PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From January 11, 1866, *to May* 23, 1867, *inclusive.*

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CONTENTS.

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ERRATA.

Page 25, last line (and throughout the same paper), *for* oxalyl *read* oxatyl.
— 27, fifteenth line from bottom, *for* alcohol hydrogen *read* alcohol radical.
— 169, fifth line from bottom, *after* dynamical theory *insert* comma.
— 169, fourth line from bottom, *after* specific heats *omit* comma.
— 171, sixth line from top, *omit* specific gravity is ·0069.
— 184, line 12, *for* $\frac{R^3 - {}^3}{3}$ *read* $\frac{R^3 - r^3}{3}$.
— 185, line 24, *for* $\phi 1 - \cos \theta)$ *read* $\phi(1 - \cos \theta)$.
— 185, line 29, *for* $(R^3 - r^3)$. *read* $(R^3 - r^3) . r$.
— 196, thirteenth line from top, *omit* and in an opposite direction of rotation.
— 199, last line, for *asy* read *asym.*
— 200, top line, for $o\chi$ read $o\gamma$.
— 200, third line from bottom, for $o'e'^2$ read $o'e^2$.
— 200, bottom line, for $o'v$ read $o'v'$.
— 222, line 3 from bottom, *for* Extraction Matters of the Urine *read* Extractive Matters of Urine.
— 324, line 13, *for* Chianenna *read* Chiavenna.
— 326, line 15 of column C, for 8112 *read* 1182.
— 330, line 17, for clean *read* clear.

NOTICE TO THE BINDER.

In this Volume the following pages are to be cancelled:—9, 225, 319, 355 and 389.

ERRATA.

Vol. XV. page 469, line 6 from bottom, the expression in brackets $\{\ \}$ should be raised to the nth power.

Vol. XV. p. 486, in equation (1) for $\frac{dz}{dy}$ read $\frac{dz}{dx}$.

The equation which occurs at the foot of page 488 is limited to the case where $a = x +$ a const.

Vol. XV. page 497, line 12 from bottom, *for* +0·00631 *read* +·000631.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

January 11, 1866.

Lieutenant-General SABINE, President, in the Chair.

The following communication was read:—

"On the Colouring and Extractive Matters of Urine.—Part I." By EDWARD SCHUNCK, F.R.S. Received June 29, 1865.

(Abstract.)

Notwithstanding the labour bestowed by many eminent men on the chemistry of urine during the last sixty years, there are portions of the subject of which we have but a very imperfect knowledge. Of all the properties of urine, none is more obvious, even to the ordinary observer, than its colour; and yet very little is known concerning the chemical nature of the substances to which its colour is due. Our ignorance in this respect may be ascribed to various causes, among which may be mentioned the extremely minute quantities of these substances occurring in the secretion, the facility with which some of them are decomposed, their chemical and physical properties (which present to our notice very little that is characteristic), and, lastly, the little interest which they possess for the chemist, notwithstanding their importance from a physiological and pathological point of view. According to the author, the colouring-matters peculiar to urine may be divided into three classes, viz.—

1st. Those which are only found occasionally in it, in consequence either of disease or of some abnormal state of the system.

2ndly. Those which are produced by spontaneous decomposition, or by the action of reagents on substances, either coloured or colourless, pre-existing in the urine.

3rdly. The colouring-matter or matters occurring in normal urine, and to which its usual colour is due.

The first class is again subdivided by the author into blue, purple or red, and black or brown colouring-matters. The appearance of a blue colouring-matter in urine has been frequently observed, both in ancient and modern times. By some it has been taken for indigo-blue, by others for prussian blue, while several chemists maintain that it consists of a peculiar substance, to which the name of *cyanourine* has been applied. The red colouring-matter is generally found in association with deposits of urate of ammonia and urate of soda, to which it communicates a pink or carmine tinge. Proust called it *rosacic acid*, while recent observers have given it other names, such as *uroerythrine* and *purpurine*. Very little is known regarding its true chemical nature. Prout, indeed, suggested that it might be identical with purpurate of ammonia; but he advanced no good grounds in support of this view, and it was proved to be erroneous by Berzelius. Instances of black urine are even of rarer occurrence than those of urine coloured blue. Indeed in many cases the black colour appears to have been due to red or purple pigments which communicated to the urine so deep a tint as to make it appear black. The *melanic acid* of Prout seems, however, to have been a peculiar substance, though closely resembling, as remarked by Berzelius, the black pulverulent substance which is formed by the action of concentrated acids on the extractive matters of urine.

The second class of urinary colouring-matters, comprising those which are formed by artificial means and therefore do not preexist in the secretion, may also be subdivided according to colour—those which have hitherto been observed being either blue, red, or brown. The author concedes to Heller the merit of having first obtained from urine by artificial means colouring-matters of a pure blue or red tint; but the true nature of these colouring-matters, as well as of the process by which they are formed, was not understood by him. Subsequent researches have proved that the *uroglaucine* and *urorhodine* of Heller are identical with the indigo-blue and indigo-red obtained from vegetables. After mentioning the experiments of Hassall, who observed the formation in morbid urine of a blue colouring-matter which he showed to be indigo-blue, the author refers to his own researches. In a paper published several years ago, he showed that urine contained as a never-failing constituent a body closely resembling if not identical with indican, the indigo-producing body of vegetables, and that hence the formation of indigo-blue and indigo-red from urine might easily be explained. This result has been confirmed by Carter and others. The formation of brown colouring-matters by the action of acids on urine was first observed by Proust, who obtained by this means a brown resinous body and a black pulverulent substance. The same or similar bodies were obtained by Scharling and Liebig, as well as the author, who gave a general account of them in the memoir just referred to. The simultaneous formation of glucose, or at least of a body having the same action on oxide of copper as glucose, is a fact first observed by the author. From an examination of the composition of the brown pulverulent substance resulting from the action of strong acids on urine, the author infers that it may be expressed by the formula $C_{14} H_7 NO_4$, which is also that of anthranilic acid, a product of decomposition of indigo-blue. All these products (the resin, the brown pulverulent substance which has received the name of *uromelanine,* and the glucose) are, in the author's opinion, derived from the extractive matter of urine, which by decomposition with acids yields these and perhaps other products. The conclusion formerly arrived at by the author, viz. "that the indigo-producing body will be found, as regards its formation and composition, to occupy a place between the substance of the tissues and the ordinary extractive matter of urine," is one which further research, as the author thinks, has only tended to confirm.

The urinary colouring-matters belonging to the third class, consisting of those to which the ordinary colour of the secretion is due, have been less frequently submitted to investigation than those which make their appearance only exceptionally or in consequence of some artificial process of decomposition. This circumstance may easily be accounted for. These so-called colouring-matters are all amorphous, and possess few characteristic properties; hence their separation from the other constituents of urine is attended with great difficulties, and has even been pronounced impossible. They are also compounds of very little stability—so much so that mere evaporation of the urine seems to produce a complete change in their composition, as is seen by the marked change of colour which takes place during the process. The opinions entertained on the subject by the earlier chemists, such as Fourcroy and Vauquelin and Proust, having been referred to, the author gives a short account of the experiments of Berzelius, Duvernoy, Lehmann, Scherer, Harley, Tichborne, and Thudichum. Berzelius and Lehmann both found the substance to which healthy urine owes its colour to be completely soluble in water. Subsequently, however, most of the attempts which were made to isolate the colouring-matter of urine ended in the separation of substances quite insoluble in water. These must in all cases have been products of decomposition; for the author considers it quite certain that the colouring-matters derived from urine which are insoluble in water are not contained as such in the secretion, provided the latter is in its normally acid state.

Having concluded his summary of the results obtained in previous researches, the author proceeds to give an account of his own experiments. Before doing so, he states that he shall apply the term "colouring-matter" to those bodies only which, occurring naturally in urine or else formed by processes of decomposition, are insoluble or not easily soluble in water, while the substances easily soluble in water to which the colour of normal urine is due, he shall continue for the present to call "extractive matters." The extractive matters being, in the author's opinion, the source whence most of the colouring-matters of urine are derived, he resolved to commence the investigation by a careful examination of their properties and composition. Indeed the first step which he thought it necessary to take, before proceeding with the investigation at all, was to ascertain whether these extractive matters are bodies of a definite chemical nature, or whether they are merely accidental mixtures of various excrementitious substances thrown out by the system, and differing in their nature according to circumstances. In order to arrive at a positive conclusion on this point, several series of experiments were undertaken. The method devised for the purpose of separating the extractive matters from the other constituents of urine, and obtaining them in a state of purity, presents few features of novelty as compared with those previously employed. The experiments necessarily occupied a considerable time, since the author considered it essential, in order to avoid decomposition, to evaporate all the solutions at the ordinary temperature by means of a current of air. The certainty of the conclusions arrived at afforded, however, ample compensation for the loss of time and additional labour thus occasioned. The composition of the extractive matters was determined by analyzing their lead compounds, since the substances themselves cannot be obtained in a state fit for analysis.

From the experiments described in this part of his paper the author thinks he is justified in drawing the following conclusions:—

1. Human urine contains at least two peculiar and distinct extractive matters, one of which is soluble in alcohol and ether, while the other is soluble in alcohol, but insoluble in ether. The existence of an extractive matter insoluble both in alcohol and in ether is extremely doubtful.

2. The composition of these extractive matters varies slightly, without any corresponding difference in their appearance and properties being perceptible; but these variations are not due to any difference in the quality of the urine or the source whence it was derived, but rather to the decomposition which takes place during the process employed in their preparation, and which cannot be entirely avoided.

3. When quite pure, the extractive matter soluble in alcohol and ether has a composition corresponding with the formula $C_{86} H_{51} NO_{52}$, while that of the extractive matter soluble in alcohol but insoluble in ether is expressed by the formula $C_{38} H_{27} NO_{28}$.

January 18, 1866.

Lieutenant-General SABINE, President, in the Chair.

The President stated that Dr. William Bird Herapath, who by reason of non-payment of his annual contribution ceased to be a Fellow of the Society at the last Anniversary, had applied for readmission. The Statute relating to the case was read, and, in accordance therewith, notice was given that the question of Dr. Herapath's readmission would be put to the vote at the next meeting.

The following communications were read:—

I. "Sixth Memoir on Radiation and Absorption." By Prof. J. Tyndall, F.R.S. Received December 21, 1865.

(Abstract.)

In this paper the author considers the deportment of certain additional elementary bodies towards Radiant Heat. He exposes powders and liquids of the same physical character, but differing from each other chemically, at a focus of dark rays, and describes the different effects produced. He examines and explains the experiments of Franklin on the absorption of solar heat. He then determines the radiative power of a great number of substances in the state of fine powder, and finds, contrary to the current belief, that in this state also chemical constitution exercises a paramount influence. The results obtained by previous experimenters in connexion with this subject are illustrated and explained. The reciprocity of radiation and absorption on the part of fine powders is also illustrated. It is moreover shown that the heat emitted from different sources, at a temperature of 100° C., varies in quality, this being proved by its unequal transmission through plates of rock-salt of perfect purity. The absorption by such plates varies from 4 to 30 per cent. of the incident radiation.

II. "On the Spectrum of Comet 1, 1866." By William Huggins, F.R.S. Received January 11, 1866.

The successful application of prismatic analysis to the light of the nebulæ showed the great importance of subjecting the light of comets to a similar examination, especially as we possess no certain knowledge of the intimate nature of those singular and enigmatical bodies, or of the cosmical relations which they sustain to our system. The importance of a prismatic analysis of cometary light is enhanced by the consideration of the general resemblance which some of the nebulæ present to the nearly round vaporous masses of which some comets, in some positions at least in their orbits, appear to consist,—a resemblance which suggests the possible existence of a close relation between nebulous and cometary matter.

I made several unsuccessful attempts to obtain a prismatic observation of Comet 1, 1864. The position of the comet and the weather were unfavourable. M. Donati succeeded in making an examination of the spectrum of this comet. "It resembles," says M. Donati, "the spectra of the metals; in fact the dark portions are broader than those which are more luminous, and we may say these spectra are composed of three bright lines"*.

Yesterday evening, January 9, 1866, I observed the spectrum of Comet

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* Monthly Notices, Royal Astronomical Society, vol. xxv. p. 114.

1, 1866. The telescope and spectrum-apparatus which I employed are described in my paper "On the Spectra of some of the Nebulæ" *.

The appearance of this comet in the telescope was that of an oval nebulous mass surrounding a very minute and not very bright nucleus. The length of the slit of the spectrum-apparatus was greater than the diameter of the telescopic image of the comet.

The appearance presented in the instrument when the centre of the comet was brought nearly upon the middle of the slit, was that of a broad continuous spectrum fading away gradually at both edges. These fainter parts of the spectrum corresponded to the more diffused marginal portions of the comet. Nearly in the middle of this broad and faint spectrum, and in a position in the spectrum about midway between *b* and F of the solar spectrum, a bright point was seen. The absence of breadth of this bright point in a direction at right angles to that of the dispersion showed that this monochromatic light was emitted from an object possessing no sensible magnitude in the telescope.

This observation gives to us the information that the light of the coma of this comet is different from that of the minute nucleus. The nucleus is self-luminous, and the matter of which it consists is in the state of ignited gas. As we cannot suppose the coma to consist of incandescent solid matter, the continuous spectrum of its light probably indicates that it shines by reflected solar light.

Since the spectrum of the light of the coma is unlike that which characterizes the light emitted by the nucleus, it is evident that the nucleus is not the source of the light by which the coma is rendered visible to us. It does not seem probable that matter in the state of extreme tenuity and diffusion in which we know the material of the comæ and tails of comets to be, could retain the degree of heat necessary for the incandescence of solid or liquid matter within them. We must conclude, therefore, that the coma of this comet reflects light received from without; and the only available foreign source of light is the sun†. If a very bright comet were to visit our system, it might be possible to observe whether the spectra of the coma and the tail contain the dark lines which distinguish solar light. If the continuous spectrum of the coma of Comet 1, 1866, be interpreted to indicate that it shines by reflecting solar light, then the prism gives no information of the state of the matter which forms the coma, whether it be solid, liquid, or gaseous. Terrestrial phenomena would suggest that the parts of a comet which are bright by reflecting the sun's light, are probably in the condition of fog or cloud.

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* Phil. Trans. 1861, p. 421.

† This conclusion is in accordance with the results of observations on the polarization of the light of the tails of some comets. Some of these observations appear to have been made with the necessary care. See J. P. Bond's "Account of the Great Comet of 1858," Annals of the Astronomical Observatory of Harvard College, vol. iii. pp. 305–310.

We know, from observation, that the comæ and tails of comets are formed from the matter contained in the nucleus.

The usual order of the phenomena which attend the formation of a tail appears to be that, as the comet approaches the sun, material is thrown off, at intervals, from the nucleus in the direction towards the sun. This material is not at once driven into the tail, but usually forms in front of the nucleus a dense luminous cloud, into which for a time the bright matter of the nucleus continues to stream. In this way a succession of envelopes may be formed, the material of which afterwards is dissipated in a direction opposite to the sun, and forms the tail. Between these envelopes dark spaces are usually seen.

If the matter of the nucleus is capable of forming by condensation a cloud-like mass, there must be an intermediate state in which the matter ceases to be self-luminous, but yet retains its gaseous state, and reflects but little light. Such a non-luminous and transparent condition of the cometary matter may possibly be represented by some at least of the dark spaces which, in some comets, separate the cloud-like envelopes from the nucleus and from each other.

Several of the nebulæ which I have examined give a spectrum of one line only, corresponding in refrangibility with the bright line of the nucleus of the comet referred to in this paper. Other nebulæ give one and two fainter lines besides this bright line. Whether either or both of these were also present in the spectrum of this comet I was unable to determine. The light of the comet was feeble, and the presence of the continuous spectrum made the detection of these lines more difficult. I suspected the existence of the brighter of these lines. I employed different eyepieces, and also gave breadth to the bright point by the use of the cylindrical lens, but I was not able to obtain satisfactory evidence of more lines than the bright one already described.

In my paper "On the Spectra of the Nebulæ," I showed that this bright line corresponds in refrangibility with the brightest of the lines of nitrogen. This line may perhaps be interpreted as an indication that cometary matter consists chiefly of nitrogen, or of a more elementary substance existing in nitrogen.

The great varieties of structure which may exist among comets, as well as the remarkable changes which the same comet undergoes at different epochs, will cause all those who are interested in the advance of our knowledge of the cosmical relations of these bodies, and of the gaseous nebulæ, to wait with some impatience the visit of a comet of sufficient splendour to permit a satisfactory prismatic examination of the physical state of cometary matter during the various changes which are dependent upon the perihelion passage of the comet.

January 25, 1866.

Lieutenant-General SABINE, President, in the Chair.

In accordance with the announcement made from the Chair at the last Meeting, the President read letters from Dr. William Bird Herapath, explaining the reason for non-payment of his annual contribution; and the question of his readmission was put to the vote, and was decided in the affirmative. The President accordingly declared that Dr. Herapath was readmitted into the Society.

The following communication was read:—

"Note on the Secular Change of Magnetic Dip, as recorded at the Kew Observatory." By BALFOUR STEWART, M.A., LL.D., F.R.S., Superintendent of the Observatory. Received January 10, 1866.

The President of this Society has already called the attention of the Fellows to the annual values of the magnetic inclination at Toronto, as deduced from the monthly determinations. In doing so he remarked that "the general effect of the disturbances of the inclination at Toronto is to increase what would otherwise be the amount of that element; therefore, if the disturbances have a decennial period, the absolute values of the inclination (if observed with sufficient delicacy) ought to show in their annual means a corresponding decennial variation, of which the minimum should coincide with the year of minimum disturbance, and the maximum with the year of maximum disturbance." At Toronto, where the true secular change is very small, the effect of this superimposed variation is very visible, so that the yearly values of the inclination appear to increase up to the period of maximum disturbance and to decrease after it. At Kew the general effect of disturbances is probably the same as at Toronto—that is to say, tending to increase the inclination; but the secular change being considerable, and tending to decrease the inclination, the joint effect of the secular change and the superposed variation might be expected to appear in a diminution of the yearly secular change for those years during which the disturbances are increasing from their minimum to their maximum value, and in an increase of the yearly secular change for those years during which the disturbances are decreasing from their maximum to their minimum.

The Kew records appear to exhibit a variation of this nature. Observations of dip were commenced at the Kew Observatory in 1854; and by comparing a good number of observations taken during the latter months of 1854, with two circles and four needles, with observations taken with the same circles and needles during the same months of 1855, we obtain a yearly secular change of $2'\cdot24$.

During the years from 1856 to 1859 inclusive, monthly observations were made with a circle known as the Kew circle, two needles being always used, and the mean of the two results taken as the true value of the dip.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
	° ′	′
1856.	68 27·67	
1857.	24·36	3·31
1858.	22·80	1·56
1859.	20·73	2·07

If we take the mean of these three values of yearly secular change, and also include that between 1854 and 1855, we have a mean value of yearly secular change, for the period between 1854 and 1859, amounting to 2′·29, and this value will not be sensibly altered if we omit the observations between 1854 and 1855.

