, the one member sometimes refracts differently from the other, the development being evidently in a different stage. The numerous two-, three-, and four-jointed rods attached to each other by a yielding elastic substance, which is very faintly refractive, complete the metamorphosis from the coccus to the rod.

The development of the rod appears to take place in two directions. In the one there is a formation of small segments whose fate it seems to be to be set free from each other (fig. 6); whilst in the other a formation of spores is introduced by a series of preliminary changes. The latter process begins by a lengthening out of the rod, which is frequently found of great comparative length. (These long rods are naturally liable to be broken in the preparation, but an idea of their length may be formed from fig 5, *a*.)

After the rods have attained a certain length, the protoplasm divides into separate portions in the tube (fig. 5, *b*, *c*). Minute refractive spheres then appear in the separate portions, but are also found occasionally in a long rod before the protoplasm has separated into segments (figs. 7, *a*, and 8, *a*). The delicate wall of the tube remains in the meanwhile entire.

The next stage is represented by tubes, in which the spores are so densely packed, that they almost touch (fig. 8, *b*), the wall of the tube being, however, still visible.

Two appearances, which were seen during the examination of fresh specimens, but were not noticed in the permanent preparations, have not been drawn. I several times observed tubes filled with spores, in which faint narrow lines of different refraction crossed the tube between the spores, giving the impression of the existence of a hyphen. I also several times observed tubes partially emptied of their spores, the escaped spores lying beside the empty tube, and resembling in every respect the spores still present in the portions of the tube which had not ruptured.

Amidst the mass of rods and tubes in all the different stages which are found in the scum, were found dense masses of micrococci. When they were set free by manipulation, and sufficiently isolated to be accurately observed, they were found singly and in pairs (fig. 9), and resembled in size and appearance, not only the spores which were observed in the ripe tubes, but those which were found in the meshes of the stocking.

Several cultivations were made in turnip infusion from the original stock and from various generations of the vitreous humour stock. No regular notes were taken of these cultivations, but I can state that the bacterium whilst it grows in turnip infusion grows less actively than in vitreous humour. It took from three to five days (at the same temperature at which the other cultivations were made) before a perceptible scum appeared on the surface of the fluid, and then the scum was not so thick as after a cultivation of half the time in vitreous humour. All the lengths of rod formation were found, but in none of the preparations examined did I find spores either in the long rods

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or free in the fluid. In many of the long rods segmentation of the protoplasm, by which rod-shaped masses of equal length were formed, was observed, the delicate tube wall being continued from one segment to the other unbroken. The observations were not sufficiently extended to determine whether the bacterium forms spores when cultivated in turnip infusion, but they suffice to show that if it does occur, it occurs much less actively than when the cultivation is in vitreous humour.

That the fœtid odour of the stocking is due to the development of the bacterium was shown by the characteristic fœtor being reproduced in the cultivation glasses, although the strength of the odour diminished in successive generations. The fluid of the fourth generation still smelt powerfully, and was at once recognised by several persons who had smelt the original piece of stocking; the fluid of the eighth generation still had the characteristic smell, but had it so feebly that although at once recognised by myself and the patient from whom the stocking had been obtained, it was not considered distinctive by a third person who had recognised the smell of the fourth generation. Mr. Lister, to whom I mentioned this fact, informs me, and authorises me to state, that he has made an analogous experience with the *Bacterium lactis*. This bacterium, after being cultivated in successive generations in urine, although it still retains its capacity to induce the lactic fermentation, possesses it in a less degree than when it has been grown in milk. The vitreous humour in a similar way would seem to be a less favourable medium for the *Bacterium fœtidum* (in so far as the production of the peculiar odour is concerned) than in the mixture of sweat and serum in which it develops in the stocking.

EXPLANATION OF THE PLATE.

[Figs. 1, 2, 4, 6, 7, 8, and 9 are camera drawings by Mr. Knowsley Thornton, made by the use of an excellent immersion objective by R. and J Beck, and I am much indebted to Mr. Thornton, not only for these careful and accurate drawings, but for valuable suggestions bearing on the subject of the paper. The magnifying power for these figures is 900 diameters.

In fig. 3 are forms drawn by myself without the camera. They afford no guarantee of size.

In fig. 5, drawn by myself, are camera drawings, as regards length (magnifying power 1,000 diameters), but they give no guarantee as regards breadth.]

Fig. 1. The micrococcus, from the sock uncultivated. All the other figures are drawn from cultivated specimens.

Fig. 2. Micrococcus forms in development. *a* and *b* show two phases of the same object.

Fig. 3. *a*. Further stages in the development of the micrococcus; *b*., examples of spore appearances in rods.

Fig. 4. Elongating rods.

Fig. 5. Elongated rods, three of them showing segmentation in the protoplasm.

Fig. 6. Segmented rods.

Fig. 7. Spores in the elongated rods; *a* is an example of a single spore formed at one end of each segment of protoplasm.

Fig. 8. Various phases of spore (coccus) formation in the tubes; *a* is an example of spheres in the protoplasm of an unsegmented rod; *b*, chains of spores (or cocci), in which remains of the tube, although hardly to be represented clearly in a drawing without exaggerating the appearance, are yet distinctly visible. In estimating the size of *b*, it is to be borne in mind that both the spores and the sheath make up the dimensions as they are shown.

Fig. 9. Micrococci or spores set free from scum, which is partly formed by rods in the various phases shown in the preceding figures.

III. "On the State of Fluids at their Critical Temperatures." By J. B. HANNAY, F.R.S.E., F.C.S. Communicated by Professor G. G. STOKES, D.C.L., &c., Sec. R.S. Received May 24, 1880.

In carrying out the investigations which I commenced some years since upon the phenomena presented by the flow of different liquids through capillary tubes, the question as to what constitutes a liquid—that is in what way it differs from a gas, and how the great variance of the microrheometrical laws for the two fluids can be explained—again and again presented itself to me. Seeing that solids are soluble in gases as well as in liquids, one of the chief differences supposed to exist between the two states has disappeared; and I have been compelled to adopt as the only definition of a liquid, that it is a fluid which has cohesion. Professor James Thomson, F.R.S., has suggested to me the use of the term contractility, instead of cohesion, and this term admirably defines the liquid state, but as it suggests (in a distant way perhaps) a voluntary power, and is used in connexion with organised structures, I shall retain the term cohesion at present. We have then the two states of fluids, first, the gaseous, in which the *vis viva* or heat energy of the molecules has entirely overcome cohesion, or their mutual attraction, and they are prevented from grouping; and second, the liquid where the attractive power is greater than the *vis viva*, and the molecules are enabled to group themselves, but still are in sufficient motion to prevent the grouping from being permanent, hence we have cohesion, but no rigidity. We do not yet know that all solids are not also fluids, as many of them are known to flow, but this may be from other causes, but we know that the solid state is characterised by so much cohesion as to produce more or less rigidity. The most interesting point in the consideration of a liquid is that at which it approaches to the gaseous state, where its cohesion disappears, and we have what Dr. Andrews has termed the critical point, which is the termination of that property which distinguishes a liquid fluid from a gaseous fluid, or in other words the liquid becomes a gas. But a question arises. To observe this disappearance of the cohesion of a liquid, it is requisite that it should have a free surface, and this free surface has till now only been obtained by arranging the pressure that a portion of the fluid is in the gaseous state, and this only occurs at one pressure. Now, when the temperature of a liquid is raised while it is retained under very great pressure, so that it never has a free surface, but is always retained filling the vessel, does the liquid still lose its cohesion, and become a gas at the same temperature; or, as the pressure is increased, does the temperature at which the cohesion of the liquid is overcome, also rise? In the former case, the limit of the liquid state would be an isotherm, in the latter, a continuation of the boiling line. To determine which is the object of the work here described.

With proper precautions, the loss of cohesion or capillarity can be noticed very accurately, and the level of the liquid in a fine capillary tube, seen to coincide with the plane surface of the liquid just before the final disappearance of the line of demarcation. One of the precautions to be taken is to obtain equable temperature, and while in my earlier experiments, I used a double air-bath, and considered this sufficient to obtain good results, I subsequently found that by the use of a triple bath of copper, every trace of irregularity of temperature disappeared, and I obtained results in which the line of division was admirably clear and sharp, and never became broad and hazy as in ordinary experiments. Another precaution to be taken is to have pure liquids, and this at first sight might appear to be an easy matter, but I find that in transferring a portion of a pure liquid to a tube, the momentary exposure to air, especially in the vicinity of the hands, hydrates the liquid sufficiently to render the line of demarcation rounded, and show a slightly greater refractive power in the lower part of the tube after the critical point has been passed. In the case of liquefied gases, such as carbon dioxide, ammonia, sulphur dioxide, and nitrous oxide, which are easily dried, the line is beautifully sharp, and the disappearing point easily noted. Alcohol cohobated over caustic lime for a week and transferred to a tube without contact with air, shows the disappearance of the line with great sharpness, and immediately after no difference in refractive power can be detected between the upper and lower portions. The least trace of moisture is sufficient to show such a difference. Whenever I notice any difference between the upper and lower portions after passing the critical point, I attribute it to moisture or other impurity, as careful treatment always removes the difference in density. In many organic liquids there is always a difference at the critical point, and sometimes before reaching this temperature, they form several layers, each having a different critical point as they seem to give rise on heating to new compounds, or form polymeric compounds having different critical points. Besides, many organic compounds cannot be entirely freed from impurity, as they retain it even on repeated distillation. In the following experiments, therefore, such organic compounds were never used, and only perfectly anhydrous alcohol, or carbon disulphide,* or gases which can be obtained anhydrous, CO_2, SO_2, and NH_3, being chosen. The apparatus used for obtaining pressure was that described in a former paper (" Proc. Roy. Soc.," No. 201, 1880, " On the Solubility of Solids in Gases"). In order to determine, then, whether increased pressure applied above the critical point, would have the effect of reducing the gas to a liquid, as might easily be supposed, since the rates of expansion of gas and liquid become alike at the critical point, a new form of experiment was resorted to. It had been noticed that it was easy to determine whether the tube were filled with liquid or gas, by simply reducing the pressure somewhat quickly, when, if there were liquid present, it boiled, while if the contents were entirely gaseous, simple expansion was the result. The boiling only takes place when the pressure is reduced so far as to be a little under the vapour pressure at that temperature, in other words, boiling cannot be observed, unless there exists a free surface, and this free surface cannot be obtained with the liquid alone above the "critical pressure." By the introduction of a quantity of hydrogen gas over the liquid, a free surface is obtained at any pressure, and the mixture of hydrogen and alcohol vapour being of so much less density than the alcohol, it remains divided from it by a line of demarcation for some time after the latter is undoubtedly gaseous. Now, let us see what takes place on lowering the pressure. When the temperature is even only 1°C. below the critical point, when the pressure is sufficiently reduced, the alcohol boils, showing that it still has cohesion, but if the temperature be 1° above the critical point, the fluid only expands, and no boiling is seen at any pressure, from 50 up to 200 atmospheres. Here the fluid above the critical point has just as free a surface as below it, and we see that the last trace of the liquid condition has disappeared. The line dividing the mixture of hydrogen and alcohol vapour from the pure alcohol is quite sharp, for a short time, and on altering the pressure, it moves up and down quite freely, and possesses exactly the same appearance and properties as hydrogen over carbon dioxide in a bell-jar.†

Thus we see that the liquid state seems to come to an end, and the gaseous state to supervene quite independent of pressure.

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* Even this, in later experiments, has been found to dissociate slightly. (June 14, 1880.)

† The experiments must be done quickly, before the alcohol has time to dissolve the hydrogen to any great extent, and the temperature must be equal all through the apparatus, otherwise mixture takes place and these phenomena are not seen. Experiments on mixtures obtained by shaking are in progress, but in these the critical point is much altered.

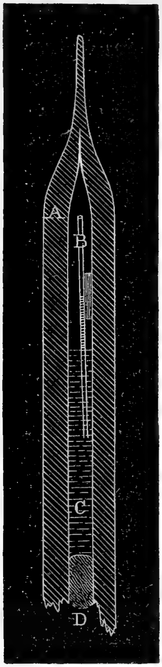
There is another mode of experimenting which illustrates this even more strikingly. A quantity of alcohol with hydrogen over it was raised to a temperature of 228° to 230° under a pressure of 120 atmospheres, and the pressure and temperature maintained for five hours, when it was seen that the line of demarcation was in the same place, and as sharp as before, and on lowering the pressure, the liquid boiled, showing that it had not mixed with the gas. The temperature was now raised to 240°, and the pressure as before, at 120 atmospheres, and at the end of a quarter of an hour, instead of a sharp line of demarcation there was only a faint, broad indication of a change of density near the middle of the tube, and at the end of an hour all mark of a difference had disappeared, the two gases having mixed. Doubtless, the mixture was not yet uniform, but it had proceeded sufficiently far to show that the two were miscible. Here we see plainly, that a few degrees below the critical point, the substance was undoubtedly liquid, and a few degrees above, undoubtedly gaseous. The experiment was repeated at 200 atmospheres' pressure, the temperature being 220° and 240°, with precisely the same result, and I have since repeated it several times. This shows that the limit of the liquid state depends upon temperature, and not upon pressure.

The method of examining the loss of cohesion of a liquid by means of a capillary tube was also applied at any pressure by the introduction of a permanent gas over the liquid, as in the foregoing experiments. This was done as follows.

In the working tube of the pressure apparatus A, a small piece of capillary tubing B, was cemented by fusing a minute piece of silicate of soda between it and the side of the tube. The liquid C, which was in this case carbon disulphide, was placed in the tube over the mercury, and readings taken of the height of the liquid in the capillary tube. In order that the same liquid should be used in both cases, and the experiments thus rendered strictly comparable, the point of A was broken off, and a quantity of pure dry nitrogen introduced. Pressure was then applied, and the capillarity noted, the temperature raised, and the result again noted. The following table illustrates the results obtained. The capillary heights are arbitrary numbers of the cathetometer: ten experiments were done in each case.

	Liquid alone.			Liquid with nitrogen.		
	Temp.	Pressure.	Height.	Temp.	Pressure.	Height.
	51°	1 atmos.	53·6	51°	81 atmos.	53·2
	220°	63 ,,	12·2	220°	180 ,,	11·9
Probable error of mean		0·7 ,,	0·2		1·1 ,,	0·25

The experiments were then pushed up to the critical point when, in both cases, the one at 80 atmospheres and the other at 194, the capillarity disappeared. This again shows that pressure has little or no effect upon the cohesion of a liquid, and that it is reduced to zero at about the same temperature notwithstanding the pressure.

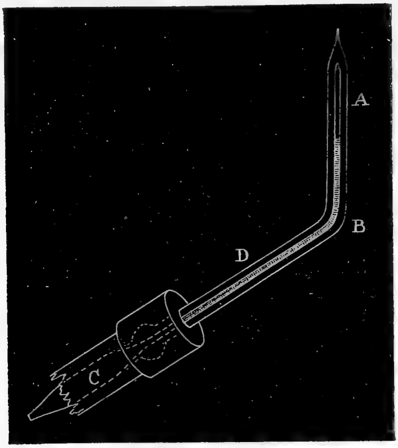


It would appear, then, that the boiling line does not extend beyond the critical point, but that the critical point lies upon an isothermal, which is the limit of the liquid state.

I have examined several liquids and liquefied gases, including CO_2, NH_3, SO_2, N_2O, CS_2, CCl_4, Cl, CH_4O, $C_2H_{10}O$, and C_2H_6O, and I find that the capillarity disappears at or near the critical point, and pressure does not cause it to reappear.

I have also measured the rate of decrease of capillarity with temperature, but have not yet had time to reduce the arbitrary readings to actual measurements to find whether or not any systematic relation exists. I shall bring this work forward at some future time.

I have still another method of examination to bring before the Society. An apparatus was arranged with the tube for experimenting with the liquid bent at an obtuse angle, as shown in the sketch. The liquid was contained in the upper part, A, inclosed by the mercury, B; while the lower part, C, inclosed in the apparatus, was filled with nitrogen or other permanent gas. On applying pressure the nitrogen is compressed into small space, and by gently tapping the tube is made to rise to about D, where it rests against the upper side of the slanting part of the tube. When the temperature and pressure required were reached the tube was gently tapped, when the bubble of nitrogen passed round the bend up into the fluid at A. If the fluid be in the liquid state, the bubble shows a meniscus; but if the fluid be gaseous, the bubble instantly diffuses into the inclosed space, showing that the fluid has no cohesion or has assumed the gaseous state. The same results were obtained as before. When the temperature was below the critical point the contents of the tube were liquid, and when over that temperature the reaction was always gaseous, notwithstanding the variations of pressure.



I think that we have in these experiments evidence that the liquid state ceases at the critical temperature, and that pressure will not materially alter the temperature at which the cohesion limit occurs. Dr. Andrews has, indeed, indicated his belief that such was the case; but as far as I am aware, there has been till now no direct experimental proof of the fact. When the line dividing the fluid from the gas, and the difference in height outside and inside the capillary tube disappear, it cannot be said that this proves the total absence of cohesion, but only that the cohesion of the two portions of the fluid becomes equal; and it is only if the gas or vapour has no cohesion that we can say that the portion which formed the liquid has none either. But by the introduction of a third substance, which, when the experiments are done quickly, acts only on one side, it seems, from the results observed, that the condition of the surface of demarcation depends mainly, if not entirely, upon the lower or liquid portion of the fluid. The critical points of liquids are thus truly the absolute boiling points, as has been aptly said, and are closely comparable to the melting points of solids, which depend almost solely upon temperature, a very great amount of pressure being required to alter them.

The difference between the fluid and gaseous states is not then entirely dependent upon the length of the mean free path, but also upon the mean velocity of the molecule.

June 14, 1880.

IV. "On the Solubility of Solids in Gases. II." By J. B. Hannay, F.R.S.E., F.C.S. Communicated by Professor G. G. Stokes, D.C.L., &c., Sec. R.S. Received May 24, 1880.

In a former paper* it was pointed out that a solid dissolved in a liquid was not deposited from solution when the liquid passed through the critical point into the gaseous state, and the conclusion drawn that solids are soluble in gases. I have shown in another paper † that it is probable that after a liquid passes its critical point it becomes a vapour or gas, and this quite independent of pressure, and that the cohesion limit for any one substance lies more in the direction of an isothermal line than a continuation of the boiling line. Before, however, I had carried out that work, I determined to make sure that a truly gaseous state had been reached, and the fluid containing the solid was raised to 150° above its critical point, and the gaseous solution expanded to nearly twice its liquid volume, and still the solid remained in solution. In these circumstances it was found that non-volatile solids were retained in solution in truly gaseous menstrua. In the second paper referred to, I have pointed out some of the difficulties which beset such an investigation; and when it is remembered that only air-baths can be used, the temperature being over 200° and sometimes over 300°, and that very many liquids dissociate under these temperatures, it will be seen that conclusions should be drawn only from substances perfectly free from suspicion. All the experiments, therefore, which I have performed in iron tubes cannot bear on a discussion in this matter; and I shall defer going into the natural minerals formed artificially from aqueous solution at a red-heat till another paper. For high temperatures glass does not do, as every variety of it is easily decomposed by water; but by the use of rock crystal blocks I have obtained some results of interest where the action of the water can be seen. However, I intend in this paper to treat in a fuller manner of work touched upon in former papers. One of the first pieces of work I set about to finish was to determine whether or not the critical points were altered by the introduction of a solid into solution.

In a former paper it was thought that the critical point of alcohol

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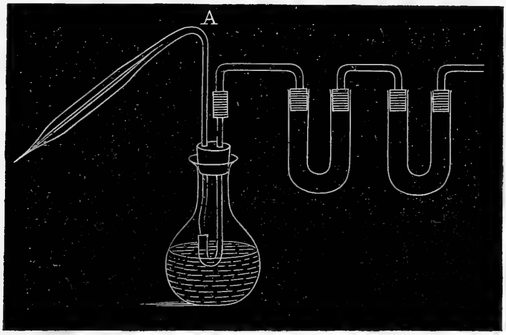
* "Solubility of Solids in Gases," "Proc. Roy. Soc.," *ante*, p. 178.

† "On the State of Fluids at their Critical Temperatures," *ante*, p. 478.

containing potassic iodide in solution was higher than pure alcohol, but subsequent experiments determined me to go into the matter again. The cause was this. I found that absolute alcohol if exposed to air for a very short time takes up sufficient moisture to alter its critical point; and I considered that in preparing the solution of the iodide some moisture might have been introduced. The experiments were repeated in the following manner. Some common absolute alcohol was placed in a flask with caustic lime, freshly burned, and cohobated for a week. The flask was then fitted with the arrangement shown in the diagram. The tube connected with the drying arrangement is turned up at the lower end, so that the exit tube may be slipped into it. A current of air desiccated over sulphuric acid is passed through this for some time, and the tube A then slipped out of the recurved end in the flask, turned round so as to clear it, and pushed down till it is below the level of the desiccated alcohol, which is forced up into the experimental tube sealed or connected to A. In this way, any quantity of alcohol, which has never been removed from contact with lime, can be introduced into the working tube. A portion of fused potassic iodide placed in the experimental tube before the experiment enables a saturated solution to be prepared out of contact with air when the liquid is sealed up. Twenty experiments were done with pure alcohol, and twenty with alcohol saturated with potassic iodide. The pure alcohol had the specific gravity ·7928 at 15°·5.



	Alcohol alone.		Alcoholic solution of iodide.
Mean temperature	234·3	....	235·1
Probable error of single experiment	0·62	....	0·50
Mean pressure	64·5	....	65·8
Probable error of single experiment	0·83	....	0·95

These numbers are very much closer than the former ones, and go to show that with proper precautions the critical point is very slightly raised indeed by the introduction of a minute quantity of solid. Were the iodide more soluble, or did its solubility increase more rapidly with temperature, I have no doubt that its effect upon the critical point would be much more pronounced.

By performing a large number of experiments I came to the following conclusions:—1st. The gas must have a certain density before it will act as a solvent, and when its volume is increased more than twice its liquid volume its solvent action is almost destroyed. 2nd. Retaining the volume the same, the higher the temperature the greater the solvent power. This was determined by placing an excess of iodide in the tube and raising the temperature to a given point and maintaining it for an hour or so, then noting portions still undissolved, raising the temperature again, and watching the effect till a temperature was reached when all the salt dissolved. As the increase in the solubility in potassic iodide is very slight, it takes a considerable rise to have any apparent effect. Thus we see that two conditions are requisite, molecular closeness and *vis viva.* We see from this that the liquid condition of fluids has very little to do with their solvent power, but only indicates molecular closeness. Should this closeness be attained by external pressure instead of internal attraction, the result is that the same or even greater solvent power is obtained; greater because of the greater activity of the molecules so compressed. That this is the case is seen well in the case of hydrogen, which, when compressed by a pressure of 1,000 atmospheres, measured by the hydrogen manometer, dissolves potassium, sodium, and lithium. The experiment is done in this way. A piece of the metal is placed in the end of the tube, and the dry hydrogen below it. The pressure is then applied, and after twenty or thirty minutes the mercury near the metal will be found to be solid amalgam. This is at the ordinary temperature; but should the temperature be raised to 100°, the mercury becomes solid in a few seconds. I intend to apply still higher pressures, and attempt to dissolve harder metals. This is a case of purely gaseous solution, the hydrogen being at a very great distance from its critical point.

The conclusions arrived at in a former paper have been called in question in a paper by Professor Ramsay, and I would ask permission to point out some errors into which I think the author has fallen. He says:—"When the tube contains a small amount of liquid the space left for gas is larger, and consequently more vapour must be given off by the liquid before enough gas can be compressed till it acquires the same specific gravity as the liquid, the *temperature* at which the meniscus disappears is *consequently* higher."* It is difficult to understand how the author came to such a conclusion, unless he was thinking of the *amount* of heat required to form more vapour, and confounding calorimetry with thermometry. His conclusion, as it stands, is at variance with all the experiments and conclusions of the

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* "Proc. Roy. Soc.," vol. xxx, page 326, lines 10 to 14. The italics are mine.

many eminent workers in the field. It would lead to the result that the boiling point of a homogeneous liquid would vary with the amount of space over the liquid, and that the vapour tensions of liquids as determined by placing some over the mercury in a barometer, would vary with the amount of liquid used. This no doubt would be true of a mixed liquid whose constituents have different boiling points, but cannot be said of a homogeneous liquid. I had often noticed that the critical point of a carefully dried liquefied gas was the same whether observed at the top of the tube or at the bottom; but any impurity or moisture would cause the critical point observed at the bottom to be high, as it would be concentrated in the last portions of the liquid. Since Dr. Ramsay's paper was published I have again gone over this ground, and to prevent mistake I used the same liquid in the same tube, varying its volume so as to make the disappearance of the division take place near the top or bottom of the tube; and I here give a series with alcohol and carbon dioxide with the results.

Alcohol.		Carbon dioxide.	
Top.	Bottom.	Top.	Bottom.
234·5	234·1	31·2	30·9
234·0	234·7	31·0	31·3
235·0	234·5	30·7	31·2
234·2	233·9	30·8	30·7
234·8	234·3	31·1	30·8

In these experiments every precaution was taken in purifying the substances, but the bath used was the old double one, otherwise closer observations might have been made. They prove exactly what was known before, and what Dr. Ramsay denies, that a homogeneous liquid has a fixed boiling point, whether that be the absolute boiling point (critical point) or a lower one. I find that liquefied gases give the best results to illustrate this, as they are more easily obtained pure and dry. I have tried ammonia and sulphur dioxide with the same results. Several ethers, especially oxalate of ethyl and methyl, failed to give the same temperature at top and bottom, a difference of several degrees often occurring. This, I have no hesitation in saying, is due to impurity or to alteration of the liquid at the elevated temperature. It is difficult to conceive how it could be imagined that the ratio of a homogeneous liquid to the size of the vessel containing it could influence the tension or density of its vapour. As experiment and theory point to the fact that at the same temperature the vapour of the same liquid must have the same tension and density, irrespective of the size of the containing vessel.

In another place, Dr. Ramsay says:—"From the first experiment it is seen that, on cooling, the liquid contracts more rapidly than the gas." In another, "Whereas, even above their critical points, liquids are comparatively incompressible." A fluid is homogeneous above its critical temperature, and it seems useless to theorise on the compressibility of a subject not known to exist. Besides, in these statements he makes temperature and pressure act in different ways. Compared with gas, the liquid is more sensitive to heat, but again compared with gas, the liquid is comparatively incompressible. The author says:—"Carbonic anhydride, examined by Dr. Andrews, is abnormal in this respect, but of this I am not certain." I must say all the bodies I have examined are also "abnormal." Dr. Ramsay's idea of the critical point is, that it is the temperature where their densities being equal, gas and liquid mix; but the entire absence of capillarity, showing no difference in the cohesion of the upper and lower portions of the fluid, and the fact that we have two portions of the same chemical compound at the same temperature, under the same pressure with the same density, would prove to any ordinary mind that they are identical, as I have shown them to be in the paper already referred to.

Dr. Ramsay seeks to explain the solubility of solids in gases by saying "these gentlemen have observed nothing unusual, but merely the ordinary phenomenon of the solubility of a solid in a hot liquid." His former explanation would not hold here however, as the liquid holding a solid in solution would have a greater density than the gas, and so would not mix. Besides, suppose his explanation were true, it would still prove the solubility of solids in gases, because, were the solid not soluble in the gas, it would be precipitated when the liquid mixed with the gas, as we know takes place when a solution is mixed with a liquid in which the dissolved body is insoluble. Again, according to Dr. Ramsay's theory, the higher the temperature the less the solvent power would be, but the opposite is the case. He attributes to me the idea that, because a solid is dissolved in a gas, it is gaseous; but I never supported that idea. Let us now consider Dr. Ramsay's repetition of my experiment with potassic iodide. The solubility of the iodide in alcohol is very low (1 in 40), and it increases very slowly with temperature, yet, when he caused a solution saturated just below the critical point to assume the gaseous condition, he evidently expected a very sudden increase in the solvent power, and such not being the case, he concludes that solids are not soluble in gases. Then, the tube was only one-third full, and the pressure in a case of that sort would be so low that the solvent power would be very weak. When Dr. Ramsay repeats the experiment with more liquid, and raises the temperature sufficiently, he will see that the solvent power is not altered in passing the critical temperature.

An important point, which is sometimes lost sight of, is that, although the pressure is independent of volume as long as the two states of fluids exist together, whenever the critical temperature is passed, the fluid in the vessel which contains the greatest amount of liquid will develop the highest pressure on raising the temperature. When, therefore, a tube contains a small amount of liquid, although below the critical temperature, it develops as much pressure as a tube nearly full, yet on passing that point it may not have sufficient molecular closeness to exhibit solvent power, the cohesion existing below the critical temperature giving it sufficient closeness there. Hence, with a small quantity of liquid, the phenomenon is seen of a liquid depositing a solid (which it held in solution) at the critical point, but redissolving it again, when a certain increase of temperature gave it sufficient *vis viva* to compensate for its want of density. With a larger volume of liquid, the solution being the same, no deposit takes place.

Then, as to the asserted difference in refractive power. I have had an apparatus made specially for this purpose and, when the liquids are pure, there is not the slightest difference in refractive power between the upper and lower layers. Liquids which may be pure at low temperatures may give rise to new products at higher temperatures, and thus produce a different result. I have not my determinations reduced yet, but will publish them shortly. They are done with a glass prism, with thick sides, and the results are reliable; but Dr. Ramsay's tube may have varied more than his liquids. In another paragraph he says:—"It would be interesting to speculate on the condition of a fluid of which two portions possess the same specific gravity, but refract light differently." How have the two portions the same specific gravity when one is always left at the *bottom* of the tube? I don't think any clearer proof is required to show that it is an impurity, or a product of decomposition at the high temperature, and its greater refraction also proves its greater density. Dr. Ramsay was very careful to expel all air, and evaporated the liquid to less than half its volume, thus concentrating the denser impurities in the remaining portion, and as momentary exposure of a pure liquid to air slightly hydrates it, we have conditions which suffice for producing the mixed liquid required for the results we observed. Lastly, Dr. Ramsay says:—"Surely no clearer proof is needed to show that the solids are not present as gases, but simply as solution in a liquid medium," than that they retain the same spectrum as in the solid state. We do not want proof that they are present in the gaseous state, which is very different from being dissolved in a gas.

In another paper, I have shown that there can be no liquid above the critical point, as this is the termination of all properties which distinguish a liquid from a gas, so that a fluid has only one state above that temperature. Solution of solids is (when chemical action does not come into play) a function of all fluids, the requisite conditions being molecular closeness and thermal activity.

June 14, 1880.

V. "Note on the History of the Carbon Spectrum." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 26, 1880.

In a "Note on the Spectrum of Carbon," read before the Society on April 29, 1880, Mr. Lockyer has in the first place alleged that we have advanced within a very short period inconsistent opinions as to spectra of compounds of carbon; he has next implied that we have given an unfair representation of the history of the carbon spectrum, and, lastly, has directly challenged the correctness of some of the conclusions we have drawn from our experiments. We propose in this paper to reply to him on the historical questions, and in another paper to deal with the experimental evidence which he has produced in contravention of our conclusions.

That the inconsistency alleged has not really existed in our opinions, will be seen by any one who reads the first passage referred to as it appeared in the published "Proceedings of the Society" (vol. xxx, p. 87), which stands as follows:—

"The well-nigh impossible problem of eliminating hydrogen from masses of carbon, such as can be employed in experiments of this kind, proves conclusively that the inference drawn by Mr. Lockyer as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, *seeing that undoubtedly hydrogen was present in the carbon** *and in all probability nitrogen in the chlorine.*"

Mr. Lockyer has quoted the *confidential, uncorrected, proof* instead of the published paper which had been issued before Mr. Lockyer's paper was read. In the published paper a clause, accidentally omitted before, and no way altering the general drift of the passage, had been inserted to make the sense clearer. The paper from which he quotes deals with the chemical interactions taking place in the arc between the matter of the poles and the constituents of the surrounding atmosphere. It is there demonstrated that specially purified carbon poles continue to produce in dry air cyanogen compounds, and that hydrogen always forms an essential ingredient of such poles. No reference was made in this paper to the specific origin of any flutings in the spectrum of the arc, the sole question under consideration being the possible existence of carbon compounds in the electric arc taken in dry chlorine, on the non-existence of which Mr. Lockyer bases his proof of the elementary character of the carbon spectrum. That part of the paper from which the quotation is taken related particularly to the combination of nitrogen with the carbon of

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* Mr. Lockyer having italicized the former half of the last clause, we italicize what he chose to omit.

the poles, and the final clause added in the published paper accords with it.

There is no inconsistency between the opinion there expressed and that subsequently given in the paper "On the Spectra of the Compounds of Carbon with Hydrogen and Nitrogen;" and that such a charge could have been made is due only to Mr. Lockyer's having quoted the *uncorrected proof.*

To come to the omissions we have made in reference to the work of others. The complaints appear in such remarks as the following, which we extract from Mr. Lockyer's paper :—

"As Messrs. Liveing and Dewar do not controvert the very definite conclusions arrived at by Attfield, Morren, Watts, and others, I can only presume that they have taken for granted that the experimental work performed by these men was tainted by the presence of impurities, and that it was impossible to avoid them."

"The only reference to this admirable work, in which vacuum tubes and the electric discharge were largely employed, which I can find in Messrs. Dewar and Liveing's paper is the following :—" The spectrum of hydrocarbon *burning in air* has been . . . described . . . . by Attfield."

It was not our intention to give a complete historical account of what had been done previous to the date of the paper by Ångström and Thalèn ("Nov. Acta Reg. Soc. Upsal.," 1875), as in that paper they had discussed the observations of experimenters who had been engaged on this subject before that time. Nor had we any desire to avoid or controvert, or take anything for granted, in the work of early investigators. We merely passed over for the sake of brevity such work as had not stood the test of subsequent investigation. We could hardly accept Dr. Attfield's work, however good for its time, as authoritative, seeing that his experiments were made, as he allows ("Phil. Mag.," 1875), with very imperfect appliances, and evidently without his being so well aware as we now are of the nicety required in regard to the purity of his materials. In fact, he found no distinction between the spectrum of carbonic oxide and that of hydrocarbons, and found the lines of nitrogen in the spectrum of the flame of cyanogen.

We can hardly be charged with ignoring Morren's work, for we have referred to it again and again. The particular passage quoted by Mr. Lockyer embodies observations and conclusions which are at variance with those of Plücker and Hittorf, as well as of Watts ; and are substantively discussed by us in our paper.

Dr. Watts' position with reference to this subject deserves very special consideration, as he has perhaps directed his attention to it more continuously and laboriously than any other investigator. And as his publications have extended over a long period of years, it is clear that a grave injustice is done him if his early work be selected for quotation instead of the maturer product of his latest study. This kind of selective quotation does not represent the true scientific spirit.

The quotation from Dr. Watts' paper ("Phil. Mag.," 1869) which Mr. Lockyer prefaces by this remark—"His work was thus summarised by himself,"—has in reality no reference to a complete summation of his experiments, but has solely to do with "the typical form of the first carbon spectrum, that obtained when olefiant gas and oxygen are burnt together in an oxyhydrogen blowpipe jet." That this is the correct view of Dr. Watts' position, the following extracts will show:—"The spectrum obtained from cyanogen varies with the mode of production. The flame of cyanogen in oxygen exhibits γ, δ, and ϵ; the red group is replaced by a series of bands which show an opposite character to the rest of the spectrum, inasmuch as each band is brightest at the most refracted edge. If cyanogen be burnt in air instead of in oxygen these bands are more numerous, extending nearly to δ and replacing γ, which is then not to be seen. Instead of the group *f* we have two very brilliant groups of lines, ξ, which includes seven lines, and θ, which is composed of six lines."

The two very brilliant groups of lines which are referred to as ξ and θ by Watts, and which distinguish the flame of cyanogen, are two of the sets of channelled groupings which we, in our paper on "The Spectra of the Compounds of Carbon with Hydrogen and Nitrogen," referred to a compound of carbon with nitrogen. Further on, in the same paper of Dr. Watts, the following occurs:—"In comparing the spectra of fig. 1, we notice that the changes take place at the ends of the spectra, the central groups, γ, δ, ϵ, remain substantially the same. If we pass from the spectrum of the olefiant gas-flame to that of the cyanogen-flame, we find the change at the blue end of the spectrum consisting in the disappearance of the group *f* and its replacement by the groups ξ and θ. The group *f* is not absolutely proved to belong to carbon (that is, it may be caused by carbonic oxide or carbonic anhydride); but the groups ξ and θ, since they are common to carbonic oxide, cyanogen, and naphthaline, must be due to carbon, and their presence may, with much probability, be attributed to the higher temperature of the cyanogen-flame." *

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* When Mr. Lockyer makes the following statement:—"I have also repeated Morren's experiment and confirmed it, and I have also found that the undoubted spectrum of cyanogen is visible neither in the electric arc nor in the surrounding flame,"—he is referring to a different part of the spectrum altogether from the one we have been discussing, and the application of the term "undoubted" to the specific part of the spectrum to which he here refers is simply an expression of his own view. Ångström and Thalèn in their "Recherches sur les Metalloïdes," 1875, have the following passage:—"Les groupes nuancés de cyanogène, situés dans les parties bleues et violettes du spectre, se montrent aussi, soit quand l'étincelle traverse la partie luisante d'une flamme à gaz, soit dans l'arc voltaïque entre des électrodes du charbon d'une pile puissante. Cependant, ce dernier spectre du

On the ground of observing these two characteristic groups of lines ξ and θ (our seven blue and six violet nitrocarbon bands) in the spark spectra of carbonic oxide, cyanogen, and naphthaline, Dr. Watts was entitled at the time to infer that they must be due to the common element carbon. Dr. Watts has, however, made many experiments on the carbon spectrum since the date of that paper, and to neglect to take into consideration papers published by him since 1869 is to misrepresent his position. In the "Phil. Mag." for 1874, he wrote in a "Note on the Carbon Spectrum" as follows:—"In the 'Phil. Mag.' for October, 1869, I described four different spectra as spectra of *carbon*. One of them was the ordinary spectrum from hydrocarbon flames, first described by Swann; the second was the spectrum obtained from vacuum tubes inclosing carbonic oxide, carbonic anhydride, or olefiant gas; the third was the spectrum of the Bessemer flame; and the fourth, the high tension spark in carbonic anhydride or carbonic oxide.

"I have since shown ('Phil. Mag.,' 1873) that the Bessemer flame spectrum, instead of being a spectrum of carbon, is the spectrum of manganese oxide; and I have had now to add the result of recent observations, which show that the second spectrum also is due, not to carbon itself, but to some *oxide* of carbon. This spectrum was held to be a spectrum of carbon because it was common to compounds of carbon with hydrogen and with oxygen.

"I have now found that it is not given by spectral tubes inclosing olefiant gas if special care be taken to exclude all traces of oxygen. . . . . We have therefore only one spectrum which can be proved to be due to *carbon*—that, namely, which is common to the flame of olefiant gas or cyanogen, the electric discharge in cyanogen or carbonic oxide at the ordinary pressure, and to the electric discharge in vacuum tubes inclosing cyanogen, olefiant gas, or hydrocarbons, such as benzol."

We infer from this, that the groups ξ and θ (our seven blue and six violet nitrocarbon bands), which are certainly not common to all the spectra here enumerated, are not included in the one spectrum which Dr. Watts at that time assigned to carbon. However that may be, it is unnecessary to say more about it here, for we have quoted enough to show that Dr. Watts' conclusions, in 1869, were not so certain that they could now be quoted as authoritative against the inferences drawn from later observations, and that we did our subject no injustice in making no more particular allusions to them than we did.

But, further, after describing an experiment with carbon tetrachloride, Mr. Lockyer says:—"This result, which entirely endorses the work of Attfield and Watts, has been controlled by many other experiments." If we may assume that the work of Attfield and Watts

cyanogène, qui est remarquable par l'éclat vif des raies, n'est pas pur, mais mêlé des raies des carbures d'hydrogène, dont le splendeur est encore plus magnifique."

alluded to is that described in the previous quotations, it is not a little remarkable that Mr. Lockyer's experiments should so "entirely endorse" what Watts himself has shown to have been in part erroneous.

Lastly, we cannot allow Mr. Lockyer's assertion that Ångström and Thalèn's conclusions "rest more upon a theory which has been shown to be false since the labours commenced, and analogy, than upon experiment," to remain uncorrected. Their conclusions are eminently inductions from experiments carried on, as they say, during several years, of which the most important are particularly described. Many of the experiments which led up to their conclusions had been previously published by Thalèn in 1866 ("Årsskrift, Upsala"), in a paper in which he correctly described and distinguished the spectra of hydrocarbons, of oxide of carbon, and of carbon respectively, and besides described the spectrum of the spark between carbon poles in nitrogen. This paper is an admirable sample of good experimental work, and its perusal, together with the later paper of 1875, leaves no doubt that, whatever theories they may have held, Ångström and Thalèn relied mainly on experiment and observation to prove the soundness of their conclusions.

In conclusion, Mr. Lockyer introduces a reference to a former work of ours on the magnesium-hydrogen spectrum, and to a theoretical deduction regarding the interactions which might produce the cometic spectrum, making the following remark:—

"From what I have shown it will be clear that the consequences drawn in the following paragraphs by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands are entirely invalid."

As this opens out a question entirely beside that in hand, depending on the validity of the premises from which Mr. Lockyer draws his conclusion, we prefer to deal with it when we discuss, in our next paper, the spectra of carbon compounds and the experimental evidence Mr. Lockyer has adduced in support of such assertions.

VI. "On the Spectra of the Compounds of Carbon with Hydrogen and Nitrogen. No. II." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received May 27, 1880.

In our last communication on this subject (*ante*, p. 152), we thus summarised the results of our observations as to the "nitrocarbon spectrum."*

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* In using this term we merely mean that we are dealing with a spectrum invariably associated with the presence of nitrogen and carbon in such conditions that chemical union takes place, without any reference to the particular compound produced.

"On a review of the whole series of observations, certain points stand out plainly. In the first place, the seven blue, the violet, and ultra-violet bands, characteristic of the flame of cyanogen, are conspicuous in the arc taken in an atmosphere of nitrogen, air, nitric oxide, or ammonia, and they disappear, almost, if not quite, when the arc is taken in a non-nitrogenous atmosphere of hydrogen, carbonic oxide, carbonic acid, or chlorine.* These same bands are seen brightly in the flames of cyanogen and hydrocyanic acid, but are not seen in those of hydrocarbons, carbonic oxide, or carbon disulphide. The conclusion seems irresistible that they belong to cyanogen; and this conclusion does not seem to us at all invalidated by the fact that they are seen weakly, or by flashes, in the arc or spark taken in gases supposed free from nitrogen, by reason of the extreme difficulty of removing the last traces of air. They are never, in such a case, the principal or prominent part of the spectrum, and in a continuous experiment they are seen to fade out in proportion as the nitrogen is removed. This conclusion is strengthened by the observations of one of us, that cyanogen (or hydrocyanic acid) is generated in the arc in atmospheric air in large quantity."

The observations and experiments which we have since made all tend to confirm the correctness of the foregoing induction, while they help to remove the apparent inconsistency of some recorded observations with our conclusions. More particularly they explain the appearance of the bands which we, in common with Ångström and Thalèn, have ascribed to a compound of carbon with nitrogen, in some cases in which nitrogen had been supposed to be absent, by showing the sensibility of the spectroscopic test for nitrocarbon compounds under varied circumstances, and the consequent necessity for something more than ordinary care in the preparation of materials and conduct of experiments, if inferences dependent on the presumed absence of nitrogen (or hydrogèn, as the case may be) are to be drawn from them.

The Flame of Cyanogen.

The accompanying diagram (fig. 1) shows approximately the relative position of the bands in that part of the spectrum of the flame of cyanogen fed with a jet of oxygen which is more refrangible than the Fraunhofer line F. Only those bands which are less refrangible than the solar line L have been before described, but photographs show another set of two shaded bands slightly less refrangible than the solar line N accompanied by a very broad diffuse band of less intensity on the more refrangible side of N; also a strong shaded band, which appears to be absolutely coincident with the remarkable shaded

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* We have repeated and confirmed the experiments with the De Meritens arc in different gases. The arc examined in different *fluids* also supports the original conclusions.

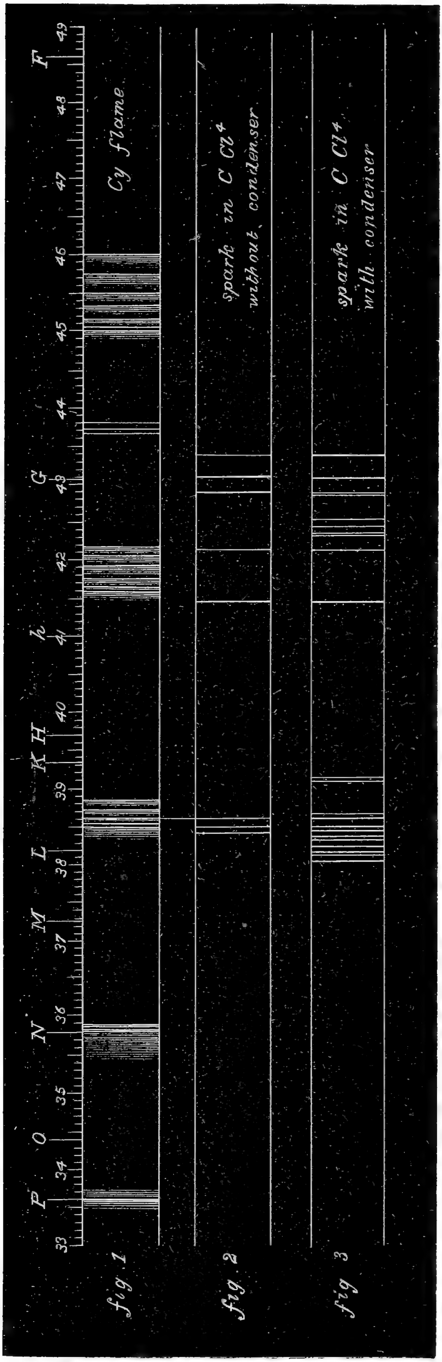
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fig 1
Cy flame
fig 2
spark in C Cl4 without condenser
fig 3
spark in C Cl4 with condenser

band in the solar spectrum, which has been designated by the letter P; and near this, on the more refrangible side, a much fainter diffuse band, which also seems to coincide with a part of the solar spectrum sensibly less luminous than the parts on either side of it. This spectrum is remarkably persistent at all temperatures of the flame. Watts found that it did not disappear when the flame was cooled down as much as possible by diluting the cyanogen with carbonic acid; and we have found that it retains its characters when the cyanogen is burnt in nitric oxide. The flame in the last case must be one of the hottest known, from the large amount of heat evolved in the decomposition of cyanogen and nitric oxide, namely, 74,500 and 43,300 units per molecule respectively. There is in the case of cyanogen, as in the case of so many other substances, a difference in the relative intensities of the different parts of the spectrum of the flame at different temperatures, but no other change of character; and we are unable to confirm the observation of Watts, that cyanogen burning in oxygen gives only the bands which we assign to hydrocarbons. It is true, that in the upper part of the flame where much or all of the cyanogen is oxidised or decomposed the spectrum is continuous, but at the base of the flame where it is issuing from the nozzle we have always seen the nitrocarbon bands, both when the cyanogen is burning in oxygen and when it is burning in nitric oxide.

On the theory that these groups are the product of an exceptional temperature in the case of the cyanogen flame, it is inconceivable that they could disappear by combustion in oxygen, instead of in ordinary air. Our observations accord with the statement of Morren, Plücker, Hittorf, and Thalèn, that a cyanogen flame, fed with oxygen, when it is intensely luminous, still yields these peculiar groups. We have found these peculiar groupings in the flame when it had a current of oxygen in the middle, and was likewise surrounded outside with oxygen. There is nothing remarkable in observing only a continuous spectrum from any hydrocarbon or nitrocarbon by using excess of oxygen, as we know from Frankland's experiments that carbonic acid and water vapour at the high temperature of flame under compression gives a continuous spectrum. In fact, this is what we should anticipate, provided intermediate, and not the final, compounds are the active sources of the banded spectrum.

Each of the five sets of bands shown in the diagram is attended on its more refrangible side by a series of rhythmical lines extending to a considerable distance, not shown in the diagram, but easily seen in the photographs.

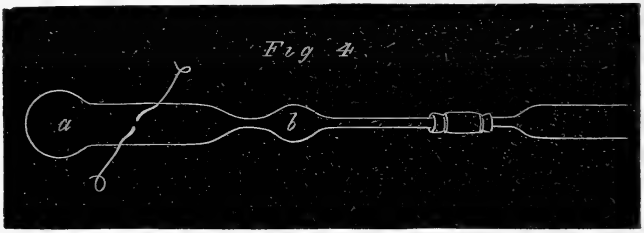
Coal gas burning in oxygen gives no bands above that near G within the range of the diagram, fig. 1, nor are the visible cyanogen bands seen in the flames of many organic compounds containing combined nitrogen; but beyond this our photographs show a spectrum of a character quite different from that at the less refrangible end. The most remarkable part of this spectrum is a long series of closely set strong lines, filling the region between the solar lines R and S, and ending abruptly with two strong lines a little beyond S. These are lines of various intensities, not regularly arranged so as to give shaded bands like those in the less refrangible part of the spectrum. Beyond these lines there is another large group of lines, not so strong or so closely set, but sharp and well defined. It will be a work of time to examine in detail this very striking and peculiar spectrum; at present we can only indicate its general character. We are not yet sure whether it is produced by pure hydrocarbons, or may not be due to impurities such as sulphur in the coal gas.*

Spark Discharge in Carbon Tetrachloride.

We have conducted the experiments with carbon tetrachloride with great care, and numerous repetitions on account of the prominence given to an experiment with this compound by Mr. Lockyer in a recent "Note on the Spectrum of Carbon," communicated to the Society, and because our results in every case differ from those which he obtained.

The form of sparking tube employed was similar to that used by Salet, and is shown in the annexed diagram, fig. 4. This was attached by thick rubber tubing to a straight glass tube of which one half, about 6 inches long, was filled with phosphoric anhydride, and the other half with small fragments of soda-lime to prevent any chlorine from the decomposition of the tetrachloride by the spark from reaching the pump. The pump was connected by rubber tubing to that end of the tube which contained soda-lime. The tetrachloride used had been prepared in our own laboratory from carbon disulphide, and fractionated until it had a constant boiling point of 77° C. Sufficient of it was introduced into the sparking tube to fill nearly one quarter of the bulb at the end, and the whole interior of the tube thoroughly wetted with it in order to facilitate the removal of the last traces of air.

Fig. 4.



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* Since this passage was written we have found reason to believe that this remarkable spectrum is due not to any carbon compound, but to water. As this takes it beyond the scope of the present paper, we purpose dealing with it hereafter in a separate communication. June 8, 1880.

The Sprengel pump employed differs slightly from those in common use. The reservoir is a globe funnel (*a*, fig. 5), and the valve formed by a glass rod (*b*) with a conical termination ground into the neck of the funnel. This rod at its upper end passes into an iron ferule (*c*), which, by means of a screw and a small frame fixed at the top of the globe, can be raised or lowered at pleasure. A little below the globe the tube expands, and the lower part of the tube enters this expansion, and terminates upwards in a narrow tube of platinum, round which the glass is fused. By this means the mercury is continually drawn from the centre of a mass of mercury, and there is less chance of air descending with it. Moreover, as the exhaustion proceeds the part of the tube *h*, above the level of the fall *k*, becomes vacuous, and is an effectual trap for any air which may be sucked down with the mercury from the globe. The necessity for such a trap is apparent, for when the pump has been in continued use for two or three days there is always a small bubble of air collected in *h*.

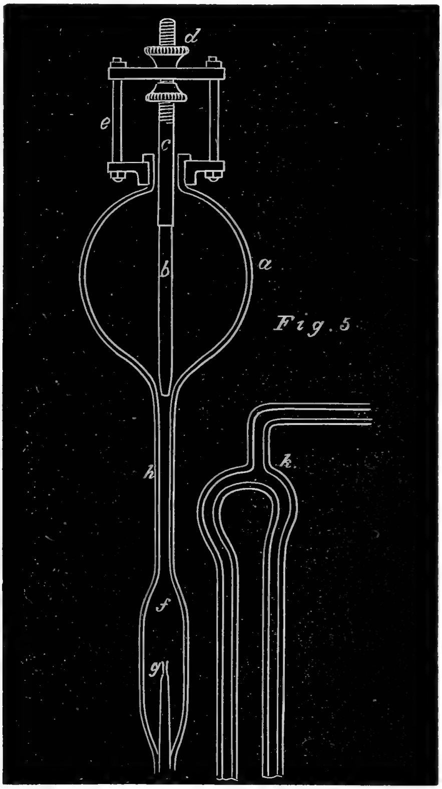

Fig. 5

When the tube containing the tetrachloride had been so far exhausted that little but condensible vapours were pumped out, the bulb was heated so as to fill the apparatus with vapour of tetrachloride, the pump still going, and this was repeated as long as any incondensible gas was extracted. Sparks were then passed through the tube for a short time, the pump still being kept going. After a short time it was unnecessary to keep the pump going, as all the chlorine produced by decomposition of the tetrachloride was absorbed by the soda-lime. On now examining the spectrum, no trace of any of the bands we ascribe to nitrocarbons could be detected, either by the eye or by photography, however the spark might be varied. The violet lines of chlorine described by Salet were more or less visible, coming out brightly when a condenser was used. Several tubes were treated in this way, and many photographs taken, but always with the same result; no trace appeared of either the seven blue, the six violet, the five ultra-violet, or of the still more refrangible bands of the cyanogen flame. It is true that all the photographs showed three lines in the ultra-violet, but these do not at all closely resemble the nitrocarbon bands, they are not shaded like the nitrocarbon bands. The least refrangible of the three is coincident with the middle maximum in the ultra-violet set of five bands, but the other two do not coincide with other of these maxima. When a condenser is used these three lines come out with much greater intensity, and two other triplets appear on the more refrangible side, as well as other lines. In order to compare the positions of these lines with the cyanogen bands, we have taken several photographs of the spark in tetrachloride simultaneously with a cyanogen flame, the latter being thrown in by reflexion in the usual way. Wratten and Wainwright's instantaneous dry plates were used, and gave a strong picture of the cyanogen flame after three seconds' exposure, but in order to ensure getting a picture of all the lines which the spark could produce, we gave an exposure of three minutes to the spark. Not one of many photographs so taken showed any traces of the cyanogen bands. The general character of the violet part of the spectrum of the spark in carbon tetrachloride taken without a condenser, but not the exact position to scale of wave-lengths of all the lines, is shown in Diagram 1, fig. 2. Fig. 3 of the same diagram shows the brightest of the additional lines which come out with the use of a condenser. Photographs of sparks taken in hydrochloric acid showed a precisely similar group of ultra-violet lines, so that we have no doubt that the three lines which our photographs show amongst the five ultra-violet nitrocarbon bands are due to chlorine.

A bulb tube was next partly filled with tetrachloride, the end drawn out to a long narrow neck, and the tetrachloride boiled out in an oil-bath (150° C.) so as to expel all air, and the tube sealed off. This tube likewise showed no nitrocarbon bands.

A bulb was then blown in the middle of a piece of narrow glass tubing, and platinum wires sealed into it; some carbon tetrachloride was introduced into the bulb, and a current of hydrogen passed through the tube at atmospheric pressure; when the air was judged to be expelled sparks were passed by means of the wires in the bulb, but no nitrocarbon bands could be detected. In this case the rapid deposit of carbon soon brought the observations to a close, but the result was the same as before.

Solid trichloride was next tried. It was introduced into a tube similar to that described above in the first experiment with tetrachloride, and treated as the tetrachloride had been, the trichloride being mostly sublimed over into the small bulb shown in fig. 4, *b*. The trichloride was found to behave as far as the spectrum is concerned very much as the tetrachloride had done. It gave no nitrocarbon bands at all.

Having satisfied ourselves by repeated trials that pure carbon chloride if free from nitrogen does not give any of the bands we ascribe to nitrocarbon compounds, our next step was to determine whether the addition of nitrogen would bring them out, and if so what quantity of nitrogen would make them visible. For this purpose we first loosened the binding of the rubber tube connecting to the pump a sparking tube containing tetrachloride, and found to give no nitrocarbon bands, and, after letting in very little air, immediately closed it again. On now passing the spark the six violet bands at once appeared, and soon became quite bright, the seven blue bands were not seen immediately the air was admitted, but they too were in a short time well seen.

After trying some other experiments of a similar kind which indicated that a very small quantity of nitrogen was sufficient to develope the nitrocarbon bands in one of these tubes, we introduced a minute fragment of bichromate of ammonia, carefully weighed, and wrapped in platinum foil, into the neck of one of the sparking tubes containing carbon tetrachloride, connected the tube to the Sprengel pump, and removed the air as before. On examination of the spark with the spectroscope no trace of any nitrocarbon band could be detected. A pinch-cock was now put on the rubber tube, and the bichromate was heated by a spirit lamp to decomposition (whereby it is resolved into nitrogen, water, and oxide of chromium). On now passing the spark the six violet bands were well seen. There was no change in the condition of the coil or rheotome, so that the spark was of the same character as it had been before when no nitrocarbon bands were visible, and the change in the spectrum cannot be attributed to any change in the spark. The weight of the bichromate was between ·0005 and ·0006 grm.; and the nitrogen this would evolve would fill just about $\frac{1}{20}$ of a cubic centimeter at atmospheric pressure. The tube held 30 cub.

centims., so that vapour of carbon tetrachloride when mixed with $\frac{1}{600}$ part of its volume of nitrogen, gives under the action of the electric spark the nitrocarbon bands distinctly. Other similar experiments confirmed this result. It is worthy of remark that the nitrocarbon bands were not seen instantaneously on the admission of nitrogen into the tube, but were gradually developed, as if it was necessary that a certain quantity of nitrocarbon compound should be formed under the influence of the electric discharge and accumulated before its spectrum became visible.

Spark Discharge in other Compounds of Carbon.

Some carbon bisulphide was introduced into another tube, which was drawn out to a long narrow neck, and the carbon bisulphide boiled out in an oil-bath at 200° C., and the tube sealed off. In the spectrum of the spark taken in this tube no nitrocarbon bands, either blue or violet, could be detected however the discharge was varied.

A similar tube, containing benzol and similarly treated, showed no trace of the nitrocarbon bands.

Another tube, containing naphthaline, previously well washed with dilute sulphuric acid, dried and resublimed, was attached to the Sprengel pump, and treated as the tubes with tetrachloride had been. The spark in this tube likewise showed no nitrocarbon bands. After a time the tube cracked, and then the nitrocarbon bands made their appearance, and on setting the pump going a good deal of gas was pumped out. When the air had again been pretty completely exhausted, the nitrocarbon bands no longer showed, but gradually reappeared again as air leaked through the crack. Another tube, containing a mixture of naphthaline and benzol, showed no trace of the nitrocarbon bands.

The observation of the nitrocarbon bands in the spectrum of the spark in naphthaline was one of the reasons which led Watts at one time to ascribe these bands to carbon only. Naphthaline is not well adapted to be the subject of a test experiment in this case, for, from its mode of preparation, it is liable to be contaminated with nitrogenous bases; and the tension of its vapour at ordinary temperatures is so small that, unless the supply is kept up by heating the tube, the spark soon so far decomposes it that the spectrum is reduced to that of hydrogen only. It appears, however, from our experiments, that if the naphthaline is sufficiently purified and freed from air it does not furnish the nitrocarbon spectrum.

As Watts laid much stress on the occurrence of the nitrocarbon bands in the spectrum of the spark taken in carbonic oxide at atmospheric pressure, though they do not appear in carbonic oxide at reduced pressures, as a proof that these bands were due to carbon only, we have made a series of careful experiments with carbonic oxide at atmospheric pressures.