In 1859 it was resolved to substitute another circle for the Kew circle, as the action of the latter was not considered to be quite satisfactory; and accordingly since this date Barrow's circle No. 33 has been employed, and monthly observations have been made with it, generally in the afternoon—two needles being used, as before.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
	° ′	′
1860.	68 20·21	
1861.	18·21	2·00
1862.	15·58	2·63
1863.	12·66	2·92
1864.	9·88	2·78

exhibiting between 1860 and 1864 a mean secular change of 2′·58.

It will be noticed from this, that the mean yearly secular change of dip at Kew appears to be greater from 1860 to 1864, a period of increasing disturbances, than from 1854 to 1859, a period of decreasing disturbances. Possibly the yearly decrement of dip has again begun to diminish, since the change from 1864 to 1865 is only 1′·32. It is, however, premature to assert that this is the case, and it can only be decided by continuing the monthly observations. At all events the Kew observations agree with those at Toronto in indicating that the yearly change of dip contains the combined result of two things—namely, the true secular change and the change due to disturbance; and this ought to be borne in mind by future observers of this magnetic element.

February 1, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Specific Gravity of Mercury." By BALFOUR STEWART, M.A., LL.D., F.R.S., Superintendent of the Kew Observatory. Received January 25, 1866.

Some time since, in connexion with a research on the fusing-point of mercury, several observations were made at Kew of the specific gravity of this fluid.

A specific-gravity bottle was used for this purpose and it was washed, in the first place with sulphuric acid, secondly with distilled water, and thirdly with alcohol; when this was done it was found to contain mercury without any air-specks or any diminution of that metallic lustre which pure mercury exhibits when in contact with a vessel of clean glass. Three different specimens of pure mercury were used and were separately weighed in the specific-gravity bottle at 62° Fahr. The following results were obtained:—

	Weighed in air. grs.
Mercury from the cistern of the old Kew standard barometer, filling the bottle, weighed at 62° F.	13591·36
Mercury from the cistern of the new Kew standard barometer weighed at 62° F.	13591·66
Mercury used in experiments with air-thermometer weighed at 62° F.	13591·96

the mean of these will be 13591·66 grs.

It was found that the specific-gravity bottle had an internal volume equal very nearly to 4 cubic inches, and assuming that a cubic inch of air weighs 0·31 gr., then the air displaced by the liquid filling the bottle would weigh 1·24 gr.

In like manner the air displaced by the Kew standard weights (sp. gr. 8·2) would have the volume of 6·6 cubic inches, and would weigh 2·04 grs.

From these premises we find that the real weight of the mercury *in vacuo* would have been 13590·86 grs.

Again, the amount of water which the same bottle held at 62° F. weighed in air 1000·53 grs.

Here the air displaced by the bottle is, as before, 1·24 grs., while that displaced by the weights is only 0·15 gr.

From this we find that the real weight of water filling the bottle at 62° F. would be *in vacuo* 1001·62 grs. We have thus—

True weight of mercury filling the bottle at 62° F. = 13590·86 grs.
True weight of the same volume of water at 62° F. = 1001·62 grs.

And hence the specific gravity of mercury at 62° F., as compared with water at the same temperature, will be 13·569 nearly.

Again, if we assume the correctness of Regnault's Table of the absolute dilatation of mercury, and also that of Despretz's Table of the absolute dilatation of water, we shall find that the weight at 32° F. of a volume of mercury weighing 13590·86 grs. at 62° F. will be

$$13590{\cdot}86 \times 1{\cdot}00298 = 13631{\cdot}361 \text{ grs.}$$

Also the volume at 4° C., or 39°·2 F., of a volume of water weighing at 62° F. 1001·62 grs., will be

$$1001{\cdot}62 \times 1{\cdot}0011437 = 1002{\cdot}766 \text{ grs.}$$

Hence the specific gravity of mercury, according to the French method of determining it, will be

$$\frac{13631{\cdot}361}{1002{\cdot}766} = 13{\cdot}594.$$

A determination by Regnault gives 13·596.

These two results agree very nearly with one another; and this agreement tends not only to verify the correctness of Regnault's determination, but to show that Regnault's Table of the dilatation of mercury, and Despretz's Table of the dilatation of water, agree together; a remark that had been previously made by Dr. Matthiessen in a paper which he recently presented to the Society.

II. "On the Forms of Graphitoidal Silicon and Graphitoidal Boron." By W. H. Miller, M.A., For. Sec. R.S., and Professor of Mineralogy in the University of Cambridge. Received February 1, 1866.

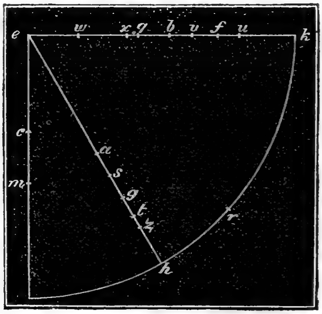
Graphitoidal Silicon.

It has been so confidently assumed that graphitoidal silicon is an allotropic condition of silicon crystallized in octahedrons, that on ascertaining by measurement of angles that some graphitoidal silicon given me by Dr. Matthiessen was in simple and twin octahedrons, I at once concluded that the substance had been wrongly named. Later, however, I obtained from Dr. Percy a supply of graphitoidal silicon of unquestionable authenticity. Its lustre was that of the crystals I had previously examined. It occurred in small scales, having for the most part the appearance of crystals of the oblique system. On measurement, however, they proved to be octahedrons in which two parallel faces were much larger than any of the other faces, and two other parallel faces were either too small to be observed or were altogether wanting. One of the scales had all the faces of a twin octahedron. It appears, then, that there is no reason, founded on a difference of form, for separating graphitoidal from octahedral silicon, the sole distinction being that the crystals of the latter are more perfect than those of the former.

Graphitoidal Boron.

The forms of boron have been described by the Commendatore Quintino Sella in two papers read before the Royal Academy of Turin on the 4th of January and the 14th of June, 1857, and by the Baron Sartorius v. Waltershausen in a paper presented to the Royal Society of Göttingen on the 1st. of August of the same year. They found independently that the adamantine boron of Wöhler and Deville, containing a variable and not inconsiderable amount of aluminium and carbon, considered by Sella as possibly a definite compound of boron with aluminium and carbon with a mechanical mixture of pure boron, crystallizes in forms belonging to the pyramidal system.

Boron containing 2·4 per cent. of carbon, the boro semplice of Sella, is described by him as occurring in crystals, the faces of which are not so perfect as to admit of a very accurate determination of the angles they make with one another. The angles approximate to some of the angles of crystals of the cubic system, but the aspect of the crystals, which are usually twins, leads to the supposition that they belong to the oblique system, and that the angle between the oblique axes differs but little from 90°.



The forms observed by Sella, considered as belonging to the oblique system, are:—

$$k\ 1\,0\,0,\ e\ 0\,0\,1,\ c\ 0\,1\,3,\ m\ 0\,2\,3,\ b\ 1\,0\,1,\ n\ \bar{5}\,0\,4,\ p\ 5\,0\,8,\ q\ 2\,0\,3,\ f\ \bar{2}\,0\,1,$$
$$h\ 1\,1\,0,\ r\ 2\,1\,0,\ g\ 1\,1\,1,\ a\ \bar{1}\,1\,2,\ d\ \bar{2}\,1\,1,\ l\ 2\,1\,2.$$

Of these, I have since reobserved all, with the exception of *a, d, l*, and perhaps *p*, the corresponding reflexion being too faint to enable me to affirm the existence of that face in the crystals I examined. I have also observed the following forms in which the distribution of the faces is in most cases, probably in all, the same as in the prismatic system, or as if the oblique form $h\,k\,l$ were always accompanied by the oblique form $\bar{h}\,k\,l$:

$$u\ 3\,0\,1,\quad w\ 1\,0\,4,\quad v\ 4\,0\,3,\quad x\ 3\,0\,5,\quad s\ 2\,2\,3,\quad t\ 3\,3\,2,\quad z\ 2\,2\,1.$$

On the same supposition regarding the distribution of the faces, the annexed figure represents an octant of the sphere of projection, the poles of some of the faces not wanted for comparison with those of graphitoidal boron being omitted. The principal angles taken or computed from the angles provisionally adopted by Sella, are:—

	°	'		°	'
e c	39	14	*e k*	90	0
e m	58	31	*k m*	90	0
e w	19	28	*k h*	60	0
e x	40	19	*e a*	54	44
e q	43	21	*e s*	62	4
e b	54	44	*e g*	70	32
e v	62	4	*e t*	76	44
e f	70	32	*e z*	79	59
e u	76	44	*e h*	90	0

Besides the two forms already mentioned, Wöhler and Deville obtained boron in extremely thin scales, which were supposed to be a different modification of boron, and was accordingly called graphitoidal. Sella, however, relying apparently upon the evidence afforded by the lustre and colour of the scales, for he was unable to obtain any measurements, expresses his conviction that they are not different from pure boron. Some scales of this substance, for which, as well as a supply of crystals of pure boron, I am indebted to Dr. Matthiessen, have faces on their edges, but so narrow that the reflected image of the bright signal is diffracted into a line of considerable length, and therefore difficult to bisect. For this reason it is not possible to determine the positions of the faces with accuracy.

One of them, about 2 millims. wide and 0·014 millim. thick, of the shape of half a hexagon divided by a line at right angles to two opposite sides, exhibited faces agreeing in position very fairly, considering the unavoidable errors of observation, with two of the faces *k*, two of the faces *e*, *c*, *m*, three of the faces *b*, two of the faces *x*, *q*, three of the faces *h*, and four of the faces *a*. Another, smaller and thinner, of the shape of a hexagon, had faces coinciding with two of the faces *k*, two of the faces *e*, *c*, *m*, *f*, *v*, and four of the faces *h*. The agreement in position of so many of the faces with those of pure boron appears to leave but little doubt of the identity of the forms of the two substances.

February 8, 1866.

Lieut.-General SABINE, President, in the Chair.

The BAKERIAN LECTURE was delivered by JAMES CLERK MAXWELL, M.A., F.R.S., "On the Viscosity or Internal Friction of Air and other Gases." The following is an abstract.

All bodies which are capable of having their form indefinitely altered, and which resist the change of form with a force depending on the rate of deformation, may be called Viscous Bodies. Taking tar or treacle as an instance in which both the change of form and the resistance opposed to it are easily observed, we may pass in one direction through the series of soft solids up to the materials commonly supposed to be most unyielding, such as glass and steel, and in the other direction through the series of liquids of various degrees of mobility to the gases, of which oxygen is the most viscous, and hydrogen the least.

The viscosity of elastic solids has been investigated by M. F. Kohlrausch* and Professor W. Thomson†; that of gases by Professor Stokes‡, M. O. E. Meyer§, and Mr. Graham‖.

The author has investigated the laws of viscosity in air by causing three horizontal glass disks, 10·56 inches diameter, to perform rotatory oscillations about a vertical axis by means of the elasticity of a steel suspension wire about 4 feet long. The period of a complete oscillation was 72 seconds, and the maximum velocity of the edge of the disks was about $\frac{1}{12}$ inch per second.

The three disks were placed at known intervals on the vertical axis, and four larger fixed disks were so adjusted above and below them and in the intervals between them, that strata of air of known thickness were intercepted between the surfaces of the moving disks and the fixed disks. During the oscillations of the moveable disks, the viscosity of the air in these six strata caused a gradual diminution of the amplitude of oscillation, which was measured by means of the reflexion of a circular scale in a mirror attached to the axis.

The whole apparatus was enclosed in an air-tight case, so that the air might be exhausted or exchanged for another gas, or heated by a current of steam round the receiver. The observed diminution in the arc of oscillation is in part due to the viscosity of the suspending wire. To eliminate the effect of the wire from that of the air, the arrangement of the disks was altered, and the three disks, placed in contact, were made to oscillate midway between two fixed glass disks, at distances sometimes of 1 inch, and sometimes of ·5 inch.

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* Pogg. Ann. cxix. (1863).

† Proceedings of the Royal Society, May 18, 1865.

‡ Cambridge Philosophical Transactions, 1850.

§ Pogg. Ann. cxiii. (1861).

‖ Phil. Trans. 1846 & 1849.

From these experiments on two strata of air, combined with three sets of experiments on six strata of thicknesses ·683, ·425, and ·1847 inches respectively, the value of the coefficient of viscosity or internal friction was determined.

Let two infinite planes be separated by a stratum of air whose thickness is unity. Let one of these planes be fixed, while the other moves in its own plane with a uniform velocity unity; then, if the air in immediate contact with either plane has the same velocity as the plane, every unit of surface of either plane will experience a tangential force μ, where μ is the coëfficient of viscosity of the air between the planes.

The force μ is understood to be measured by the velocity which it would communicate in unit of time to unit of mass.

If L, M, T be the units of length, mass, and time, then the dimensions of μ are $L^{-1}M\,T^{-1}$.

In the actual experiment, the motion of the surfaces is rotatory instead of rectilinear, oscillatory instead of uniform, and the surfaces are bounded instead of infinite. These considerations introduce certain complications into the theory, which are separately considered.

The conclusions which are drawn from the experiments agree, as far as they go, with those of Mr. Graham on the Transpiration of Gases*. They are as follows:—

1. The coefficient of viscosity is independent of the density, the temperature being constant. No deviation from this law is observed between the atmospheric density and that corresponding to a pressure of half an inch of mercury.

This remarkable result was shown by the author in 1860† to be a consequence of the Dynamical Theory of Gases. It agrees with the conclusions of Mr. Graham, deduced from experiments on the transpiration of gases through capillary tubes. The considerable thickness of the strata of air in the present experiments shows that the property of air, to be equally viscous at all densities, is quite independent of any molecular action between its particles and those of solid surfaces, such as those of the capillary tubes employed by Graham.

2. The coefficient of viscosity increases with the temperature, and is proportional to $1+\alpha\theta$, where θ is the temperature and α is the coefficient of expansion per degree for air.

This result cannot be considered so well established as the former, owing to the difficulty of maintaining a high temperature constant in so large an apparatus, and measuring it without interfering with the motion. Experiments, in which the temperature ranged from 50° to 185° F., agreed with the theory to within 0·8 per cent., so that it is exceedingly probable that this is the true relation to the temperature.

The experiments of Graham led him to this conclusion also.

3. The coefficient of viscosity of hydrogen is much less than that of

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* Phil. Trans. 1846.

† Phil. Mag. Jan. 1860.

air. I have never succeeded in filling my apparatus with perfectly pure hydrogen, for air leaks into the vacuum during the admission of so large a quantity of hydrogen as is required to fill it. The ratio of the viscosity of my hydrogen to that of air was ·5156. That obtained by Graham was ·4855.

4. The ratio for carbonic acid was found to be ·859. Graham makes it ·807. It is probable that the comparative results of Graham are more exact than those of this paper, owing to the difficulty of introducing so large a volume of gas without letting in any air during the time of filling the receiver. I find also that a very small proportion of air causes a considerable increase in the viscosity of hydrogen. This result also agrees with those of Mr. Graham.

5. Forty experiments on dry air were investigated to determine whether any slipping takes place between the glass and the air in immediate contact with it.

The result was, that if there were any slipping, it is of exceedingly small amount; and that the evidence in favour of the indicated amount being real is very precarious.

The results of the hypothesis, that there is no slipping, agree decidedly better with the experiments.

6. The actual value of the coefficient of viscosity of dry air was determined, from forty experiments of five different kinds, to be

$$\mu = \cdot 0000149\ (461^\circ + \theta),$$

where the inch, the grain, and the second are the units, and the temperature is on Fahrenheit's scale.

At 62° this gives $\mu = \cdot 007802$.

Professor Stokes, from the experiments of Baily on pendulums, has found

$$\sqrt{\frac{\mu}{\rho}} = \cdot 116,$$

which, with the average temperature and density of air, would give

$$\mu = \cdot 00417,$$

a much smaller value than that here found.

If the value of μ is expressed in feet instead of inches, so as to be uniform with the British measures of magnetic and electric phenomena, as recorded at the observatories,

$$\begin{aligned} \mu &= \cdot 000179\ (461 + \theta) \\ &= \cdot 08826 \text{ at } 32^\circ. \end{aligned}$$

In metre-gramme-second measure and Centigrade temperature,

$$\mu = \cdot 01878\ (1 + \cdot 00366\ \theta).$$

M. O. E. Meyer (Pogg. Ann. cxiii. (1861) p. 383) makes μ at 18° C. $= \cdot 000360$ in centimetres, cubic centimetres of water, and seconds as units, or in metrical units, $\mu = \cdot 0360$.

According to the experiments here described, μ at 18° C. $= \cdot 02$.

M. Meyer's value is therefore nearly twice as great as that of this paper, while that of Professor Stokes is only half as great.

In M. Meyer's experiments, which were with one disk at a time in an open space of air, the influence of the air near the edge of the disk is very considerable; but M. Meyer (Crelle, 59; Pogg. cxiii. 76) seems to have arrived at the conclusion that the additional effect of the air at the edge is proportional to the thickness of the disk. If the additional force near the edge is underestimated, the resulting value of the viscosity will be in excess.

7. Each of the forty experiments on dry air was calculated from the concluded values of the viscosity of the air and of the wire, and the result compared with the observed result. In this way the error of mean square of each observation was determined, and from this the "probable error" of μ was found to be ·036 per cent. of its value. These experiments, it must be remembered, were made with five different arrangements of the disks, at pressures ranging from 0·5 inch to 30 inches, and at temperatures from 51° to 74° F.; so that their agreement does not arise from a mere repetition of the same conditions, but from an agreement between the properties of air and the theory made use of in the calculations.

February 15, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communication was read:—

"Further Observations on the Spectra of some of the Nebulæ, with a Mode of determining the Brightness of these Bodies." By WILLIAM HUGGINS, F.R.S. Received January 30, 1866.

(Abstract.)

In the first part of this paper the author continues his observations on the spectra of nebulæ and clusters. The results already presented by him to the Royal Society are confirmed by his new observations, namely, that with his apparatus clusters and nebulæ give either a continuous spectrum or a spectrum consisting of one, two, or three bright lines. The positions in the spectrum of these lines are the same as those of the bright lines of the nebulæ described in his former papers.

On account of the faintness of these objects the author was not able to ascertain whether the continuous spectra which some of the nebulæ give are interrupted by dark lines in a manner similar to the spectra of the sun and fixed stars. Some of these spectra appear irregularly bright in some parts of the spectrum.

The nebulæ which follow have a spectrum of one, two, or three bright lines; in addition to which, in the case of some of them, a faint continuous spectrum was visible. These bodies are probably gaseous in constitution.

No. 2102	27 H. IV.	No. 4499	38 H. IV.
4234	5 Σ.	4827	705 H. I.
4403	17 M.	4627	192 H. I.
4572	16 H. IV.		

The following nebulæ and clusters give a continuous spectrum:—

No. 105	18 H. V.	No. 4315	14 M.
307	151 H. I.	4357	190 H. II.
575	156 H. I.	4437	11 M.
1949	81 M.	4441	47 H. I.
1950	82 M.	4473	Auw. N. 44.
3572	51 M.	4485	56 M.
2841	43 H. V.	4586	2081 *h.*
3474	63 M.	4625	51 H. I.
3636	3 M.	4627	192 H. I.
4058	215 H. I.	4600	15 H. V.
4159	1945 *h.*	4760	207 H. II.
4230	13 M.	4815	53 H. I.
4238	12 M.	4821	233 H. II.
4244	50 H. IV.	4879	251 H. II.
4256	10 M.	4883	212 H. II.