In our first experiments with this substance the gas was made by the action of sulphuric acid on dried formiate of sodium. The dried formiate was placed in a flask (fig. 6) fitted with a rubber stopper, through which passed the tube of a globe funnel, with a stop-cock for the admission of the sulphuric acid, and an exit tube for the gas. The latter was plugged with asbestos to catch any liquid drops which might be thrown up, and was connected by a thick rubber tube to a tube of the form shown in fig. 6. That part of this tube marked *a* was filled, one-half with soda-lime to catch acids, and the other half with phosphoric anhydride to catch moisture; the gas, after passing through this tube, was burnt as it issued from the narrow opening at *b*.

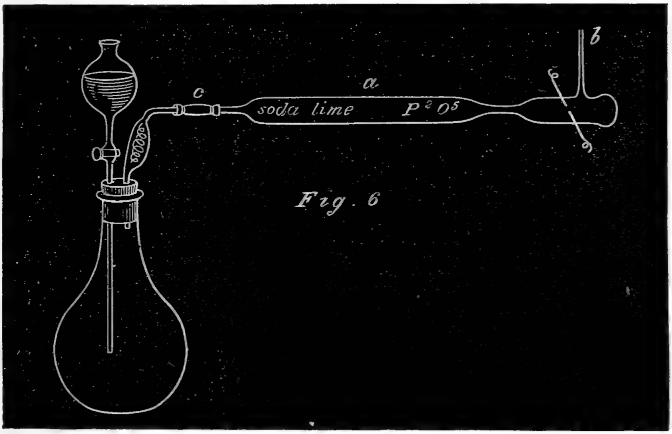

Fig. 6

The spectrum of the spark was observed through the small bulb blown on the end. The sulphuric acid was introduced gradually through the funnel, and the stream of gas allowed to pass through the tube for a long time. Sparks were passed between the wires, and the spectrum observed from time to time. At first the six violet bands were well seen, and the seven blue bands faintly; but gradually, as the air became more completely expelled, the blue bands disappeared entirely, and then the violet bands so far died out that it was only by manipulating the coil that they could be made visible, and then only very faintly indeed. A bubble of air, estimated by the length of the tube it filled to be about $\frac{1}{400}$ of the volume of gas in the flask and tube, was now introduced through the funnel, and almost immediately the bands reappeared brightly. As the stream of gas however continued, they again gradually died away until they were represented only by a faint haze. It was subsequently found that each introduction of fresh acid into the flask was attended with a marked increase in the brightness of the nitrocarbon bands, which died away again when the current of gas was continued without fresh introduction of acid. On testing the acid it was found to contain, as is frequently the case with sulphuric acid, a small quantity of oxides of nitrogen. This quantity was, however, so small that the amount of nitrogen introduced at each introduction of acid must have been but a small portion of the amount introduced in the bubble of air above mentioned, yet it gave a very sensible increase in the brightness of the nitrocarbon bands. The difficulty of getting all the air expelled from the apparatus led us next to adopt a modification of this experiment. The carbonic oxide was generated by heating in a tube of hard glass in an ordinary combustion furnace a mixture of pure and dry potassium oxalate with one quarter of its weight of quicklime, the mixture having been previously heated for some time so as to expel traces of ammonia. The rest of the apparatus was similar to that used in the previous experiment, but in order to expedite the removal of the air the narrow tube, *b*, was connected with a Sprengel pump, and the air exhausted before heating the oxalate. The distant end of the tube with the oxalate was then heated, and the whole apparatus filled with carbonic oxide; it was then again exhausted with the pump, refilled by heating more oxalate, and the gas allowed to stream out through the pump for some time. The heat was then lowered, sparks were passed and the spectrum observed. No trace whatever of the nitrocarbon bands could be detected, however the spark might be varied. The pump was now set going again, and the pressure of the gas reduced to 1 inch of mercury, while the spectrum was observed from time to time. Still no trace of the nitrocarbon bands could be detected. More of the oxalate was next heated, and the observations repeated again and again, always with the same result. We came, therefore, to the conclusion that carbonic oxide, if quite free from nitrogen, does not give, at the atmospheric or any less pressure, the nitrocarbon spectrum.

From Dr. Watts' account of his experiments, it appeared that he had used carbonic oxide prepared by the action of sulphuric acid on ferrocyanide, and it was probable that it might have been contaminated with nitrogen, or with nitrogenous compounds, from the ferrocyanide. We accordingly repeated our experiments with carbonic oxide so prepared, and found that the nitrocarbon bands were then always distinctly seen.

We have also repeated Ångström and Thalèn's experiments with the spark between carbon poles in nitrogen and carbonic acid gas. For this we used a wide glass tube (fig. 7), to one end of which a quartz lens, *a*, was cemented, while the other was stopped with a cork, through which were passed two stout platinum wires with little cones of carbon (*b*), purified in Bunsen's manner, fastened, as shown in the figure, on to their ends. Two narrow glass tubes were also passed through the cork, one reaching the whole length of the bigger tube for the introduction of gas, and the other a short one for exit. The tube was first filled with nitrogen. On passing the spark we observed, as Ångström and Thalèn say, that about the poles the nitrocarbon bands were plainly seen; and they were visible through a great range of variations of the character of the spark; even the use of a condenser of moderate size (a litre flask full of sulphuric acid and coated to the neck with tinfoil) did not diminish them. Photographs were then taken, with and without the use of the condenser, and these showed the violet and ultra-violet nitrocarbon bands including those near N and P. The nitrogen was now swept out by a current of carbonic acid gas, and on now passing the spark the nitrocarbon bands could no longer be detected, and photographs taken as before showed no trace of any of them.

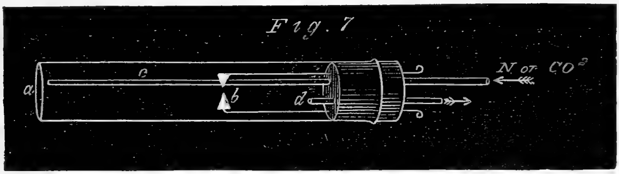
Fig. 7

In order further to test the sensibility of the spectroscopic test for nitrocarbon compounds, and the permanence of such compounds under variations in the electric discharge, we drew out, so as to form long narrow necks, both ends of a glass tube of 110 cub. centims. capacity which had wires sealed into it in the same way as before, and into one of these necks we introduced a minute quantity, ·002 grm., of mercury cyanide; a current of hydrogen was then passed through the tube at atmospheric pressure, and when it was judged that the air must be thoroughly expelled, the tube was closed by the blowpipe. On passing the spark no trace of nitrocarbon bands could be seen. The mercury cyanide was then heated to decomposition by a spirit lamp. The nitrocarbon bands immediately made their appearance, and were well, even brightly seen, and they did not disappear when a strong spark from a very powerful induction coil was passed, or even with the use of a condenser consisting of two such flasks as are above mentioned. There was no sign of any permanent decomposition of the cyanide; the bands continued to show well after the passage of the spark had been maintained for a long time. The whole quantity of cyanogen which the amount of cyanide introduced would give if it were wholly decomposed, which is not the case at the temperature employed, is less than $\frac{1}{600}$ of the volume of the whole tube, *i.e.*, of the hydrogen with which it was mixed.

It appears from this experiment not only that the test is an extremely sensitive one for nitrocarbon compounds, but that cyanogen is far more permanent under the electric discharge when mixed with a large bulk of hydrogen than when the discharge is passed through cyanogen alone.

In all the foregoing experiments the bands which Ångström and Thalèn ascribe to hydrocarbons were always more or less plainly seen; and we have come to the conclusion that much more care than has generally been thought necessary is needed if the last traces of hydrogen and its compounds are to be removed from spectral tubes. Indeed, we do not think that all water can be removed from apparatus and reagents which do not admit of being heated to redness. To illustrate this, we may mention one or two experiments.

Into the bulb of a sparking tube similar in form to that of fig. 4, but rather longer, some loose phosphoric anhydride was introduced and then some well dried and redistilled carbon bisulphide. The end of the tube having then been drawn out the carbon bisulphide was boiled out in an oil-bath at a temperature of 200° C., and the tube sealed off. On passing the spark without condenser the hydrocarbon bands were discernible, and on putting on the condenser the hydrogen line C came out plainly and was identified by comparison with an ordinary vacuum tube containing hydrogen. One would have supposed that the phosphoric anhydride would have retained all the moisture, and that there could be no other source of hydrogen in the tube.

In another experiment a mixture of carbonate of sodium and boric anhydride, previously to admixture heated red hot, was introduced into one end of a piece of combustion tube, near the other end of which wires had been sealed, and the open end drawn out; the mixture was then heated, and when it was judged that all the air was expelled, the tube was sealed off at atmospheric pressure. On passing sparks through it carbonic oxide bands and oxygen lines could be seen, but no hydrogen, hydrocarbon, or nitrocarbon bands could be detected. It appears, therefore, that the application of a red heat is likely to prove a more effectual means of getting rid of moisture than the use of any desiccating agent.

The first point we have had before us in these investigations is whether the groups of shaded bands seen in the more refrangible part of the spectrum of a cyanogen flame, of which the three which can be detected by the eye are defined by us in our previous paper on this subject by their wave-lengths (4600 to 4502, 4220 to 4158, and 3883 to 3850), are due to the vapour of carbon uncombined, or, as we conclude, to a compound of carbon with nitrogen.

Now, the evidence that carbon uncombined can take the state of vapour at the temperature of the electric arc is at present very imperfect. Carbon shows at such temperatures only incipient fusion, if so much as that, and that carbon uncombined should be vaporised at the far lower temperature of the flame of cyanogen is so incredible an hypothesis, that it ought not to be accepted if the phenomena admit of any other probable explanation. On the other hand, we have shown that cyanogen or hydrocyanic acid is generated in large quantity in the electric arc taken in nitrogen, and Berthelot has shown that hydrocyanic acid is produced by the spark discharge in a mixture of acetylene and nitrogen, so that in the cases in which these bands shine out with the greatest brilliance, namely, the arc in nitrogen and the cyanogen flame, we know that nitrocarbon compounds are present. Further, we have shown that these bands fade and disappear in proportion as nitrogen is removed from the arc. Ångström and Thalèn had previously shown the same thing with regard to the discharge between carbon electrodes; and the conclusion to which they and we have come would probably have commanded universal assent if it had not been for the fact that these bands had been seen in circumstances where nitrogen was supposed to be absent; but where, in reality, the difficulty of completely eliminating nitrogen, and the extreme sensibility of the spectroscopic test, had been inadequately apprehended.

To clear up the question from this point of view, the experiments now detailed have been made, and they appear to us quite conclusive. Were the evidence less conclusive than it is, it would still be as rash and as illogical to conclude from the appearance of the nitrocarbon bands in a case where nitrogen was presumed, not proved, to be absent, that they were not due to a nitrocarbon, as it would be to deny that the well-known yellow lines were due to sodium, because they had been seen in cases where sodium was supposed to be absent. Our argument is an induction from a very long series of observations which lead up to one conclusion, and hardly admit of any other explanation. But Mr. Lockyer attempts to explain the disappearance of the bands when nitrogen is absent, by the statement, "that the tension of the current used now brings one set of flutings into prominence, and now another." This is no new observation. It is well known that variations in the discharge produce variations in the relative intensities of different parts of a spectrum. Certain lines of magnesium, cadmium, zinc, and other metals, very brilliant in the spark, are not seen, or are barely seen, at all in the arc. His remark might be applied to the spectra of compounds as well as to those of elements. Variation in the discharge accounts very well for some of the variations of intensity in the bands if they be due to a nitrocarbon; it will not, however, account for the fact observed by us, that the bands, or those of them which have the greatest emissive power, and are best developed by the particular current used, come out on the addition of a minute quantity of nitrogen, when there is every reason to think that no variation of the current occurs. *

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* It is worthy of note, that if we number the sets of bands of the spectrum of hydrocarbons, beginning with the orange set, and those of cyanogen, beginning with

Much the same may be said with regard to the changes of the spectrum produced by changes of temperature. We cannot infer from any of these changes that the spectrum is not due to a compound. The bands in question are singularly persistent through a great range of temperatures, from the temperature of a cyanogen flame cooled by admixture with carbonic acid gas, as related by Watts ("Phil. Mag.," 1869, p. 258), to that of the spark of an induction coil with condenser.

But again, Mr. Lockyer attempts to get over the difficulties of his case by the supposition that "the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum."

Now, until independent evidence that carbon can exist at all in the state of vapour uncombined at the temperature of a cyanogen flame can be adduced, and further independent evidence of the existence of different groupings in such vapour, the hypothesis here enunciated is a gratuitous one, so long as any other hypothesis for which independent evidence can be adduced, as is true of the existence of nitrocarbon compounds in the flame, arc, and spark, will sufficiently explain the facts.

Whether or no the nitrocarbon bands are visible in the solar spectrum is not in dispute, for we have never hitherto expressed any opinion on that point. The observation above recorded, that there is in the spectrum of cyanogen a strong shaded band coincident with the very characteristic dark shaded band P, strengthens materially the evidence in favour of the existence of these bands in the solar spectrum; the more so, as the series of lines at P has far more of the distinctive character of the cyanogen spectrum than any other series in the ultra-violet part of the solar spectrum.

However that may be, we contend against the hypothesis that if present they can be due to any vapour of carbon uncombined in the upper cooler region of the chromosphere. One object of our investigations has been to determine the permanence of compounds of non-metallic elements, and the sensitiveness of the spectroscopic test in regard to them. It appeared probable that if such compounds existed in the solar atmosphere their presence would be most distinctly revealed in the more refrangible part of the spectrum, and this is a subject with which we purpose to deal in a further communication. In the meantime it is sufficiently clear that the presence of nitrogen in the solar atmosphere may be recognised through cyanogen when free nitrogen might escape detection.

In a footnote of his paper Mr. Lockyer assigns the wave-length 3881

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the least refrangible of those shown in our diagram, fig. 1, then the most persistent in the hydrocarbon flame-spectrum are 2 and 5, while in the visible part of the spectrum of cyanogen they are 3 and 4, but there are conditions of the nitrocarbon spectrum in which the three lines near G seem to be most persistent.

as approximately that of the least refrangible edge of the ultra-violet set of bands next above K, whereas in our former paper we had given it as 3883·5. Now, as we have before explained, we have never set ourselves to the exact determination of wave-lengths, we have rather used them as convenient handles by which to designate particular lines; but a repetition of the measures of our photographs does not confirm Mr. Lockyer's number. The photographs were those of the arc from a De Meritens machine in one of our lime crucibles, and gave the iron lines well developed; and we used the iron lines, wave-lengths 3906, 3902, 3898·5, 3894·8, and the pair between 3877 and 3878 as lines of reference. These measures give, as a minimum, the wave-length 3882·7. In Cornu's map a manganese line is given with the wave-length 3881. None of our photographs show this line, though manganese was purposely introduced into the crucible, and the other manganese lines came out well and strong. We should not put a determination made in this way against a direct determination of the wave-length by a diffraction grating; but unless there be an error in the identification of the iron lines, or Cornu's wave-lengths for those lines be erroneous, neither of which suppositions is at all probable, we conclude that the wave-length is more nearly 3883 than 3881. If this be correct, the band covers a markedly luminous portion of the solar spectrum as shown in Cornu's map.

Mr. Lockyer has forced us to go again over some of the ground which we had previously traversed, by making such a statement as the following:—

"I have therefore thought it desirable to go over the ground again, modifying the experimental method so as to demonstrate the absence of impurities. Indeed, I have started upon a somewhat lengthy research, and have taken a large number of photographs under various conditions, so that to reduce them all will be a work of time. I beg permission, therefore, in the meantime, to submit to the notice of the Society an experiment with a tube containing CCl_4, which, I think, establishes the conclusions arrived at by prior investigators. And I may add that it is the more important to settle the question, as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views of a kind which appear to me calculated to mislead, and which I consider to have long been shown to be erroneous."

As we have now demonstrated the utterly unsatisfactory character of the crucial experiment on the strength of which Mr. Lockyer condemns in so sweeping a manner our conclusions, it is unnecessary to add any further comment.*

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* Mr. Lockyer has communicated to the Society a "Further Note on the Spectrum of Carbon," since the above was written. We see no reason to modify our conclusions regarding the carbon spectrum on account of any new facts brought forward. The discussion of the paper we leave until a future occasion.

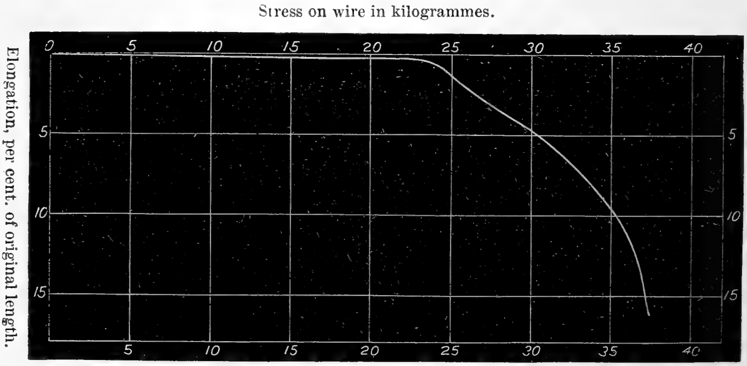
VII. "On certain Effects of Stress on Soft Iron Wires." By J. A. Ewing, B.Sc., F.R.S.E., Professor of Mechanical Engineering in the University of Tokio, Japan. Communicated by Fleeming Jenkin, F.R.S., Professor of Civil Engineering in the University of Edinburgh. Received May 24, 1880.

The following is a preliminary notice of some of the results of experiments which are still in progress. As I hope to publish later a detailed account of the whole investigation, it is unnecessary to describe here, in more than brief outline, the apparatus which has been employed.

The wires experimented on were of soft Japanese iron, drawn in Japan, and annealed shortly before being tested. Each wire was stressed separately by being hung vertically from a strong frame, a tank capable of holding 100 kilos. of water being attached to the lower end of the wire. The weight of the empty tank was balanced, so that the stress on the wire was due only to the weight of water in the tank. The tank was circular and of uniform diameter from top to bottom, and the stress on the wire was therefore proportional to the height of water in the tank. This height was recorded on a sheet of paper drawn along by a float, which rose as the water was run in. A pencil travelled transversely across the paper through distances proportional to the elongation of the wire. In this way a continuous diagram was automatically drawn, showing the relation of the strain or elongation to the stress from zero stress up to the breaking point.

Fig. 1.

Stress increased uniformly up to breaking point, at the rate of 1 kilogramme in 15 seconds.



Water was run into the hanging tank from a cistern at a considerable height above it. This gave a convenient means of applying stress slowly or quickly at will, and at a sensibly uniform rate. When not otherwise specified, the rate of flow of water into the tank is to be understood to have been about 1 kilo. in 15 seconds.

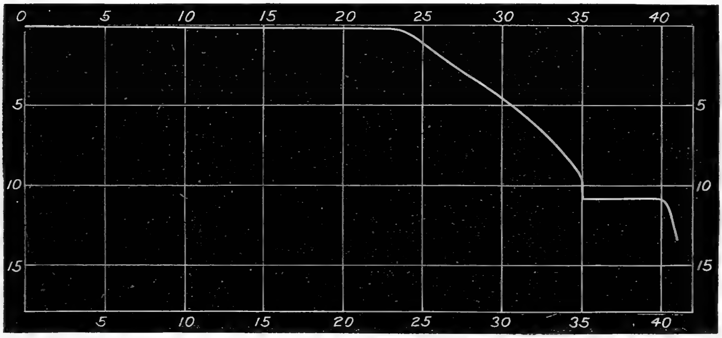
The iron wires were 1·1 millim. in diameter, and the length under test was 250 millims.

Fig. 1 shows one of the diagrams of a continuous test, in which the flow of water was continued uniformly at the above rate until the wire broke. The horizontal ordinates give the stress in kilogrammes, the vertical ordinates give the elongation in percentages of the original length. The line is sensibly straight until the stress is about 21 kilos.; after that it curves rapidly but rather irregularly. The breaking stress in this example was 37·5 kilos., and the final elongation 16·3 per cent.

Fig. 2 shows the effect of maintaining a constant stress on the wire for a considerable time. When the stress reached 35 kilos. the flow of water was stopped, and this load was allowed to remain on the wire for an interval of 45½ hours, after which the flow of water was resumed at the same rate as before, and continued until the wire broke. The effect was most remarkable. Instead of continuing to lengthen at once when the flow of water was resumed, the wire refused to stretch further until the stress rose to about 40 kilos. There was, in fact, a new "limit of elasticity" at this point. When the stress exceeded 40 kilos. the wire ran down rapidly, and finally broke with a load of 41 kilos. and a total elongation of 13½ per cent. The breaking strength was therefore greater, and the elongation less, than when the wire was tested continuously.

FIG. 2.

Showing effect of maintaining a stress of 35 kilogrammes constant for 45½ hours.



This effect, which I shall call the "hardening effect," depends on the length of time during which the test is interrupted. Fig. 3 shows the effect when the same stress (35 kilos.) was maintained constant for 5 minutes only. I have also obtained diagrams showing the effect of keeping a load of 35 kilos. on wires for 2 minutes, 10 minutes, 30 minutes, 1 hour, 3 hours, and 141 hours. In all these cases the hardening effect is greater for greater lengths of time, but the increase of the effect is much more rapid at first than it is later. A curve showing the relation of the hardening effect (measured by the length of the straight line in the diagram after the stress has been kept constant, and before the wire begins to stretch again) to the time rises rapidly at first, but afterwards becomes nearly parallel to the line along which time is measured.

FIG. 3.

Showing effect of maintaining a stress of 35 kilogrammes constant for 5 minutes.

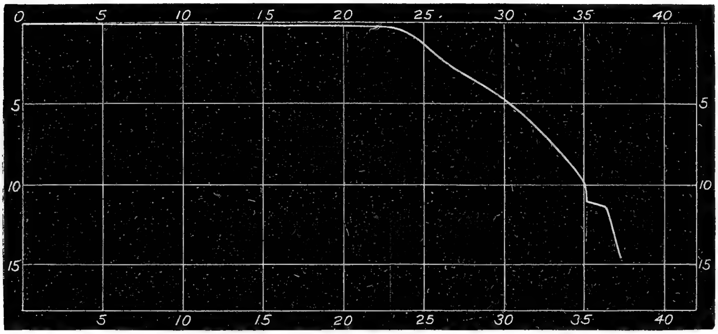
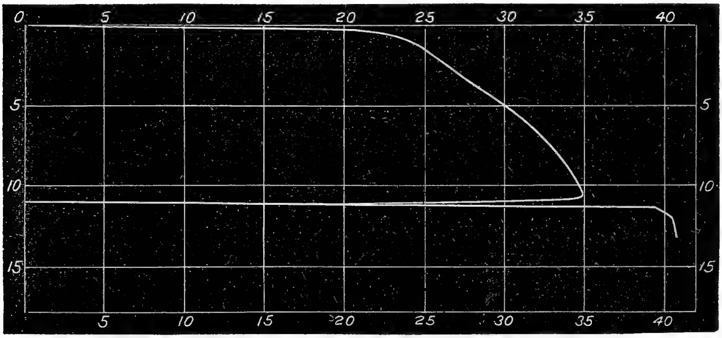


FIG. 4.

Showing effect of removing the load when it reached 35 kilogrammes and replacing it after 19¼ hours, during which the wire was free from stress.



An interval of constant stress of even five seconds produces a perceptible "knee" in the curve similar to, but of course much smaller than, that shown in fig. 3.

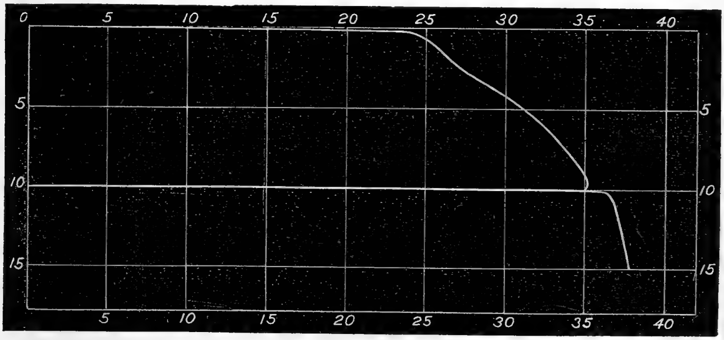
Experiments have also been made to determine the effect produced by completely relieving the wire of stress for a time, and then continuing the test. Fig. 4 is an example of the effect observed. In this example, when the stress reached 35 kilos., the flow of water into the tank was stopped, and the tank was emptied by opening a stop-cock at the bottom. This brought the line in the diagram back to the line of no stress. The wire was then left without stress for 19¼ hours, after which water was run into the tank again, at the usual rate, until the wire broke. The result, as may be seen from the diagram, was that no considerable lengthening took place until a stress far exceeding 35 kilos. was reached. It was not until the stress reached nearly 40 kilos. that the wire began to stretch again, and then it stretched rapidly, and broke with the comparatively small final elongation of less than 13 per cent.

Here the interval of *no* stress had an hardening effect exceedingly similar to the effect produced by an interval of *constant* stress.

In the test shown in fig. 4 there was a gradually diminishing stress on the wire while the tank was being emptied, and a gradually increasing stress on it while the tank was being refilled. To show that the hardening effect observed was not due to these stresses, I made the experiment whose diagram is given in fig. 5. Here the wire was stressed up to the same point as before (35 kilos.), the water was then run out of the tank at the same rate as in fig. 4, and *immediately* run in again. The circumstances of this experiment were, therefore, identical with those of the preceding one, except that now there was no sensible interval during which the stress was *nil*. There was in this case a small hardening effect, such as might have been anticipated

FIG. 5.

Showing effect of removing the load when it reached 35 kilogrammes, and replacing it without leaving any sensible interval during which the wire was free from stress.



from the results mentioned above, but nothing like so much as in fig. 4. The difference can only be due to the long interval of no stress.

An investigation is now being made of the hardening effects of equal intervals of constant stress and no stress at different points along the curve. So far as the experiments have yet gone, it appears that outside the so-called elastic limit, that is, for stresses greater than about 22 kilos., the effect of 19 hours of constant stress is not greatly different, whether the stress is great or small. Inside the elastic limit the effect of constant stress is to prolong that limit, that is, the wire then requires a greater stress to make it begin to stretch considerably. This effect is greater the nearer to the elastic limit the interval of constant stress occurs.

By intermitting the flow of water for a succession of short intervals during one test we get a stepped curve, having a number of "knees" upon it, like those in figs. 2 and 3.

A wire to which a continuously but very slowly increased stress is applied, gives a curve in which the elongation corresponding to each stress (beyond the elastic limit) is less than when the increase of stress is rapid. In other words, when the flow of water is very slow, the curve is bent upwards to the right. The final elongation is diminished, and the breaking strength is increased. It will be observed that the last-mentioned results are in perfect agreement with the remarkable experiments described by Mr. J. T. Bottomley, in the Report of the British Association Committee for commencing secular experiments upon the elasticity of wires (1879).

When a stress is applied and maintained constant it produces two effects. It produces a gradual viscous elongation, at first rapid, afterwards slow. This I shall call for brevity the viscous effect. It also produces the hardening effect described in this paper. The hardening effect, like the viscous effect, is greatest at first. The viscous effect is quite visible in the diagrams. Thus, in figs. 2 and 3, especially in fig. 2, the wires have stretched considerably during the time that the stress has been constant; and in figs. 4 and 5 the wires have gone on stretching during part of the time that the water took to run out of the tank.

If the viscous effect existed alone, and there were no hardening effect, a wire would show a higher breaking strength and less final elongation the more quickly the increase of stress took place. For, with a quickly increased stress, the wire would not have time to take the elongation, and consequently the diminished cross-section proper to each value of the stress.

If, on the other hand, the hardening effect existed alone, a wire would show a higher breaking strength and less final elongation the more slowly the increase of stress took place.

We may, therefore, expect that whenever the viscous effect is great relatively to the hardening effect, a wire will show to greater advantage as regards its breaking strength, and to less advantage as regards its elongation, when tested quickly than when tested slowly. This is probably the case with tin, copper, and other metals in which the viscous effect is great. [I have not tested tin wires, but copper and also brass wires show a perceptible hardening effect under constant stress, much less, however, than that shown by soft iron.]

When, on the contrary, the hardening effect is great relatively to the viscous effect, a wire will show to greater advantage as regards breaking strength, and to less advantage as regards elongation the more slowly it is tested. This is the case with soft iron, at least in circumstances like those of the above experiments.

The experiments show that even such variations in the rate of applying stress, or such intermissions of stress as are liable to occur in practical testing, are sufficient to affect most materially the results of the test.

A batch of specimens cut from the same coil, and similarly treated in every way, give very closely accordant results when tested with the same rate of increment of stress. The examples quoted above are all taken from the same batch of specimens. One peculiarity is common to them and to all the other specimens in the same batch, namely, the double curvature, which appears in the diagrams soon after the limit of elasticity has been passed. On the other hand, other specimens cut from the same bundle of wire, but annealed at another time, do not show this peculiarity.

The examples given may fairly be taken as representative of a large number of tests. I am now extending the experiments; but it appeared to me that the results already arrived at were of sufficient interest to excuse their presentation in this somewhat crude form.

VIII. "Memoir on Abel's Theorem." By R. C. Rowe, Fellow of Trinity College, Cambridge. Communicated by A. Cayley, LL.D., F.R.S., Sadlerian Professor of Pure Mathematics in the University of Cambridge. Received May 27, 1880.

(Abstract.)

The object of this paper is to present in a shortened and simplified form the processes and the results of Abel's famous memoir, "Sur une propriété générale d'une classe très-étendue de fonctions transcendantes," composed and offered to the French Institute in 1826, but first published in the "Mémoires des Savans Étrangers" for 1841.

The generality and the power of this memoir are well known, but its form is not attractive. Boole, indeed, in a paper on a kindred subject (Phil. Trans. for 1857, pp. 745—803) says, "As presented in the writings of Abel . . . the doctrine of the comparison of transcendants is repulsive, from the complexity of the formulæ in which its general conclusions are embodied." Boole's theorems, however, escape this charge only with loss of the generality which makes Abel's valuable.