The second part of the paper contains an account of a mode of determining approximatively the intrinsic brightness of some of the nebulæ.

Analysis by the prisms shows that some of the nebulæ consist of luminous gas existing in masses, which are probably continuous; and the nebulæ in the telescope present not points, but surfaces, in some cases, subtending a considerable angle. As long as an object remains of sensible size in the telescope it retains its original brightness, except as this may be diminished by a possible power of extinction belonging to celestial space, and by the absorptive power of the earth's atmosphere.

By means of a special apparatus the light of three nebulæ was compared with the light emitted by a sperm candle, burning at the rate of 158 grs. per hour. The results are that—

The intensity of nebula, No. 4628 1 H. IV. $=\frac{1}{1508}$th part of that of the candle.
,, ,, annular nebula in Lyra $=\frac{1}{6032}$nd ,, ,,
,, ,, Dumb-bell nebula $=\frac{1}{19604}$th ,, ,,

The estimation in each case refers to the brightest part of the nebula. The amounts are too small by the unknown corrections for the loss which the light has sustained in its passage through space and through the earth's atmosphere. These values have an importance in connexion with the gaseous nature of the source of the light, which the spectroscope indicates. Similar estimations made at considerable intervals of time might show whether the brightness of these bodies is undergoing increase, diminution, or a periodic variation.

The paper concludes with some observations on the measures of the diameters of some of the planetary nebulæ. A very careful set of measures of 4232, 5 Σ, by the Rev. W. R. Dawes, F.R.S., is given, which makes the equatorial diameter$=15''{\cdot}9$. Also measures by the author of 1414, 73 H. IV. which give its diameter in R. A.$=30''{\cdot}8$.

February 22, 1866.

J. P. GASSIOT, Esq., Vice-President, in the Chair.

The following communications were read:—

I. "Account of Experiments on the Flexural and Torsional Rigidity of a Glass Rod, leading to the Determination of the Rigidity of Glass." By JOSEPH D. EVERETT, D.C.L., Assistant to the Professor of Mathematics in the University of Glasgow. Communicated by Professor WILLIAM THOMSON, F.R.S. Received February 1, 1866.

(Abstract.)

In these experiments a cylindrical rod of glass is subjected to a bending couple of known moment, applied near its ends. The amount of bending produced in the central portion of the rod is measured by means of two mirrors, rigidly attached to the rod at distances of several diameters from each end, which form by reflexion upon a screen two images of a fine wire placed in front of a lamp-flame. The separation or approach of these two images, which takes place on applying the bending couple, serves to determine the amount of flexure.

In like manner, when a twisting couple is applied, the separation or approach of the images serves to determine the amount of torsion.

The flexural and torsional rigidities, f and t, which are thus found by experiment, lead to the determination of Young's Modulus of Elasticity, M (or the resistance to longitudinal extension), and the absolute rigidity, n (or resistance to shearing); M being equal to f divided by the moment of inertia of a circular section of the rod about a diameter, and n being equal to t divided by the moment of inertia of a circular section about the centre. The "resistance to compression," k, is then determined by the formula

$$\frac{1}{3k}=\frac{3}{M}-\frac{1}{n},$$

and the "ratio of the lateral contraction to longitudinal extension," σ, by the formula

$$\sigma=\frac{M}{2n}-1.$$

The values found for the flint-glass rod experimented on were, in grammes' weight per square centimetre,

$$M = 614,330,000,$$
$$n = 244,170,000,$$
$$k = 423,010,000,$$
$$\sigma = \cdot 258.$$

The mode of experimenting is somewhat similar to that by which Kirchhoff investigated the value of σ for steel and brass; but there are several points of difference, especially this—that the portion of the glass rod, whose flexure and torsion are measured, is sufficiently distant from the places where external forces are applied, to eliminate the local irregularities produced by their application.

II. "Note on the relative Chemical Intensities of direct Sunlight and diffuse Daylight at different altitudes of the Sun." By HENRY E. ROSCOE, F.R.S., and JOSEPH BAXENDELL, F.R.A.S. Received February 8, 1866.

The method of determining the chemical intensity of daylight described by one of us* presents a convenient means of experimentally comparing the intensity of the chemically active rays which reach the earth's horizontal surface directly from the sun with that of the same rays reflected from the atmosphere and constituting diffuse daylight. For this purpose it is only necessary alternately to expose pieces of the standard sensitive paper, according to the method described in the memoir above mentioned, to the action of the total light of day, and to the diffuse daylight alone, which is easily done by cutting off the sun's direct rays from the sensitive paper, by throwing upon the paper a shadow cast by a small screen, having an apparent diameter slightly greater than that of the solar disk. In the first case the chemical intensity of the total daylight, in the second that of the diffuse light is determined; the difference between these two observations giving the chemical intensity of the direct sunlight. As the experiments which we have already made in this direction have led us to conclusions differing altogether from those derived from theoretical considerations concerning the relative chemical intensities of direct and diffuse sunlight, we think that, although this investigation is incomplete, the results are worthy of the attention of the Society. No direct photometrical determinations of the relative intensity of sun and diffuse light have up to this time been made; but Clausius† has calculated this relation for varying altitudes of the sun, founding his calculations upon the hypothesis (generally adopted by meteorologists to explain the red tints of the morning and evening sky) that the diffused light is reflected, not from the par-

* Bakerian Lecture, 1865. Phil. Trans. 1865, p. 605.

† Poggendorff's 'Annalen,' Bd. lxxii. p. 294.

ticles of air or solid floating material, but from the minute vesicles of water which are supposed to be always contained in large quantities in the atmosphere. According to this hypothesis, Clausius obtained the following numbers as expressing the intensities of direct sunlight and diffused daylight for altitudes varying from 20° to 60°:—

	Calculated Intensities of		
Sun's Altitudes.	Total Daylight.	Diffuse Light.	Direct Sunlight.
20°	0·10049	0·06736	0·03313
25°	0·17808	0·09291	0·08517
30°	0·25933	0·11184	0·14749
35°	0·34049	0·12654	0·21395
40°	0·41957	0·13832	0·28125
50°	0·56686	0·15599	0·41087
60°	0·69442	0·16822	0·52620

(The intensity of sunlight at an altitude of 90°, unweakened by atmospheric absorption, is taken as the unit.)

The measurement of the relative chemical intensities were made at three localities: (1) Owens College, Manchester, 53° 29′ N., and $0^h\ 9^m\ 0^s$ W.; (2) the Observatory, Cheetham Hill, near Manchester; and (3) the summit of the Königstuhl, near Heidelberg, 1900 feet above the sea, in 49° 24′ N., and $34^m\ 48''$ E. We are indebted for the latter observations to Dr. Wolkoff, who kindly forwarded us his results through Professor Bunsen.

The following experimental numbers, obtained at Owens College, may serve to illustrate the method adopted; in most cases several (four or five) observations of the intensities of the total and diffuse light were made quickly one after the other, and the mean of all the readings taken.

TABLE I.—Observations at Owens College, Manchester,
53° 29′ N. $0^h\ 9^m\ 0^s$ W.

Date.	Greenwich Mean Time of Observation.	Sun's Hour Angle.	Sun's Altitude.	Intensity of total Daylight.	Number of Observations.	Intensity of diffused Light.	Number of Observations.	Intensity of direct Sunlight.
1865.	h m	° ′	° ′					
Oct. 6.	12 0	0 44 W.	31 17	·073	3	·068	4	·005
7.	9 30	36 42 E.	23 23	·060	1	·056	1	·004
	12 0	0 48 W.	30 54	·063	1	·057	1	·006
18.	11 25	7 18 E.	26 30	·075	2	·056	2	·001
	11 45	2 17 E.	26 46	·111	2	·089	2	·022
	12 30	8 58 W.	26 20	·088	4	·087	4	·001
	1 19	21 13 W.	24 15	·071	4	·067	5	·004
	2 45	42 43 W.	17 8	·062	2	·053	2	·009
24.	0 45	12 51 W.	23 42	·139	3	·113	5	·026
	1 20	21 41 W.	22 4	·123	5	·115	4	·008
Nov. 15	12 0	1 33 W.	17 55	·101	5	·082	4	·019
	12 40	11 33 W.	17 15	·065	4	·063	5	·002
	1 15	20 18 W.	15 50	·063	4	·058	5	·005
21.	12 10	3 43 W.	16 27	·056	5	·055	4	·001
	12 30	8 43 W.	16 8	·066	4	·058	5	·008
	12 45	12 28 W.	15 44	·058	4	·050	5	·008

As the altitudes here observed vary only from 15° 44′ to 31° 47′, we thought it best to collect the results into two groups, containing the eight highest and eight lowest observed altitudes.

TABLE II.—Results of Observations at Owens College.

	Number of Observations.		Mean Altitude. of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	33	34	17° 8′	·066	·007	0·106
2	20	24	26 38	·074	·008	0·108

The determinations made at Cheetham Hill (53° 30′ 50″ N., and $0^h\ 8^m\ 56^s$ W.) were sixty-three in number, in which the altitude varies from 16° 8′ to 46° 14′, and these are divided into three groups, as follows:—

TABLE III.—Results of Observations at Cheetham Hill.

	Number of Observations.		Mean Altitude of Sun.	Intensity of Sky or diffused Daylight	Intensity of direct Sunlight.	Ratio of Sun to Sky.
	Sky.	Sun.				
Group 1	23	24	19° 30′	·064	·012	0·187
2	22	22	25 31	·091	·019	0·208
3	18	17	34 8	·104	·026	0·250

The range of altitude in the Heidelberg experiments being a much wider one (viz. from 0° to 63° 49′), we have been able to arrange these (containing ninety-nine observations) in five groups, as follows:—

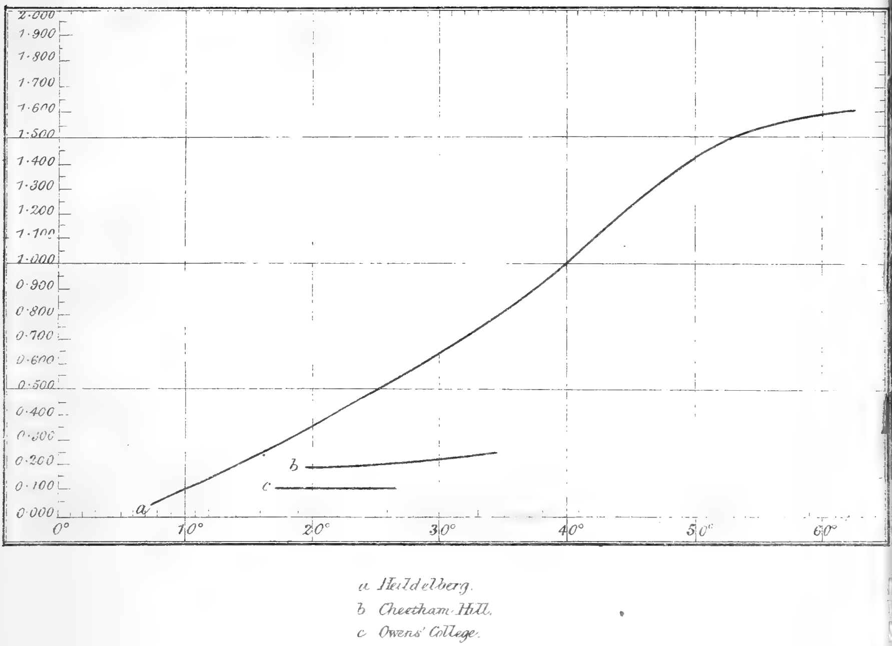
TABLE IV.—Results of Observations at Heidelberg.

	Number of Observations.	Range of Altitude of of Sun.	Mean Altitude of Sun.	Intensity of Sky or diffused Daylight.	Intensity of direct Sunlight.	Ratio of Sun to Sky.
Group 1	10	0° to 15°	7° 15′	·048	·002	0·041
2	19	15 — 30	24 43	·134	·066	0·472
3	31	30 — 45	34 34	·170	·136	0·800
4	22	45 — 60	53 37	·174	·263	1·511
5	17	above 60	62 30	·199	·319	1·603

The curves on Pl. I. fig. 1 are derived from the foregoing numbers, the ordinates representing the intensities and the abscissæ denoting the corresponding altitudes. The curves marked *a*, *b*, *c* give respectively the observations at Heidelberg, Cheetham Hill, and Owens College; the dotted curves represent the intensities of the diffuse light, the black curves those of the direct sunlight. The ratio of the sun and skylight for the same places is represented by the curves *a*, *b*, and *c*, fig. 2.

In the following Table the experimental ratios are compared with those calculated by Clausius.

RATIO OF DIRECT TO DIFFUSED LIGHT AT DIFFERENT ALTITUDES OF THE SUN.



CHEMICAL INTENSITY OF DIFFUSED AND DIRECT SUN-LIGHT AT DIFFERENT ALTITUDES OF THE SUN.

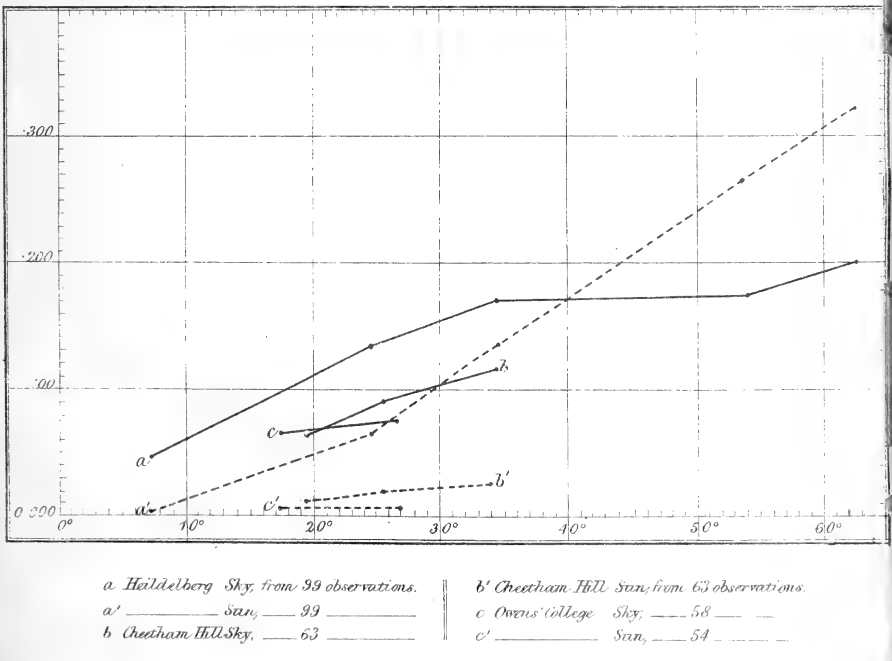


Table V.—Ratio of Chemical Intensities of direct Sunlight to diffuse Light.

Sun's Altitude.	Calculated (Clausius).	Experiments.		
		Heidelberg.	Cheetham Hill.	Owens College.
20°	0·491	0·350	0·19	0·10
25	0·896	0·480	0·20	0·11
30	1·320	0·650	0·23	
35	1·690	0·820	0·26	
40	2·032	1·00	—	
50	2·634	1·37	—	
60	3·129	1·60	—	

These numbers show that whilst at 20° of altitude, according to theory, the relation of the intensities of diffuse light to direct sunlight is as 100 to 49·1, the experiments at Heidelberg give a relation of 100 to 35; those at Cheetham Hill 100 to 19, and those in Manchester 100 to 10. If we compare the theoretical ratio for higher altitudes, we find that in our latitudes the ratio even at 35° of altitude is only as 100 for diffuse light to 26 for sunlight, whereas theory gives the relation as 100 to 169. The Heidelberg observations show indeed a more rapid rise in the intensity of the direct sun's rays, the ratio reaching 100 to 82 for 35° of altitude. The great difference between these and the other experimental results must doubtless be ascribed to the considerable elevation (1900 feet above the sea) at which these observations were made.

Even at Heidelberg, however, no less than eight observations show that at low elevations the chemical action of the sun becomes altogether inappreciable, whilst that of the diffuse light is still considerable; and the same inactive condition of direct sunlight at low altitudes has been frequently observed both at Owens College and Cheetham Hill. In these cases the intensity of the sun's direct visual rays was considerable, and a strong shadow was cast; but the more highly refrangible rays were altogether absent, and the ratio became infinite.

Heidelberg Observations.

Altitude.	Direct Sun.	Diffuse Light.
0° 34′	0·000	0·026
1 32	0·000	0·024
2 29	0·000	0·038
3 27	0·000	0·028
6 0	0·000	0·030
10 40	0·000	0·083
11 51	0·000	0·079
12 58	0·000	0·080

In some of the experiments made at Cheetham Hill the shadow of a small disk was thrown on a horizontal surface of white paper, and careful estimations made of the relative brightness of the shaded and unshaded portions of the surface. A comparison of these results with those obtained at the same time for the chemical rays showed that with the sun at a mean altitude of 25° 16′, the mean ratio of the chemical intensities of direct and diffused light being 0·23, that of the luminous intensities was 4·00, or that the action of the atmosphere was 17·4 times greater on the chemically active than on the luminous rays of sunlight. A series of photometrical experiments made afterwards at Owens College gave the following results:—

Mean altitude of the sun	12° 3′
Mean ratio of chemical intensity	0·053
Mean ratio of luminous intensity....	1·400

It appears therefore that with the sun at an altitude of 12° 3′, the action of the atmosphere was 26·4 greater on the chemical than on the luminous rays.

The foregoing experiments appear to prove—

I. That the effect of the atmosphere upon the highly refrangible and chemically active solar rays is regulated by totally different laws from those founded upon the hypothesis of the reflexion by means of hollow vesicles of water.

II. That the ratio of the chemical intensity of direct to diffuse sunlight for a given altitude of the sun at different localities is not constant, varying with the transparency, &c., of the atmosphere.

III. That this ratio of "chemical" intensity does not in the least correspond to the ratio of "visible" intensity as estimated by the eye; the action of the atmosphere being 17·4 times greater upon the chemical than on the luminous rays when the sun's altitude is about 25° 16′, and 26·4 times greater when the sun's altitude is 12° 3′.

March 1, 1866.

Lieut.-General SABINE, President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Patrick Adie, Esq.
Alexander Armstrong, M.D.
George Bishop, Esq.
Sir Charles Tilstone Bright.
Samuel Brown, Esq.
Francis T. Buckland, Esq.
John Charles Bucknill, M.D.
Lieut.-Col. John Cameron, R.E.
Sir Thomas Edward Colebrook, P. R. Asiat. Soc.
W. Boyd Dawkins, Esq.
Henry Dircks, Esq.
Thomas Rowe Edmonds, Esq.
Rev. Frederick William Farrar, M.A.
Alexander Fleming, M.D.
Peter Le Neve Foster, Esq.
Sir Charles Fox.
William Withey Gull, M.D.
William Augustus Guy, M.B.
Julius Haast, Ph.D.
Capt. Robert Wolseley Haig, R.A.
James Hector, M.D.
Jabez Hogg, Esq.
Edward Hull, Esq.
John William Kaye, Esq.
John Robinson M^{c}Clean, Esq.
Hugo Müller, Ph.D.
Charles Murchison, M.D.
James Robert Napier, Esq.
Rear-Admiral Erasmus Ommaney.
George Wareing Ormerod, Esq.
William Henry Perkin, Esq.
Thomas Lambe Phipson, Ph.D.
Ven. Archdeacon Pratt, M.A.
Charles Bland Radcliffe, M.D.
Capt. George Henry Richards, R.N.
Benjamin Ward Richardson, M.D.
Thomas Richardson, M.A.
William Henry Leighton Russell, Esq.
Rev. William Selwyn, D.D.
Rev. Richard Townsend, M.A.
Rev. Henry Baker Tristram.
Edward John Waring, M.D.
Henry Watts, Esq.
Charles Wye Williams, Esq.
Henry Worms, Esq.

The following communication was read:—

"Researches on Acids of the Lactic Series.—No. I. Synthesis of Acids of the Lactic Series." By E. FRANKLAND, F.R.S., and B. F. DUPPA, Esq. Received February 14, 1866.

(Abstract.)

In the first part of this paper the authors give the details of the synthetical production of numerous acids of the lactic family, which have been briefly described in a series of notes already published in these Proceedings during the years 1863, 1864, and 1865. In the concluding portion of the present paper, they discuss the theoretical considerations which arise out of these investigations. They call attention to the existence of a group of elements, to which they give the name oxalyl and the formula $(\mathrm{C\,O\,Ho})$, and which exists not only in all acids of the lactic series, but also in nearly every known organic acid. The isolated molecule of this radical is oxalic acid,

$$\left\{\begin{matrix} C\,O\,Ho \\ C\,O\,Ho, \end{matrix}\right.$$

in proof of which they show that when oxalic ether is acted upon by nascent amyl, it is converted into caproic ether:

$$\underset{\text{Oxalic ether.}}{\left\{\begin{matrix} C\,O\,Eto \\ C\,O\,Eto \end{matrix}\right.} + \underset{\text{Amyl.}}{\left\{\begin{matrix} C\,Bu\,H_2 \\ C\,Bu\,H_2 \end{matrix}\right.} = 2\;\underset{\text{Caproic ether.}}{\left\{\begin{matrix} C\,Bu\,H_2 \\ C\,O\,Eto. \end{matrix}\right.}$$

Oxalyl is closely related to cyanogen, the two radicals passing into each other in a host of reactions; hence the production of cyanides from the ammonium-salts of the fatty acids on the one hand, and the synthesis of acids from certain cyanogen compounds on the other,—a reaction first pointed out by Kolbe and Frankland*, and which has of late yielded such important results in the hands of Maxwell Simpson† and of Kolbe and of Hugo Müller‡.

$$\underset{\text{Cyanogen.}}{\left\{\begin{matrix} C\,N''' \\ C\,N''' \end{matrix}\right.} \qquad\qquad \underset{\text{Oxalyl.}}{\left\{\begin{matrix} C\,O\,Ho \\ C\,O\,Ho. \end{matrix}\right.}$$

The researches of these chemists prove that the introduction of cyanogen into an organic compound, and its subsequent transformation into oxalyl, converts that compound into an acid, or, if already an acid, increases its basicity by unity (for each atom of oxalyl so developed), this result being apparently quite independent of the position of the oxalyl in the molecule. The atom of oxalyl (as the above molecular formula shows) may be regarded as methyl ($C\,H_3$), in which two atoms of hydrogen have been replaced by one of oxygen, and the third by hydroxyl (Ho).

It may be objected that the group of elements, which is thus invested with radical functions, lacks one of the fundamental characteristics of a radical by its proneness to change; but this characteristic is exhibited by the commonly received radicals in a very varied degree. And even methyl itself, which certainly possesses it in the most marked manner, readily permits of its hydrogen being replaced by chlorine or bromine on the one hand, and by sodium on the other. All compound radicals, the authors remark, are purely conventional groupings of elements intended to simplify the expression of chemical change; and in this respect they believe the group oxalyl, entering as it does into the constitution of nearly every organic acid, has as valid a claim to a distinct name as the most universally recognized radical. Its admission renders possible the following very simple expression of the law governing the basicity of nearly all organic acids—*an organic acid containing* n *atoms of oxalyl is* n *basic.*

The authors classify all acids of the lactic series at present known, or

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* Memoirs of Chem. Soc. vol. iii. (1847) p. 386.

† Phil. Trans. 1861, p. 61; and Journ. Chem. Soc. vol. xviii.

‡ Journ. Chem. Soc. vol. xvii. p. 109.

which could be obtained by obvious processes, into the following eight divisions :—

General formula.

1. Normal acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}H\,Ho} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
2. Etheric normal acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}H\overset{\pm}{R}o} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
3. Secondary acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}_2\,Ho} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
4. Etheric secondary acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}_2\overset{\pm}{R}o} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
5. Normal olefine acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}H\,Ho} \\ \mathrm{(C\,H_2)_n} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
6. Etheric normal olefine acids........ $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}H\overset{\pm}{R}o} \\ \mathrm{(C\,H_2)_n} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
7. Secondary olefine acids .... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}_2\,Ho} \\ \mathrm{(C\,H_2)_n} \\ \mathrm{C\,O\,Ho} \end{array}\right.$
8. Etheric secondary olefine acids...... $\left\{\begin{array}{l} \mathrm{C\overset{+}{R}_2\overset{\pm}{R}o} \\ \mathrm{(C\,H_2)_n} \\ \mathrm{C\,O\,Ho.} \end{array}\right.$

A normal acid of the lactic series may be defined as one in which an atom of carbon is united with oxalyl, hydroxyl, and at least one atom of hydrogen. In the above general formula for these acids $\overset{+}{\mathrm{R}}$ may be either hydrogen, or any alcohol hydrogen.

An etheric normal acid is constituted like a normal acid, but contains a monatomic organic radical, chlorous or basylous, in the place of the hydrogen of the hydroxyl.

A secondary acid is one in which an atom of carbon is united with oxalyl, hydroxyl, and *two* atoms of an alcohol radical.

An etheric secondary acid stands in the same relation to a secondary, as an etheric normal does to a normal acid.

A normal olefine acid is one in which the atom of carbon united with oxalyl is *not* united with hydroxyl, and in which the atom of carbon united with hydroxyl is combined with not less than one atom of hydrogen. In this formula $\overset{+}{\mathrm{R}}$ may be either hydrogen or a monatomic alcohol radical, and the olefine, or diatomic radical of these acids, may belong to either the ethylene, or the ethylidene series.

An etheric normal olefine acid differs from a normal olefine acid only in having the hydrogen of the hydroxyl replaced by an organic radical positive or negative.

A secondary olefine acid is one in which the atom of carbon united with oxalyl is not combined with hydroxyl, and in which the atom of carbon united with hydroxyl is also combined with two monatomic alcohol radicals. In the above formula $\overset{+}{R}$ must be a monatomic alcohol radical.

An etheric secondary olefine acid is related to the secondary olefine acids in the same way as the etheric normal olefine acids are related to the normal olefine acids.

The numerous cases of isomerism in the lactic series are next examined and explained; and the authors then show how the radicals which are employed for the production of the synthesized acids may again be separated, thus affording analytical as well as synthetical proof of the constitution of these acids.

These investigations have conducted the authors to the following conclusions:—

1. All acids of the lactic series are essentially monobasic.

2. These acids are of four species, viz. normal, secondary, normal olefine, and secondary olefine acids; and each of these species has its own etheric series of acids, in which the hydrogen of the hydroxyl contained in the positive or basylous constituent of the acid is replaced by a compound organic radical, either positive or negative.

3. The normal acids are derived from oxalic acid by the replacement of one atom of oxygen, either by two atoms of hydrogen, or by one atom of hydrogen and one atom of an alcohol radical.

4. The secondary acids are derived from oxalic acid by the replacement of one atom of oxygen by two atoms of monatomic alcohol radicals.

5 The olefine acids are derived from oxalic acid by a like substitution of two monatomic positive radicals for one atom of oxygen, with the addition of a diatomic radical ($C_n H_{2n}$) between the two atoms of oxalyl.

6. The acids of the lactic series stand in the very simple relation to the acids of the acetic series first pointed out by Kolbe, viz. that by the replacement, by hydrogen, of the hydroxyl, ethoxyl, &c. contained in the positive radical of an acid of the lactic series, that acid becomes converted into a member of the acetic series.

7. The acids of the lactic series stand in an almost equally simple relation to those of the acrylic series, as is seen on comparing the two following formulæ:—

$$\left\{\begin{array}{l} C\,(C\,H_3)\,H\,Ho \\ C\,O\,Ho \end{array}\right. \qquad\qquad \left\{\begin{array}{l} C\,(C\,H_2)''\,H \\ C\,O\,Ho. \end{array}\right.$$

Lactic acid. Acrylic acid.

March 8, 1866.

Lieut.-General SABINE, President, in the Chair.

Mr. Archibald Geikie was admitted into the Society.

The following communications were read:—

I. "Note on a Correspondence between Her Majesty's Government and the President and Council of the Royal Society regarding Meteorological Observations to be made by Sea and Land." By Lieutenant-General SABINE, P.R.S. Received March 8, 1866.

Her Majesty's Government having been pleased to consult the Royal Society on several occasions in the last few years regarding the proper steps to be taken by this country, under the sanction and authority of its Government, for the prosecution, in cooperation with the Governments of other States in Europe and America, of systematically conducted meteorological observations by Land and Sea, it may be desirable to offer to the Fellows a *résumé* of the correspondence, and of the suggestions which from time to time have been tendered on the part of the Society to the several departments of the State.

The correspondence commenced by a communication from the Foreign Office in March 1852, transmitting, by direction of the Earl of Malmesbury, several documents received from foreign governments in reply to a proposition which had been made to them by Her Majesty's Government, for their cooperation in establishing a uniform system of recording meteorological observations; and requesting the opinion of the President and Council of the Royal Society in reference to these documents, and more especially in reference to a communication from the Government of the United States of America respecting the manner in which the proposed cooperation might be carried out.

The reply of the President and Council was dated May 10, 1852. It recognized fully the importance of well-directed and systematically conducted meteorological observations, not only for their scientific value, but also on account of the important bearing which correct climatological knowledge has on the welfare and material interests of the people of every country. With reference to a specific proposal for the adoption, by all countries, of a uniform plan in respect to instruments and modes of observation in meteorological researches on land, the President and Council expressed a doubt whether any practical advantage was likely to be gained by pressing such a recommendation in the then state of meteorological science. Many of the principal Governments of the European Continent had already organized systematic climatological researches throughout their respective States under the superintendence of men eminently qualified by theoretical and practical knowledge, and whose previous publications had obtained for them a general European reputation. The instruments employed in each country had been constructed under the care of the Superintendents, and the instructions for their use drawn up and published by them; the observations were also received by them and reduced and coordinated, and were in this state published periodically by the respective governments. To call on countries so advanced in systematically conducted meteorological observations to remodel their instructions and instruments with the view of establishing uniformity in these respects, was scarcely likely to be successful, especially if the request proceeded from a country which had no similarly organized system extending over its own area. Moreover the discussions which had taken place at the Magnetical and Meteorological Congress assembled at the Cambridge Meeting of the British Association in 1845, which was attended by the Superintendents of the different continental systems, had manifested so marked a disposition on the part of the meteorologists of the different countries to adhere to their respective arrangements in regard to instruments, times of observation, and modes of publication, as to produce a strong conviction that the suitable time for pressing a proposal for the substitution of a uniform scheme, however advantageous in some respects, had not then arrived.

But in respect to *Marine* Meteorology the case was widely different; and the suggestions which the President and Council felt it their duty to offer on that subject had a much more positive character, and were directed to immediate action. An application had been made by the Government of the United States to that of our own country to give a greater extension and a more systematic direction to meteorological observations *made at sea.* Apart from the important scientific bearing of such researches, the well-known publications of Lieutenant Maury had given to the Government of the United States a fair claim to make such an application to that of our own country, to whose commercial interests a practical knowledge of the meteorological statistics of the ocean was not less important than to those of the United States. Accordingly, in their reply to the Foreign Office, the President and Council did not fail to express emphatically their hope that the application for cooperation, thus earnestly addressed by the Government of the United States, might not be addressed in vain.

The following extracts from a memoir addressed by Lieutenant Maury to the Secretary of the United States Navy, printed in "Papers presented to the House of Lords by command of Her Majesty, pursuant to an address dated February 21, 1853," will show that the suggestions thus made by the President and Council, both in regard to Land and to Sea Meteorological Observations, were fully concurred in by Lieutenant Maury, on his return from an official mission to visit and report upon the meteorological establishments of the principal States of Europe. Lieutenant Maury's memoir is dated November 6, 1852.

"I would recommend that the United States should abandon, for the present at least, that part of the 'Universal System' which relates to the Land, and that we should direct our efforts mainly to the Sea, where there is such a rich harvest to be gathered for navigation and commerce. . . . . I am induced to make this recommendation in consequence of the evident reluctance with which Russia, Austria, Bavaria, Belgium, and other powers seem to regard any change in their systems of meteorological observations on shore. . . . . Each country seems to have adopted a system of its own, according to which its labourers have been accustomed to work, and to which its meteorologists are more or less partial. Any proposition having in view for these systems a change so radical as to bring them to uniformity, and reduce them to one for all the world, would, I have reason to believe, be regarded with more or less jealousy by many. . . . . Not so, however, with regard to the Sea; that proposal meets with decided favour and warmest support."

Lieutenant Maury then quotes largely from the Report of the President and Council of the Royal Society (already referred to), and from the anniversary address of the President of the British Association at the Belfast Meeting in 1852, in illustration of the advantages which navigation and commerce may derive from the extension of maritime researches by the proposed cooperation of Great Britain and the United States.

In June 1854 the Board of Trade informed the President and Council by letter that they were about to submit to Parliament an estimate for an office for the discussion of observations on Meteorology to be made at Sea in all parts of the globe, in conformity with the recommendation of a conference held at Brussels in the preceding year; and that it was their intention to publish from time to time, and to circulate, such statistical results, obtained by means of the observations referred to, as might be considered most desirable by men versed in the science of meteorology, in addition to such other information as might be required for the purposes of navigation. To this end, the Board of Trade were desirous of obtaining the opinion of the Royal Society as to what were the great desiderata in that science, and as to the forms which may be best calculated to exhibit the great atmospheric laws which it may be most desirable to develope.

The Board further stated that, "as it may possibly happen that observations on land upon an extended scale may hereafter be made and discussed in the same office, it is desirable that the reply of the Royal Society should keep in view and provide for such a contingency."

In their reply to this communication, dated February 22, 1855, the President and Council kept carefully in view the relative importance which the Board of Trade attached to the suggestions which might be offered respecting *observations to be made at sea,* and *observations to be made on land*; the *immediate* bearing of the one, and the *contingent* and *eventual* bearing of the other. Their reply consequently bore chiefly on points to be investigated by marine observations,—under the several heads of barometric variations; of those of the dry air and of aqueous vapour; of the mean temperature of the air; of the temperature of the sea, and investigations regarding currents; of storms and gales; thunderstorms; auroras and falling stars; and charts of the magnetic variation. (Proc. Roy. Soc., vol. vii. pp. 342–361.)

These were treated of in their generality, as subjects of investigation to be made (in the words of the Board of Trade) "at sea in all parts of the globe." Nor was the contingency of observations to be made on land upon an extended scale, "which may hereafter be made and discussed in the same office," overlooked; and to enable the Royal Society to be more fully prepared, and to be ready "to provide for the contingency" whenever it should present itself, the President and Council put themselves in communication with several of their foreign members who were known as distinguished cultivators of meteorological science,—having always in view the possibility that, when the occasion should occur, they might be prepared to offer such suggestions as might facilitate the purpose which had been deemed so desirable by Her Majesty's Government in the preceding year. The chief difficulty which had then presented itself had been the desire shown by the meteorologists of the different European States to adhere to the instruments, and more particularly to the hours of observation, to which they had been accustomed. The hours had been selected partly on grounds of convenience, and partly in the belief that they were those best suited to receive the corrections required in different localities for diurnal and casual variations. The most hopeful way of surmounting the difficulties in question would obviously be the introduction of instruments which should be *continuously self-recording*; such instruments would in addition supply the exact instants of the maxima and minima of the different phenomena—points greatly required in the discussion of the laws of extensive atmospherical disturbances. At the epoch of the Cambridge Congress in 1845 it was understood that no perfectly reliable instruments for continuous record were in use amongst the continental observers. The self-recording instruments of M. Kreil, employed at the observatories of Prague, Senftenberg, Vienna, and Munich, and which had also been in use for many months at the Kew Observatory, were not *continuously* self-recording, and were also, in the opinion of many, not altogether free from objections. The construction of instruments both in magnetism and meteorology which should serve this important purpose and at the same time be free from causes of error in other ways, had been a cherished object of the Committee of the Kew Observatory from the commencement of that institution. The advance which had been accomplished at a very early period may perhaps best be stated in the following extract from the fourth report of Mr. Ronalds, its Director, published in the volume of the British Association for 1847, Trans. of Sections, page 30. "The preliminary experiments on the photographic registration of the atmospheric electrometer, the thermometer, the barometer, and the declination-magnet having been long since completed and published, and their results having warranted the cost and trouble of constructing apparatus of a durable and convenient character, a declination-magnet and a barometer have been mounted at Kew which scrupulously fulfil the requisite conditions without the intervention of those friction-rollers, levers, pivots, or other mechanism which have hitherto rendered self-registering apparatus so objectionable." Mr. Ronalds added that it was his "intention to provide during the ensuing year complete apparatus on like principles for registering as many of the other meteorological and magnetical instruments as funds will permit."

The instruments which would be required for a complete equipment of a meteorological observatory would be those which should automatically and continuously record (1) the variations of the atmospheric pressure; (2) those of the dry and wet thermometers; (3) those of the force and direction of the wind; and (4) those of the atmospheric electricity. Of these, Nos. 1 and 2, as has been already said, had been devised at Kew in or before 1847. Dr. Robinson's hemispherical-cup anemometer, of which a description was published in 1851 in the Transactions of the Royal Irish Academy, and which was immediately adopted at Kew, records, in a manner which I believe is universally held to be unexceptionable, the direction and force of the wind at every instant. The electrometer to which Mr. Ronalds referred in his report of 1847, though highly ingenious and yielding very instructive results, was not *continuously self-recording*. An electrometer fulfilling this condition was consequently a desideratum until Professor William Thomson devised, and caused to be constructed under his own superintendence at Kew, in the spring of 1861, the self-recording electrometer which has been subsequently in successful work at that observatory; thus supplying the fourth apparatus required for a complete meteorological record. It may be added that the Kew Barograph photographs its records self-compensated for temperature; its curves consequently are in immediate readiness for the lithographer or engraver.

Such was the state of instrumental preparation at the Kew Observatory when the untimely death of Admiral FitzRoy, who had been placed by the Board of Trade in charge of the meteorological office established in 1855, occasioned a renewal of the communications which had taken place on the formation of the office, between the Board of Trade and the Royal Society. In a letter dated May 26, 1865, the Board of Trade recalled to the recollection of the Royal Society the recommendations regarding marine meteorology contained in the letter of the President and Council of February 22, 1855, stating that those recommendations had been adopted by the Board as the basis of the proceedings of the meteorological department; and that in conformity therewith instruments and logs had been prepared and furnished to ships proceeding on distant voyages, and had been returned to the office; and that some progress had been made in carrying into effect the original programme of tabulating these in readiness for statistical charts.