But this complexity is rather apparent than fundamental. It is attempted, by rearrangement of parts, by separation of essential from non-essential steps, by changes of notation, in particular by the introduction of a symbol and a theorem discussed by Boole in the paper already referred to, and by the addition of examples of the processes and results, to reduce this part of an important subject to a shape more simple, while no less general, than the original.

Of the three sections which compose the paper the first contains the discussion of the main question,

Is it always possible to establish between the values for different variables of the integral of an algebraic function however complex, algebraic relations, the variables themselves being connected by any requisite algebraic laws?

If, for example, $\int X dx = F(x)$,

where X is an algebraic function of x, rational or irrational, integral or fractional, is it necessarily possible by connecting x_1, x_2, . . . x_n by any requisite algebraic laws to obtain an algebraic (or logarithmic) expression for the sum $F(x_1)+F(x_2)+ \ldots +F(x_n)$?

This question is suggested on the one hand by such well-known results as

$$F(x_1)+F(x_2)=\text{constant, where } X=\frac{1}{\sqrt{1-x^2}}, \text{ if } x_1^2+x_2^2=1,$$

and $$F(x_1)+F(x_2)+F(x_3)=0 \text{ where } X=\frac{1}{\sqrt{1-x^2 \,.\, 1-k^2x^2}}$$

if $$(1-x_1^2)(1-x_2^2)(1-x_3^2)=(2-x_1^2-x_2^2-x_3^2+k^2x_1^2x_2^2x_3^2)^2;$$

and on the other hand by the possibility of finding algebraical expressions for many symmetric functions of the roots of equations, though these roots may not be separately determinable.

This combination of the theory of integrals and the theory of equations furnishes in fact the key to the problem, as enabling us to express the "requisite algebraic laws" very concisely by a *single* equation of which the variables are roots, and whose coefficients are not independent but connected by a corresponding number of relations.

The notation is this:—

We first, to escape the inconvenience of irrational and fractional forms, introduce two functions, f, χ, where X, the subject of integration $\equiv f(x, y)$, y being determined as a function of x by the equation

$$\chi(x, y) \equiv y^n + p_1 y^{n-1} + \,.\,.\, + p_{n-1} y + p_n = 0,$$

where $p_1, p_2 \,.\,.\,.\, p_n$ are rational integral functions of x, and χ therefore a rational integral function of x, y; while f is rational, but not necessarily integral. This assumption will cover all cases.

[For example, in the case $\mathrm{X} = \frac{1}{\sqrt{1-x^2}}$,

we have the equations $\qquad f(x, y) \equiv \frac{1}{y}$,

$$\chi(x, y) \equiv y^2 - (1-x^2) = 0].$$

We next change $f(x, y)$ so that its denominator shall be the product of $\chi'(y)$, the differential coefficient of χ with respect to y, and a function of x only, and write

$$f(x, \quad \equiv \frac{f_1(x, y)}{f_2(x) \,.\, \chi'(y)}.$$

This is shown to be always possible.

We then introduce the equation whose roots are the variables of the functions we add. This equation is obviously not arbitrary. It is necessary and sufficient to take the result of eliminating y between χ and any other function of x, y which by the use of χ can be made of (at most) the $n-1$th degree in y.

Let this function be

$$\theta(y) \equiv q_{n-1} y^{n-1} + \,.\,.\,.\, + q_1 y + q_0,$$

and let the result of elimination be called E.

Here $q_{n-1}, q_{n-2} \,.\,.\,.\, q_1, q_0$ are rational integral functions of x, and E is a function of x and of their coefficients.

It may happen that, owing to relations between these coefficients, the equation $\mathrm{E} = 0$ is satisfied by values of x independent of them. Let $\mathrm{F}_0(x)$ be the product of the corresponding factors, and write

$$\mathrm{E}(x) = \mathrm{F}_0(x) \mathrm{F}(x).$$

It only remains to define a symbol Θ, used by Boole in his paper on the comparison of transcendants. The following is his definition:—

"If $\phi(x) f(x)$ be any function of x composed of two factors $\phi(x)$, $f(x)$, whereof $\phi(x)$ is rational, let

$$\Theta[\phi(x)] f(x)$$

denote the result obtained by successively developing the function in ascending powers of each simple factor $x-a$ in the denominator of $\phi(x)$, taking in each development the coefficient of $\frac{1}{x-a}$, adding together the coefficients thus obtained, and subtracting from the result the coefficient of $\frac{1}{x}$ in the development of the same function $\phi(x)\,f(x)$ in descending powers of x."*

Then the theorem of this section is

$$\Sigma\int f(x, y)dx = \Theta\left[\frac{1}{f_2(x)\mathrm{F}_0(x)}\right]\mathrm{F}_0(x)\Sigma\frac{f_1(x, y)}{\chi'(y)}\log\theta(y) + \mathrm{C}.$$

This formula is more general than either of Boole's, while it seems more concise and intelligible than that of Abel to which it corresponds.

The rest of this section is occupied with the application of the theorem to several simple cases. Those have been chosen of which the results were well known (*e.g.*, the circular and elliptic functions), with a view to the comparison of method.

In the second section it is shown to follow from the results of Section I that the sum of any number of integrals of the form considered may be expressed in terms of a number of such integrals equal to the number of conditions necessarily connecting the variables in the proposition already discussed; and the question is investigated.

What is the least value of which this number of conditions is susceptible?

It is proved independent of everything but the form of the equation $\chi(x, y)=0$ by which y is determined as a function of x; the answer being that *if this equation has* $n_1\mu_1$ *roots of the form* $y=\mathrm{C}x^{\frac{m_1}{\mu_1}}$, $n_2\mu_2$ *of the form* $\mathrm{C}x^{\frac{m_2}{\mu_2}}$, *and so on, then this least number is*

$$\sum_{s>r} n_r m_r n_s \mu_s + \tfrac{1}{2}\Sigma n^2 m\mu - \tfrac{1}{2}\Sigma nm - \tfrac{1}{2}\Sigma n - \tfrac{1}{2}n + 1.$$

The expressions for the roots are according to descending powers of x; each $\frac{m}{\mu}$ is a fraction in its lowest terms with denominator positive; the series $\frac{m_1}{\mu_1}$, $\frac{m_2}{\mu_2}$, etc., are in descending order of magnitude; and this equality of powers in sets is shown to subsist.

This section concludes with an example of its processes.

The third section contains three distinct parts: first, a generalization of the theorem of Section I, showing that a similar expression to that

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* Cauchy had used in his "Calcul des Résidus" a symbol which only differs from this Θ in the absence of the subtractive term last mentioned.

obtained there may be found for the sum of any number of such functions each multiplied by any rational number, positive or negative, integral or fractional; secondly, an investigation of the conditions that the algebraic expression obtained for the sum of the integrals considered in Section I may reduce to a constant; and lastly, a discussion, as an example of all the results of the paper, of the case

$$X=\frac{\phi}{\psi^{\frac{m}{n}}}$$

where $\phi=\frac{\phi_1(x)}{\phi_2(x)}$, ϕ_1 and ϕ_2 being rational integral functions, while y is a root of the equation

$$\chi\equiv y^n-\psi=0,$$

and $$\theta\equiv q_{n-1}y^{n-1}+q_{n-2}y^{n-2}+\ \ldots\ +q_1y+q_0.$$

A considerable simplification is introduced into the second part of this section by placing it after (instead of, with Abel, before) Section II.

In the Appendix will be found notes on three points in the paper; and a list—it is hoped complete—of the errata in Abel's memoir.

There follows an addition from Professor Cayley, wherein it is shown that the foregoing expression for the least value of the number of conditions is equal to the deficiency (*Geschlecht*) of the curve represented by the equation $\chi(x, y)=0$. That this least value is equal to the deficiency is a leading result in Riemann's theory of the Abelian integrals; the before-mentioned assumptions as to the form of the roots of the equation $\chi(x, y)=0$ considered as an equation for the determination of y are equivalent to the assumption that the curve $\chi(x, y)=0$ has certain singularities; and it is in the addition shown that the resulting value of the deficiency, as calculated by the formulæ in Professor Cayley's paper "On the Higher Singularities of a Plane Curve," Quart. Math. Journ., vol. vii (1866), pp. 212–222, has in fact the foregoing value.

June 17, 1880.

THE PRESIDENT in the Chair.

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

Prof. Charles Niven and Dr. William A. Tilden were admitted into the Society.

The Right Hon. Alexander James Beresford Hope, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Notes of Observations on Musical Beats." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received May 28, 1880.

During the last three years I have been greatly occupied with observing and counting musical beats, for the purpose of discovering the cause and amount of error in Appunn's reed tonometer, and of measuring the number of vibrations made in a second by tuning-forks and organs, as materials for my "History of Musical Pitch."* The following are brief notes of some of the observations then made:—

When two musical notes nearly but not accurately form a consonance, or are in unison, they beat. Under ordinary circumstances the number of beats in a second of a disturbed unison is equal to the difference of the number of double vibrations in a second made by each note. It is not so always, as will be shown later on. If x and y be the "pitch" or number of vibrations in a second, made by two musical tones, of which y is the sharper; then, if $my-nx=0$, the tones form what I have termed a *considence*, that is, the nth partial of x falls on the same rank or seat as the mth partial of y. Considences are not always consonances, because other partials of the notes may beat roughly, as when $m:n=8:9$ or $9:10$ or $15:16$, which are well known dissonances, but give appreciable considences. But if the pitch of either x or y be slightly altered, so that $my-nx=\pm b$, the two consident partials become what I have termed *dissident*, or placed on different ranks or seats, and b beats in a second are heard, being called "sharp" when positive, that is when $my>nx$, and "flat" when negative, that is when $my<nx$. This includes the unison for which

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* A paper read before the Society of Arts on March 3, 1880, and printed in their Journal for March 5, with an appendix on April 2, 1880.

$m=n$. *Hence all beats heard are beats of simple partial tones*, however compound may be the tones which contain them. This agrees thoroughly with my observations.

Tuning-forks are comparatively simple but always possess an audible second partial or octave, and sometimes higher partials still, capable of being so reinforced by resonance jars properly tuned to them, that beats can be separately obtained from them and counted. This is a matter of great importance in the construction of a tuning-fork tonometer. When the tone is very compound, as in the case of bass reeds (especially those of Appunn's tonometer, furnished with a bellows giving, when properly managed, a perfectly steady blast for an indefinite length of time), beats can be obtained and counted from the 20th to the 30th and even the 40th partial, without any reinforcement by a resonance jar.

Taking tuning-forks first, I find it advantageous to hold the beating forks over one or two resonance jars, tuned, by pouring in water, to the pitch of the partial to be observed, whether it be the prime of both or the prime of one and the second (or octave) of the other. There may be small differences, but I have not found any difference appreciable by my methods of observation in the number of beats in a second, whether the resonance jar is the same or different for the two forks, and whether it is exactly or very indifferently tuned to each fork, but a tolerably accurate tuning much improves the tone and length of the beat. In that case the resonance jar practically quenches all other partial tones, and the beats are distinctly heard as loudnesses separated by silences. If no jar is used, the other partials are heard. In the case of the octave, the low prime becomes a drone and fills up the silences. In the case of beating primes, the octaves, which are beating twice as fast, tend to confuse the ear. Sometimes the second partial of a fork is so much stronger than the prime, that when the fork is applied to a sounding-board, only the octave is heard, which is inconvenient. This is entirely avoided by the resonance jar. Beats being a case of interference, the amplitude of the beating partials should be equalised as much as possible. With two forks of very different size and power, it is easy to regulate the amplitude by holding the louder fork further from the jar. Otherwise the beats become blurred and indistinct. For powerful reeds or organ pipes, beating with forks, it is best to go to a considerable distance from the reed or pipe and hold the fork close to the ear or over a jar. I find 30 or 40 feet necessary for organs; in Durham Cathedral, where the pressure of wind was strong and my forks weak, I found 60 or 70 feet distance much better. As I was not able latterly to go to a distance from Appunn's reed tonometer, having to pump it myself, I found it impossible to count the primes of the upper reeds by the octaves of my forks, which were completely drowned.

I find beats of all kinds most easy to count (by the seconds hand of a chronometer) when about 4 in a second. They can be counted well from 2 to 5 in a second. Above 5 they are too rapid for accuracy; below 2 and certainly below 1, they are too slow, so that it is extremely difficult to tell from what part of the swell of sound the beat should be reckoned. Partly from this reason, perhaps, I have found great variety in counting successive sets of such slow beats. I never use beats of less than one in a second, if I can avoid it. When the beats are slow it is difficult to discover by ear which of the two beating tones is the sharper; and even fine ears are often deceived. It is easy to discover, however, by putting one of the forks under the arm for a minute. This heats and flattens it by 2 or 3 beats in 10 seconds. Hence if the beats with the heated fork are slower, it was sharper, because it has been brought nearer the other; if faster, it was flatter and has been brought further away. Count for 10, 20, or 40 seconds, according to the fork. Up to 20 or 30 beats in 10 seconds it is easy to count in ones, but from 30 to 50 it is best to count in twos, as one-ee, two-ee, &c., beginning with *one*, and hence throwing off one at the end. When counting for 20 seconds I always count in twos, and for 40 seconds in fours, as one-ee-ah-tee, two-ee-ah-tee, &c., because I have to divide the result by 20 or 40; and this division is avoided by the count itself. Owing to difficulties in beginning and ending the count, I find the possible error per second to be 2 divided by the number of seconds through which the count extends; and that it is best to take a mean of 5 to 10 counts for each set of beats.

Temperature plays an important part. Forks should not be touched with the unprotected hand; they otherwise easily flatten by 2 beats in 10 seconds. Interpose folds of paper. I use two folds of brown paper stitched between two pieces of wash-leather. Large forks are generally on resonance boxes and need not be touched, otherwise the same precautions should be used, as they are very sensitive, and retain the heat longer than small forks. Scheibler's forks are fitted with wooden handles. In tuning, the file heats and flattens; the result, therefore, can seldom be known for a day or two, when the forks have cooled and "settled," as they will be sure to "jump up." I find it best to leave off filing when the forks are two or three tenths of a vibration too flat. In sharpening there is, therefore, great danger of doing too much, as the fork remains apparently at the same pitch, the flattening by heat balancing the sharpening by filing. Hence all copies should be compared some days after, by means of a third fork about four vibrations flatter or sharper than each, to avoid the slow beats of approximate unisons. The filing also seems to interfere with the molecular arrangement of the forks.

The thermometer should be always consulted when beats are taken. But if the beats are between two forks, of which the pitch of one at a given temperature is known, and both forks may be assumed to be altered in the same ratio by heat, then the temperature need not be observed; but the unknown fork may be presumed to be as many vibrations sharper (or flatter) than the measured fork at the temperature at which the latter was measured, as beats in a second were observed to take place. This is because the alteration is very small, and would be quite inappreciable for the few vibrations between them. But for tonometrical purposes an allowance must be made.

The coëfficient of temperature has not been satisfactorily determined. It varies from ·00004 to ·00006 for each vibration and each degree Fahrenheit. Possibly the mean ·00005 is the best number to take, but I have used 1÷21000=·0000476. The flattening seems to be chiefly due to the effect of temperature on elasticity. A large fork of about 435·44 vibrations at 59° F., grew sharper and beat more and more brightly as the temperature descended to 10° or 15° F., being easily counted for 20 seconds and more. At 104° F. it could scarcely be counted for 20 seconds, at 112° F. scarcely for 10 seconds, and at 164° F. I could not count it at all, the sound not lasting more than 2 or 3 seconds, and the beats varying during that time. The effect also seems to vary with the metal and make of the fork and its size, and the coefficient to be greater for high than for low temperatures. For a fork of about 256 vibrations at 59° F., the coefficient from 14° to 59° F. was about ·0000305 or 1÷32,760, but from 59° F. to 175° F. about ·0000548 or 1÷18,280. These experiments, which I made by dipping the forks in freezing mixtures and hot water, and beating them with a fork at mean temperature, are unsatisfactory. In the same way M. Aristide Cavaillé-Coll, the well-known organ-builder of Paris (private letter), experimenting on two forks of about 435 vibrations, one of Scheibler's and one a large-sized diapason normal, found—

	Coeff. for 1° F. and 1 vib.
1st fork, from 59° F. to 194° F. ..	·0000567=1÷17,650
2nd „ „ 60°·8 F. to 194° F. ..	·0000589=1÷16,970

Scheibler ("Tonmesser," p. 50) himself found for a rise of 45° F.—

	Coeff. for 1° F. and 1 vib.
1st fork, about 440 vibrations ..	·00006=1÷16,670
2nd „ „ 220 „ ..	·00005=1÷20,000

Kayser, for two large forks, furnished with mirrors, &c. ("Wied. Ann.," 1879, p. 444)—

	Coeff. for 1° F. and 1 vib.
1st fork, about 72 vibrations ..	·0000494=1÷20,250
2nd „ „ 85 „ ..	·0000566=1÷18,000

Koenig's recent careful experiments ("Wied. Ann.," 1880, p. 413), not made by freezing mixtures and hot water, give two different coefficients, deduced originally from a fork of 64 vibrations and extended to one of 256.

	For 1° C. and 1 vib.		For 1° F. and 1 vib.
1st or general coefficient ...	1÷8943	....	·00006212=1÷16,097
2nd or particular ,, ...	1÷8951	....	·00006207=1÷16,112

These are practically the same, but it is the latter which he uses in his reductions. Professor McLeod by his machine ("Proceedings," 1879, vol. xxviii, p. 291, and "Phil. Trans.," 1880, p. 1) at first obtained a result nearly identical with Koenig's, but afterwards, when experimenting on one of my forks of about 440 vibrations, he found (private letter) the coefficient to be ·00004882=1÷20,490. While Professor Alfred Mayer (private letter) by repeated experiments on Koenig's and other forks, exposed to the cold of American nights, and counted by beats with forks at mean temperature, obtained the coefficient ·00004545=1÷22,000, and I adopted 1÷21,000 as a mean of those of Professor McLeod and Professor Mayer. The error must certainly be very small for all Scheibler's forks.

For organ pipes where great exactness does not seem to be possible, I find the coefficient ·00104 for 1° F. and 1 vibration quite sufficiently accurate and to give concordant results generally. In some organs where the air for blowing is cooler than the air of the room, this is too large, according to the observations of Mr. A. J. Hipkins, at St. James' Hall, the coefficient is nearer ·0005 (private letter). In taking the pitch of organ pipes by measured forks, I usually neglect the small alterations of the pitch of the fork, and allow fully for the change of pitch in the pipe. It is probable that this coefficient does not answer for the stopped and fancy pipes. I have applied it only to open, and generally metal cylindrical pipes.

To construct the fork tonometer, invented by J. Heinrich Scheibler (*b.* 1777, *d.* 1837), a silk manufacturer at Crefeld, obtain a set of about 70 good forks with parallel prongs, and of a tolerably large size; tune the lowest to about the C (or B for English high pitch) between the bass and treble staves of any organ or piano, and tune the rest roughly each about four beats in a second sharper than the preceding. Then fit them with wooden collars or handles, and allow them to rest for three months, if possible in the same temperature at which they will be counted. This was not the process adopted by Scheibler, but is much simpler. Then count the beats between each set most carefully, at a temperature which remains as uniform as possible. It may be necessary to use a high temperature; thus Scheibler's was from 15° R. to 18° R.=65·75 to 72°·5 F., which I reckon at 69° F. as a mean; and Koenig now works at 20° C.=68° F., but announces that his former 256 vibrations was only correct at 26°·2 C.=79°·16 F. ("Wied. Ann.," 1880, p. 413.) Count on one day the beats between forks 1 and 2, 3 and 4, &c., and on another between forks 2 and 3, 4 and 5, &c., so that the same fork is not used for two counts on the same day. Excite by striking with a soft ball of fine flannel wound round the end of a piece of whalebone, as a bow is not convenient unless the forks are tightly fixed. Each blow or bowing heats, and hence flattens, and this tells if the experiments on any one fork are long continued. Count each set of beats for 40 seconds if possible, and many times over, registering the temperature and the beats, and take the mean. Scheibler counted by a graduated metronome, set constantly to an astronomical clock, when the weight was at 60, to eliminate the effect of temperature, and he altered the position of the weight (finally by a micrometer screw) so that there were always four beats to each swing of the pendulum. He seems to have attained extraordinary accuracy. Having counted all, observe those forks which are near the octave of the lowest fork. Find two such, beating with the octave (that is, the second partial tone) of the lowest fork less than they beat with each other. Then the sum of all the beats from the lowest fork to the lower of the two forks, added to the beats of the octave (that is, the second partial tone) with that fork, is the pitch of the lowest fork. Hence the pitch of all the forks is known. The extra high forks are for verifying by the octaves of several low forks, and for the purpose of subsequently measuring. From such a tonometer any other can be made, and the value of each fork at another temperature calculated.

Scheibler made a 52-fork tonometer with infinite trouble, on another plan, and counted it with marvellous accuracy. This tonometer, which I have made many efforts to find, has absolutely disappeared and his family knows nothing of it. But he left behind him a 56-fork tonometer, believed to proceed from 220 to 440 vibrations, and through the kindness of Herr Amels, an old friend of the Scheibler family, who obtained it from Scheibler's grandson, I have had the use of it for a year. I had to count it as well as I could, just as if it had been a set of forks such as I have described, and I found it was not what was thought, but that only 32 sets of beats were 4 in a second, and the other 23 sets varied from 38 to 42 in 10 seconds. I found also that the extremes were probably of the same pitch as in the original 52-fork tonometer. After then counting it as well as I could, and obtaining 219·27 vibrations in place of 219·67, at 69° F., I distributed the error of 4 beats in 10 seconds, as 2 in 100 seconds, among 20 of the 23 sets which were not exactly 4 beats in 10 seconds, leaving the first 3 sets, which I had repeatedly counted and felt sure of, unaltered. Then I reduced all the values from 69° to 59° F. Finally to verify my result I measured by beats with Scheibler's forks as thus determined; first 5 large forks of various pitches, which I had had made for me in Paris, and then 4 forks of Koenig's belonging to Professor McLeod. Professor McLeod himself kindly measured all of them, also, by his machine, and Professor Mayer, of the Stevens Institute, Hoboken, New Jersey, U.S., kindly measured the first 5 forks by his electrographic method, both with the greatest care and precaution. The three sets of measurements agreed to less than 1 beat in 10 seconds, and more often less than 1 beat in 20 seconds, when reduced to the same temperature. Thus the value of the tonometrical measurement by beats only, and the possibility of counting a tonometer sufficiently, was fully established. Koenig's measurements of his own forks reduced to 59° F., and of the actual Diapason Normal at the Conservatoire, Paris, intended to be used at the same temperature, also agree with mine within less than the same limits. By these forks I have counted 75 standard forks of Messrs. Valantine and Carr, music smiths, 76, Milton Street, Sheffield, who are thus in a position to make small copies, probably not more than half a vibration wrong at most, at a cheap rate.

When forks are counted without a resonance jar, they should not be applied to a sounding board, or held one to one ear and one to the other, but should both be held about six inches from the same ear, and their strengths should be equalised by holding the weaker fork closer to the ear than the stronger.

When the forks are screwed on and off a sounding board or resonance box, there is great danger of wrenching the prongs, unless they are held below the bend, but I have constantly seen this precaution neglected. A wrench immediately affects the pitch and duration of sound of a fork, and renders it comparatively worthless. Such cases have come within my observation. The next enemy to be guarded against is rust. Forks should be kept dry, and occasionally oiled with gun-lock oil. Rust towards the tip affects the fork much less than rust at the bend. My observations and experiments show that errors from rust can scarcely exceed a flattening of 1 vibration in 250, and are generally very much less. But as the amount is uncertain, rust spoils a fork for accurate tonometrical purposes.

My observations on reed tones are confined to those in Appunn's tonometers at the South Kensington Museum, the Museum of King's College, London, and Lord Rayleigh's, where there are copies, all of which I have counted. They consist of oblong boxes containing the reeds placed side by side. The wind pumped into a large reservoir, is driven from it by a spring, and received into this box, the heavy lid of which, separated from the body by the usual bellows-folds of leather, presses on the wind and drives it on the reeds with a very constant pressure. Below each reed is a pallet which, when pulled out by a valve to its full extent, allows the reed to sound at its highest pitch to which it is tuned. If the pallet be slid in somewhat, the pitch can be flattened by as much as two vibrations without much affecting the quality of tone, and even as much as three vibrations with considerable loss in quality. This power of reducing the pitch slightly and instantly restoring it, is of great service in experiments upon considence and dissidence, which the relations of the pitch of the reeds allow of being tried in a very large number of cases. It also enables considences to be rendered perfect when the instrument, as is necessary generally the case, is slightly out of tune, as the intervals can be made closer by flattening the upper, and wider by flattening the lower reed.

The nominal values of the reeds are as follows:

Bass tonometer, 57 reeds, numbered by their nominal value in double vibrations :—8, 9, 10, 11, 12, 13, 14, 15—16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31—32, 34, 36, 38, 40, 42, 44, 46, 48, 50, 52, 54, 56, 58, 60, 62—64, 68, 72, 76, 80, 84, 88, 92, 96, 100, 104, 108, 112, 116, 120, 124, 128.

Tenor tonometer, 33 reeds, numbered 0 to 32, nominal value=4 × number +128=128, 132, 136, 140, 144, 148, 152, 156, 160, 164, 168, 172, 176, 180, 184, 188, 192, 196, 200, 204, 208, 212, 216, 220, 224, 228, 232, 236, 240, 244, 248, 252, 256.

Treble tonometer, 65 reeds, numbered 0 to 64, nominal value =4 × number +256=256, 260, 264, 268, 272, 276, 280, 284, 288, 292, 296, 300, 304, 308, 312, 316, 320, 324, 328, 332, 336, 340, 344, 348, 352, 356, 360, 364, 368, 372, 376, 380, 384, 388, 392, 396, 400, 404, 408, 412, 416, 420, 424, 428, 432, 436, 440, 444, 448, 452, 456, 460, 464, 468, 472, 476, 480, 484, 488, 492, 496, 500, 504, 508, 512.

The correctness of these numbers had to be proved in the first place by counting the beats. The sum of the various sets of beats in the treble tonometer should be 256, in the tenor tonometer 128, in the bass tonometer, from 8 to 16, 8 beats; from 16 to 32, 16 beats; from 32 to 64, 32 beats; from 64 to 128, 64 beats. I counted the beats in the treble tonometer at South Kensington several times. On 27th October, 1876, each set of beats being counted for 20 seconds, I obtained 256 exactly. From 5th to 24th September, 1877, I counted each set of beats for a minute, and many times over, and, owing to alterations in the pitch of the reeds, the beats varied from 3·85 to 4·27 in a second, their sums being 256·27. On 25th and 28th September, 1877, I again counted each set for one minute, and obtained 256·28 as the sum. On the 10th and 12th September, 1877, I counted the beats on Lord Rayleigh's copy, and found the sum 256·38. My count of the King's College copy, 13th November to 20th December, 1877, at two minutes for each set, gave 254·75, and was certainly erroneous. The beats $\pm b$, of a disturbed considence, as $y : x$, which is supposed to be near $n : m$, joined with the sum of the beats d, between y and x, when b, d, m, n are known, give y and x by the equations—

$$my-nx=\pm b, \quad y-x=d,$$

and hence the value of l, the lowest note, is known, when $y-l$ is counted. Of thirty determinations of l thus made for the South Kensington instrument, the highest was 256·65, the lowest, 253·05, and the mean, 255·85. Hence the value of l could apparently be not far from 256, its nominal value. But when compared with an Ut_3 of Koenig's, the reed beat 2·4 flat. Supposing Koenig's fork to have been 256 truly, this made Appunn's $l=253{\cdot}6$, far less than any of my counts and most of my calculations. Lord Rayleigh and Mr. R. H. M. Bosanquet had already suggested to me that the confined air in the box of the tonometer, and the vibration of the whole instrument during the beats, "drew" the notes. I believe they thought that it altered the pitch of the notes, and hence the number of the beats. I began to entertain the same opinion, and devised the following experiment, the Lords of the Committee of Council on Education allowing me to remove the treble and tenor tonometers from South Kensington to the Museum of King's College, where Professor W. G. Adams, F.R.S., allowed me to compare these instruments with those in the Museum for many weeks. Suppose L, M, N, are adjacent reeds on one instrument, and L′, M′, N′ reeds of nominally the same pitch on the other. Practically, they were not quite of the same pitch, a circumstance which showed inaccuracy of construction. Then I took the beats between L and M, M and N, and thus by addition obtained the *internal* beats between L and N, that is, those which occurred within the box of the tonometer. When I took the beats between L and M′, M′ and N, and thus obtained the *external* beats between L and N, that is, beats which were formed in the uncompressed atmosphere of the museum external to the tonometers, *the number of the internal beats always exceeded that of the external.* On taking a mean of my observations, which extended to every set of beats, I found that I could reduce the internal to the external by diminishing the number of the internal beats by 76 in 10,000. I was not completely satisfied with the accuracy of my observations, or with what I considered the rather hazardous mean, but was unable to repeat the very long course of observations. In November, 1879, however, I was able to examine every reed in all the tonometers by means of Scheibler's forks, and I found that the nominal values of the reeds in the treble tonometer could be reduced to those of the forks by a mean subtraction of 76 in 10,000; in the tenor tonometer (which was altogether flatter), by a mean subtraction of 83 in 10,000; in the bass tonometer, octave 64 to 128, by a mean subtraction of 76 in 10,000; octave 32 to 64, of 64 in 10,000; octave 16 to 32, of 57 in 10,000; the octave 8 to 16 was too uncertain to deduce a mean. These results singularly well confirm the former. The nominal values had been deduced from observed internal beats of reed with reed; the external beats agreed whether they were taken between reed and reed, or reed and fork. It is certain, therefore, that the internal beats were accelerated, whether the pitch of the reeds themselves was raised or not; I think it was not. But the nominal value, depending on the accelerated beats was, of course, increased, and the experiment of the forks shows that it was increased exactly in the ratio of the internal to the external beats. At any rate, therefore, if the pitch of the tones was increased, it was not caused by the confined air acting upon a single speaking reed, but was occasioned by the joint action of two speaking reeds.

This joint action was very powerful. It shook the whole instrument violently. When reeds 256 and 260 (I cite them always by their nominal value) were sounded together, at first only a crash was audible, but after about a second the ear could distinguish the beating of the two primes, closely resembling the beating of two tuning-forks held over resonance jars, but accompanied throughout by a great crash, which made the simple beats difficult to keep well in the ear. This was totally different from the effect when a fork was substituted for one of the reeds. The simple beats remained, but the crash disappeared. Moving of the head caused considerable difference in the loudness of the simple beats, due perhaps, to placing the ear at or near a node of the sound wave, and removing it again. It was useless to attempt to count the beats till these simple beats were well recognised. As I ascended the scale, the crash became less. It was much less for reeds 272 and 276, and the contrast of these beats with those of 256 and 260 on the one hand, and 288 and 292 on the other, was very striking. For reeds 320 and 324 the crash was comparatively faint. I could not distinguish the beats of the second partials or octaves from the general crash arising from the beats of all the other partials. The beats of the primes were quite separate, slow (4 in a second) and distinct, and they seemed to give the time to the other beats. For reeds 376 and 380, the beat of the primes overpowered the crash, which became comparatively light, and after this point, the beats of the primes were always easy to find. From reeds 412 and 416, the beats of the primes were the principal phenomena, and after 492 and 496, the beats were practically simple.

In the tenor and bass tonometers, the beats were still more distressing to the ear, for even for single notes the upper partials beat clearly and slowly enough, after reed 64, to be distinctly perceived as beats; and the beats of two reeds sounded almost like a continually reiterated *feu de joie*. Below reed 16, the primes could not be heard at all, but down to 8 the beating upper partials could be heard.

The effect of the external beats on the ear was distinctly different from the effects of the internal beats. The *surge* of the other partials was not so strong, and the beats were much easier to count. At one time I placed the two tonometers fully 50 feet apart, and stood half way between them to count the beats, which were remarkably clear, the surge becoming indistinct, very like that of the distant waves on a sea-shore, and forming a running accompaniment, totally dissimilar from the bell-like beat of the partials.

The pitch of the reeds on the treble tonometer furnishes not only numerous cases of considences, but numerous cases of disturbed considences, beating four times in a second, when the reeds are in order. Thus $3 \times 256 = 2 \times 384$, and hence 256 and 384 are a perfect considence. But $3 \times 260 - 2 \times 388 = +4$, so that 260 and 388 form a disturbed Fifth, beating 4 times in a second, the upper note being too flat; while $3 \times 260 - 2 \times 392 = -4$, another disturbed Fifth, also beating 4 times in a second, the upper note being too sharp. The fact of the upper note being too flat or too sharp is shown immediately by flattening it, as previously described; the first beats are then made more rapid and the second more slow, but it is impossible to destroy them entirely, as the upper note cannot be sufficiently flattened. It is delightful, however, to take what should be a perfect considence, as all were when I first examined the instrument, and throw it out of tune by flattening either the upper or lower note, or both unequally, producing the dissident beats, and then to sharpen these notes gradually, and listen to the beats growing slower and slower till they finally entirely disappear, and then reappear as the sharpening is carried too far. The nature of considence and dissidence is thus distinctly felt, and the delimitation of a considence is determined by the possibility of hearing these dissident beats when one of the extreme notes is flattened. The beats are clear, distinct, and simple, and can be made very slow; their pitch is also exactly what has to be expected by the number of the partials. The other partials of the two notes in the meantime beat roughly, strongly, and very much faster than the dissident beats. Thus for the Fourth 4 : 3, we may take reeds 264 and 352, then the partials will be—

(1)	(2)		(3)	(4)	(5)	
264	528		792	1,056	1,320, &c.	
352		704		1,056		1,408, &c.
(1)		(2)		(3)		(4)

Even when the 1,056 is consident, the 88 beats of 704 and 792, and of 1,320 and 1,408 are easily heard, producing the well-known "roughness" of the Fourth, while the 264, 352, and 528 boom along loudly and independently. But if the upper note is flattened, the rate of the two first beats is altered, one becoming faster and the other slower, while slow beats of an entirely different character are introduced at the high pitch of about 1,056, by the tearing apart of these formerly coincident partials. I have watched the phenomenon over and over again for different considences, and cannot imagine a better demonstration of Helmholtz's theories.

The following, among numerous other considences, have all been clearly delimitated by me in the way mentioned, and most of the forms of dissidences which should beat 4 in a second have also been investigated: Fifth 3 : 2, Fourth 4 : 3, Major Third 5 : 4, Minor Third 6 : 5, Major Sixth 5 : 3, Sub-Fifth 7 : 5, Super-Fourth 10 : 7, Super-major Third 9 : 7, Sub-minor Sixth 14 : 9, Sub-minor Third 7 : 6, Super-major Sixth 12 : 7, Sub-minor or Harmonic Seventh 7 : 4, Super-major Second 8 : 7, Major Tone 9 : 8, Minor Tone 10 : 9, Small Major Seventh 9 : 5, and Diatomic Semitone 16 : 15. The latter was most difficult, on account of the great roughness of the intervening beats, and succeeded best in the highest case, reeds 480 and 512. The Major Sevenths 16 : 9 and 15 : 8, however, baffled me, from the excessive roughness of the other beats. I have frequently shown these effects to others, and as the instruments are accessible at the South Kensington Museum, they can easily be repeated.*

In counting the beats of the reeds with forks, I was unable to use the octaves of the forks, as they were entirely drowned by the primes of the reeds, and hence above reed 440 I was obliged to use other forks which had been previously counted with the octaves of Scheibler's forks; but below reed 220 I always counted by the partials of the reeds. By this means I was able to determine the pitch as far as 12 vib. in a sec., with tolerable certainty. Occasionally I determined the pitch of a single reed by means of several partials, beating, of course, with the primes of different forks. The following table gives the nominal numbers of some of these low reeds, with the partials used, the mean pitch determined, and the decimals of a vibration determined from the different partials, showing the close agreement of the several determinations.

Nominal number of vibrations.	Mean number of vibrations in the prime by Scheibler's forks.	Partials used, of which the pitch was determined.	Decimals of the numbers of vibrations of the prime as calculated from the pitch of the partials.
88	87·34	3, 4, 5	·34, ·33, ·36
72	71·46	4, 5, 6	·45, ·45, ·47
64	63·52	4, 5, 6	·52, ·52, ·53
48	47·68	5, 6, 7, 8, 9	·67, ·68, ·67, ·69, ·71
40	39·92	6, 7, 8, 9, 10, 11	·91, ·93, ·90, ·92, ·93, ·93
36	35·74	7, 8, 9, 10, 11, 12	·73, ·73, ·73, ·735, ·745, ·74
32	31·47	7, 8, 8, 9, 10, 11, 12, 13	·47, ·48, ·46, ·47, ·49, ·45, ·47, ·45
23	22·88	13, 19	·88, ·88
16	15·94	25, 27, 25	·94, ·94, ·92
13	12·90	20, 25	·89, ·91
12	11·90	20, 28	·88, ·91

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* Since this paper was sent in to the Royal Society, I have handed to the Secretary of the Science and Art Department for the use of the South Kensington Museum, a detailed account of the method of making these experiments, with tables showing how to bring the proper reeds into action.—June 15, 1880.

After reed 12 the results were very uncertain. After reed 32 the primes were scarcely audible, and after reed 15 they were utterly inaudible. All that could be distinguished was the thumping of the beats of the upper partials, and these became gradually fainter and fainter, but were always audible even for reed 8.

The mode of finding the proper forks for any partials of reeds was simple, since the approximate pitch of the reed and the actual pitch of the fork were known. Thus for reed 12, the 20th partial would be nearly 20 × 12=240, and hence would lie between the forks 239·66 and 235·69. On trial I found the beats to be respectively 2·00 and 1·96 (the last a mean of several counts). Then 239·66−20× reed 12=2·00, and 20× reed 12−235·69=1·96. These give 20× reed 12=237·66 and 237·65 respectively, and consequently reed 12=11·88 vibrations. The ease and certainty with which the partials could thus be picked out was delightful to observe. As no resonance boxes or jars were used for the reeds, the objection sometimes made, that such partials are created by multiple resonances within the resonance cavity itself, falls to the ground, and the practical objective existence of the partials is established. The practical coincidence of the values of the prime from several distinct partials shows that there was no error in assigning the pitch to the proper partial. In the case of the four last reeds, 11, 10, 9, 8 only, where successive partials are so very close, did I feel any uncertainty, and hence I have not cited these results. It was for these cases extremely difficult to hear any beats at all, as distinct from the beats of the partials of the single notes themselves, as the partials that had to be used were very high and very weak. From and after reed 32 there were no musical sounds at all; indeed, even reed 64 scarcely deserved the name of a musical sound, so strong were the beats of the upper partials.

As the coefficient of temperature for reeds is unknown, a suspicion of error to a small amount attaches to all these determinations of pitch, which were made at artificial temperatures varying from 45° to 55° F. This want of correction for temperature, and liability to lose pitch from unknown circumstances, militate against the use of the reed tonometer for scientific purposes, but on account of its mumerous partials it is admirably adapted for many purposes which the stabler tuning-fork, with its small number of available partials, cannot subserve. How stable tuning-forks are, it is difficult to say. The lowest and highest forks of Scheibler's tonometer do not seem to have varied, by so much as the twentieth of a vibration since 1837, judging by my own measurements and by Professor McLeod's measurements of a fork in absolute union with the highest. A good fork, marked 438 simple vibrations (that is, 219 double vibrations) in Scheibler's own handwriting, probably about fifty years ago, is now considerably rusty, but I measure it as 218·77 double vibrations, hence it cannot have altered more than 0·23 vibration, for which the rust fully accounts. At the same time five other forks, of a large size and very different make, after having journeyed to America and back to be measured by Professor Mayer, have, according to Professor McLeod's measures, lost ·0015, ·165, ·0205, ·0285, and ·014 vibration respectively. The second fork was not so good as the rest, and may possibly have been slightly wrenched, as it had to be screwed in and out of a wooden holder. The other losses scarcely exceed errors of observation and differences of estimation of the effects of temperature.

The points to which I wish to draw attention are, the establishment of the acceleration of beats which take place in confined spaces, and the corroboration of Helmholtz's theory of the objective existence of partial tones, by means of beats of these partial tones, either with one another or with those of other compound tones.

II. "On the Lowering of the Freezing-Point of Water by Pressure." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge. Received June 10, 1880.

The Cailletet pump may be conveniently employed to observe the thermal effects of compression on solid and fluid substances. Before engaging in an investigation on this subject, it was necessary to test the apparatus, and especially the manometer. For this purpose it seemed, on theoretical grounds, that observations on the lowering of the freezing-point of water by pressure would be a severe test of the accuracy of the pressure gauge, and the constancy of the records of the thermo-junctions under pressure. I am not aware of any quantitative experiments on this subject having been made under high pressures. Sir William Thomson carried the proof of the accuracy of Professor James Thomson's great theoretical discovery to a pressure of 17 atmospheres.* The experiments of Mousson ("Pogg. Annalen," 1858) were not of a quantitative character, being merely intended to show that ice at a temperature of −18° C. might still be liquefied by the application of an enormous pressure. The following experiments appear to show that a convenient manometer for very high pressures, based on the observation of the freezing-point, may be easily constructed.

In all the following experiments the galvanometer, moving to the negative side, represents a cooling effect on the junction inside the

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* "The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated." "Phil. Mag.," 1850.

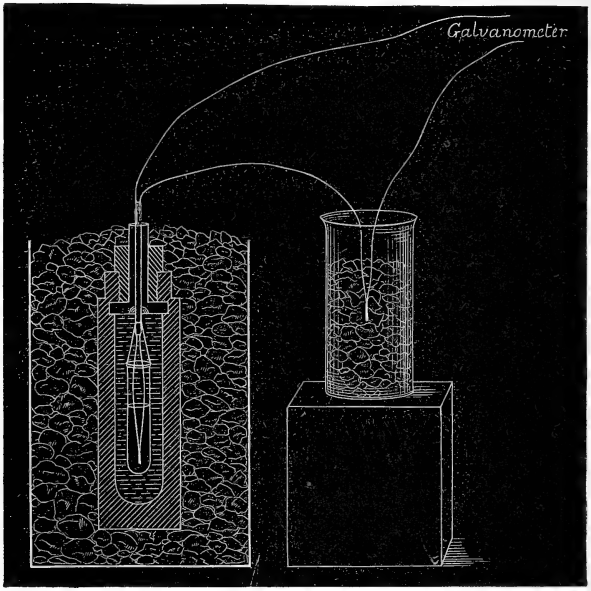
bottle. One division on the arbitrary scale represented about $\frac{1}{112}$ of a degree C. Two thermo-junctions, made of iron-copper wires, insulated by a covering of marine glue, the junctions themselves being covered with a thin layer of gutta-percha dissolved in benzol, were employed in the experiments.

One junction was placed in the iron bottle, to record the effect of pressure under different circumstances, the other was kept in a beaker outside at the constant temperature of melting ice.

Series I.

(Fig. 3 shows the general arrangement of bottle and thermo-junctions.)

FIG. 3.



A junction, fixed in a brass flange with marine glue, was frozen in a test-tube containing boiled water, and placed in the iron bottle of the Cailletet apparatus, surrounded with water at 0° C., the bottle itself being packed round with ice.

The pressure was increased by 25 atmospheres at a time. The galvanometer showed a deflection immediately after the pressure was applied, rapidly coming to rest, and it remained perfectly steady at the lower temperature as long as the pressure was maintained constant.

The following table gives the results of one of many series of experiments, which agreed very nearly. Pressures were recorded by a metallic manometer which had been chequed on several occasions. The records of the first 25 atmospheres' pressure are not reliable.

Galvanometer zero +1.

					Increase.
25 atmospheres	=	− 26	position on scale	....	17
50 ,,	=	− 43	,, ,,	....	19
75 ,,	=	− 62	,, ,,	....	20
100 ,,	=	− 82	,, ,,	....	18
125 ,,	=	−100	,, ,,	....	20
150 ,,	=	−120	,, ,,	....	22
175 ,,	=	−142	,, ,,	....	21
200 ,,	=	−163	,, ,,	....	22
225 ,,	=	−185	,, ,,	....	18
250 ,,	=	−203	,, ,,	....	20
275 ,,	=	−223	,, ,,	....	20
300 ,,	=	−243	,, ,,	....	

This represents a mean deflection of 19·7 on the scale, being 0·18 of a degree C. for 25 atmospheres, or a lowering of the freezing-point by 2°·1 C. for the total range of pressure. The mean reduction for one atmosphere of pressure is 0°·0072.

These experiments were afterwards repeated, with a stronger pump, to 700 atmospheres. Exactly the same mean deflection was observed, and the galvanometer moved with the same regularity.

Series II.

The junction in the flange was placed in a piece of quill tubing, open at top and bottom, in order to insulate it from the sides of the iron bottle, and surrounded with water at 0° C. instead of ice, the iron bottle being afterwards packed, as before, in ice. On applying pressure a very slight decrease of temperature occurred, the whole deflection for 200 atmospheres being only about 4 divisions, or $\frac{1}{28}$ of a degree C. This agrees with Joule's* experiments on the compression of water about the same temperature.

Series III.

A mixture of finely powdered ice and water was placed round the junction inside the bottle, which was, as usual, packed afterwards in ice. It will be seen, from the following set of readings, that the deflection was about the same as when the junction was frozen into a block of ice, the only difference being that, whereas the galvanometer in the first series assumed its new position immediately after the pressure was applied, the instrument now required from two to three minutes before coming finally to rest, the action being far more sluggish.

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* "On the Thermal Effects of Compressing Fluids." "Proc. Roy. Soc.," vol. ix, p. 496.

Pressure.	Position on Scale. Zero = +6.	Difference.
25	− 17	
50	− 36	19
75	− 55	19
100	− 74	19
125	− 91	17
150	−110	19
175	−130	20
200	−149	19

Mean deflection for 25 atmospheres is 19·4 divisions of the scale.

Series IV.

Brine was placed round the junction in the bottle, the latter being afterwards packed in ice and salt, to reduce the temperature of the brine to −18° C., the other junction being placed in ice and salt in a beaker outside the iron bottle. The junctions were then connected up with the galvanometer, and allowed to remain until the temperature became equalised :—

Zero +85.

Atmospheres.	Position on Scale.	Increase.
25	+ 98	
50	+106	8
75	+115	9
100	+121	6
125	+128	7
150	+134	6
175	+138	4
200	+142	4

In this case a heating effect was produced, which seemed, however, to decrease as the pressure was increased. It represents an increase of $\frac{1}{25}$ of a degree Centigrade for the whole 200 atmospheres.

Series V.

The junction in the flange was frozen into a solid block of ice as in Series I, and placed in the bottle, surrounded with brine at −20° C., the whole apparatus packed in ice and salt. A very slight heating effect was produced, which only amounted to about $1\frac{1}{2}$ divisions for 200 atmospheres.

Series VI.

These experiments were made with the junction frozen into a test-tube as in Series I, with mercury surrounding the test-tube instead of water. This gave exactly the same deflection of the galvanometer as when water was employed, only the experiment could not be carried on for a long time, as the ice melted with considerable rapidity, from the heating of the mercury by compression.

Series VII.

As in several cases the junction that was placed inside the bottle, and which had been several times subjected to high pressures, appeared to be affected by the compression and worked somewhat irregularly, it was thought advisable to subject both the junctions to the same compression, so as to have them both under the same conditions. For this purpose both junctions were passed through the brass flange (fig. 1), being well insulated by marine glue, the one remaining outside of the test-tube in which the other was frozen.

Fig. 1.

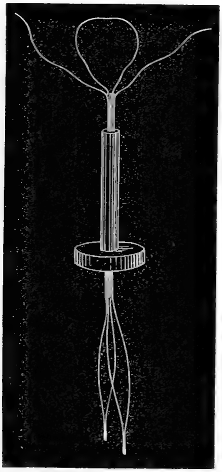
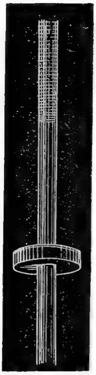


Fig. 2.



The results with ice were exactly the same as when one junction was placed outside of the bottle. Pressure still appeared to have an effect upon the junctions, as after one or two series of compression experiments they could be no longer relied upon, and worked very irregularly. The only way to obviate this difficulty was to prevent the junctions being submitted to pressure at all, and this was effected by soldering a stout iron tube (fig. 2), about $\frac{1}{4}$ inch internal diameter, into the brass flange, which reached to about the centre of the bottle. The tube was closed at the bottom and contained a few cubic centimetres of alcohol, into which the junction was lowered from the outside. The part of the tube which went into the bottle was then frozen into a mass of ice, and the iron tube sustained all the interior pressure. The alteration of temperature was conveyed slowly through the iron to the junction.

This method worked exceedingly well, giving exactly the same results as before, the only drawback being that the action was very sluggish, the galvanometer taking about two minutes to come to rest after each addition of pressure. The above results seem to prove that the calculated value of the variation of the freezing-point of water, deduced from the observed difference of volume of ice and water, and the latent heat of fluidity at the melting-point under one atmosphere of pressure, is identical with the mean experimental value obtained from a series of observations extending to 700 atmospheres. From Clapeyron's formula we are thus entitled to infer that $\frac{TV}{L}=\text{constant}$, where T is absolute temperature, V the difference of volume of the two states, and L the latent heat of fluidity. If V is assumed to be approximately constant, then T varies as L. Thus the latent heat of ice diminishes as the freezing-point is lowered by pressure. This is in accordance with the deductions of Clausius* from other considerations.

III. "On the Critical Point of Mixed Vapours." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge. Received June 10, 1880.

The following experiments regarding the behaviour of carbonic acid in presence of different vapours above the temperature of the critical point of the pure gas, were undertaken to ascertain if any optical discontinuity could be observed in such mixtures. As the object was intentionally a qualitative investigation, many of the pressure observations have been taken with the metallic manometer.

The Liquefaction of Carbonic Acid in presence of other Bodies.

1. *Carbonic Acid and Bisulphide of Carbon.*

Carbonic acid liquefied in presence of a small quantity of bisulphide of carbon at a pressure of 49 atmospheres and a temperature of 19° C. It floated on the convex surface of the bisulphide, the line of separation being sharp and well defined.

At 35° C. liquid condensed on the surface of the bisulphide in the same way at a pressure of 78 atmospheres, at 40° C. it still appeared at 85 atmospheres, at 55° C. there seemed to be a distinct appearance of two liquids, and at 58° C. there was still the same apparent separation under a pressure of 110 atmospheres. Observed at 47° C. and a

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* "Phil. Mag.," 1851.

pressure of 80 atmospheres, there was a distinct layer of a separate fluid on the surface of the bisulphide, the bisulphide surface however was not so well defined.

By keeping the temperature at 47° C., on increasing the pressure to 110 atmospheres, the upper surface of the liquid floating on the bisulphide almost entirely disappeared. By reducing the pressure again to 80 atmospheres, the surface of demarcation did not appear, but on reducing the pressure another 5 atmospheres, the line of demarcation again appeared very sharply and remained. A quick withdrawal to 58 atmospheres, followed by a gradual increase of the pressure to 85 atmospheres, did not make the liquid remain, but on reducing it again slowly to 80 atmospheres the definition became perfectly sharp.

2. *Carbonic Acid and Chloroform.*

Carbonic acid in presence of chloroform at 18° C. liquefied at 25 atmospheres, forming a distinct layer on the surface of the chloroform. On further compression, the manometer rose rapidly to 50 atmospheres, at which pressure the two liquids mixed completely, after being left for a few minutes. When the pressure was rapidly withdrawn, distinct layers of what appeared to be carbonic acid were always formed, which however became rapidly dissolved in the chloroform on standing a few seconds.

At 33° C. liquid began to appear at a pressure of 35 atmospheres, and on increasing the pressure to 55 atmospheres it behaved in exactly the same way as at the lower temperatures, except that the layer was, if anything, more distinct and mixed more rapidly with the chloroform on standing.

At 55° C. a layer of liquid was still formed at a pressure of 50 atmospheres; at 67° C. it behaved in the same way, except that there was a smaller quantity of liquid formed, and the pressure rose to 85 atmospheres.

In every case it rapidly mixed with the chloroform when left for a few seconds.

3. *Carbonic Acid and Benzol.*

At 18° C. the carbonic acid commenced to liquefy at a pressure of 25 atmospheres, and at the moment of liquefaction the surface of the benzol became violently agitated, the carbonic acid falling through the benzol in an oily stream, and becoming completely mixed with it.

When by further condensation more liquid was formed, the agitation almost entirely ceased, the liquid carbonic acid forming a distinct layer on the saturated benzol. On leaving this for about five minutes, the line of demarcation disappeared, and the two liquids formed a perfectly homogeneous fluid.

On again increasing the pressure so as to get a layer of carbonic acid, and then releasing the pressure gradually, the liquid carbonic acid on the surface first fell in oily streams through the saturated benzol; but when all this had disappeared, the carbonic acid then commenced to boil from the bottom of the benzol, and continued to do so until it was again entirely vaporised.

At 35° C. liquid commenced to appear at 35 atmospheres, forming a distinct layer on the surface of the benzol, which was not in the least agitated. On further compression the liquid layer increased in volume, but no oily streams were seen to fall through the benzol; and on leaving it for about ten minutes, the layer of liquid was almost just as distinct, showing that it was not nearly so soluble in the benzol at this temperature.

On the pressure being now reduced, the liquid quietly evaporated away from the surface; but when all this had disappeared, carbonic acid commenced to boil out of the benzol, showing that it had dissolved a considerable amount.

At 52° C. the liquid appeared at 60 atmospheres, forming a layer which mixed with the benzol on standing; and at 70° C. and 85 atmospheres a distinct layer was also formed, which however rapidly mixed with the benzol.

4. *Carbonic Acid and Ether.*

A tube was filled with carbonic acid and a little ether introduced. At 20° C., and at a pressure of 20 atmospheres, the carbonic acid liquefied and fell through the ether, mixing with it in all proportions.

At 42° C. liquid was condensed on the surface of the ether at a pressure of 55 atmospheres, forming a distinct layer; the upper surface of the ether was, however, kept in continual oscillation from the apparent solution of the carbonic acid in it. No currents due to the falling of the carbonic acid through the ether were visible. At 68° C., and a pressure of 110 atmospheres, a perfectly separate layer of fluid was formed on the surface of the ether, and no currents descended through the ether.

5. *Carbonic Acid and Nitrous Oxide.*

When a tube was filled with equal volumes of carbonic acid and nitrous oxide, and the gas liquefied, the fluids mixed together in all proportions, no difference being perceptible; but when the pressure was suddenly withdrawn, the one gas boiled before the other, and for a few seconds a distinct line of separation was seen.

Liquefaction of Carbonic Acid with Trichloride of Phosphorus.

At 16°·20 C., and 42·95 atmospheres' pressure, the carbonic acid commenced to condense on the sides before the trichloride of phosphorus came in sight, and when the latter was visible, a slight indistinct layer of fluid was seen on the surface, distinguishable by the different refractive index of the two liquids, there being no sharp line of demarcation. On standing a few minutes, the liquids became quite homogeneous. On increasing the pressure, more carbonic acid was condensed, forming a more or less distinct layer on the surface of the trichloride of phosphorus. This, however, rapidly disappeared on standing. On releasing the pressure, the carbonic acid boiled first on the surface, but afterwards through the liquid, the trichloride of phosphorus at the same time falling in heavy striæ to the bottom.

At 23° C. the carbonic acid appeared to liquefy at 46·91 atmospheres, exactly the same appearances taking place as at 16° C.

At 30° C. the carbonic acid liquefied at 49·94 atmospheres, forming a rather more distinct layer, and not mixing so readily. On increasing the pressure to 90 atmospheres, the surface of the carbonic acid disappeared, it being near its critical point, the top part of the tube being filled with a homogeneous mass. The trichloride of phosphorus could also not be distinguished on the surface of the mercury, its upper surface being entirely mixed up with the carbonic acid, the whole space above the mercury forming one homogeneous mass.

On releasing the pressure, a cloud first appeared, and then the surface of the carbonic acid became visible; it boiled away first from the surface, and afterwards through the trichloride of phosphorus.

At 33°C.,	liquid	carbonic acid	appeared at	50·84	atmospheres.
At 40	,,	,,	,,	56·88	,,
At 50	,,	,,	,,	66·53	,,

At all the temperatures above 30°C., the appearances were the same, except that as the temperature increased, the quantity of fluid liquefied diminished, and it took a greater pressure to make the surface of the trichloride of phosphorus disappear.

The following are pressures taken with a smaller quantity of carbonic acid and trichloride in the tube, and were read off when the surface of the trichloride was first agitated, thus showing that the carbonic acid had commenced to condense.

At 10°·5 C.	22·70	atmospheres.
,, 16·5 ,,	24·70	,,
,, 22·8 ,,	32·18	,,
,, 30·0 ,,	33·88	,,
,, 40·0 ,,	36·36	,,
,, 50·0 ,,	49·67	,,
,, 70·0 ,,	76·61	,,

Liquefaction of Carbonic Acid with Tetrachloride of Carbon.

The quantity of tetrachloride of carbon was a little less than the volume of the liquid carbonic acid.

At 12°·8 C. the surface of the liquid appeared agitated as soon as it appeared in sight, and on increasing the pressure, a distinct layer of carbonic acid was formed on the surface of the tetrachloride; on increasing the pressure, a still more distinct layer was formed which, however, on standing, rapidly commenced to dissolve in the tetrachloride, and in about ten minutes it was perfectly homogeneous.

At 21°·4 C. the surface of the tetrachloride appeared agitated when it came in sight, a layer of liquid being formed on increasing the pressure, as at 12° C.

At 30° C. liquid was also formed, which, however, rapidly diffused into the tetrachloride of carbon.

At 40° C. the liquid also appeared agitated, and on increasing the pressure rapidly, a small quantity of fluid was condensed, which, however, rapidly disappeared in the tetrachloride.

At 52° C. the liquid again became agitated, and on increasing the pressure, a distinct layer of liquid was formed.

The same took place at 58° C.

Liquefaction of Carbonic Acid and Chloride of Methyl.

When chloride of methyl was compressed in a tube by itself, it became liquid before the pressure could be registered, and must have been below 10 atmospheres.

When compressed with about twice its volume of carbonic acid at 13°·5 C., the chloride of methyl, of course, liquefied first, and at about 27·67 atmospheres, its surface became agitated, showing that the carbonic acid had commenced to liquefy, but the exact point was difficult to ascertain, as it dissolved so very rapidly in the chloride of methyl.

At 20°·05 C. this point appeared to be at 28·57 atmospheres.

At 30° C. some liquid was also condensed, but the pressure at commencement of liquefaction could not be taken, as it mixed so rapidly with the chloride of methyl.

That more liquid was in reality condensed, was seen by the lengthening of the liquid column, and by its boiling out of the chloride of methyl when the pressure was reduced.

At 40° C. exactly the same took place.

Carbonic Acid and Acetylene.

About equal volumes of these gases were compressed together; they liquefied and mixed completely at all the temperatures given below, and no appearance of two different gases being liquefied could be detected, except that the liquid was strongly agitated during condensation. The pressure at the point of liquefaction was, as in the former cases, lower than either of the gases liquefied by themselves. Thus :—

At	13°·5 C.,	the	pressure was	25·23 atmospheres.
„	21·0	„	„	26·8 „
„	26·8	„	„	34·1 „
„	31·9	„	„	40·26 „
„	39	„	„	55·3 „
„	41	„	„	75·32 „

The critical point was, on the other hand, heightened, being 41° C.; that of carbonic acid being 31° C., and of acetylene, 37° C.

Carbonic Acid and Hydrochloric Acid Gases.