It was further stated that "in 1859 or 1860, the French Government having adopted a system of telegraphing and publishing the actual state of the weather from one place to another, in which Admiral FitzRoy's cooperation had been sanctioned, a considerable part of the vote previously applied to obtaining and digesting observations was devoted to these telegrams; and further, that in 1861, Admiral FitzRoy having grafted on this system of telegraphic communication a system of forecasting the weather, and, on occasions of anticipated storms, the giving of special warnings, communicated by telegraph to the different ports and there made known by hoisting certain signals,—the whole or almost the whole of the funds originally voted for the purpose of observations had thus been diverted from their original scientific purpose to an object deemed more immediately practical."

The decease of Admiral FitzRoy afforded, in the judgment of the Board of Trade, a fitting opportunity to review the past proceedings and present state of the meteorological department, and rendered them desirous of again consulting the Royal Society on the constitution and objects of the department, and the mode in which those objects might be most effectually attained.

The points on which the opinion of the Royal Society was specially requested were the following:—

1. Are the points specified in the letter of the Royal Society of the 22nd of February 1855 still deemed as important for the interests of science and navigation as they were then considered?

2. To what extent have any of these objects been answered by what has already been done by the meteorological department?

3. What steps should be taken for making use of the observations already collected?

4. Is it desirable to make any, and what, further observations on any of the subjects mentioned in the Royal Society's letter of the 22nd of February 1855?

5. What is the nature of the basis on which the system of daily forecasts and storm-warnings established by Admiral FitzRoy rests? Are they founded on scientific principles, so that they, or any part of them, may be carried on satisfactorily notwithstanding Admiral FitzRoy's decease?

6. Can the Royal Society suggest any improvement in the form and manner of the process pursued in forecasts and storm-warnings?

7. Have the Royal Society any general suggestions to make as to the mode, place, or establishment in, at, or by which the duties of the meteorological department can best be performed? it being understood that the Admiralty are willing to undertake to place in the hands of the Hydrographer all those observations which can properly be used in framing charts for the

purposes of navigation, but not those which relate to meteorology proper or meteorological observations on land.

Several documents accompanied this communication,—amongst them a statement by Mr. Babington, chief clerk in Admiral FitzRoy's office, regarding the method adopted in the department in regard to forecasts and storm-warnings; and returns, exhibiting a comparison of the probable force of the wind indicated by signals in the years ending March 31, 1864, and March 31, 1865, and its actual state as reported in the three days following the exhibition of the signals.

The reply of the President and Council was dated June 15, 1865. It suggested the continuance, for the present, of the practice of forecasts and storm-warnings as before, and the continued issue of instructions and forms to such masters of vessels proceeding on distant voyages as might be expected to make a profitable use of them; both these duties to be continued under Mr. Babington's superintendence, by whom in effect they had been carried on for some time previous to Admiral FitzRoy's decease. And it was further recommended that both the system under which the forecasts and storm-warnings had been hitherto carried on, and the extent and value of the information regarding ocean-statistics which had been accumulated in the office of the Board of Trade, should be subjected to a careful examination. These recommendations were adopted; and a Committee has been appointed, of three members, one nominated by the Board of Trade, a second by the Admiralty, and a third by the Royal Society, to report on what has been done, and to suggest any modifications which may appear desirable for the future. The report of this Committee is expected to appear very shortly.

With reference to the subject of Land Meteorology, the President and Council had been apprized by the Board of Trade, in February 1855, that "observations on land upon an extended scale might hereafter be made and discussed in the meteorological department of the Board," and had been requested to be "prepared for such a contingency." In the more recent correspondence in 1865, the subject of land observations was again brought forward, and the Royal Society was invited to offer suggestions in reference to it. Thus appealed to, the President and Council would have failed in their duty if they had not replied fully and explicitly to a request proceeding from Her Majesty's Government,—carefully restricting themselves, in their reply, to such suggestions as their own knowledge enabled them to affirm with confidence could be carried into practical operation, and which at the same time enabled them to respond to the more general inquiry in the letter from the Board of the 26th of May 1865, viz. "Have the Royal Society any suggestions to make as to the mode, place, or establishment in, at, or by which the duties of the meteorological department can best be performed?"

The reply of the President and Council was as follows:—"There remain, therefore, to be noticed solely the considerations which relate to 'Meteorology proper,' *i. e.* to the Land Meteorology of the British Islands. We find that the principal states of the European Continent have almost without exception formed establishments for the collection and publication periodically of the meteorology of their respective countries. The arrangements consist usually of a central office, at which instruments and instructions are provided for a number of stations, greater or less, according to the area which they represent; at which stations observations are made and transmitted to the central office, where the results of all are reduced, coordinated, and published. The small extent of the area comprised by the British Islands in comparison with the territories of many of the European States may require *fewer* stations; but in a matter now so generally attended to and provided for, it seems scarcely fitting that our country should be behind others. There is, moreover, a peculiarity in the meteorological position of the British Islands in respect to Europe generally as its north-western outpost, in consequence of which an especial duty appears to devolve upon us. M. Matteucci, in a very recent publication, has already made the important remark that extensive atmospheric disturbances which first invade Ireland and England are those which, in winter more especially, extend to and pass the Alps (although somewhat retarded by them), and spread over Italy; and that storms so telegraphed from England have actually reached Italy, and have been found to correspond with the accounts subsequently received from Italian Mediterranean Ports.

"A few stations—say six, distributed at nearly equal distances in a meridional direction from the south of England to the north of Scotland, furnished with self-recording instruments supplied from and duly verified at one of the stations regarded as a central station, and exhibiting a *continuous* record of the temperature, pressure, electric and hygrometric state of the atmosphere, and of the force and direction of the wind—might perhaps be sufficient to supply authoritative knowledge of those peculiarities in the meteorology of our country which would be viewed as of the most importance to other countries, and would at the same time form authentic points of reference for the use of our own meteorologists. The scientific progress of meteorology from this time forward requires indeed such continuous records—first, for the sake of the knowledge which they alone can effectively supply, and, next, for comparison with the results of independent observation not continuous. The actual photograms or other mechanical representations, transmitted weekly by post to the central station, would constitute a lithographed page for each day in the year, comprehending the phenomena at all the six stations, each separate curve admitting of exact measurement from its own base-line, the precise value of which might in every case be specified.

"The President and Council suggest that the observatory of the British Association at Kew might with much propriety and public advantage be adopted as the central meteorological station."

It has been already shown, in the earlier part of this communication, that the Kew Observatory possesses all the instruments required in a complete system of continuous self-recording meteorological observation. These are well known to the Directors of many meteorological observatories both at home and abroad, who in several instances, after personal examination, have applied for and obtained for their own establishments similar instruments prepared under the Superintendent of the Kew Observatory, Mr. Balfour Stewart. Thus, Barographs on the Kew pattern have been supplied to Oxford, St. Petersburgh, and Coimbra; Anemometers to St. Petersburgh, Odessa, Melbourne, Coimbra, Ascension, Madras, Agra, and two meteorological stations established by Admiral FitzRoy; Electrographs to Lisbon and Coimbra. There would be no difficulty (as was stated in the letter to the Board of Trade) in preparing instruments at Kew for affiliated meteorological stations in Britain, and in arranging for their verification and comparison with the Kew standards, as well as in giving to those in whose hands they may be placed such instructions as may ensure uniformity of operation. Such functions constitute in fact part of the original purposes of the Institution at Kew, and are in continual exercise both for magnetism and for meteorology.

It is not unreasonable to anticipate that the success of such a system of continuous self-recording meteorological observation, exemplified over the limited area of the British Islands, might occasion its wider extension, and thus contribute to the virtual fulfilment of the desire expressed by Her Majesty's Government in 1852 "for the adoption of a general and uniform plan of making and recording meteorological observations."

Postscript, March 23.—Since this "Note" was communicated to the Royal Society, I have read with great satisfaction the opinion expressed in the following extract from the "Report of a Committee assembled at the Board of Trade to consider certain questions relating to the Meteorological Department of the Board," pages 52, 53:—

"There is no doubt that self-recording instruments are urgently needed in the present state of meteorological science, and that they will soon in all probability be largely employed both in this country and abroad. Their advantages are manifest. By reason of the continuity of their records, no wave or variation of any description in any of the meteorological elements can escape notice, and the course of that wave or variation can be tracked with certainty from station to station, and its modification at the time of reaching each station in succession can be accordingly observed. For the same reason one difficulty, now seriously felt, in charting the weather, viz. that which arises from observers in different places and countries adopting different hours of observation, would wholly disappear; and a further difficulty, viz. that which arises from observers being unpunctual to their professed hours of observation, would disappear also. The unvarying accuracy of the record is an advantage of still greater importance than might be expected by those who have had no experience of the frequent errors to be found in meteorological registers. Each error creates considerable confusion; it throws doubt on the observations accurately made at neighbouring places; and that doubt cannot be removed except by the continuity of the records at those places. This continuity is unattainable unless the weather happens to be uniform over a wide district, or unless observations are made at many more places than would be needed, if reliance could be placed upon the accuracy of the observers. Another advantage of self-recording instruments is that their records are independent of particular scales. Their notation is in lines and curves that can be measured with equal facility according to any desired scale. The thermometer lines could be measured at pleasure according to Fahrenheit's scale, as used in England; to the Centigrade, as in France; or to Reaumur's, as in Germany. The barometer lines could be measured with equal ease in English inches, in millimetres, or in Paris feet. For the various reasons we have mentioned, self-recording instruments are of eminent local and international utility. The establishment of a series of them in England would confer a wide benefit. They would give precision and fulness to the charts of our own weather; they would set an example that foreign governments would soon follow; and they would afford material in a very acceptable form to meteorologists at home and abroad for the discussion of the weather of Europe at large."

II. "On the Action of Compasses in Iron Ships." By Mr. JOHN LILLEY. Communicated by Sir W. SNOW HARRIS, F.R.S. Received February 9, 1866.

Although many ably-written papers upon this subject have at various times appeared, none of them seem to be of that simple practical character as to supersede the necessity of any further investigation of the subject, or deter the author from submitting to the Royal Society the results of many years' practical experience in the construction of the mariner's compass, and its adjustment in iron ships. These results are given with a view to advance our knowledge of this important and great practical scientific question, and to add still more to the security of life and property. In the present day, when iron shipbuilding is so widely extending, it is presumed that the most humble offering tending to place the directive action of the compass beyond the reach of disturbing magnetic forces may not be unacceptable.

It is unnecessary here to enter into a mathematical investigation of the properties or magnetic condition of iron ships, this part of the subject having been already fully treated and developed by many learned men. The author rather proposes to confine himself to the consideration of the probable causes of the disasters so frequently attendant on the navigation of iron and other ships, through defective compass guidance; such disasters, according to the author's experience, may be traced, in a large majority of cases, to one or other of the following causes:—

1. The improper construction and position of the steering binnacle and compass.
2. The too frequent habit of placing all reliance upon the steering binnacle compass.
3. Allowing the compasses to be too long in use without due examination.
4. The improper manner in which the compasses have been adjusted.

1. The construction of the compass is at all times a matter of great importance, but when applied to the case of an *iron* ship its value is increased tenfold; it is most essential that the compass should be of the best attainable description, and so constructed that its centre and pivot should be subject to the smallest possible amount of friction, in order that the needles may be entirely free to follow the directive force of the earth, and have at the same time great retentive power. Some years since the author observed, when adjusting an iron steamer's compass furnished by him with very powerful needles, discarded from some previous experiments in magnetism, and which were only 4 inches in length, that more than usually satisfactory results were obtained; the deviations were of a smaller value, and far more uniform than when using needles of greater length. He has since adopted the practice of employing needles not exceeding 6 inches in length, even for cards of the largest diameter.

The position of the compass is also important; it is an objectionable practice of too frequent occurrence, even with the ordinary plane spindle and barrel, to place the binnacle near the steering-wheel; with the screw apparatus, arms, and levers the practice becomes extremely dangerous, the whole mass, from the process of manufacture, being found highly magnetic. The idea, however, prevails that all this is a matter of indifference, since the counteracting magnets are supposed to neutralize all magnetic disturbance. The reverse, however, is the case; many instances have come under the author's notice practically in which errors have arisen solely from this cause, but which ceased to exist on the removal of the binnacle to a distance of 4 feet from the spindle, or twice the original distance.

It would be far better, in all cases, if the steering-wheel could be placed before the mizen-mast of ships, instead of abaft and close to the stern, as is generally the case; compasses would invariably act better and be subject to smaller changes.

2. It is too frequently the practice to place exclusive reliance upon the *steering*-compass; this may be attended with less trouble to the navigator, but is, as regards the safety of the ship, very perilous. As ships are now fitted, the steering-compass is placed very near the stern; and since in iron ships the magnetic forces are generally concentrated at the two ends of the vessel, it must be obvious that it is placed at that spot where the greatest variations may be expected. Every iron ship should be furnished with a properly constructed standard binnacle not less than 5 feet high above the deck, suitably placed, and containing a compass fitted in the most con-

venient manner for taking observations without the aid of compensating magnets. By this compass *alone* should the ship be *navigated*; the steering-compass is simply the helmsman's guide. Many iron ships have been fitted with two navigating binnacles, the one for steering, and the other at the fore part of the poop, both furnished with compensating magnets, and obviously both subject to the same changes incident to the change of hemisphere; cases of ships thus fitted have come under the author's notice, in which many providential escapes from wreck have occurred on the return passage from India; in one particular instance, land was made on the coast of Ireland when the commander imagined he was entering the channel, an error that could not have arisen had the ship been furnished with an uncompensated standard compass instead of a compensated compass.

In the uncompensated compass, the changes of deviation are far less than would be found in a compensated compass placed not more than 2 feet above iron beams. Very full directions on this subject will be found in the valuable work of the late Capt. E. J. Johnson, R.N., and it is much to be regretted that this work is not more studied by commanders and officers of iron ships.

3. Frequent disasters have been found to occur in consequence of allowing the compass to be too long in use without due examination, more particularly in steam-vessels going short voyages; the author is led to this conclusion from the fact of having readjusted iron vessels' compasses stated to be largely in error, but subsequently found, on swinging the ship, to have a comparatively small error, the supposed error being referable to a defective centre and pivot in the compass itself, which prevented the needle from reaching its meridional position. In such cases an error of 8° or 10° has been often observed; the importance of such an error in narrow channels is too obvious to require comment.

There are valuable and simple appliances which to a very great extent remove this difficulty, and which are comparatively inexpensive; it is to be much regretted that a great indifference to the state of vessels' compasses exists in the minds of those under whose care the compasses are often placed, and who, it might be expected, would from experience have been more sensitively alive to the absolute necessity of keeping the compasses, as far as possible, in a state of efficiency. If a compass looks clean upon its surface, it is believed to be in a perfect state, although it may have been stowed away with the card upon the pivot without any protection, thus interfering with the most important of its working parts; as a consequence, the card, when required for use, will be sluggish upon its pivot, and a false indication of the direction of the vessel's head is an unavoidable result.

4. A defective adjustment of the compass is again a vital source of error. This mainly depends, 1, on the locality in which the vessel is swung, which is too often a dock, where other iron vessels lie in dangerous proximity; 2, a want of due precaution on the part of the adjuster, in the

place selected for the shore-compass. This is a matter of great importance, far more than is often supposed. As the corrections made on board necessarily depend upon the bearings given from the shore, if these are faulty, the ship's compass cannot be otherwise than incorrect.

The author has more than once seen a shore-compass within a very short distance of an iron shed. The prevailing practice of swinging a ship in the short space of two hours is also very objectionable; it is utterly impossible to regulate compensating magnets, and to make the requisite observations for the use of soft iron, in so short a time; it does not allow the vessel to be stayed upon the several points for a sufficient scrutiny of the compasses, sometimes three in number, and the results are too often merely guessed at, and thus all the advantages of that admirable system of adjusting iron vessels' compasses, introduced by the Astronomer Royal, and which are invaluable to our coasting and short voyage vessels, are, to a great extent, negatived.

London is unfortunately very badly provided with the means of adjusting the compasses of iron vessels, and, as their number is so much increasing, this subject is daily becoming more important; the docks are crowded, and it is by mere chance that in the Victoria Dock, the only available dock at our command, a sufficiently clear space is to be met with. Greenhithe remains as the last resource in this dilemma; here moorings have been laid down by the City authorities for the use of merchant-vessels, but they are too near the edge of the tide. A very strong down current exists; the entire space of slack water in the Reach is occupied by *two* sets of moorings for the use of the Royal Navy. It would be a very great boon if one of these sets of moorings were allowed to be used by the merchant service, when not required for Her Majesty's Navy. It rarely happens that both sets of moorings are at the same time required for use. As the swinging a ship, moreover, at Greenhithe is entirely dependent upon the tide, it must be commenced with the flood, added to which, the operation is often much impeded by the wind, so that in the winter months, when the days are short, and the seven hours of ebb occur during nearly the whole of daylight, much serious delay is the result; the author has often occupied three days in adjusting a ship's compasses.

The construction of a dock for this purpose would be very desirable; the expense may be urged against the proposal; but surely in London, one of the greatest mercantile cities in Europe, such an undertaking should be regarded as a national matter, and it is believed that shipowners would not object to pay a small rate of charge for the use of such a dock which might render it self-supporting.

An opportunity for constructing such a dock may possibly arise in the formation of the new docks at Dagenham, where the expense of an entrance would be saved by simply making a basin from the dock large enough to swing a ship 400 feet in length round a dolphin placed in the centre, and well supplied with mooring-posts for ropes to be made fast to, and a suitable spot for the shore observer—of course removed from all possibility of attraction by iron in the immediate vicinity; such basin to be kept exclusively for this purpose, and known as "The Ship-swinging," or "The Compass-adjusting" Dock.

With the view of deducing a practical result from what has been advanced in the foregoing remarks, the author would most strongly urge the necessity of some official inspection of iron vessels with reference to their compass-fittings. A code of rules and instructions might be laid down for this purpose; this would be essential in the cases of *new* ships; *old* ships should be examined at stated times, and certificates of compass adjustment be recognized by the appointed officer solely from those who can furnish evidence of having been properly instructed by competent persons, so that such an important work should not be allowed to be taken up and carried on by any amateur as his fancy may dictate.

It may be a question how far a Government inspection would be cordially received by shipowners or public companies; it might by some be regarded as an undue interference; the Government also might be indisposed to incur the cost of an extra office, with all its details; it might be more properly thought a matter for the consideration of Lloyd's. It is a question assuredly in which underwriters are largely and personally interested, and they already hold arbitrary powers as concerns the construction of the hull of a ship. The simple question of the compass as a means of safety is comparatively disregarded.

The above suggestions are offered with great deference, and the author would rather leave the subject in the hands of those more conversant with legislation than himself; but he cannot refrain from repeating that the results of his own practical experience (which has been of no small amount) convince him that an official supervision of the compass-fittings of iron ships has become, from various causes, absolutely requisite.