A mixture of equal volumes of these gases was filled into a tube, the liquefied gases mixed completely together at all temperatures below the critical point, which was 36° C., forming a perfectly homogeneous fluid; in fact, it was impossible to tell that two different gases were present, as even at the point of liquefaction no difference was discernible.

The following are the pressures at which the mixture liquefied :—

At	0°·0 C..................	36·0 atmospheres.
„	5·0 „	39·0 „
„	8·0 „	43·8 „
„	10·1 „	48·2 „
„	18·5 „	59·0 „
„	34·0 „	83·0 „
„	35·5 „	90·0 „

Carbonic Acid and Bromine.

A tube filled with a mixture of carbonic acid and bromine vapour by passing dry carbonic acid through a tube containing dry bromine before entering the liquefying tube. A little strong sulphuric acid was also introduced, to protect the mercury. On compressing this mixture at 11°·5 C. the sulphuric acid appeared in sight with a layer of liquid at 50 atmospheres, the liquid having a decidedly red colour.

On increasing the pressure the liquid became more highly coloured, while a little pure bromine liquid fell through the liquid carbonic acid, remaining a short time on the surface of the sulphuric acid, through which a globule also sank.

As this tube was spoiled through the amount of bromine which fell through the sulphuric acid, another was put up in the same way, and heated at once to about 40° C., before compressing the mixture.

On the sulphuric coming in sight a small layer of bromine was seen on the surface, which was surmounted by a layer of darkish red liquid, about $\frac{1}{2}$ inch long. The pressure was about 60 atmospheres.

On increasing the pressure to 90 atmospheres, the upper liquid increased a good deal in quantity and then disappeared entirely, but immediately reappeared on reducing the pressure a few atmospheres, and remained permanently.

Carbonic Acid and Camphor.

Some small pieces of camphor were placed in the capillary part of an ordinary Cailletet tube, near the end, and fused so as to adhere to the sides. The tube was then filled with carbonic acid gas.

On compressing this mixture in the pump at 12° C. the camphor was seen to melt and run down the sides of the tube before the mercury appeared in sight (in this experiment it was not observed whether liquid carbonic acid had commenced to form in the tube and thus dissolve the camphor, or whether the latter was dissolved in the gas). On continuing the pressure so as to almost fill the tube with liquid, two distinct layers of liquid were seen, the lower one being slightly cloudy, containing the dissolved camphor, the upper liquid being perfectly clear. On compressing at different temperatures up to 55° C., the lower cloudy liquid was always present, the upper layer diminishing gradually in quantity as the temperature rose, but at 40° C. there was still a slight layer on increasing the pressure to about 125 atmospheres.

On withdrawing the pressure very suddenly when the tube was full of liquid at 50°, the sides of the tube became coated with crystallised camphor, which rapidly dissolved again on increasing the pressure. After doing this several times, a very small quantity of camphor was seen to crystallise out, and in taking down the tube, the most of it was found to have crystallised out in the top part of the reservoir near the joining with the capillary part, thus allowing the mercury to get past it on again increasing the pressure.

Carbonic Acid and Camphor (second tube).

Another tube was filled in the same way as the last (a rather larger quantity of camphor being placed in the capillary part), with this exception, that the carbonic acid, after being dried, was passed through a tube, containing fragments of solid camphor, which were gently heated, so as to fill the tube with carbonic acid gas, saturated with the vapour of camphor.

When this tube was compressed in the pump at a temperature of 15° C., and when the pressure reached 27·7 atmospheres, the camphor was seen to gradually melt and run down the sides of the tubes before the mercury appeared in sight.

On increasing the pressure to 37·0 atmospheres, the mercury appeared in sight with about three-quarters of an inch of a turbid liquid on the surface. On still further increasing the pressure, two distinct layers of fluid were formed, which, however, became quite homogeneous after a short time. On mixing them up by a rapid decrease and increase of pressure, the two fluids soon mix. At 28° C., two layers of liquid were distinctly visible at 65 atmospheres, the lower layer being visible as soon as the mercury appeared in sight.

At 35° C. fluid was condensed on the surface of the lower liquid at 80 atmospheres, but when the pressure was increased to 100 atmospheres, the surface of the carbonic acid became undefined.

At 45° C. fluid is seen to condense on the sides of the tube at a pressure of 100 atmospheres, running down and forming a slight layer on the surface of the camphor liquor, which, however, disappears on still further increasing the pressure.

When the temperature was 42°·5 and the pressure was suddenly reduced, the inside of the tube became covered with crystals of camphor. On now increasing the pressure very carefully, the camphor was seen to melt or liquefy and run down the sides of the tube at a pressure of 37·6 atmospheres, and this pressure scarcely increased at all until all the camphor was thus liquefied.

A diminution of the pressure by two or three atmospheres was sufficient to bring out the crystals of camphor.

At 60° C. the lower layer of liquid still remained, and on increasing the pressure to 100 atmospheres there was an appearance of liquefaction of carbonic acid on the surface.

Carbonic Acid, Air, and Camphor (third tube).

A quantity of camphor was placed in the capillary part of a tube as before, which was afterwards filled with a mixture of four volumes of carbonic acid, saturated with camphor vapour, and one volume of air.

The tube was surrounded with water at 25° C., so as to be far above the critical point of the carbonic acid and air mixture, and on now increasing the pressure the camphor liquefied and ran down the sides of the tube as before. At 50° C. a quantity of liquid, about $\frac{1}{2}$ inch long, appeared on the surface of the mercury when it came in sight, the pressure being now 65 atmospheres.

On leaving the pressure the same, the mercury being just in sight, and increasing the temperature gradually to 60° C., the inside of the tube above the liquid became covered with camphor crystals, which on increasing the pressure another five atmospheres (viz., to 70 atmospheres) again dissolves.

At 65° C., the pressure standing at 70 atmospheres and the mercury and liquid being in sight, on reducing the pressure to 65 atmospheres, the camphor crystals separated out, being again suddenly dissolved on again increasing the pressure to 73 atmospheres. On now rapidly letting down the temperature to 15° C. a white mass of camphor separated out from the liquid, which again dissolved on a slight increase of pressure, although it could not be again separated out by diminishing it. These actions may be due, in part, to supersaturation and the effect of pressure in aiding solubility when contraction takes place during solution.

These experiments show that carbonic acid at high pressures in presence of various substances acts as if it produced a series of unstable chemical compounds, which are decomposed and recomposed according to the conditions of temperature and pressure in the medium. Further observations are required to elucidate many obscure actions taking place under such conditions of experimenting as are detailed in the above communication.

IV. "The Aluminium-Iodine Reaction." By J. H. GLADSTONE, Ph.D., F.R.S., and ALFRED TRIBE, F.C.S., Lecturer on Chemistry in Dulwich College. Received June 10, 1880.

About four years ago we pointed out a reaction of iodide of aluminium which, as far as we are aware, has no precise analogue in the science of chemistry, and which has led to the discovery of several volatile aluminium alcohols. It is well known that neither water, alcohol, nor ether is decomposed by metallic aluminium, and that each of these bodies will dissolve iodine without entering further into combination with it; but we found that by the joint action of these two elements, it was possible to split up the above-mentioned liquids. Water, though forming a definite hydrate with the iodide of aluminium, is decomposed by it in the presence of an excess of the metal, hydrogen being evolved and aluminic hydrate formed. This takes place at the ordinary temperature.

Alcohol in a similar manner is decomposed by the joint action of metallic aluminium and its iodide, with the ultimate production of hydrogen gas, aluminic ethylate, and varying amounts of aluminic iodoethylate proportional to the quantity of aluminic iodide employed. A small quantity of the iodide suffices to bring about the formation of a very large amount of the ethylate; in fact, the process would be a continuous one, were it not that the solid products gradually put a stop to the reaction.

Ether is not affected by aluminic iodide and aluminium, but when exposed to iodine and aluminium simultaneously it suffers a violent decomposition, iodide of ethyl and the aluminic iodoethylate being the ultimate products.

Amylic ether behaves in a similar manner, and the acetates of ethyl and amyl give analogous results; but the reaction is not an ordinary double decomposition, for it does not take place when ready-formed iodide of aluminium is employed.

The several changes thus described are typical of our "aluminium-iodine reaction." The reaction is quite distinct from that which has lately given beautiful results in the hands of Friedel; and we have investigated its applicability both for the production of new compounds, and for distinguishing between different classes of organic bodies, as well as for throwing light on their comparative constitution.

Neither zinc nor iron, nor, as far as we know, any other metal can be substituted for aluminium in this reaction; but the chloride or bromide may be used instead of the iodide, though with less advantage.

Recent Results.

For decomposing an alcohol a small quantity of iodine is dissolved in it, the necessary excess of aluminium is added, and the mixture is heated. Evolution of hydrogen gas begins immediately, and proceeds somewhat rapidly until the whole of the metal has passed into combination. In this way the aluminium derivatives of the following alcohols have been prepared:—Ethylic, normal propylic, iso-butylic, amylic, benzylic, phenylic, cresylic, and thymolic. The first four of these aluminium derivatives may be distilled *in vacuo*, and they have thus been separated from the other solid products of the reaction, and obtained in a pure condition. The other aluminic alcohols cannot be distilled, at any rate not without very considerable decomposition. The following alcohols, however, behaved in a different way with the reagent:—

Methyl alcohol is not decomposed by aluminic iodide and aluminium, but in presence of free iodine it parts slowly with hydrogen, and the same happens when an aluminium-platinum couple is substituted for the metallic aluminium.

Iso-propyl alcohol.—This is not acted upon in the least by the reagents.

Cetyl alcohol.—On heating this compound with the reagent, hydrogen is slowly set free, until the temperature reaches about 200° C., when another chemical change is set up, resulting in the formation of cetyl iodide and aluminic hydrate.

Allylic alcohol.—The first action is identical in character with that which takes place with the alcohols of the $C_nH_{2n+1}OH$ series. Instead, however, of the whole of the liberated hydrogen escaping, about 30 per cent. of it acts upon the excess of alcohol, splitting it up into propylene and water.

Ethene alcohol.—The reagent does not liberate hydrogen from this substance, and has but a very slight action upon it.

Propenyl alcohol.—No hydrogen is evolved, but a double decomposition ensues at about 140° C., yielding allyl iodide, free iodine, and aluminium hydrate; if aluminium be in excess, aluminic hydrate and allylic iodide are the sole products.

Aldehyde.—Hydrogen is not set free from this compound.

The general result, then, of these observations is that the reagent substitutes aluminium for the basic hydrogen of water, and of all the alcohols hitherto tried, whether of the methyl, allyl, benzyl, or phenyl series, with the remarkable exception of isopropyl alcohol. On the other hand, it does not substitute aluminium for hydrogen in the dihydric or trihydric alcohols, nor yet with aldehyde. The reaction with the ethers and glycerine is of a different character, as, in addition to an aluminium compound, the iodides of the positive radicals are formed.

General Properties of the Aluminium Alcohols.

The aluminium alcohols are solid at the ordinary temperature, and fuse generally into clear liquids. They possess, in a marked degree, the property of remaining fluid far below their melting points. Those of the methyl series distil unchanged at reduced pressures, affording the first organic compounds which contain both oxygen and a metal and are capable of distillation. They are soluble more or less in ether, alcohol, and benzol, but are decomposed by water with the formation of aluminium hydrate and the alcohols.

They were found to have the following specific gravities at 4° C.:—

Ethylate	1·147	Phenylate	1·25
Propylate	1·026	Cresylate	1·166
Butylate	0·982	Thymolate	1·04
Amylate	0·980		

Action of heat.—All these alcohols are decomposed at a temperature somewhere about their boiling points, and it became an interesting subject of inquiry whether they were resolved into alumina, and the alcohol and its olefine, or into alumina and the ether. Both these actions seem to take place. Thus aluminic ethylate appears to be decomposed by heat in both ways, but mainly according to the following scheme:—

$$(C_2H_5O)_6Al_2 = Al_2O_3 + 3C_2H_4 + 3C_2H_6O.$$

Aluminic phenylate on the other hand is capable of decomposition mainly in the following way:—

$$(C_6H_5O)_6Al_2 = Al_2O + 3(C_6H_5)_2O.$$

It must not however be supposed that these decompositions take place without other reactions. Some of the bodies thus produced are new ones, and we are at present engaged in their investigation.

The most interesting decomposition is that of the thymolate. When heated it splits up into alumina, propylene, and bodies of the cresylic group. One of these is a solid body, which when resublimed or crystallised from alcohol presents itself in most beautiful pearly plates. Its properties resemble rather those of an ether, and concordant analyses of different specimens and determinations of vapour density give the anomalous molecular formula $C_{14}H_{13}O$, but its real constitution is still the subject of inquiry.

Theory of Reaction.

The reaction by which the compounds described above are formed is by no means clear at first sight. It is difficult to imagine that the presence of a haloid salt should determine the direct replacement of hydrogen by aluminium in water or in alcohol. We believe however that the presence of hydriodic acid in the reaction with the aromatic alcohols furnishes the true key to the chemical change. The first action is in all probability that of a reciprocal decomposition between two binary compounds—the alcohol and aluminium iodide—forming a certain proportion of aluminium alcohol and hydriodic acid. But the metallic aluminium present at the same time decomposes the hydracid with evolution of hydrogen and the production of more aluminium iodide, which brings about a further redistribution of the elements and the formation of more of the aluminium alcohol and the hydriodic acid, and so on till the chemical change is complete. The following equations express these changes, which take place alternately or rather continuously:—

$$(1) \quad 6(C_nH_{2n-7}O.H)+Al_2I_6=Al_2(C_nH_{2n-7}O)_6+6HI.$$
$$(2) \quad 6HI+Al_2=Al_2I_6+6H.$$

It is evident that as the iodine does not enter into the final products a very small quantity of it is sufficient to carry on the continuous chemical change.

There can be little doubt that the same reactions occur in the case of the alcohols of the $C_nH_{2n+1}O.H$ series, but the amount of hydriodic acid formed in the reciprocal decomposition is perhaps very small, and the tenacity with which it is held by the alcohol has made it impossible for us to prove its separate existence. The same remark applies also to water, which we believe to act in the same way. Indeed we know from the experiments of Roscoe that there would be no chance of separating minute quantities of acid from an aqueous solution under the circumstances.

The part which the free elements take in the decomposition of the ethers is doubtless analogous to that which the combined elements play in the action with water and alcohol. The reason why the free elements do, and aluminic iodide does not attack the ethers, is probably owing to the available energy being greater in the former than in the latter case.

In conclusion, we would commend this aluminium-iodine reaction to the consideration of other chemists, who may be investigating organic compounds containing oxygen.

V. Preliminary Note on the Ossification of the Terminal Phalanges of the Digits." By E. A. SCHÄFER, F.R.S., and F. A. DIXEY, B.A. Received June 3, 1880.

The diaphyses of the ungual phalanges of the digits offer an exception to the usual mode of ossification of diaphysial bones (including the other phalanges) in the fact that the calcification of the cartilage and its attendant changes begins at the tip and not in the centre of the diaphysis. The subperiosteal intramembranous ossification also commences at the same point—the tip, namely, of the cartilage—as a cap-like expansion over the end of the cartilage. The irruption of the osteoblastic subperiosteal tissue also first occurs here, so that this part seems to correspond morphologically with the centre of the shaft of other long bones. The expanded portion of the phalanx which bears the nail, claw, or hoof, is entirely formed by an outgrowth of the subperiosteal bone, and is not preceded by cartilage.

A detailed account of the mode of ossification of these phalanges will be shortly published.

VI. "On the Organisation of the Fossil Plants of the Coal-measures. Part XI." By W. C. WILLIAMSON, F.R.S., Professor of Botany in the Owens College, Manchester. Received June 3, 1880.

(Abstract.)

M. Renault has recently published a memoir, in which he reproduces the views of M. Brongniart respecting the relations which the Lepidodendra bear to the Sigillariæ, still insisting that the former are cryptogamic Lycopods, whilst the latter are exogenous Gymnosperms. In endeavouring to establish this position, the French palæo-botanist concludes that if the exogenous Diploxyloid stems (*i.e.*, Sigillarian ones) are but matured states of some Lepidodendra, every Sigillarian type of organisation ought to be found in a young or Lepidodendroid form, because, he contends, the type of the central organisation, once established, undergoes no further change with advancing age. In support of his position, he affirms that there are three such Sigillarian

types, viz., Sigillaria vascularis; 2. Diploxyloid stems; 3. Favularia and Leiodermaria. At present he contends that only the second of these forms has been discovered in Lepidodendron Harcourtii. He further believes that there are three types of Lepidodendron known, represented by—1. L. Rhodumense, with a solid central vascular axis, in which the vessels are not intermingled with medullary cells; 2. by L. Harcourtii, in which the vascular axis is a cylinder surrounding a cellular medulla; and, 3. An undescribed plant, which he names L. Jutieri, in which the vascular cylinder is broken up into detached bundles of vessels.

The author of the present paper considers that the above conclusions are not in accordance with the facts, and he proceeds to give his reasons for this conclusion by demonstrating that we certainly have *two* of the three supposed Sigillarian types represented in a young or Lepidodendroid state—the first by Lepidodendron vasculare of Binney, and the second by L. Harcourtii, whilst, judging from M. Renault's own description, the L. Jutieri represents the third type. On the other hand, the author believes that of M. Renault's three Lepidodendroid types the first is only a young state of the second, as illustrated by the development of the Burntisland and Arran Lepidodendra described in previous memoirs, whilst the able Frenchman appears not to have been acquainted with the existence of the very characteristic type of the L. vasculare of Binney.

The author gives the series of facts upon which his opinions are based by tracing the history of the development, first, of Lepidodendron Selaginoides, the L. vasculare of Binney, and, second, of L. Harcourtii.

Commencing with the declaration that the Lepidodendron vasculare of Mr. Binney is but the young state of the Sigillaria vascularis of the same author, he proceeds to show the successive stages by which the vasculo-cellular medullary axis of the former becomes not only enclosed within the exogenous cylinder of the latter, but that this cylinder ultimately develops into a very conspicuous example of the Diploxyloid form of stem. The growth of the exogenous cylinder begins at one point of the periphery of the vasculo-medullary axis, from which point it extends both laterally and radially. The exogenous growth thus first appears in the transverse section of the Lepidodendroid twig as a small crescent, thickest at its centre, but whose two horns creep gradually round the medullary axis, its constituent vascular wedges also growing radially as the lateral growth advances, until at length the exogenous zone forms a complete ring, enclosing the vasculo-medullary axis, in which state it becomes the Sigillaria vascularis of Mr. Binney and M. Renault. The various stages of this growth are represented in the plates, in addition to which a section is described and figured of a branch about to dichotomise, in which process the vasculo-medullary axis has divided into two equal halves, one being destined for each branch. One of these halves of the vasculo-medullary axis displays, with the utmost distinctness, the characteristic crescentic commencement of an exogenous zone, whilst the other half retains its primary non-exogenous state. The latter condition thus belongs to the Lepidodendron vasculare of Binney, whilst the former as clearly represents the Sigillaria vascularis of the same author, and the Sigillarian character of which is recognised by M. Renault. We thus have in one stem two branches, one of which, according to the views of the French savant, is a Cryptogamic Lycopod, and the other a Gymnospermous Sigillaria. The remarkable peculiarities characterising the central axis of these specimens makes it absolutely certain that they all belong to one species of plant.

The typical Lepidodendron Harcourtii is then examined in a similar manner. In the details of its organisation it differs materially from L. Selaginoides, nevertheless as its growth progresses it displays typically similar changes. It attains to much larger dimensions than the latter plant does before developing its exogenous zone, corresponding in this respect with the Arran plant. Its earlier changes are chiefly seen in the rapid development of the bast or prosenchymatous layer of the outer bark and in the increase in the size and number of the vessels constituting its vasculo-medullary cylinder or medullary sheath—the "*étui medullaire*" of Brongniart; but in more advanced specimens a cylindrical zone of centrifugally developed vascular wedges begins to make its appearance in a quasi-cambian zone of the cells of the inner bark, these cells being arranged in more or less regular radiating lines. In this state the rudimentary vascular zone corresponds very closely to what is seen in young stems and roots of some Cycads.

The author shows that, contrary to the views of M. Renault, very marked changes take place in the development of the vascular bundles destined for the secondary branches of the plant. In the first instance, each of these is but a concavo-convex segment of the entire vasculo-medullary cylinder, whose detachment leaves a large gap in the continuity of that cylinder, which, however, soon becomes closed again by the convergence of the disconnected ends of the broken vascular circle. The concavo-convex, detached segment undergoes a similar change. Its two extremities meet, and before it escapes from the outermost bark, it has assumed the cylindrical form of its parent stem.

The rootlets of Stigmaria ficoides, now well known to belong alike to Lepidodendron and to Sigillaria, present some peculiarities of structure which are only found in the Lycopodiaceæ and the Ophioglossæ, amongst living plants.

The vascular bundle in the interior of each Stigmarian rootlet is enclosed within a very regularly circular cylinder, composed of the cells of the innermost bark; but the position of the bundle in relation to the cylinder is always, unless accidentally disturbed, an eccentric one. This position has not escaped notice, but it was regarded as accidental, but it now proves to be a normal one. The bundle begins to appear in very young roots, as one or two very small vessels developed in close union with the innermost cells of one side of the cylinder within which it is located; newer and larger vessels are gradually added centripetally, until the bundle occupies a considerable portion of the area enclosed by the inner bark cylinder. The remaining space is usually empty, but occasionally specimens are found in which it is filled with small delicate cells that have escaped destruction. These represent what in the living Lycopods are liber-cells. The outer cortical layer of the root, composed of well preserved and rather thick walled cells, is usually separated from the inner cylinder by a similar lacuna; but in a few specimens the cells of this usually destroyed middle bark are retained in good preservation. They consist of very delicate thin-walled parenchyma, separated by a sharp line of demarcation equally from the innermost and outermost cortical cylinders. The number of the vessels in each of the vascular bundles given off from any one section of a Stigmarian root is found to vary but little, but they steadily increase, both in number and size, with the size and age of the root. Young specimens of Stigmarian roots are described, the smallest of which is not more than one-fifth of an inch in diameter, and the vascular bundles of its small rootlets consists each of from three to five minute vessels. In the largest rootlets from old roots they number about forty, most of the additional ones being of larger size; intermediate examples exhibit a regular gradation on all these points.

The only living plants which possess rootlets with this structure being Lycopodiaceæ and Ophioglossæ, and it being sufficiently clear that the Lepidodendra belong to the former and not to the latter order of Cryptogams, the existence of this Lycopodiaceous feature in the rootlets of Sigillaria is another indication of the Lycopodiaceous affinities of these plants.

Many of the Diploxyloid forms of the Lycopodiaceous stems of the Coal-measures have an abundant development of spiral or barred cells in their numerous medullary rays. Among living plants this characteristic seems to be almost, if not wholly, confined to the Gymnosperms.

Two important additional observations have been made in reference to the structure of the curious Strobilus, Calamostachys Binneyana. The exact mode of the attachment of its sporangia to the Equisetiform sporangiophores has been ascertained; but what is still more important, it has also been discovered that it is provided with both micro- and macro-spores—an additional indication of its probable Lycopodiaceous affinities, already suggested by other features of the fruit.

The recently discovered fungi of the Coal-measures are investigated, especially the Pernosporites antiquiorum of Mr. Worthington Smith. The author finds, in the specimens he has examined, including that described by Mr. Smith, no traces of septa in the hyphæ or of Zoospores in the Oogonia. He concludes that its affinities are probably with the Saprologniæ, and not with the Pernosporeæ.

VII. "Note on the Discovery of a Fresh Water Medusa of the Order Trachomedusæ." By E. Ray Lankester, F.R.S. Received June 15, 1880.

[Publication deferred.]

VIII. "Agricultural, Botanical, and Chemical Results of Experiments on the Mixed Herbage of Permanent Meadow, conducted for more than Twenty Years in Succession on the same Land. Part II. The Botanical Results." By J. B. Lawes, LL.D., F.R.S., F.C.S., J. H. Gilbert, Ph.D., F.R.S., F.C.S., F.L.S., and M. T. Masters, M.D., F.R.S., F.L.S. Received June 17, 1880.

(Abstract.)

Part II.—*The Botanical Results.*

In Part I (" Phil. Trans.," Part I, 1880), under the title of the "*Agricultural Results*," a general description of the experiments, with full particulars of the conditions of manuring of each of the more than twenty plots, was given. The effects of each condition of manuring on the character of growth of the herbage, as illustrated in the quantities of produce yielded, and in the amounts of nitrogen and of mineral matter taken up, on each plot, were also fully considered. But, so varied were the components of the mixed herbage, both as to the species grown, and as to the character of development of the plants, that, to render the "*Agricultural results*" sufficiently intelligible, and to prevent misconception, if the element of quantity only were taken into account,

it was found necessary to describe, in general terms, the differences in the botanical composition, in the character of development, and in some points in the chemical composition of the produce also. The object of the present section is to describe and discuss, more in detail, what may be called the *botany* of the plots; that is, to show both the normal botanical composition of the herbage, and the changes induced, by the application of the different manuring agents, and by variations in the climatal conditions of the different seasons; and, as far as may be, to ascertain what are the special characters of growth, above-ground or under-ground, normal or induced, by virtue of which the various species have dominated, or have been dominated over, in the struggle which has ensued.

The method of taking the samples, and of conducting the botanical analyses and observations, is described. The characters of the seasons in which complete botanical separations were made, as well as those of some of the seasons leading up to the years of separation themselves, are discussed. The flora of the collective plots is described; and the organization by means of which the constituent plants may maintain themselves, or succumb in the competition, is considered. The characteristics of the individual dominant plants are pointed out; and, finally, the botany of each of the twenty-two plots is fully detailed, and the changes induced, by season or manuring, are discussed.

IX. "Preliminary Note on some Points in the Pathology of Anthrax, with especial reference to the Modification of the properties of the *Bacillus anthracis* by Cultivation, and to the Protective Influence of Inoculation with a Modified Virus." By W. S. GREENFIELD, M.D., F.R.C.P., Professor Superintendent of the Brown Institution. Communicated by Professor BURDON-SANDERSON, F.R.S. Received June 17, 1880.

In the course of some experimental investigations into the pathology of anthrax at the Brown Institution, made during the past twelve months, two series of phenomena have been the subject of study, and in each some results which I believe to be novel have been attained. These results have not only a considerable practical importance, if verified by other observers, but their interest in relation to the pathology of anthrax and other diseases appears to me sufficient to warrant their communication to the Royal Society in the form of a preliminary note, leaving the full detail of the experiments for a future occasion.

The practical purpose of these investigations was to ascertain (1) by what means the virus of splenic fever may be so modified as to be capable of inoculation without fatal result, and (2) whether a modified attack, produced by inoculation, exerts any protective influence against a future inoculation with unmodified virus.

The conclusions arrived at by the experiments made with this object were as follows:—

1. That anthrax may be artificially communicated to bovine animals by inoculation with the blood or spleen of the guinea-pig which has died of the disease artificially induced, and that the same result may be attained by inoculation with the *Bacillus anthracis* cultivated from the fluids of a rodent; the disease thus induced being severe, but rarely fatal to previously healthy bovine animals.

This result had already been attained by Dr. Burdon-Sanderson in some previous experiments, with the details of which I was not acquainted when my own experiments were made.

2. In all the cases thus inoculated, the animals appeared to have acquired either a considerable degree of protection or entire immunity from the results of subsequent inoculation, although much larger doses of the virus were employed.

Some of my experiments on these subjects have been already published in the Journal of the Royal Agricultural Society.

In the course of these experiments, I employed on several occasions the *Bacillus anthracis* artificially cultivated in successive generations in aqueous humour, and finding that the results appeared to vary considerably with the stage of the cultivation, those furthest removed from the original parent source being more frequently inactive, I was led to make a series of observations, of which I now desire to communicate the results. They may be stated as follows:—

That when the *Bacillus anthracis* is artificially grown in successive generations in a nutrient fluid (aqueous humour) it maintains its morbific properties through a certain number of generations, but each successive generation becomes less virulent than its predecessor, requiring both a longer time and a larger quantity to exert its morbific action; and after continuous diminution of virulence, at a certain stage in the successive cultivations, the *Bacillus*, though maintaining all its morphological characters and its power of growth, becomes completely innocuous even to the most susceptible class of animals.

It may be added that the modified virus produces forms of modified disease which differ widely from ordinary splenic fever, both in the distribution of the *Bacilli* and in the nature of the symptoms and pathological appearances.

It does not appear necessary at the present moment to enter with further detail into the evidence upon the subject of modified inoculation and protection from further attack, for this could only be done by a detail of the experiments, which is beyond my present purpose. But it is desirable that I should briefly state the general method employed in the determination of the gradual diminution of virulence by successive artificial cultivations, and the evidence upon which the conclusion is based.

The experiments were performed in a very simple manner. The cultivating fluid employed was aqueous humour, the animals inoculated were chiefly mice, though guinea-pigs, rabbits, cows, and on one occasion a sheep were also used. But mice, being especially susceptible to the poison, were mainly relied on. The cultivations were performed in closed tubes, half filled with the cultivating fluid. Every precaution was taken both to prevent the entrance of other organisms and to detect and eliminate from the series any cultivations which proved to be contaminated.

The cultivations were continued to the nineteenth generation, each successive generation presenting identical morphological characters at the various stages of its growth, and showing no diminution in the capacity for growth, nor marked variation in the time and temperature relations of its germination. From each successive cultivation fresh cultivations were made, a microscopic examination was performed, some capillary-tubes were filled with the cultivation and preserved for future use, and, in most cases, a portion used for inoculation of a mouse.

The first series of experimental inoculations were made, step by step, as the cultivations proceeded, and when the results showed that the *Bacillus*, though still actually growing, might be inoculated in large quantities with impunity, a second series of inoculations was made with the material which had been preserved in capillary tubes. To this second series the objection might be made that the material had deteriorated by keeping. The reply is to be found in the fact that the earlier the cultivation the longer had it been kept, so that the first cultivation might be two or three weeks older than the sixth, and would have had more chance of deterioration, whereas it was found to be equally virulent at the end of three months as when first used.

In no case was I able to produce any symptoms or a fatal result by inoculation with a later generation than the twelfth; beyond that stage a large quantity of actively germinating rods and spores producing no result whatever. The diminution of virulence was very marked, even at the eighth generation, both as regards the proportion of animals affected and the rapidity of action with an equal dose.

I do not desire at present to dwell upon any conclusions to be drawn from these experiments. I would merely observe that, while they suggest a means of protective inoculation with a modified virus, the activity of which may be capable of exact determination, they also point to a modification of the views which have been entertained on the relations of the *Bacillus anthracis* to splenic fever. Whether the facts are explicable on the hypothesis of alteration of the habitat of the *Bacillus*, acclimatization, and incapacity for transplantation to a different soil; whether the same result would be attained by cultivation in other media; and, lastly, whether by any means the *Bacillus* might again acquire the power of producing the disease—these are questions which can only be determined by further experiment.

X. "On the Miocene Plants discovered on the Mackenzie River." By Professor OSWALD HEER, of Zürich. Communicated by ROBERT H. SCOTT, F.R.S. Received June 16, 1880.

On his return from his "searching expedition" to Arctic America of the year 1848, Sir John Richardson brought to London a box of fossil plant-remains, which he had collected on the Mackenzie River, between Fort Norman and the Great Bear Lake River, in latitude 65° N.

The deposits from whence the specimens were obtained had been discovered by Sir Alexander Mackenzie, in 1785, and visited by Sir J. Franklin and Sir John (then Dr.) Richardson in 1825; but it was not until his second visit that Richardson was able to bring away specimens (Journal, vol. i, p. 186).

This first collection contained seventeen specimens, of which only fourteen allowed of exact determination, but it presented features of great interest, as it gave us the first indications of the character of the Miocene Flora of these inaccessible regions of North America.

The importance of procuring more copious materials for the study of these deposits hardly requires to be stated, and, accordingly, in the year 1871, Mr. Robert H. Scott, F.R.S., and Dr. John Rae, F.R.S. (who had himself accompanied Sir John Richardson on his expedition in 1848, when the first specimens were collected), obtained from the Government Grant Committee the sum of £50 to defray the cost of procuring and forwarding to London a collection of these plant-remains. The actual expenses incurred have, however, fallen far short of the amount received.

The kind offices of Mr. W. Hardisty, Chief Factor of the Hudson's Bay Company's Service, were enlisted for the task, which was not an easy one to carry out. The locality is distant from any inhabited port, and is merely passed by the boats on their annual trading journeys up and down the river, when the men are fully employed in making the best use they can of the short period available for navigation, and have but little time to look for fossils.

More than one attempt was made to send boxes to London; but as this involved a long journey by boat before they reached the port of shipment, it is not to be wondered at that two or three cases failed to reach their destination. At last, in the summer of 1879, Dr. Rae learnt that a box of fossils, addressed to him, was lying at Manitoba which eventually was sent by rail to New York, and thence by steamer to London, its contents reaching me in the course of the present spring (1880).

The specimens consisted of silicified wood with a number of leaves, partly in soft, but partly also in baked clay. The nature of the stone corresponds exactly with that in which Richardson's specimens are embedded; and as, of the fourteen species represented in the new collection, seven are found in Richardson's, it is evident that all the specimens come from the same locality. The same is also true of a few plant fossils recently submitted to me, which have been received by Mr. Scott from the Bishop of Athabasca, who, before leaving England, had readily consented to do what he could to forward the study of the geology of this part of his immense diocese.

If we summarise all the known facts of the Fossil Flora known to occur on the Mackenzie River, we find that there are twenty-three species, of which six are widely disseminated in the Miocene deposits of Europe:—*Taxodium distichum miocenum*, *Glyptostrobus Ungeri*, *Sequoia Langsdorfii*, *Corylus M'Quarrii*, *Platanus aceroides*, and *Juglans acuminata*.

Not a single one of the species is found in the Eocene of Europe.

The Mackenzie Flora contains six species present also in the Browncoal Flora of Alaska, viz., *Tax. distichum*, *Glypt. Ungeri*, *Seq. Langsd.*, *Coryl. M'Quar.*, *Jugl. acuminata*, and *Viburnum Nordenskiöldi*.

Eight appear also in the Flora of Saghalien:—*Tax. dist.*, *Glypt. Ung.*, *Sequ. Langsd.*, *Corylus M'Quar.*, *Jugl. acum.*, *Hedera M'Clurii*, *Quercus Olafseni*, and *Magnolia Nordenskiöldi*.

The Flora exhibits just as close a connexion with that of the Arctic Miocene beds. Thirteen species from Greenland, fourteen from Spitzbergen, five from Grinnell Land, and four from Iceland make their appearance.

In Canada, to the south of the Mackenzie River, in lat. 49° N., Tertiary plants have been found in some localities, *e.g.*, at Porcupine Creek, at Great Valley, and on the Milk River. The last-mentioned localities have yielded only a few species, and Professor Dawson has determined fifteen from Porcupine Creek.* Of these six are still existing, and one of these species (*Onoclea sensibilis*, L.) has also been found in the Miocene Tuffs of Mull, in Scotland, and at Fort Union. To these should be added three species known as Miocene, *Glyptostrobus Europæus*, *Sequoia Langsdorfii*, and *Populus Richardsoni*.

Not one of the plants described by Dawson occurs in the Eocene of Europe, and in the United States themselves four of the species have been found in the Miocene, while only one (the *Sequoia*) is possibly admissible as Eocene.

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* *Vide* "Report on the Geology and Resources of the Region in the vicinity of the 49th Parallel." By G. M. Dawson. Page 328.

When we, moreover, consider that, according to Dawson's determinations, one-half the species agree with living forms, while not an individual existing plant is known to occur in the Eocene of Europe, we must certainly range these Porcupine Creek beds with the Miocene and not with the Eocene, where Professor Dawson places them.

Professor Dawson was led to this incorrect conclusion by some remains of Vertebrata found at Milk River. These, however, occur on a lower horizon than the plant beds on the Porcupine River, as the author himself points out (*loc. cit.*, p. 152), and these Milk River beds *may* be Eocene.

Two species are common to the Mackenzie River and Porcupine Creek, and when Professor Dawson, from this circumstance, concludes that the plants from the former locality are Eocene, he has reasoned on entirely mistaken grounds. The same remark may be made when he assigns the Tertiary Flora of the Arctic regions to the Lower Eocene.

The plant-remains found at Porcupine Creek, on the Mackenzie River, and in Alaska belong to the northern Miocene Flora of North America, which has a considerable number of species in common with the Miocene Flora of the Frigid Zone, and belongs to a period at which in the Rocky Mountains, in Greenland, and Iceland, as well as in Scotland, Ireland, and on the continent of Europe extensive volcanic eruptions occurred, in which these plant-remains are occasionally found embedded.

If we compare the plants from the Mackenzie River with the Tertiary Flora of the United States, which has been very thoroughly worked up by Professor L. Lesquereux,* we find eight species in common, viz., *Tax. dist.*, *Sequ. Langsd.*, *Glypt. Ung.*, *Cor. M'Quar.*, *Pop. Richards.*, *Populus Arctica*, *Betula macrophylla*, and *Platanus aceroides* (?).

All these species appear in the Miocene beds of America, but one of them (*Pop. Arctica*) reaches down into the Oligocene and a second (the *Sequoia*) possibly even down to the Eocene.

The comparison with the Tertiary Flora of the United States, therefore, tends to place the white clay beds of the Mackenzie River among the Miocene, and accordingly corroborates the result based on the proved agreement of the fossils therein contained with the Miocene Flora of Europe and of the Arctic regions.

The specimens of silicified wood have been submitted for microscopic examination to M. C. Schröter, Assistant at the Botanical Laboratory of the Polytechnicum of Zürich, and I shall hope to communicate a notice of any results he may attain to the Society.

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* L. Lesquereux, "Contributions to the Fossil Flora of the Western Territories. II. The Tertiary Flora." Washington, 1878.

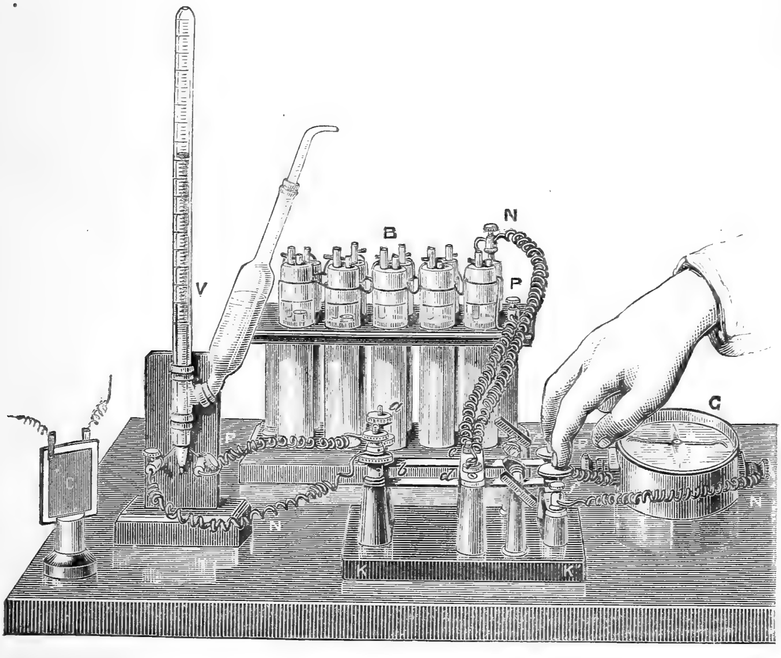
XI. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery." By WARREN DE LA RUE, M.A., D.C.L., F.R.S., and HUGO W. MÜLLER, Ph.D., F.R.S. Received June 16, 1880.

SUPPLEMENT TO PART III.

The Electric Condition of the Terminals of a Vacuum-Tube after their connexion with the Source of Electricity has been broken.

The question has been mooted whether there is a polarisation of the terminals of a vacuum-tube after the discharge through it has been interrupted by breaking connexion between it and the source of electricity. This question we understand in the sense—does there exist a *chemical* polarisation as is the case for instance with the terminals of a voltameter under similar circumstances? It is evident that if a polarisation of this kind could be proved, the formula used for determining the tube potential ("Phil. Trans.," Part I, vol. clxxi, p. 65) would no longer hold good.

FIG. 1.



The problem is by no means easy of solution, and it has engaged our attention for the last five months. We believe, however, that the few experiments we are about to describe, and which we select from a long series, will show that there is really no such polarisation of the terminals.

We will first take the case of the voltameter; the arrangement of the apparatus employed in our experiments is shown in fig. 1, where K K′ represent a special key for breaking connexion between any piece of apparatus and the battery, B, and connecting it afterwards instantaneously with a galvanometer, G. The terminals of the battery are attached respectively to the screw-clamps, *c* and *d*, carried by an ebonite bar, supported on two ebonite columns, touch-points in metallic contact with *c* and *d*, pass through this bar, so that when the brass springs, *ae* and *bf*, are allowed to rise and press against these touch-points, the battery is in metallic connexion with the screw-clamps, *a* and *b*, and any piece of apparatus attached to them, as, for example, the voltameter, V. The voltameter employed has a resistance of 20 ohms; its platinum electrodes are 2·5 inches long and 0·025 inch in diameter, and are separated 0·25 inch; it is charged with dilute pure sulphuric acid in the proportion of 1 volume of acid, sp. gr. 1·84, to 8 volumes of water. When the springs are suddenly pressed down by the fingers placed on the ebonite disks, *e* and *f*, the connexion is broken between the voltameter and the battery, and it is connected instantaneously with the galvanometer, G. The galvanometer actually employed was not that shown in the figure, but a Thomson galvanometer, whose constant, $C, = \frac{1 \text{ volt}}{1 \text{ megohm}} = 1{,}874$ scale divisions. On connecting a battery of 10 chloride of silver cells with the voltameter for a minute or less, and then suddenly pressing down the springs *e* and *f*, there was a deflection to the left, say, of more than 1,000 scale divisions, although the $\frac{1}{999}$ shunt was used to reduce the current through the galvanometer; therefore, the deflection without the shunt would have been more than 1,000,000 divisions. By comparing this deflection with that produced by a half microfarad condenser, charged with 240 cells, it was ascertained that the deviation produced by the voltameter was equivalent to that of 111 microfarads.

The small condenser shown at C, fig. 1, was substituted for the voltameter; it is made of a thin plate of glass 2 inches square and 0·0115 inch thick; the tinfoil coatings being 1·5 inch square, its capacity was found to be 0·00055 m.f. When charged with 3,600 cells, and afterwards connected with the Thomson galvanometer through the $\frac{1}{9}$ shunt, by pressing down *e* and *f*, the deflection was 136·5 divisions to the left; this multiplied by 9·92, the value of the shunt was equivalent to 1,354 divisions.

The apparatus, shown in fig. 2, which was constructed for another object already described,* was connected with *a* and *b*; it consists

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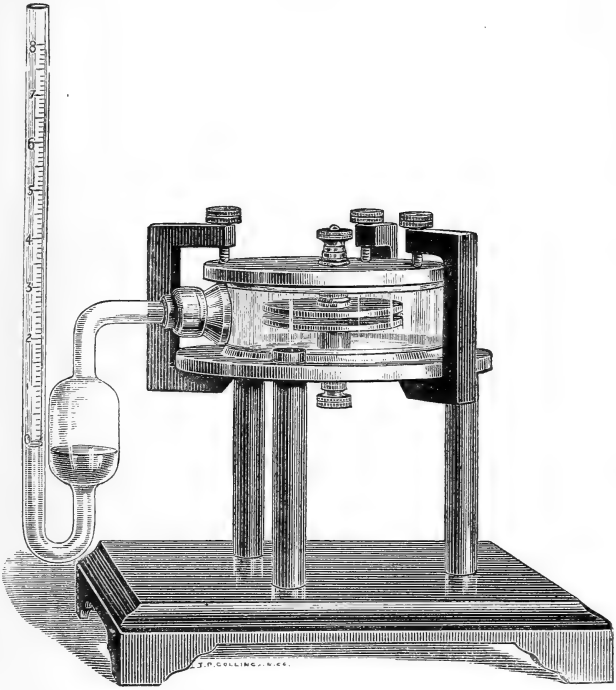
* "Phil. Trans.," vol. clxxi, p. 76.

essentially of two disks, 3·1 inches diameter, placed 0·13 inch apart. The capacity of this apparatus, when used as an air-condenser, was determined, and found to be 0·000058 m.f. With 3,600 cells no discharge took place, and it merely charged up as a condenser. The deflection produced, when the keys *e* and *f* were pressed down, was (without shunt) 150 divisions, still to the left. It is evident, therefore, that the direction of the deviation throws no light on the question, for it is the same with the voltameter as with the condenser.

Fig. 2.

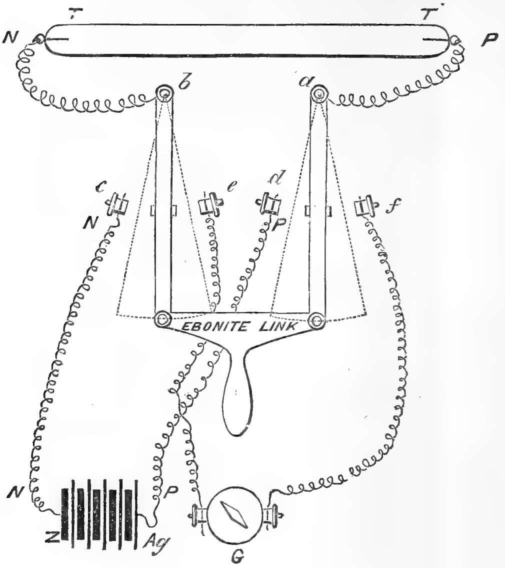


Tube 73, containing a residuum of acetylene, was now substituted for the air-condenser; it is 26·5 inches long and 1·5 inch diameter, the distance between the terminals 23 inches; this tube was connected with 3,600 cells, current 0·00681 W. The tube potential was found to be 2,980 cells, and its resistance 449,500 ohms. On pressing down the springs *e* and *f*, so as to break connexion with the battery and connect the tube with the galvanometer, there was a deflection of 11 divisions to the left, the same as before. The current was only $\frac{11}{1874 \times 1{,}000{,}000}$

$$= \frac{1}{170{,}000{,}000} \text{ W.}$$

The diagram, fig. 3, will illustrate the action of the special key, fig. 1. When the handle is moved to the left, the tube TT′ is placed in metallic connexion with the battery, whose terminals are shown attached to *c* and *d* (this is equivalent to the springs being allowed to press upwards against *c* and *d*, fig. 1); when the handle is moved to the right, then the discharge of the battery through TT′ ceases, and the terminals of the tubes N and P are connected with the galvanometer, the extremities of whose coil are attached to *e* and *f* (this is equivalent to pressing down the springs in fig. 1).

Fig. 3.



On another occasion with the same tube, No. 73, with a discharge from 2,400 cells, the deviation to the left on passing down *e* and *f* was 20 divisions.

Tube 199, with a hydrogen charge, was now substituted, fig. 4; pressure 2 millims., 2,632 **M**, 5,100 cells, current 0·01639 W. This tube has already been described;* it is 37 inches (94 centims.) long, and $5\frac{13}{16}$ inches (14·8 centims.) in diameter. The distance between the terminals, a ring and a straight wire, is 33·5 inches (85 centims.). In the first place the battery was connected direct to the galvanometer, the positive to *e* and the negative to *f* (that is, in the same direction as if the positive were attached to *c* and the negative to *d*). A short piece of wire was inserted between *e* and *f* as a shunt, and the $\frac{1}{999}$

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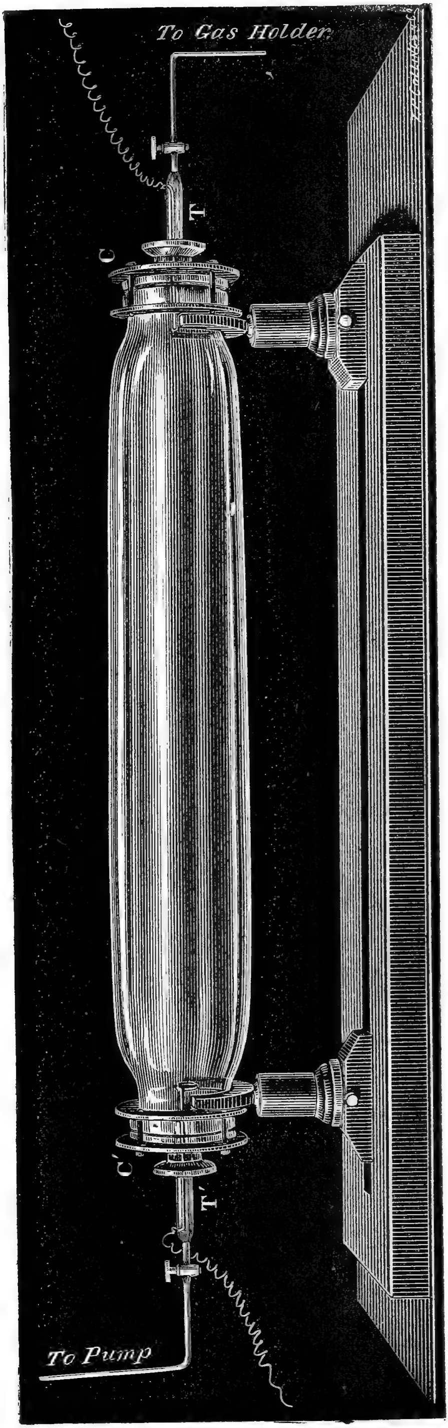
* "Phil. Trans.," vol. clxxi, p. 104.

FIG. 4.



shunt was also used with the galvanometer, the direction of deviation was found to be to left, which it was desired to know.

On pressing down *e* and *f*, the deviation was to the left and only amounted to 2 divisions. On another occasion, at the same pressure, 3,900 cells, current 0·02925 W., the deviation to the left on pressing down *e* and *f* was from 3 to 5 divisions.

The same tube, with a charge of coal-gas, pressure 3 millims., 3,947 **M**, current 0·01705 W., deflection, on pressing down *e* and *f*, 7 divisions to left.

The same tube, with air, pressure 1·5 millims., 1,974 **M**, current 0·02728 W., on pressing down *e* and *f*, deviation 20 divisions. The deviation was, therefore, greatest with air; but if due to a chemical polarisation, it would have been *à priori* expected to be greatest with coal-gas, which is a mixture of decomposable molecules. The result of the experiments with tube 199 gave the following deviations:—

Hydrogen.		Coal-gas.		Air.
3	....	7	....	20

Again, tube 199, 2,400 cells, pressure 1 millim., 1,316 **M**, current 0·02456 W., deflection on pressing down the keys, 16 to left; with 1,500 cells no discharge, yet a deflection of 10 divisions to the left was produced on pressing down the keys. The deflection in the latter case being clearly due to a static charge.

Experiments were now made in air with the apparatus (already described *) shown in fig. 5, in order to obtain a vacuum vessel with larger terminals. The terminals are two disks a little over 4¼ inches (11 centims.) in diameter, and in the first instance were placed 3 inches (7·62 centims.) apart. Pressure 4·5 millims., 5,921 **M**, 2,400 cells, current 0·04366 W., deviation on connecting the disks with the galvanometer, 10 divisions to left.

With 1,200 cells, no discharge took place, but on connecting the galvanometer a deviation of 27 divisions to the left was, nevertheless, produced.

At a pressure of 20 millims., 26,316 **M**, with 3,900 cells, no current passed, but the deviation on connecting the galvanometer, was 66·6 divisions to the left.

The disks were now placed at a distance of 2 inches (5·08 centims.), the pressure being still 20 millims., and the battery 3,900 cells, the discharge now took place, current 0·03896 W., deflection on connecting galvanometer 8·5 to left.

On reducing the battery to 2,400 cells, the discharge did not take place, but a deviation of 43 divisions was produced on pressing down *e* and *f*.

The disks were placed at 1 inch (2·54 centims.) apart, same pressure,

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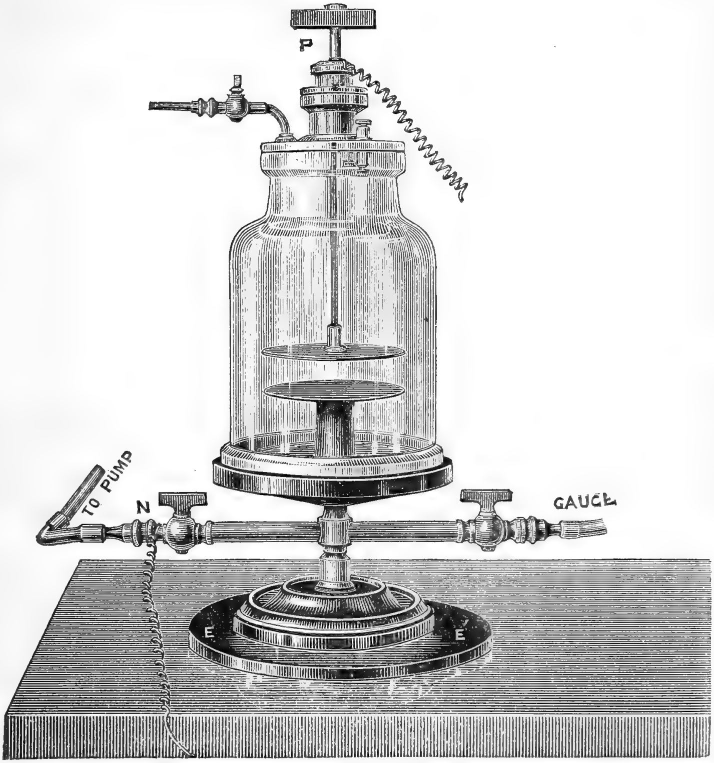
* "Phil. Trans.," vol. clxxi, p. 75.

with 3,900 cells, the current was 0·04201 W., deviation to left on connecting the galvanometer 13 divisions.

FIG. 5.



2,400 cells would not pass, but a deviation to the left of 47 divisions was obtained on connecting the galvanometer.

The following are the results of these experiments with the disks at various distances :—

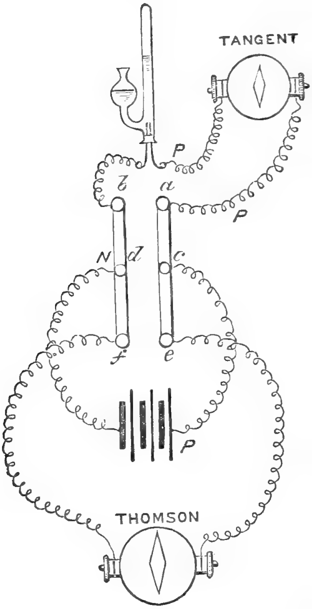
Distance, inches.	No. of cells.	Pressure. mm.	Current.	Deflection on connecting galvanometer, divisions.
3	1,200	4·5	did not pass	27·0
3	2,400	4·5	passed	10·0
3	3,900	20·0	did not	66·6
2	3,900	20·0	passed	8·5
2	2,400	20·0	did not pass	43·0
1	3,900	20·0	passed	13·0
1	2,400	20·0	did not pass	47·0

So that in every case when the discharge did not take place, the deflection, on connecting the terminals with the galvanometer, was the greatest.

If the fact which we have already pointed out be taken into account, that with a constant battery-potential, the difference of potential between the terminals of a vacuum-tube varies, in the same gas, according to the degree of exhaustion, it follows that as soon as a discharge takes place, the potential of the terminals will be lowered. We should therefore expect to find what the before-cited experiments indicated, namely, that the static charge of the terminals would be greater when no discharge takes place than after it has occurred, notwithstanding that a larger number of cells may have been employed in the latter case than in the former, for we have shown ("Phil. Trans.," vol. clxxi, p. 67), that a tube-potential may be only 430 cells, although the battery connected with its terminals is 11,000 cells.

We believe, therefore, that the experiments point conclusively to the deduction that the current obtained from the terminals of a vacuum tube, after having been disconnected from the battery, is solely due to a static charge and not to a *chemical* polarisation.

Fig. 6.



An experiment was made with the apparatus arranged as in fig. 6, a tangent galvanometer being inserted in the circuit between the battery and the voltameter, the $\frac{1}{999}$ shunt being used with the Thomson galvanometer.

No. of cells.	Current indicated by the tangent galvanometer. W.	Deflection of Thomson galvanometer, with shunt $\frac{1}{999}$, on pressing down *e* and *f*.	Divisions.
1	0·00000	220 × 995·3*	218,930
2	0·00415	765 ,,	761,380
3	0·03463	984 ,,	978,820
10	0·14660	990 ,,	985,150

On keeping down the keys *e* and *f* after the voltameter had been connected with three cells, the deflection, which at first was 984 divisions, fell in—

1 minute to	110
2 minutes to	80
3 ,,	68
4 ,,	60
5 ,,	55
6 ,,	52
7 ,,	49

so that it was evident that a current was kept up by the voltameter for a considerable time after the battery had been disconnected.

In order to render evident the direction of the current of polarisation of the voltameter the apparatus was arranged as in fig. 7, that is, the Thomson galvanometer, with $\frac{1}{999}$ shunt, was inserted in the circuit between the battery and the voltameter. An adjustable shunt was fixed between *a* and *b* to permit the greater part of the current to pass through it. A plate of metal, *ss*, was provided to slip under *e* and *f* to short circuit the return current through the galvanometer. The shunt which was found just sufficient to carry the major part of the current, and yet permit sufficient to traverse the voltameter to produce a just visible evolution of gas, was 13 ohms.

On connecting the battery, the current was in the direction of the lower arrow, and the deviation 133 divisions say to the left; on pressing down the keys on to the metal slip placed under them to short circuit the current, after the battery had been disconnected, the return current was in the reverse direction, as shown by the upper dotted arrow, the deviation being 425 divisions to the right. Even without pressing down the keys on to the metal slip nearly the same deviation was obtained on disconnecting battery for the return current then traversed the shunt from *a* to *b*.

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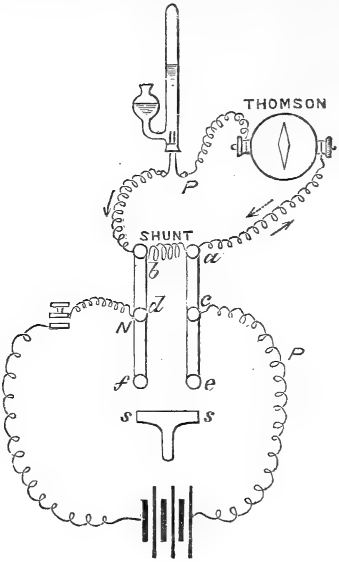
* The value of the $\frac{1}{999}$ shunt.

Fig. 7.



On substituting either the bell-jar, with disk terminals, or the tube 199 for the voltameter, the deflection of the return static charge was in the same direction as the return current from the voltameter, so that, as we before stated, no inference can be drawn from the direction of the current as to its being produced by polarisation or a static charge. We think, however, that we have shown conclusively that the effect in the case of a vacuum-tube is due to a static charge, and not to a polarisation of the terminals. We rest our opinion mainly on the fact that a greater deflection is produced, when the potential has not been lessened by a discharge through the tube, than that which occurs after the discharge has taken place, which, it had been surmised, might possibly produce a chemical polarisation.

XII. "On the Constants of the Cup Anemometer." By the Rev. T. R. Robinson, D.D., F.R.S., &c. Received June 13, 1880.

(Abstract.)

In a previous paper the author detailed experiments made by attaching anemometers to a whirling machine, and the conclusions to which they led. He was however doubtful of the accuracy of the method, and proposed one depending on the action of natural wind.

He has tried this, and he thinks successfully. Two instruments of the Kew type, differing only in friction, were established 22 feet asunder on the roof of the house and 16 feet above it: the number of turns made by each, and the time, were recorded by a chronograph, and from these, v and v', the velocity in miles per hour of the centres of the cups was known.