III. "On the Tidal Currents on the West Coast of Scotland." By ARCHIBALD SMITH, M.A., F.R.S. Received March 1, 1866.

The tidal currents on that part of the west coast of Scotland which is comprised between the Mull of Cantyre and the Island of Mull run in general with great velocity. Their velocity, direction, and the time of their change, or of slack water, are therefore matters of great importance to navigators. On the other hand, the rise and fall of the tide is so small, and the depth of water in the channels and the harbours so considerable, that the times of high and low water are of comparatively small importance.

While the laws of the currents are thus of more importance than the laws of the rise and fall of the tide, they are also much more simple. The times of high and low water are very different at different parts of the coast, while the times of slack water are nearly the same throughout the whole region in question. In a great part of this region the current, which sets for six hours in one direction, has no distinct title to be considered either a flood tide or an ebb tide. The consequence is, that to describe the laws of the currents by reference to the time of high and low water, introduces great and unnecessary complexity. The application to the currents of the method first applied by Admiral Beechey to the tidal stream of the English Channel and German Ocean (Phil. Trans. 1851, p. 703) introduces at once order and simplicity, and makes that intelligible which before was only a confused maze.

In the following paper an attempt is made, from the materials to be found in the charts of the Admiralty Survey of the west coast of Scotland, now nearly completed, to obtain a first approximation to a tidal chart of the west coast of Scotland. For this purpose I have, with the kind assistance of Commander Evans, F.R.S., the first Naval Assistant to the Hydrographer of the Navy, deduced from the charts all the information to be there found as to the direction and times of change of the tidal streams, as well as the times of high and low water. The latter are indicated in the usual way by Roman numerals, which, to avoid confusion, are always within the land. The times of change and direction of the currents are described by a particular symbol which I have found convenient for the purpose, and which I will now describe.

In the seas which we are considering, the stream at any point generally flows for six hours in one direction and for six hours in the opposite direction. This may be conveniently indicated by the following symbol:—

which indicates a stream flowing from XII. o'clock to VI. towards the east, and from VI. to XII. towards the west.

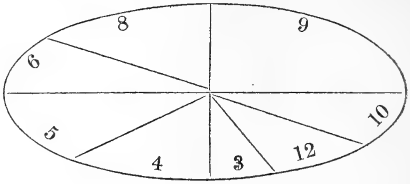
In this notation, for simplicity, the interval of the tide is considered as 12 hours instead, as it really is, about $12^h\ 25^m$. The hours are expressed in Greenwich mean time.

The same symbol is adapted to the case of a stream flowing longer in one direction than in the other. Thus in the Sound of Sanda the stream at full and change may be indicated by

indicating that it flows seven hours towards the west and five hours to the east.

The velocity of the stream may be expressed by the length of the figure, or sometimes more conveniently by separate lines, the terminations of which are well marked as |———|, the length of the lines indicating either the velocity at the middle of the stream, or, if it is found more convenient, the whole distance which a particle of water moves in one tide.

I may observe that an analogous symbol may be used to express the more complicated tides which occur where different streams meet. Thus near the Eddystone the stream may be expressed by a symbol of this kind :—



the bearing and distance of the centre of the diagram from any numeral indicating the direction and rate of stream at that hour.

The time of high and low water in the region which we are considering may be thus described. Near the two extremities, viz. the Giant's Causeway and the Island of Eysdill, the time of high water at full and change is nearly V½ Greenwich time, being very nearly that due to the Great Atlantic tidal wave propagated from S.W. to N.E., and the same is very nearly the hour of high water on the chain of islands of which Isla, Jura and Scarba are the chief. But along the coast of the mainland of Ireland and Scotland the case is very different. Between these two countries is the great opening into the Liverpool basin, in which it is high water about XI. The change in the time of high water takes place by the following gradations :—At Giant's Causeway it is high water about VI., at Ballycastle VII., Torpoint X., Mull of Cantyre XI., Gigha II., Loch Killispoint IV., Eysdill and Scarba V, Jura and Islay V½. But while the hour of high water varies, the stream through nearly the whole of the region runs from X. to IV. in one direction and from IV. to X. in the other.

Between the Mull of Cantyre and the N.E. coast of Ireland, the X. to IV. stream runs to the north.

The most westerly part turns to the west, and runs through the Sound of Rathlin along the north coast of Ireland; the central part flows to the N.W. past the Rhynns of Islay; the easterly part, which has flowed partly through the Sound of Sanda, turns sharply round the Mull of Cantyre, and flows to the northward, pouring with great velocity through the narrow openings in the chain of islands, viz., the Sound of Islay between Islay and Jura, the Gulf of Corry Vreckan between Jura and Scarba, the little Corry Vreckan between Scarba and Lunga, the Slate Isles and the Cuan Sound; of these the little Corry Vreckan is quite impassable; and Corry Vreckan and the Cuan Sound are seldom attempted except near slack water.

These channels open into the basin which lies between Jura and Iona—a comparatively tideless sea, owing apparently to the circumstance of the ocean tide from the outside of Isla rising to nearly the same height as that which pours through the openings, so that the tidal stream would be little altered by building a dam from Islay to the Ross of Mull.

The question may now be asked, Is the great X. to IV. stream which has just been described a flood or an ebb tide? So far as regards the Mull of Cantyre, Fairhead, and all that lies to the south or east, it is a true ebb tide. So far as regards Jura, Scarba, and the coast to the north and east, it is a true flood tide; but as regards a great part of the region in question, it cannot be called either a flood or an ebb tide, and much confusion is occasioned in the Charts by attempting so to distinguish it.

At the south end of Gigha in the Admiralty charts an arrow is laid down, indicating that the flood tide runs to the northward; and a few miles south of this, another arrow is laid down, indicating that the flood tide runs to the southward. From these arrows we might expect to find at this place a meeting of the tide and a sudden change in the direction of the flood stream. But the stream which is indicated by these arrows is nothing more than the great X. to IV. northerly current which we have described. The spot which is treated as a meeting of the tides, is merely that at which it is high water at I. North of this, for more than three hours of the X. to IV. stream, the tide is rising, and it is indicated as a flood tide. South for more than three hours the tide is falling, and it is indicated as an ebb tide.

On the south and west coast of Isla the confusion is greater. In some of the charts, the incoming stream is marked as the flood, in others, and perhaps with better reason, the outgoing tide. It is in truth neither the one nor the other.

The extreme complication which arises from describing the time of change of the stream by reference to the time of high and low water will now appear; thus we should have to say that in the Sound of Sanda, the ebb stream begins two hours before high water; at the Mull of Cantyre, one hour before high water; a little north of this again two hours before high water. At the south of Gigha we might say indifferently, that the flood tide runs to the south and begins three hours before low water, or that it runs to the north and begins three hours after low water; in the Sounds of Islay and the Gulf of Corry Vreckan that it begins an hour before low water; and in describing the streams along the north coast of Ireland, we have even greater complication.

The direction of the tidal streams on the rest of the west coast of Scotland is easily described. The X. to IV. stream, through the course which I have described, becomes an XI. to V. stream at the outside of Isla, and through the Sound of Iona. The stream which sets to the northward up the Sound of Jura fills the Linnhe Loch, and causes high water at the south end of the Sound of Mull at half-past V., whilst the high water caused by the ocean tide at the north end of the Sound of Mull is an hour later; the consequence, as may easily be seen, is that nearly the whole flood tide through the Sound of Mull runs to the northward, and the nearly whole ebb tide runs to the southward.

The tides round the island of Skye are comparatively simple. The V. to XI. tide from the outside of Mull is gradually retarded to a VI. to XII. stream at the outside of Skye, and then as it rounds the north end of Skye, it is met by the tidal stream which has rounded the north end of the island of Lewis, and bends round into the inner Sound of Skye, where it becomes a VII. to I. tide; the course of both streams being nearly the same as if there were an embankment from Loch Shell in the island of Lewis to Ru Rea on the coast of Ross-shire. At the same time, another branch of the tide which has rounded the point of Ardnamurchan flows through the Sound of Skye as a XII. to VI. tide, and being an hour earlier than the tide which has rounded the north end of Skye, it pours with great velocity through Kyle Rea, but only to fill Loch Alsh and Loch Duich; the retardation which it meets with in so doing, making the rise inside of the narrows at Kyle Akin so nearly contemporaneous with the rise outside, that there is little stream through that narrow opening; the flood stream, as I am informed, sometimes flowing in one direction and sometimes in the other, according to the prevailing winds.

There are many more minute details in these streams which have features of great interest. I have not, however, ventured in the present imperfect state of the data which we possess to enter upon these. I venture, however, to express a hope that before the survey is completed, the data may be obtained for showing, and that the charts may show the direction and rate of the stream at every place and at any time.

March 15, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communication was read:—

"On a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust." By John Evans, F.R.S., Sec. G.S. Received February 28, 1866.

At a time when the causes which have led to climatal changes in various parts of the globe are the subject of so much discussion, but little apology is needed for calling the attention of this Society to what possibly may have been one of these causes, though it has apparently hitherto escaped observation.

That great changes of climate have taken place, at all events in the northern hemisphere of the globe, is one of the best established facts of geology, and that corresponding changes have not been noticed to the same extent in the southern hemisphere may possibly be considered as due, rather to a more limited amount of geological observation, than to an absence of the phenomena indicative of such alterations in climatal conditions having occurred.

The evidence of the extreme refrigeration of this portion of the earth at the Glacial Period is constantly receiving fresh corroboration, and various theories have been proposed which account for this accession of cold in a more or less satisfactory manner.

Variations in the distribution of land and water, changes in the direction of the Gulf-stream, the greater or less eccentricity of the earth's orbit, the passage of the Solar System through a cold region in space, fluctuations in the amount of heat radiated by the sun, alternations of heat and cold in the northern and southern hemispheres, as consequent upon the precession of the equinoxes, and even changes in the position of the centre of gravity of the earth and consequent displacements of the polar axis, have all been adduced as causes calculated to produce the effects observed; and the reasoning founded on each of these data is no doubt familiar to all.

The possibility of any material change in the axis of rotation of the earth has been so distinctly denied by Laplace* and all succeeding astronomers, that any theory involving such a change, however tempting as affording a solution of certain difficulties, has been rejected by nearly all geologists as untenable.

Sir Henry James†, however, writing to the 'Athenæum' newspaper in 1860, stated that he had long since arrived at the conclusion that there was no possible explanation of some of the geological phenomena testifying to the climate at certain spots having greatly varied at different periods, without the supposition of constant changes in the position of the axis of the earth's rotation. He then, assuming as an admitted fact that the earth is at present a fluid mass with a hardened crust, showed that slaty cleavage, dislocations, and undulations in the various strata are results which might be expected from the crust of the earth having to assume a new external form, if caused to revolve on a new axis, and advanced the theory that the elevation of mountain-chains of larger extent than at present known produced these changes in the position of the poles.

The subject was discussed in further letters from Sir Henry James, the Astronomer Royal, Professors Beete Jukes and Hennessy, and others, but throughout the discussion the principal question at issue seems to have been whether any elevation of a mountain-mass could sensibly affect the position of the axis of rotation of the globe as a whole, and the general verdict was in the negative.

At an earlier period (1848) the late Sir John Lubbock, in a short but conclusive paper in the 'Quarterly Journal of the Geological Society'‡ pointed out what would have been the effect had the axis of rotation of the earth not originally corresponded with the axis of figure, and also mentioned some considerations which appear to have been absent from Laplace's calculations.

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* Méc. Cél., vol. v. p. 14.

† Athenæum, Aug. 25, 1860, &c.

‡ Vol. v. p. 5.

Sir John Lubbock, however, in common with other astronomers, appears to have regarded the earth as consisting of a solid nucleus with a body of water distributed over a portion of its surface; and there can be but little doubt that, on this assumption of the solidity of the earth, the usually received doctrines as to the general persistence of the direction of the poles are almost unassailable.

Directly, however, that we argue from the contrary assumption that the solid portion of the globe consists of a comparatively thin, but to some extent rigid crust with a fluid nucleus of incandescent mineral matter within, and that this crust, from various causes, is liable to changes disturbing its equilibrium, it becomes apparent that such disturbances may lead, if not to a change in the position of the general axis of the globe, yet at all events to a change in the relative positions of the solid crust and the fluid nucleus, and in consequence to a change in the axis of rotation, so far as the former is concerned.

The existence in the centre of the globe of a mass of matter fluid by heat, though accepted as a fact by many, if not most geologists, has no doubt been called in question by some, and among them a few of great eminence. The gradual increase of temperature, however, which is found to take place as we descend beneath the surface of the earth, and which has been observed in mines and deep borings all over the world, the existence of hot springs, some of the temperature of boiling water, and the traces of volcanic action, either extinct or still in operation, which occur in all parts of the globe, afford strong arguments in favour of the hypothesis of central heat.

And though we are at present unacquainted with the exact law of the increment of heat at different depths, and though, no doubt, under enormous pressure the temperature of the fusing-point of all substances may be considerably raised, yet the fact of the heat increasing with the depth from the surface seems so well established that it is highly probable that at a certain depth such a degree of heat must be attained as would reduce all mineral matter with which we are acquainted into a state of fusion. When once this point was attained, it seems probable that there would be no very great variation in the temperature of the internal mass; but whether the whole is in one uniform state of fluidity, or whether there is a mass of solid matter in the centre of the fluid nucleus, are questions which do not affect the hypothesis about to be considered.

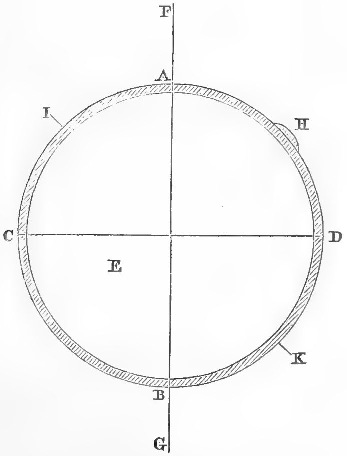
Those who are inclined to regard the earth as a solid or nearly solid mass throughout, consider that many volcanic phenomena may be accounted for on the chemical theory, which has received the support, among others, of Sir Charles Lyell. But apart from the consideration that such chemical action must of necessity be limited in its duration, the existence of local seas of fluid matter, resulting from the heat generated by intense chemical action, would hardly account for the increase of heat at great depths in places remote from volcanic centres; and the rapid transmission of shocks of earthquakes and the enormous amount of upheaval and subsidence as evidenced by the thickness of the sedimentary strata, seem inconsistent either with the general solidity of the globe or any very great thickness of its crust.

The supposition that the gradual oscillations of the surface of the earth, of which we have evidence all over the world as having taken place ever since the formation of the earliest known strata up to the present time, are due to the alternate inflation by gas and the subsequent depletion of certain vast bladdery cavities in the crust of the earth, can hardly be generally accepted.

Those who wish to see the arguments for and against the theory of there being a fluid nucleus within the earth's crust, will find them well and fairly stated in Naumann's 'Lehrbuch der Geognosie'*. My object is, not to discuss that question, but to point out what, assuming the theory to be true, would be some of the effects resulting from such a condition of things, more especially as affecting climatal changes. The agreement or disagreement between these hypothetical results and observed facts may ultimately assist in testing the truth of the assumption.

The simplest form in which we can conceive of the relations to each other of a solid crust and a fluid nucleus in rotation together is that of a sphere.

Let A C B D be a hollow sphere composed of solid materials and of perfectly uniform thickness and density, and let it be filled with the fluid matter E, over which the solid shell can freely move, and let the whole be in uniform rotation about an axis F G, the line C D representing the equator.



It is evident that in such a case, the hollow sphere being in perfect equilibrium, its axis and that of its fluid contents would perpetually coincide.

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* 2nd edit., 1858, vol. i. p. 36.

If, however, the equilibrium of the shell or crust be destroyed, as, for instance, by the addition of a mass of extraneous matter at H, midway between the pole and the equator, not only would the position of the axis of rotation be slightly affected by the alteration in the position of the centre of gravity of the now irregular sphere, but the centrifugal force of the excess of matter at H would gradually draw over the shell towards D until, by sliding over the nucleus, it attained its greatest possible distance from the centre of revolution by arriving at the equator. The resultant effect would be that though the whole sphere continued to revolve around an axis as nearly as possible in the line F G, yet the position of the pole of the hollow shell would have been changed by 45°, as by the passage of H to the equator the points I and K would have been brought to the poles by spirals constantly decreasing in diameter, while A and B, by spirals constantly increasing, would have at last come to describe circles midway between the poles and the equator.

The axis of rotation of the hollow sphere and that of its fluid contents would now again coincide, and would continue to do so perpetually unless some fresh disturbance in the equilibrium of the shell took place.

If instead of the addition of fresh matter at H we had supposed an excavation or removal of some portion of the shell, a movement in the axis of rotation of the shell would also have ensued, since from the diminished centrifugal force of that portion of the hollow sphere where the excavation had taken place, it would no longer equipoise the corresponding portion on the opposite side at I, and the excavated spot would eventually find its way to the pole.

In order more clearly to exhibit these effects, I have prepared a model in accordance with a suggestion of Mr. Francis Galton, F.R.S., in which a wheel representing a section of a hollow sphere has its axis, upon which it can freely turn, fixed in a frame, which is itself made to revolve in such a manner that the axis of its rotation passes through one of the diameters of the wheel, and coincides with what would be the axis of the sphere of which the wheel is a section.

In the periphery of the wheel are a number of adjustable screws with heavy heads, so that, by screwing any of them in or out, the addition of matter or its abstraction at any part of the sphere may be represented.

If by adjusting these screws the wheel could be brought into perfect equilibrium, its position upon its own axis would remain unchanged in whatever position it was originally placed, notwithstanding any amount of rotation being given to the frame in which it is hung; but practically it is found that with a certain given position of the screws a certain part of the wheel coincides with the axis of the frame, or becomes the pole around which the sphere revolves. The rim of the wheel is graduated so as to show the position of the poles in all cases, and generally speaking the wheel always settles down after rotation with the pole within three or four degrees of the same spot, if no alteration has been made in the adjustment of the screws, though of course what was the uppermost pole may become the lower one; and in some cases the wheel may be *in equilibrio* with a projecting screw either above or below the equator, in which case there may be four readings on the circle at the index-point, according as the one pole or the other is uppermost, and the projecting screw is above or below the equator.

With the screws on the wheel evenly balanced, a slight alteration in the adjustment of any of them immediately tells upon the position of what, for convenience sake, may be called the poles, except, indeed, in such cases as screwing outwards those already at the equator, or making similar alterations in the adjustment of two screws at equal distances on either side of one of the poles. If a screw be turned outwards so as notably to project at any spot, no matter how near to the pole, it will be found, after the machine has been a short time in revolution, in the region of the equator. Or again, if one or, better still, two opposite screws at the equator be turned inwards, they will be found after a short period of revolution at the poles.

Now let us assume for a moment that, though the crust was partially covered by water, the earth, instead of being a spheroid, was a perfect sphere, consisting of a hardened crust of moderate thickness supported on a fluid nucleus over which the crust could travel freely in any direction, but both impressed with the same original rotatory motion, so that without some disturbing cause they would continue to revolve for ever upon the same axis, and as if they were one homogeneous body. Let us assume, moreover, that this crust, though in perfect equilibrium on its centre of rotation, was not evenly spherical externally, but had certain projecting portions, such as would be represented in Nature by continents and islands rising above the level of the sea.

It is evident that so long as those continents and islands remained unaltered in their condition and extent, the relative position of the crust to the enclosed fluid nucleus would remain unaltered also. But supposing those projecting masses were either further upheaved from some internal cause, or worn down and ground away by the sea or by subaërial agency and deposited elsewhere, it seems impossible but that the same effects must ensue as we see resulting upon the model from the elevation and depression of certain screws, and that the axis of rotation of the crust of the sphere would be changed in consequence of its having assumed a fresh position upon its fluid nucleus, though the axis of the whole sphere might have retained its original direction, or have altered from it only in the slightest degree.

An irregular accumulation of ice at one or both of the poles, such as supposed by M. Adhémar, would act in the same manner as an elevation of the land; and even assuming that the whole land had disappeared from above the surface of the sea, yet if by marine currents the shallower parts of the universal ocean were deepened and the deeper parts filled up, there would, owing to the different specific gravity of the transported soil and the displaced water, be a disturbance in the equilibrium of the crust, and a consequent change in the position of its axis of rotation.

Now if all this be true of a sphere, it will also, subject to certain modifications, be true of a spheroid so slightly oblate as our globe.

The main difference in the two cases is, that in a sphere the crust may assume any position upon the nucleus without any alteration in its structure, while in the case of the movement of a spheroidal crust over a similar spheroidal nucleus, every portion of its internal structure must be more or less disturbed as the curvature at each point will be slightly altered.

The extent of the resistance to an alteration of position arising from this cause will depend upon the oblateness of the spheroid and the thickness and rigidity of the crust; while the thicker the latter is, the less also will be the proportionate effect of such elevations, subsidences, and denudations as those with which we are acquainted. The question of friction upon the nucleus is also one that would have to be considered, as the internal matter though fluid might be viscous.

It will of course be borne in mind that the elevations and depressions of the surface of the globe are not, on the theory now under consideration, regarded according to the proportion they bear to the earth's radius, but according to their relation to the thickness of the earth's crust; and that, even assuming Mr. Hopkins's extreme estimate to be true, yet elevations or depressions, such as we know to have taken place, of 8000 or 10,000 feet, bear an appreciable ratio to the 800 or 1000 miles which he assigns as the thickness of the earth's crust.

It is, however, to be remarked that the extremely ingenious speculations of Mr. Hopkins are based on the phenomena of precession and nutation, and that if once the possibility of a change in the position of the axis of rotation of the earth's crust be admitted, it is not improbable that the value of some of the data upon which the calculations of these movements are founded may be affected.

The supposition of the thickness of the crust being so great seems also not only entirely at variance with observed facts as to the increase of heat on descending beneath the surface of the earth, but to have been felt by Mr. Hopkins himself to offer such obstacles to any communication between the surface of the globe and its interior, that he has had recourse to an hypothesis of large spaces in the crust at no great depth from the surface, and filled with easily-fusible materials, in order to account for volcanic and other phenomena.

But though it may be possible to account for volcanoes upon such an assumption, yet, as already observed, the phenomena of elevation and depression, such as we find to have taken place, and more especially the existence of vast geological faults, cannot without enormous difficulty be reconciled with such a theory.

Taking the increment of heat as 1° Fahrenheit for every 55 or 60 feet* in descent, a temperature of 2400° Fahr. would be reached at about 25 miles, sufficient to keep in fusion such rocks as basalt, greenstone, and porphyry; and such a thickness appears much more consistent with the fluctuations in level, and the internal contortions and fractures of the crust which are everywhere to be observed. Sir William Armstrong, on the assumption of the temperature of subterranean fusion being 3000° Fahr., considers that the thickness of the film which separates us from the fiery ocean beneath would be about 34 miles.

Even assuming a thickness of 50 miles, so as to make still greater allowance for the increased difficulty of fusion under heavy pressure, the thickness of the crust would only form one-eightieth part of the radius of the earth; or if we represent the earth by a globe 13 feet in diameter, the crust would be one inch in thickness, while the difference between the polar and equatorial diameters would be half an inch.

In such a case, the elevation or wearing away of continents such as are at present in existence, rising, as some of them do, nearly a quarter of a mile on an average above the mean sea-level, would cause a great disturbance in the equilibrium of the crust, sufficient to overcome considerable resistance in its attempts to regain a state of equilibrium by a movement over its fluid nucleus.

Whether the thickness of the earth's crust was not in early geological times less than at present, so as to render it more susceptible of alterations in position—whether the spheroid of the fluid mineral nucleus corresponds in form with the spheroid of water which gives the general contour of the globe—whether or no there are elevations and depressions upon the nucleus corresponding to some extent with the configuration of the outer crust, and whether the motion of the crust upon it, besides effecting climatal changes, might not also lead to some elevations and depressions of the land, and produce some of the other phenomena mentioned by Sir Henry James, are questions which I will leave for others to discuss.

My object is simply to call attention to what appears to me the fact, that if, as there seems reason to suppose, our globe consists of a solid crust of no great thickness resting on a fluid nucleus, either with or without a solid central core, and if this crust, as there is abundant evidence to prove, is liable to great disturbances in its equilibrium, then it of necessity follows that changes take place in the position of the crust with regard to the nucleus, and an alteration in the position of the axis of rotation, so far as the surface of the earth is concerned, ensues.

Without in the slightest degree undervaluing other causes which may lead to climatal changes, I think that possibly we may have here a *vera causa* such as would account for extreme variations from a Tropical to an Arctic temperature at the same spot, in a simpler and more satisfactory manner than any other hypothesis.

* Page, 'Advanced Text-book of Geology,' p. 30.

The former existence of cold in what are now warm latitudes might, and probably did in part, arise from other causes than a change in the axis of rotation, but no other hypothesis can well account for the existence of traces of an almost tropical vegetation within the Arctic circle.

Of the former existence of such a vegetation, the evidence, though strong, is not conclusive. But if the fossil plants of Melville Island, in lat. 75° N.*, which appear to agree generically with those from the English coal-measures, really grew upon the spot where they were now discovered, they seem to afford conclusive evidence of a change in the position of the pole since the period at which they grew, as such vegetation must be considered impossible in so high a latitude.

The corals and Orthoceratites from Griffiths Island and Cornwallis Island, and the liassic Ammonites from Point Wilkie, Prince Patrick's Island, tell the same story of the former existence of something like a sub-tropical climate at places at present well within the Arctic circle.

To use the words of the Rev. Samuel Haughton†, in describing the fossils collected by Sir F. L. M^cClintock, "The discovery of such fossils *in situ*, in 76° N. latitude, is calculated to throw considerable doubt upon the theories of climate, which would account for all past changes of temperature by changes in the relative position of land and water on the earth's surface;" and I think that all geologists will agree with this remark, and feel that if the possibility of a change in the position of the axis of rotation of the crust of the earth were once admitted, it would smooth over many difficulties they now encounter.

That some such change is indeed taking place at the present moment may not unreasonably be inferred from the observations of the Astronomer Royal, who, in his Report to the Board of Visitors for 1861, makes use of the following language, though "only for the sake of embodying his description of the observed facts," as he refers the discrepancies noticed to "some peculiarity of the instrument. . . . . The Transit Circle and Collimators still present those appearances of agreement between themselves and of change with respect to the stars which seem explicable only on one of two suppositions—that the ground itself shifts with respect to the general Earth, or that the Axis of Rotation changes its position."

March 22, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

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* Lyell, 'Principles of Geology,' 1853, p. 88.

† Journal of the Royal Dublin Society, vol. i. p. 244.

I. "On the Action of Trichloride of Phosphorus on the Salts of the Aromatic Monamines." By A. W. Hofmann, LL.D., F.R.S., &c. Received March 3, 1866.

The starting-point of the following experiments was an accidental observation. Whilst investigating the chlorine-, bromine-, and nitro-derivatives of aniline, I had prepared a large quantity of phenylacetamide by the action of chloride of acetyl on aniline. From the hydrochlorate of aniline, abundantly produced as a by-product in this reaction, the aniline was recovered by treating the mother-liquors with hydrate of sodium. During the distillation, after the greater part of the aniline had passed over and collected in the receiver, a tenacious oily fluid began to come over, adhering to the tube of the condenser and gradually becoming a crystalline mass. It was easily purified by washing with cold, and crystallization from hot alcohol.

Beautiful white leafy crystals were thus obtained, fusible at 137°, and volatile without decomposition at a temperature beyond the range of the mercury-thermometer. These crystals are almost insoluble in water, difficultly soluble in cold, but soluble in hot alcohol, and also soluble in ether. The solutions are neutral.

In acids the crystals are also easily soluble; an alkali precipitates the original substance unaltered from the solutions. The hydrochloric-acid solution yields, with tetrachloride of platinum, a difficultly soluble crystalline precipitate. The new substance thus exhibits the deportment of a well-characterized base. Its composition was readily determined by combustion with oxide of copper, the analytical results pointing unequivocally to the formula

$$C_7 H_7 N$$

as the simplest atomic expression for the new body. But the whole behaviour of the substance, and more especially its transformation, by means of concentrated sulphuric acid, into aniline and acetic acid, leave no doubt that the above expression must be doubled, and that the new base is represented by the formula

$$C_{14} H_{14} N_2.$$

This formula was confirmed by the analysis of the platinum-salt already mentioned, and that of the nitrate which separates from the solution as an oily fluid, gradually changing to a beautiful crystalline compound; the former contains

$$2(C_{14} H_{14} N_2 HCl), Pt^* Cl_4,$$

the latter

$$C_{14} H_{14} N_2, HNO_3.$$

Whence is this body derived? and how is its formula to be interpreted? An answer to these questions was furnished by examining the chloride of acetyl employed in preparing the phenylacetamide. When on distilling the chloride the principal product had passed over, the thermometer gra-

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* Pt=198.

dually rose from 55° to 78°. The last portion which came over was pure trichloride of phosphorus. It was obvious that this substance must have played a part in the formation of the new compound.

I therefore submitted phenylacetamide to the action of trichloride of phosphorus. The new body was formed, but in very unsatisfactory quantity. The result of the experiment was essentially different when phenylacetamide and aniline in varying proportions were jointly submitted to the action of trichloride of phosphorus. The amount of substance obtained varied with the composition of the mixture, and appeared greatest when the mixture was made in the proportion of one part of trichloride of phosphorus, two parts of aniline, and three parts of phenylacetamide. Hence the reaction had taken place according to the following equation:

$$3C_6H_7N+3C_8H_9NO+PCl_3=3C_{14}H_{14}N_2+H_3PO_3+3HCl.$$

Perfectly similar results were obtained when a proportionate quantity of hydrochlorate of aniline was employed instead of aniline in this experiment.

The idea naturally presented itself to produce the same result without taking the trouble of preparing and purifying the phenylacetamide by including its preparation in the very process of forming the new compound. For this purpose 6 molecules of aniline were added to 3 molecules of chloride of acetyl, and the dense liquid thus obtained mixed with 1 molecule of trichloride of phosphorus. The result could not have been better:

$$6C_6H_7N+3C_2H_3OCl+PCl_3=3C_{14}H_{14}N_2+H_3PO_3+6HCl.$$

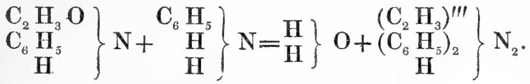
A simple additional step and the true mode of preparing the substance, and with it the general method for the production of an endless variety of analogous bodies, was found. Evidently it was not even necessary to prepare the chloride of acetyl separately. The new body must be as easily obtained by the direct action of trichloride of phosphorus on aniline and acetic acid. The mixture had only to be made in such a manner that, after transforming the acetic acid into chloride of acetyl, there was a sufficient amount of trichloride of phosphorus left to perform the rest of the work. In this case, therefore, 6 molecules of aniline and 3 molecules of acetic acid had to be added to 2 molecules of trichloride of phosphorus,

$$6C_6H_7N+3C_2H_4O_2+2PCl_3=3C_{14}H_{14}N_2+2H_3PO_3+6HCl.$$

The reaction is violent, and the operation must be performed with care. The substances are mixed according to the above equation. For this purpose three parts by weight of aniline are added to one part of acetic acid and into the mixture surrounded by cold water two parts of trichloride of phosphorus are slowly poured; in which proportion the latter compound is in slight excess. The tenacious fluid thus obtained is then heated for a couple of hours to 160°. On cooling, it solidifies to a hard, friable, translucent, resinous mass of a light-brown colour, which dissolves in boiling water almost without residue, leaving only traces of an amorphous yellow substance containing phosphorus behind. The clear filtered solution, after cooling, yields, on the addition of soda, a white crystalline precipitate, which requires only to be washed and recrystallized from alcohol.

The foregoing equations give a pretty clear idea of the qualitative and quantitative nature of the experiment, but they do not afford us an insight into the true mechanism of the reaction. This, nevertheless, is a very simple one. Trichloride of phosphorus acts water-generating and water-withdrawing. The oxygen required for this purpose is supplied by the phenylacetamide; as, however, the molecule of this compound contains but one atom of hydrogen belonging to the original ammonia skeleton, a molecule of aniline is required in addition to supply the second atom of hydrogen. In this manner a diamine is formed, in which two atoms of the hydrogen of the double ammonia type are replaced by two univalent phenyl-residues, and three of the remaining hydrogen atoms by the trivalent group C_2H_3, to which the term *ethenyl** may for the present be applied.

The new body would thus become *ethenyldiphenyldiamine*, the formation of which in the most simple form would depend upon the removal of 1 mol. of water from 1 mol. of phenylacetamide and 1 mol. of aniline.



$$\left.\begin{matrix} C_2H_3O \\ C_6H_5 \\ H \end{matrix}\right\}N+\left.\begin{matrix} C_6H_5 \\ H \\ H \end{matrix}\right\}N=\left.\begin{matrix} H \\ H \end{matrix}\right\}O+\left.\begin{matrix} (C_2H_3)''' \\ (C_6H_5)_2 \\ H \end{matrix}\right\}N_2.$$

I was anxious to test this assumption by experiment.

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* The term *ethenyl* proposed for the group C_2H_3, which in the new compound functions with the value of 3 atoms of hydrogen, is framed according to a system of nomenclature to which I have occasionally resorted, in the boundless confusion of names now prevailing in organic chemistry, as a means of communication with my pupils. Perhaps this system is capable of further development.

It is a peculiar feature of the development of modern chemistry that more than ever before is felt the necessity of grouping the organic compounds round the hydrocarbons. The question therefore may be said to reduce itself to the discovery of a good principle of nomenclature for the compounds of hydrogen and carbon. Many attempts have been made in this direction, as yet without any acceptable results.

For the purpose of framing my names I fuse the method originally employed by Laurent with the principle proposed by Gerhardt, and more or less adopted by his successors.

An example will illustrate my mode of proceeding. Let us consider the most important of all the series of hydrocarbons, the homologues of marsh-gas. All the members of this series I make terminate in *ane*, distinguishing the order of succession by prefixing the first syllable of the Latin numeral corresponding to the number of carbon atoms in the molecule. From this rule the first three members of the series are conveniently excepted, their names having been so long in use as to render it desirable to embody them in the system.

By the removal of 1 atom of hydrogen from the hydrocarbon, the latter ceases to be a *saturated* compound, and the remaining group of atoms becomes *univalent*. The termination *yl* now takes the place of *ane*. A second atom of hydrogen is removed, the group becomes *bivalent* and now terminates in *ene*; a third hydrogen atom is separated, the group is again raised in value by one becoming in fact *trivalent*, and acquires the termination *enyl*. By the removal of the fourth and fifth atom of hydrogen the

At 100° iodide of ethyl has no action on ethenyldiphenyldiamine, but at about 150° the two bodies react on one another. The mixture, after being heated for five or six hours, yielded, upon cooling, beautiful crystals of an iodide. By treatment with chloride of silver this iodide was converted into the corresponding chloride, and then into the platinum-salt. Analysis of this salt showed that the ethyl group had been once taken up. On the addition of caustic soda the corresponding base was separated. It is a thick oily fluid, which, when shaken with water, *does not impart to it the slightest alkaline reaction.* By renewed treatment with iodide of ethyl an iodide, it is true, was separated, but it was proved on examination that the ethyl group had not been assimilated a second time. In the sense of the above assumption, this nevertheless ought to have taken place. The experiment was therefore repeated with iodide of methyl which is well known to act much more powerfully than the ethyl-compound. This body attacks the ethylated base even at 100°. The iodide thus obtained, when decomposed by oxide of silver, yielded a *strongly alkaline liquid,* whence

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quantivalence of the residuary group again increases, the groups becoming *quadrivalent* and *quintivalent,* and acquiring the terminations *ine* and *inyl,* &c.

According to this principle the following names are formed:

Methane,	$(C\ H_4)^0$	Sextane,	$(C_6\ H_{14})^0$
Ethane,	$(C_2\ H_6)^0$	Septane,	$(C_7\ H_{16})^0$
Propane,	$(C_3\ H_8)^0$	Octane,	$(C_8\ H_{18})^0$
Quartane,	$(C_4\ H_{10})^0$	Nonane,	$(C_9\ H_{20})^0$
Quintane,	$(C_5\ H_{12})^0$	Decane,	$(C_{10}\ H_{22})^0$

And further:

Methane,	$(C\ H_4)^0$	Ethane,	$(C_2\ H_6)^0$	Propane,	$(C_3\ H_8)^0$	Quartane,	$(C_4\ H_{10})^0$
Methyl,	$(C\ H_3)'$	Ethyl,	$(C_2\ H_5)'$	Propyl,	$(C_3\ H_7)'$	Quartyl,	$(C_4\ H_9)'$
Methene,	$(C\ H_2)''$	Ethene,	$(C_2\ H_4)''$	Propene,	$(C_3\ H_6)''$	Quartene,	$(C_4\ H_8)''$
Methenyl,	$(C\ H\ )'''$	Ethenyl,	$(C_2\ H_3)'''$	Propenyl,	$(C_3\ H_5)'''$	Quartenyl,	$(C_4\ H_7)'''$
		Ethine,	$(C_2\ H_2)^{iv}$	Propine,	$(C_3\ H_4)^{iv}$	Quartine,	$(C_4\ H_6)^{iv}$
		Ethinyl,	$(C_2\ H\ )^{v}$	Propinyl,	$(C_3\ H_3)^{v}$	Quartinyl,	$(C_4\ H_5)^{v}$
				Propone,	$(C_3\ H_2)^{vi}$	Quartone,	$(C_4\ H_4)^{vi}$
				Proponyl,	$(C_3\ H)^{vii}$	Quartonyl,	$(C_4\ H_3)^{vii}$
						Quartune,	$(C_4\ H_2)^{viii}$
						Quartunyl,	$(C_4\ H\ )^{ix}$

This is not the place to develope this subject further. The short notice I have given must suffice. A superficial examination of the system shows, however, how large a number of groups of atoms may be clearly and succinctly expressed in it.

It appeared convenient to submit the plan to a provisional test by framing some of the names required for the substances which were furnished by the above experiments.

Bodies containing oxygen may be as simply nominated according to this plan.