The friction of one of these (K) was constant; that of the other (E) was varied by applying to a disk on its axle Prony's brake, which was connected with a spring balance whose tension was recorded during the time of experiment by a pencil moved by clockwork. Thus the mean friction was obtained. It ranged from 353 grains to 4,982.

The equation of an anemometer's motion is $V^2+v^2-2Vvx-\frac{f}{\alpha}=0$ where V is the unknown velocity of the wind, α and x two constants which are to be determined. Each observation gives two equations in which there are four unknown quantities, for it is found that the value of V changes from one instrument to another. This is partly owing to eddies caused by the buildings, but also in great measure to irregularity of the wind itself. It is, however, also found that these wind-differences are as likely to have + as − signs, and therefore it may be expected that their sum will vanish in a large number of observations. The ordinary methods of elimination fail here even to determine with precision a single constant, and he has proceeded by approximation.

Assuming the value of α given by the actual measurements in his paper $=15{\cdot}315$ at 30″ and 32° for 9-inch cups, and that there is no resistance as v^2 except that in the equation, and assuming an approximate value for x, we can compute V and V′. The difference between these must be due to an error in x and to w the wind error, and taking the sum of a series we have

$$S(V'-V)+Sw=\Delta x\times S(e-e')\,;\ e \text{ being } \frac{V}{\sqrt{x^2-1+\frac{f'}{\alpha v'^2}}}.$$

If the observations are sufficiently numerous $Sw=0$, with the assumed $x+\Delta x$ thus found, recompute the V till the sum of $V'-V$ is insensible, and the final x will give V with a high degree of probability. Twenty-one observations gave a value of x considerably larger than what was obtained with the whirling machine, and of course the limiting factor (that when v' is so large that $\frac{f'}{\alpha v'}$ may be neglected). It is for the Kew type 9″ cups 24″ arms $=2{,}831$. In this series the differences are so evidently casual as to show that neither α or x change with v.

With this x, K gives the true value of V at it; therefore if any other type be substituted for E′ it is easy to find its x, for its α is as area of cups, its f' is known, and assuming its x' and computing as before, we get similarly its Δx. He tried five different types and obtained very unexpected results, for he found that the x varied as some inverse function of the diameter of the cups and of the arms. He gives its values.

No. 1.	Original instrument	12″	cups	23·17	arms,	x=1·5880,	limit	2·812
,, 2.	Kew	9	,,	24,	,,	1·5919,	,,	2·831
,, 3.	,, ...	9	,,	12,	,,	1·7463,	,,	3·035
,, 4.	,, ...	9	,,	8,	,,	2·1488,	,,	4·051
,, 5.	,, ...	4	,,	26·75,	,,	1·8587,	,,	3·425
,, 6.	,, ...	4	,,	10·67,	,,	2·5798,	,,	4·958

No. 6 is similar to No. 2, and it might be expected that their constants would be equal. The cause of these differences is partly the eddies caused by the cups being more powerful when the arms are short, but still more the presence of high powers of the arm and diameter occurring in the expressions of the mean pressures on the concave and convex surfaces of the hemispheres. In the present state of hydrodynamics we cannot assign these expressions, but we know enough to see that such powers may be present.

As each type of anemometer has its own constants, the author would suggest to meteorologists the propriety of confining themselves to one or two forms. For fixed instruments he considers the Kew one as good as any, and would wish to see it generally adopted. For portable ones he has no experience except with Casella's 3″ cups 6″ arms, which he found very convenient; he has not however determined its constants. Some selection of the sort seems necessary if it is wished to have an uniform system of wind-measures.

XIII. "Note on the Bearing on the Atomic Weight of Aluminium of the Fact that this Metal occludes Hydrogen."

By J. W. MALLET, F.R.S. Received June 14, 1880.

In a recent communication to the Académie des Sciences,[*] it has been pointed out by Dumas that metallic aluminium and magnesium, as well as silver, may contain sensible quantities of occluded gas, that obtained from aluminium being almost pure hydrogen.

In a paper on the atomic weight of aluminium read before the Royal Society on the 22nd of April last I gave, among other data, the results of several experiments on the quantity of hydrogen liberated by a known weight of aluminium from a strong solution of sodium hydrate.

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[*] "Comptes Rendus," 3 Mai, 1880, p. 1027.

In examining beforehand the purity of the specially prepared metal used, warned by Dumas' previous results as to gaseous occlusion by silver, of which due account was taken in the paper, I did not neglect to test in like manner the aluminium, but obtained an entirely negative result. As, however, I heated the metal in a Sprengel vacuum only to the highest temperature which a hard Bohemian glass tube would bear, while M. Dumas says that gas is only given off suddenly "*vers le rouge blanc*," I have since seeing his paper repeated the experiment in a porcelain tube, and at this higher temperature, obtained by means of an excellent gas furnace, with a small remnant of the same material used in the atomic weight determinations.

4·783 grms. of the metal thus treated gave a small bubble of gas, which measured but ·75 cub. centim. at 24° C. and 752 millims. pressure. Transferred to a miniature eudiometer it was exploded with oxygen, and seemed to be pure hydrogen. This is but little more than one-third the quantity of gas obtained by Dumas, but his experiments and my own with silver prove that the amount occluded varies with the conditions under which the metal has been fused, and two circumstances in the preparation of my aluminium probably tended to reduce the proportion of hydrogen taken up—namely, that, fearing possible contamination of the metal by its alloying itself with sodium at the moment of reduction from the bromide, I used a considerable excess of the latter, and that, not only were the ingots of sodium wiped free of naphtha with a cloth, but the outside surface was pared off with a knife, and only quite clear and solid pieces of the alkaline metal were used for the reduction.

The above volume of hydrogen only represents a weight of 0·000061 grm.; the corresponding weight from the largest amount of aluminium used in the atomic weight experiments of Series 3, A (in which the gaseous hydrogen evolved was *measured*), would be but 0·000010 grm., and the corresponding weight from the largest amount of aluminium used in Series 3, B (in which the hydrogen evolved was burned and weighed as water), would be 0·000067 grm.—both quantities too small to be determined by the balance used.

Even if it were possible to correct for this occluded hydrogen the weight of metallic aluminium taken, the greatest error in the volume of hydrogen obtained in Series 3, A, would only represent 0·12 cub. centim., which would be barely measurable in the flask used, and would affect the atomic weight deduced for aluminium to an extent less than one-tenth of the probable error of the mean result. And, in Series 3, B, the greatest error in the weight of the water produced by combustion of the hydrogen would be 0·0006 grm., measurable by the balance, but affecting the result by less than three-fourths of the probable error of the mean.

Hence it appears that, although there is undoubtedly a constant error due to the cause pointed out by the great French chemist, its influence is in the present case inappreciable by existing means of measurement, and no correction of the numbers obtained for the atomic weight in question can be applied which shall have any real meaning.

The other series (1 and 2) of experiments made to determine this atomic weight do not involve the question of gaseous occlusion, at any rate in the same form, as in them the metal itself was not used, but certain of its compounds only.

XIV. On the Spectrum of the Flame of Hydrogen." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received June 16, 1880.

Messrs. Liveing and Dewar state, in a paper read before the Royal Society on June 10 (*ante* p. 494), that they have obtained a photograph of the ultra-violet part of the spectrum of coal gas burning in oxygen, and in a note dated June 8 they add that they have reason to believe that this remarkable spectrum is not due to any carbon compound but to water.

Under these circumstances I think that it is desirable that I should give an account of some experiments which I made on this subject some months since without waiting until the investigation is more complete.

On December 27, 1879, I took a photograph of the flame of hydrogen burning in air. As is well known, the flame of hydrogen possesses but little luminosity, and shows no lines or bands in the visible part of the spectrum, except that due to sodium as an impurity.

Professor Stokes, in his paper "On the Change of Refrangibility of Light,"* had stated that "the flame of hydrogen produces a very strong effect. The invisible rays in which it so much abounds, taken as a whole, appear to be even more refrangible than those which come from the flame of a spirit lamp." I was not, however, prepared for the strong group of lines in the ultra-violet which, after an exposure of one minute and a half, came out upon the plate.

Two or three weeks later, about the middle of January, 1880, I showed this spectrum to Professor Stokes, and we considered it probable that this remarkable group was the spectrum of water. Professor Stokes permits me to mention that, in a letter addressed to me on January 30, he speaks of "this novel and interesting result," and makes some suggestions as to the disputed question of the carbon spectrum.

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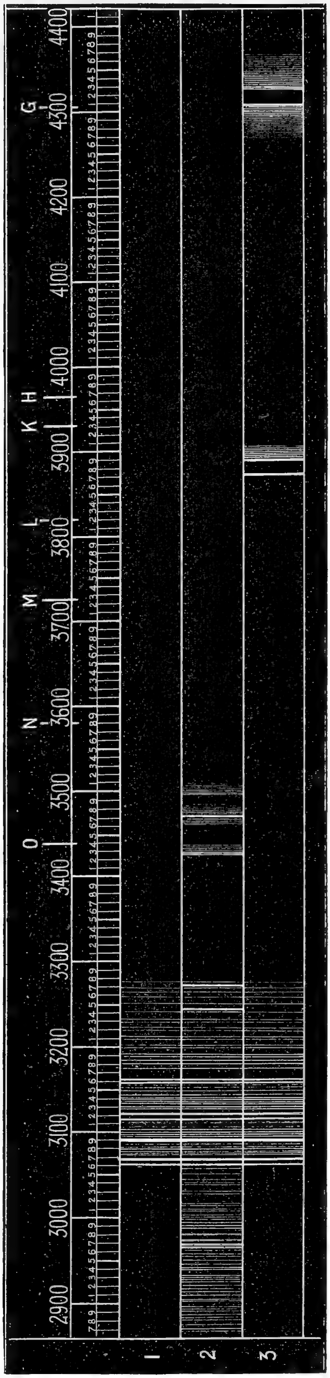
* "Phil. Trans.," 1852, p. 539.


2900
3000
3100
3200
3300
3400
3500
3600
3700
3800
3900
4000
4100
4200
4300
4400
O
N
M
L
K
H
G
1
2
3

I have since that date taken a large number of photographs of the spectra of different flames, in the hope of being able to present the results to the Royal Society, when the research was more complete. I think now that it is desirable that I should describe the spectrum of the flame of hydrogen, but I shall reserve for the present the experiments which relate to the presence of carbon and its compounds.

The spectrum of the flame of hydrogen burning in air is represented in the diagram, Spectrum No. 1. It consists of a group of lines which terminates at the more refrangible limit in a pair of strong lines, λ 3062 and λ 3068. At a short distance, in the less refrangible direction, what may perhaps be regarded as the group proper, commences with a strong line, λ 3090. Between the strong line λ 3068 and the line λ 3090 there is a line less bright, λ 3080. Less refrangible than the line λ 3090 are finer lines at about equal distances. The lines are then fine and near each other, and appear to be arranged in very close pairs. There is a pair of fine, but very distinct lines, λ 3171 and λ 3167. In this photograph the group can be traced to about λ 3290. This group constitutes the whole spectrum, which is due probably to the vapour of water.

I then introduced oxygen into the flame, leaving a small excess of hydrogen. A spectrum in all respects similar came out upon the plate. I repeated the experiment, taking both spectra on the same plate. Through one-half of the slit the spectrum of the oxyhydrogen flame was taken. This flame was about 7 inches long, and the spectrum taken of a part of the flame 2 inches from the jet. The oxygen was then turned off, and the quantity of hydrogen allowed to remain unaltered. A second spectrum with an exposure of the same duration was then taken through the second half of the slit. On the plate the two spectra are in every respect similar, and have so exactly the same intensity, that they appear as one broad spectrum.

In all these experiments a platinum jet which had been carefully cleaned was used.

In these experiments the two gases met within the blowpipe and issued in a mixed state.

The jet was removed, and a flame of hydrogen was surrounded with oxygen. The spectrum, No. 2 in the diagram, shows some additional lines. In this case the jet was brass, and in this or some other way impurities may have been introduced; and I should, at present, incline to the view that the additional lines about λ 3429 and λ 3473, and the groups more refrangible than λ 3062, do not belong to the water spectrum, but to impurities.

Coal-gas was substituted for hydrogen in the oxyhydrogen blowpipe, and oxygen admitted in as large a proportion as possible. The inner blue flame rising about 2 inches above the jet showed in the visible part of the spectrum the usual "five-fingered spectrum." The light from this part of the flame was projected upon the slit. The spectrum, No. 3 in the diagram, contains the water group already described, and in addition a very strong line close to G, and two lines, λ 3872 and λ 3890; this latter line is seen to be the more refrangible limit of a group of fine lines shading off towards K.

The ultra-violet group when carefully compared with the group in the spectrum of pure hydrogen, shows several small differences. I am inclined to believe that there is the superposition of a second fainter group. There is strong evidence of this in some spectra of hydrogen taken under other conditions. There is also a broad band less refrangible than the strong line at G, and the light extends from this line on its more refrangible side.

A double Bunsen burner (Fletcher's form) with a strong blast of air was then fitted up. The spectrum was taken of the intense blue flame. It resembles the one last described. All the distinctive features are intensified, and a continuous spectrum and groupings of very fine lines fill up all the intervals between the groups already described, so that there is an unbroken strong spectrum throughout the whole region which falls upon the plate.

A spirit lamp was arranged before the slit. The spectrum is essentially the same as No. 3, but as it is less intense only the strongest lines are seen. The water group, the strong line at G, and the pair of lines rather more refrangible than K, are seen. Probably with a longer exposure the finer lines would also show themselves.

The distinctive features of spectrum No. 3 appear to be connected with the presence of carbon.

Table of Wave-lengths of the Principal Lines of the Spectrum of Water. No. 1.

3062
3068
3073
3074
3077·5
3080
3082
3085
3090
3094
3095
3099
3102
3105
3111
3117
3122·5
3127
3130
3133
3135
3139
3142·5
3145
3149·5
3152·5
3156
3159·5
3163
3167
3171
3175
3180
3184
3189
3192·5
3198
3201
3207·5
3211
3217·5
3223
3228
3232
3242·5
3252·5
3256
3262
3266
3276

Wave-lengths of other Lines in Spectrum. No. 2.

2869·5
2872·5
2876
2880
2883
2887·5
2892
2895
2897
2904
2907·5
2910
2913
2917·5
2922·5
2925·5
2929
2932·5
2935·5
2940
2943
2947
2951
2955
2959
2966
2967·5
2970·5
2975·5
2981
2989
2991
2994
2999
3002
3005
3010
3013
3017
3019·5
3029
3031
3039
3042
3046
3051
3057·5
3246
3271
3429·5
3473

Wave-lengths of other Lines in Spectrum. No. 3.

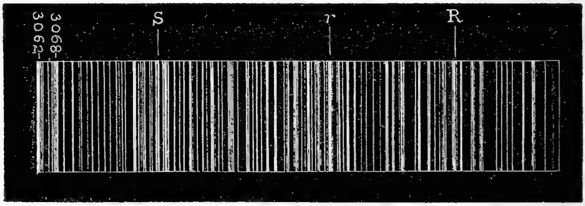
3872
3890
4310

XV. “On the Spectrum of Water.”

By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received June 17, 1880.

In our last communication to the Society, “On the Spectrum of the Compounds of Carbon with Hydrogen and Nitrogen, No. II,” we noticed that a remarkable series of lines, extending over the region between the lines S and R of the solar spectrum, were developed in the flame of coal-gas burning in oxygen. The arrangement of lines and bands, of which this spectrum consists, is shown in the accompanying diagram,* fig. 1. It begins at the more refrangible end with two strong bands, with wave-lengths about 3062, 3068, and extends up to about the wave-length 3210. It is well developed in the flame of hydro-

Fig. 1.



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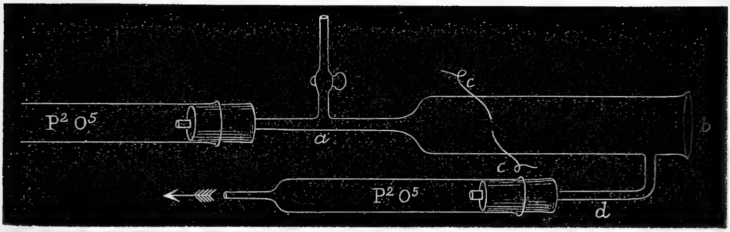
* This diagram gives tolerably well the general character of the spectrum, but the artist has not in all cases correctly rendered the relative strength of the lines. Those of the less refrangible half are almost too strongly rendered.

gen as well as of hydrocarbons, burning in oxygen, and less strongly in the flames of non-hydrogenous gases, such as carbonic oxide and cyanogen, if burnt in moist oxygen. Special pains were taken to purify the gases from sulphur. The same spectrum is given by the electric spark taken, without condenser, in moist hydrogen, oxygen, nitrogen, and carbonic acid gas, but it disappears if the gas and apparatus be thoroughly dried. We are led to the conclusion that the spectrum is that of water. The accompanying diagram, fig. 2, shows the apparatus used for comparing the spectra of the spark in the gases, dry and moist. The tube in which the sparks were passed was closed at the end *b* by a quartz lens cemented to the glass; *c c* were platinum wires sealed into the glass. The gas was introduced through the narrow tube *a*, provided with a branch closed by a glass stopcock. The gas was passed from the gas-holder through two long drying-tubes, of which the first was filled with calcium chloride, and the second with phosphoric anhydride. The joints were all made with dry corks, in order to avoid the hydrocarbons which contaminate gases passed through rubber tubes. The exit-tube *d* was armed with a tube of phosphoric anhydride, to prevent moisture entering that way. It was necessary to pass a current of dry gas for fully an hour through the warmed apparatus before the moisture was sufficiently expelled. When this was done, photographs of the spark showed either no trace, or only the faintest traces, of the spectrum above described. On now introducing a drop of water through stopcock in the branch at *a*, and letting it spread over a plug of asbestos placed in the tube at *a*, the current of gas of course passed into the sparking tube moist instead of dry; and on now passing the spark, the spectrum above described at once imprinted itself on the photographic plate. The effect was the same, whether the gas used were hydrogen, oxygen, nitrogen, or carbonic acid. In the case of nitrogen, some of the channelled bands due to that gas overlap the water spectrum, and partly obscure it, but not so much but that it can be still very distinctly recognised. When a condenser is used, the water spectrum disappears. The same spectrum

FIG. 2.



seems to appear in the De Meritens arc, but is much less fully developed. The spectrum we have figured does not by any means exhaust the ultra-violet spectra of the flames we have observed, but it is as much as we have been able at present to observe closely and to trace to water as its cause. In writing of this and other spectra which we have traced to be due to compounds, we abstain from speculating upon the particular molecular condition or stage of combination or decomposition, which may give rise to such spectra. The interest of finding an ultra-violet spectrum of water in a flame at all seemed to justify our laying before the Society so much as we have already determined. It opens up questions as to the emissive power for radiation of short wave-length of gaseous substances at the comparatively low temperatures of flames, with regard to which we are accumulating facts, which we propose to discuss in a future communication. It is obvious, however, that this opens up a very large and extensive field of inquiry which will necessitate great precautions in the conduct of the experiments; for instance, we are not prepared to guarantee that oxides of nitrogen from traces of air may not have something to do with some parts of the spectrum here described. Such facts as we have placed before the Society will completely modify the inferences which have been drawn as to the assumed continuity of flame spectra even under compression, and as to the character of the specific absorption of the vapour of water.

The Society adjourned over the Long Vacation to Thursday, November 18th.

Presents, June 10, 1880.

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John Allan Broun was born on 21st September, 1817, at Dumfries, where his father held a school intended for young men about to enter the Navy.

On the death of his father, Mr. Broun, who was then about twenty years of age, became a student at the University of Edinburgh, where he speedily distinguished himself. Here his attachment towards physical science more especially developed itself and procured for him the friendship of the late James D. Forbes, who was then Professor of Natural Philosophy at Edinburgh.

About this time (1842) the scientific world became awake to the importance of cultivating terrestrial magnetism as a branch of cosmical inquiry, and Sir Thomas MacDougal Brisbane in a very generous manner agreed to establish and maintain a highly equipped observatory at his house at Makerstoun, Dumfriesshire.

Mr. Broun was appointed director of this observatory after a course of preliminary training spent at the Royal Observatory, Greenwich, from which he derived much information.

He continued for eight years to be director of this establishment, and in addition to his other duties he discussed his observations at great length in a series of volumes which were published in the "Transactions of the Royal Society of Edinburgh," of which body Sir Thomas Brisbane was the President.

The production of these volumes must have cost Mr. Broun a great amount of time and thought, inasmuch as while published at an early stage of an important inquiry, they were conspicuous for the skill employed in the development of new methods.

John Allan Broun had now in virtue of what he had done acquired a high reputation as one of the leaders in the science of terrestrial magnetism, and was regarded as one peculiarly well adapted to fill a prominent scientific position whether at home or abroad. About this time two Professorships in Scotland became vacant, for either of which he would have been a very desirable candidate; but he had conscientious scruples which prevented him from subscribing to certain formulæ which were then insisted on from the holders of all Scotch Chairs.

In 1850 Broun left Makerstoun and went to Paris, where he formed the acquaintance of the leading men of science of that capital.

There also he met with the lady who became his wife, Isaline Vallouy, the daughter of a clergyman in the Canton du Vaud, and belonging to an old Protestant family of Dauphiné (du Val Louise), who had fled from France at the Revolution. This lady and a family of three sons and two daughters are now left to mourn his loss.

In 1851, through the influence of Colonel Sykes, Broun was appointed Director of the Trevandrum Observatory, an institution supported by His Highness the Rajah of Travancore, and he went to

India in the same year. His work in India was attended with many difficulties, but it was of very great scientific value.

Among other things he wished to obtain observations at different heights, and the following short statement will exhibit in his own words the nature of the difficulties he met with in carrying out this project, all of which were successfully overcome.

"It had appeared to me necessary," he says, in the preface to his observations, "for a complete examination of the questions to be solved, that observations should not be limited to a single station, but that the standard observatory should serve not merely for the determination of the laws and physical constants at one point, but also for the comparison and co-ordination of the laws depending on differences of height, of latitude, and of longitude.

"With reference to questions connected with difference of height, the Agustia Malley, about twenty-two miles E.N.E. of Trevandrum, the highest mountain in the narrow chain of the Ghats, rising rapidly from the plains of Coromandel and Malabar, possessed the great advantage of being free from the effects of neighbouring high tablelands. The chief objection to this locality lay in its position, without roads, a day's journey from the nearest cultivated grounds, surrounded by forests inhabited by elephants and tigers. The construction of an observatory on this nearly inaccessible rocky peak presented considerable difficulties. It was doubted, also, whether native observers, accustomed only to the climate of the plains, could live, or could be induced to try to do so, on a mountain top, which for months of each year remains buried in cloud.

"These and many other difficulties were, however, vanquished, and the observatory, built of wood in the forests about 2,300 feet below the summit, was taken to pieces, carried to the top, and rebuilt there in the first month of 1855."

Broun was not likely to spare himself, even under such a climate as that of India, and a deafness, which never left him, was caught in one of his excursions on the hills for the purpose of observing. He came back to Europe in 1866, in the hope that medical treatment might remove his deafness. In this he was disappointed; he then returned to India for three years more.

On his final return from India he resided, for a time, at Lausanne, and afterwards at Stuttgart, where his whole time was devoted to the preparation of the first volume of the results of the Trevandrum observations. Of this volume, it is sufficient to say that it is no less admirable than those in which the Makerstoun observations were discussed.

The last six years of Broun's life were spent for the most part in London, where he gave his whole time and energy to the prosecution of scientific research. Upwards of two years ago his health began to give way, and during a stay at Lynton (Devonshire) he had, in 1878, a kind of nervous attack, which was the beginning of his last illness. He died suddenly on the 22nd November, 1879.

If Broun occupied the position of a leading pioneer in observational inquiry, he also shared the discomforts of such a post. He considered no trouble too great in order to procure trustworthy observations, and he would spend an immense amount of labour upon them, when once obtained. On one occasion, with the purpose of making observations, he lived for about six weeks in a hut on the Cheviot Hills, performing his toilet in the open air, sometimes with mists around him and water nearly freezing.

It has been said of Faraday that his failures must have cost him even more thought than his successes, and the same remark will apply, with even greater force, to an original thinker in observational science like Broun. Here the testing of every idea entails a laborious investigation, and Broun, whose mind was at once very fertile and very scrupulous, must often have overworked himself before dismissing a subject from his thoughts. It seems likely that this combination of qualities eventually contributed to undermine his health.

During the latter part of his life he received from the Government Fund of the Royal Society a sum of money to enable him to improve and complete the reduction of the colonial magnetic observations. But this immediate and apparent responsibility was peculiarly trying to a man like Broun. The work seemed to go on growing the more he examined it, and he was never satisfied without going still more deeply into the subject than he had already gone. At length his health began to give way, and the thought that he had not been able to finish these investigations unfortunately hastened the progress of his malady.

It must not be supposed that Broun, while combining scrupulosity and intense devotion to science, was by nature a recluse. He was, on the contrary, a man of delicate social instincts and well calculated to take an active part in scientific administration. His deafness, however, prevented this, and during the latter years of his life he was best known to a small, but attached, circle of friends.

Broun was both a magnetician and a meteorologist. His Makerstoun observations have been already mentioned as being peculiarly valuable for the light they threw, at a comparatively early period, on the changes of the magnetism of the earth.

In 1861 he contributed two very valuable memoirs to the "Transactions of the Royal Society of Edinburgh." In one of these he discussed the errors and corrections of the bifilar magnetometer, including the determination of its temperature coefficient, which he showed might be found by a more correct method than that hitherto adopted. His second memoir was on the horizontal force of the earth's magnetism, for which he established the annual laws from a discussion of observations taken at various places. He likewise showed that the variations of this element from day to day are nearly the same over all the world.

For these discoveries the Keith Medal of the Royal Society of Edinburgh was awarded to him.

In his first volume of the "Results of the Trevandrum Observations" Broun has given an original and exhaustive investigation of the influence of the sun and moon upon the direction of the magnetic needle; but his researches regarding the lunar-diurnal variation of this element form perhaps the most original part of the volume. Besides having claims to be considered an independent discoverer of this variation, he has certainly increased our knowledge of its laws more than any other magnetician. He showed that near the equator its law in December was the opposite of that in June. His observation that the lunar action was reversed at sunrise, and that it was much greater during the day than during the night, whether the moon was above or below the horizon, may be noted as peculiarly likely to throw much light on the theory of the subject.

Broun's discovery of the simultaneity all the world over of the changes from day to day of the earth's horizontal force has already been mentioned. Certain of these changes he found to be due to the moon, while others had a period of nearly twenty-six days. These last Broun attributed to solar action, and in discussing the subject, in a memoir published in the "Philosophical Transactions" for 1877, he found that the greater magnetic disturbances were apparently due to actions proceeding from particular meridians of the sun. This is a subject of very great importance, and its exact meaning has yet to be discovered.

In meteorology Broun has done some very important work. He has shown the apparent simultaneity of the changes of mean barometric pressure over a great part of the globe, and here too he has discovered a period of nearly twenty-six days.

He was the first to commence those systematic observations of clouds at various altitudes that are now so common, and he has likewise pointed out certain relations between atmospheric motions and the directions of the lines of equal barometric pressure.

For his various researches one of the Royal Medals of this Society was conferred on him in 1878.

It is gratifying to be able to state that Her Majesty has been graciously pleased to mark her sense of the labours of this distinguished physicist by bestowing upon his widow a pension of £75 per annum.

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which will leave differences of about the same character as those given by the old formula.

The very nature of these functions precludes them from answering for any length of time, and as soon as the case becomes ripe, I shall substitute harmonic functions.

The following is an improved formula for the declination at Washington, prepared for a new edition of the paper in the 1874 Report:

$$D = +2^\circ{\cdot}19 + 2^\circ{\cdot}22 \sin(1{\cdot}4m - 6^\circ{\cdot}6),$$

it represents the observations between 1792 and 1879; $m = t - 1850{\cdot}0$.

These observations are difficult to manage on account of considerable local deflection in this vicinity produced by iron (bog-ore) which in many places crops out to the surface.

Our observations compare as follows:

In latitude 38° 53′·2 : longitude 77° 00′·4.

		Declin.	Dip.	Hor. Inten.
June 14, 15, 17.	C. A. Schott	3° 47′	70°·49	4·366
Sept. 8, 1878.	T. E. Thorpe	3 43	70·47	4·361

Yours very truly,
CHAS. A. SCHOTT.

Dr. T. E. THORPE,
Yorkshire College, Leeds, England.

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HARRISON AND SONS, 45 & 46, ST. MARTIN'S LANE, W.C.,
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Journals.

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Journal of Medicine and Dosimetric Therapeutics. Nos. 1–3. 8vo. *London* 1880. The Editor.

Cialdi (A.) Note sur les Moles à Piles et Arceaux dans les Ports à Bassin sur l'usage qu'en ont fait les Romains. 8vo. *Saint-Germain* 1879. C. W. Merrifield, F.R.S.

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The Author.

Liversidge (A.) Disease in the Sugar Cane, Queensland. 12mo. *Sydney.* And a Volume containing twenty-one Pamphlets by Professor Liversidge. The Author.

Spratt (Rear-Admiral T.), F.R.S. A Suggestion for the Improvement of the Entrance to the Mersey. 8vo. *London* 1880.
The Author.

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Direction de l'Administration Générale. Bulletin de Statistique Municipale. Oct.–Dec., 1879. 4to. *Paris*. The Direction.

Dépôt de la Marine. Publications Nos. 447*b*, 602*b*, 602*c*, 602*d*, 603–609, 615 ; and 39 Charts. Le Dépôt.

Sydney :—Intercolonial Meteorological Conference. Minutes of Proceedings. 4to. *Sydney* 1879. The Conference.

Zi-ka-wei :—Observatoire. Bulletin Mensuel. Sept.-Nov. 1879. 4to. Le Typhon du 31 Juillet, 1879. par Le P. Marc Dechevrens, S.J. 4to. *Zi-ka-wei* 1879. The Observatory.

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Carmichael (Neil), M.D. An Experimental Investigation into the Trap System, and its relation to Sewage Products. 8vo. *Glasgow* 1880. The Author.

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