The acid derived from *ethylic alcohol* is *ethoxylic acid* (acetic acid), the first acid corresponding to *ethenic alcohol* would be *ethoxenic acid* (glycolic acid), the second being *ethdioxenic acid* (oxalic acid). We speak of the *oxylic*, *oxenic*, and *dioxenic acids* of a series, of the *quartane series*, for instance, and any one would understand that by these expressions are meant butyric, butylactic, and succinic acids.

it was at once inferred that the methyl group had been added to the ethyl group already present in the substance. This conclusion was amply corroborated by the analysis of the platinum-salt precipitated from the liquid.

By this experiment the nature of ethenyldiphenyldiamine is most satistorily elucidated. The action of iodide of ethyl had converted this base into the tertiary diamine *ethenylethyldiphenyldiamine,*

$$\left.\begin{matrix}(C_2H_3)'''\\(C_2H_5)\\(C_6H_5)_2\end{matrix}\right\}N_2\,;$$

the latter, under the influence of iodide of methyl, yielding the compound

$$\left.\begin{matrix}[(C_2H_3)'''(C_2H_5)(C_6H_5)_2N_2](C\,H_3)\\H\end{matrix}\right\}O,$$

which is soluble in water with a strongly-marked alkaline reaction.

The stability of ethenyldiphenyldiamine is remarkable. As I have already mentioned, this base distils at a very high temperature without decomposition. It is moreover scarcely attacked by fusion with hydrate of potassium. Concentrated sulphuric acid, on the other hand, decomposes it easily. When gently heated, the solution of ethenyldiphenyldiamine in sulphuric acid evolves acetic acid, and, on addition of water, the slightly-coloured liquid solidifies to a white crystalline mass of *sulphanilic acid,*

$$\left.\begin{matrix}(C_2H_3)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2+2H_2SO_4=\left.\begin{matrix}C_2H_3O\\H\end{matrix}\right\}O+2\left[\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N,SO_3\right]$$

It need scarcely be mentioned that, by reactions similar to that of trichloride of phosphorus on acetate of aniline, an almost endless variety of new compounds may be obtained. By changing the acid, or base, or both, a series of substances is formed, the composition of which in each case is fixed in advance by theory. I have worked only very little in this direction.

Toluidine acts in a manner precisely similar to that of aniline. The base formed can scarcely be distinguished from the phenyl base. Analysis of the platinum-salt has led to the formula

$$C_{16}H_{18}N_2=\left.\begin{matrix}(C_2H_3)'''\\(C_7H_7)_2\\H\end{matrix}\right\}N_2.$$

With *naphthylamine* the reaction is less smooth. The product obtained by acting with 1 molecule of trichloride of phosphorus on 3 molecules of chloride of acetyl and 6 molecules of naphthylamine, was an unenjoyably viscous scarcely crystalline mass which retained, after repeated solution and precipitation, its amorphous character. An analysis of the platinum-salt led, however, to the formula

$$C_{22}H_{18}N_2=\left.\begin{matrix}(C_2H_3)'''\\(C_{10}H_7)_2\\H\end{matrix}\right\}N_2.$$

Aniline, toluidine, and naphthylamine being *primary* monamines, it seemed of interest also to extend the examination to a *secondary* one. For this purpose I selected *diphenylamine*. When a mixture of diphenylamine and phenylacetamide, in the proportion of their molecular weights, was submitted to the action of trichloride of phosphorus, the reaction took place in the ordinary way, but the mass precipitated from the solution of the chloride by ammonia could not be crystallized. It had therefore to be analyzed as platinum-salt. Determination of the platinum as well as combustion showed, however, that the expected *ethenyltriphenyldiamine* had been formed,

$$\left.\begin{array}{c}C_2H_3O\\C_6H_5\\H\end{array}\right\}N+\left.\begin{array}{c}C_6H_5\\C_6H_5\\H\end{array}\right\}N=\left.\begin{array}{c}H\\H\end{array}\right\}O+\left.\begin{array}{c}(C_2H_3)'''\\(C_6H_5)_2\\(C_6H_5)\end{array}\right\}N_2.$$

An entirely unexpected result, on the other hand, was obtained by the action of trichloride of phosphorus on a mixture of acetic acid and *methylaniline*. Working, as I did, exclusively with a secondary monamine, I had expected to see the reaction take place according to the following equation:—

$$3\left[\left.\begin{array}{c}CH_3\\C_6H_5\\H\end{array}\right\}N\right]+\left.\begin{array}{c}C_2H_3O\\H\end{array}\right\}O=2\left[\left.\begin{array}{c}H\\H\end{array}\right\}O\right]+\left.\begin{array}{c}(C_2H_3)'''\\(C\,H_3)_3\\(C_6H_5)_3\end{array}\right\}N_3.$$

But this was not the case; the action was found to have been very irregular; and amongst the products a chloride was observed, the base of which, when liberated by oxide of silver, dissolved in water *with an alkaline reaction*. When analyzed in the form of a platinum-salt, this body proved to be ethenyldiphenyldiamine, which had twice appropriated the methyl-group, having the composition

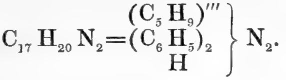
$$\left.\begin{array}{c}[(C_2H_3)'''(C_6H_5)_2(CH_3)N_2]\,CH_3\\H\end{array}\right\}O.$$

In this case evidently chloride of methyl had been eliminated from one of the molecules of methylaniline, which, acting on the ethenyldiphenylmethyldiamine, had given rise to the formation of the chloride corresponding to the above-mentioned oxide,

$$\begin{aligned}2[(C_6H_5)(CH_3)HN]+C_2H_3OCl=H_2O\\+[(C_2H_3)'''(C_6H_5)_2(CH_3)N_2](CH_3)Cl.\end{aligned}$$

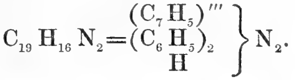
A few experiments made with the derivatives of valeric and benzoic acids are still to be mentioned.

Quintenyldiphenyldiamine.—For the preparation of this substance, 3 molecules of valeric acid were mixed with 6 molecules of aniline, and to the liquid, after cooling, 2 molecules of trichloride of phosphorus were added. This mixture, on being submitted for a couple of hours to a temperature of 150°, yielded a viscous mass soluble in water. From the solution hydrate of sodium precipitated a crystalline base almost insoluble in water, which was recrystallized from alcohol. This substance fused at 111°. The combustion of the body and the analysis of its platinum-salt, which crystallized in rhombic plates difficultly soluble in water and almost insoluble in alcohol, led to the formula



$$C_{17}H_{20}N_2=\left.\begin{matrix}(C_5H_9)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2.$$

Benzyldiphenyldiamine.—By substituting benzoic acid for valeric acid in the reaction just described, the corresponding benzyl-compound is obtained. I have prepared this substance by the action of 1 molecule of trichloride of phosphorus on a mixture of 3 molecules of phenylbenzamide and 3 molecules of hydrochlorate of aniline. The reaction takes place in the ordinary way. The product is a very weak base crystallizing in fine silky needles. The hydrochlorate forms thin brilliant plates difficultly soluble in water, which upon re-crystallization lose their acid. The analysis led to the formula

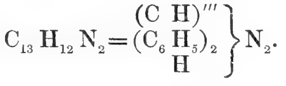


$$C_{19}H_{16}N_2=\left.\begin{matrix}(C_7H_5)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2.$$

This compound has been already observed by Gerhardt. He obtained it whilst examining the action of pentachloride of phosphorus on the amides, the last experiments which he performed before his death. A short notice of this investigation, found after his death, has been published by M. Cahours *.

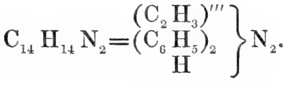
The phenyl-compounds of the acetic and valeric groups above described are naturally linked with a compound which several years ago I procured by an essentially different reaction. This substance, which at the time I described under the name of *formyldiphenyldiamine* †, but to which, in accordance with my present ideas on nomenclature, I would give the name *methenyldiphenyldiamine,* is obtained by the action of chloroform on aniline; its relation to the compounds before mentioned is seen by a glance at the following formulæ:—

Methenyldiphenyldiamine,



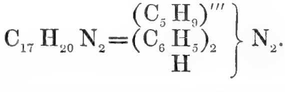
$$C_{13}H_{12}N_2=\left.\begin{matrix}(C\ H)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2.$$

Ethenyldiphenyldiamine,



$$C_{14}H_{14}N_2=\left.\begin{matrix}(C_2H_3)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2.$$

Quintenyldiphenyldiamine,



$$C_{17}H_{20}N_2=\left.\begin{matrix}(C_5H_9)'''\\(C_6H_5)_2\\H\end{matrix}\right\}N_2.$$

It seemed worth while to establish by a special experiment the analogy of methenyldiphenyldiamine, obtained in so different a reaction, with the substances just described. For this purpose I submitted *phenylform-*

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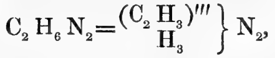
* Ann. Ch. Phys. [3], vol. liii. p. 302.

† Proceedings of the Royal Society, vol. ix. p. 229.

*amide** to the action of a mixture of aniline and trichloride of phosphorus. The result proved that the methenyl-compound can be thus prepared even more easily than by means of chloroform.

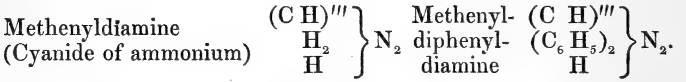
In conclusion, the relation must be mentioned which the compounds just described bear to the base obtained by Professor Strecker †, when acting with gaseous hydrochloric acid on acetamide. The body thus formed is



$$C_2H_6N_2 = \left.\begin{array}{c}(C_2H_3)'''\\ H_3\end{array}\right\} N_2,$$

and has been called *acediamine,* for which name, in accordance with the proposed nomenclature, I would now substitute the term *ethenyldiamine.* When compared with the analogous aniline-compound, the very slight stability of acediamine, which splits up with the greatest facility into acetic acid and ammonia, deserves to be noticed.

A *quintenyldiamine,* corresponding to the quintenyldiphenyldiamine, has not as yet been prepared. Methenyldiamine, on the other hand, is known, although the compound which I have in view has scarcely been looked upon as such. The body in question is no other than *cyanide of ammonium.*



$$\begin{array}{l}\text{Methenyldiamine}\\ \text{(Cyanide of ammonium)}\end{array} \quad \left.\begin{array}{c}(CH)'''\\ H_2\\ H\end{array}\right\} N_2 \quad \begin{array}{c}\text{Methenyl-}\\ \text{diphenyl-}\\ \text{diamine}\end{array} \quad \left.\begin{array}{c}(CH)'''\\ (C_6H_5)_2\\ H\end{array}\right\} N_2.$$

The facility with which this substance decomposes is well known; amongst the products *formic acid* and *ammonia* are invariably found.

It is further known that by heating ammonia with chloroform (trichloride of methenyl), cyanide of ammonium is formed, a reaction which is perfectly similar to that by which the analogous phenyl-base was originally obtained in the corresponding experiment with aniline.

In conclusion, I beg to thank Messrs. Tingle and Fischer for their valuable assistance during the performance of the experiments described.

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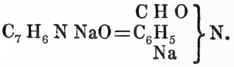
* On this occasion I prepared larger quantities of phenylformamide, which can be produced much more easily by digesting formic ether with aniline than by the process hitherto employed (distillation of oxalate of aniline). Phenylformamide has the remarkable property (not as yet observed) of being precipitated from its aqueous solution as a solid scarcely crystalline mass on addition of a strong solution of caustic soda. By separating this compound from the liquid, and purifying it as far as possible by rapid pressure between folds of bibulous paper, it was possible to make an analysis of it. Its composition was found to be



$$C_7H_6NNaO = \left.\begin{array}{c}CHO\\ C_6H_5\\ Na\end{array}\right\} N.$$

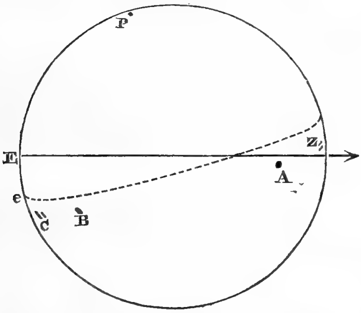
By the action of water upon it phenylformamide and hydrate of sodium are reproduced.

† Ann. Chem. Pharm., vol. ciii. p. 321.

II. "Notice of a Zone of Spots on the Sun." By John Phillips, M.A., LL.D., F.R.S., Professor of Geology in the University of Oxford. Received March 22, 1866.

During the latter half of February and the first half of March, spots of extremely varied character have appeared on the sun, and have been seen with great distinctness, in good observing weather, through the whole or parts of two semi-rotations. On the 13th of February, at 10^h 25^m, four spots were visible on the disk, in the situations marked Z, A, B, C in the diagram No. 1. In that diagram the apparent course of the sun's equator is marked by the curved line e, and the pole of rotation at P. Thus the four spots indicated for observation being all on the same side of the sun's equator, and all within the latitude of 10°, constitute a zone of spots. Since that date a fifth spot, D, still in the same zone, has appeared, following C. Of these Z was about to disappear; its reappearance was noted, and several remarkable changes in form were observed while it traversed half the disk, till its contraction to a black speck 1000 miles in diameter, after which it was obliterated. The spot B had advanced some distance on the disk, and was followed till the 21st of February, when it approached the edge, with indications of being a shallow concavity. It was not observed to reappear. The spots A and C require longer notice, both on account of their persistence through more than a rotation-period, and because of the remarkable changes which they have undergone.

Diagram No. 1.



The spot D is now under observation.

The spot A, visible from the 4th to the 16th of February, and again reappearing early in March, was solitary, and approximately round, measuring, on February 10, about 23,000 miles across the penumbra, and about 8000 across the umbra.

It had a clear brown tint over the whole penumbra, the body of the sun appearing fairly white, and a deeper brown tint over part of the umbra, the remaining and larger space of the umbra being black. The penumbral space was marked with the broken structure represented in a former communication*. The edges of the umbra and penumbra were much broken, the former running out into a sharp point on the apparent left, and including a lighter brown space.

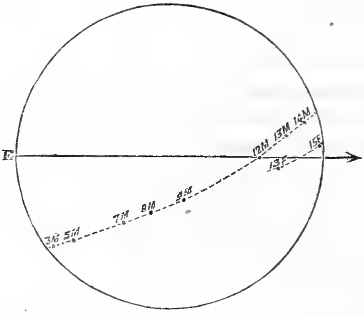
Except in this part, the dark umbra was uniformly and equally distant from the penumbral border, so that it offered an excellent object, large enough and distinct enough to be employed with confidence as a test of the depth of the umbra beneath the luminous photosphere.

In a former communication I assigned to an umbra of good figure, carefully observed, a depth of only 300 miles†. Since then M. Chacornac‡ has given the depth of 400 miles as an approach to average, though in some cases 1000 miles might be nearer. That the spots are most frequently sunk below the photosphere, as Wilson long since asserted, can no longer be doubted, since Mr. De la Rue, Mr. Stewart, and Mr. Loewy have recorded, after many measures of photographs taken at Kew, as the general result that the interior parts of the solar spot are sunk below the general surface§.

The spot now under consideration approached the edge of the disk on the 15th of February, when the umbra seemed nearly bridged across by a lighter part, and was perceptibly but very slightly nearer to the following, or left-hand side.

The reappearance of this spot was anxiously looked for. It came into view on the morning of March 3, having when observed passed the limb about 6° 16′. Its appearance was sketched on this occasion, and again on successive days to the 14th of March. Its apparent magnitude was diminished, but the main features were less altered than is usual with sunspots. The umbra still appeared in such relation to the penumbral border as to confirm the opinion of its being but very little depressed below the photosphere. Remarkable changes happened between the 7th and 10th of March. On the 8th two broad penumbral extensions appeared; on the 9th these were separated into two detached masses, and much altered in figure.

Diagram No. 2.



The path of spot A in February and March.

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* Proc. Roy. Soc. Nov. 23, 1865.

† Ibid. Jan. 26, 1865.

‡ Bull. des Obs. 1865.

§ Researches in Solar Physics, 1865.

The great spot, or rather aggregation of spots, marked C, was observed from the 13th to the 24th of February, as often as good opportunities occurred in the extremely variable weather. When fully expanded, about the 17th, 18th, and 19th of February, it measured about 12° on the surface of the sun, and occupied a space about 100,000 miles long, lying not quite parallel to his equator, and including forty or more black, dark, and dusky umbral tracts, in a complicated penumbral area. The definition was often excellent, so as to show the granular surface of the photosphere with more than ordinary distinctness.

The same fine brown tint already referred to was observed in the penumbral spaces, and there was every gradation of depth in this tint observable in the many specks and spots, till in a few only it seemed to be black. The penumbral tracts were plainly broken up into a kind of network of granulation; and the largest spot, chosen for special study, threw out long black digitations into the surrounding granulated space of the penumbra, like slits in a solid substance, 1000 or 2000 miles in length.

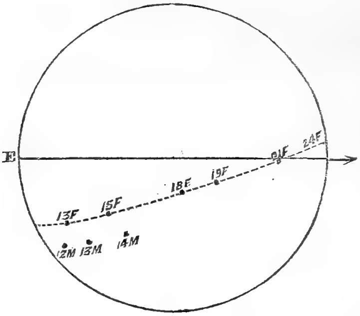
These characters of the largest spot in the group C became very prominent on the 19th of February, and were accompanied by others of an unusual character, which seem to deserve special attention in the question of the nature and history of these black spaces. In the drawing for this date these appearances are sketched with a power of 135. The spot was somewhat rhomboidal, the extreme length from angle to angle being about 15,000 miles, the least breadth about 4500. On the right the boundary was gently convex; parallel to it was a bright facular tract of uniform breadth; this was margined on the right by a nearly continuous very narrow black band, 17,500 miles long, extending in both directions beyond the spot, and slightly ramose in the (apparently) upper part. Parallel to this again was a curiously interrupted series of black angularly bent sharp cuts, ending upwards in a larger subdigitated mass, near which were some other small spots, forming broken chains, which turned off in curves to the right for about 40,000 miles (Pl. II. fig. 7).

On the 20th of February the appearances had changed to those represented in another sketch (Pl. II. fig. 8), where the great spot, something reduced in magnitude and altered in figure, shows very long digitations on all sides; the facular space on the right is broader; the long very narrow black band shows two internal extensions; the outer crested ridge has gathered itself into a shorter figure, 10,000 miles long, and has lost the character of angular tegulation which was so remarkable on the 19th.

On the 21st the spot had approached enough toward the limb to undergo some apparent change of general figure, by contraction perpendicular to the edge; the facular space on the right was entirely free from the narrow curved black divisional band; and the summit of the outer broader band was bent away from the great spot, to which it had been parallel (Pl. II. fig. 9). The disappearance of the spot amidst large elevated bright faculæ and depressed broader shaded tracts, was sketched on the

24th of February. Reappearing with similar splendid companion ridges of mountainous clouds, but much reduced in size, and altered in every part, it was observed again from the 12th to the 17th of March, after which the weather allowed no further opportunity. Two sets of drawings of this remarkable spot are presented to show its growth, development, and decay (Pl. II. figs. 7, 8, 9, and 10, 11, 12). The apparent path on the sun's disk is given for each period of appearance—the two paths differing by reason of the change in the apparent place of the sun's equator (see Diagram No. 3).

Diagram No. 3.



But few examples occur of such large penumbral tracts grouped about so many dark and half-darkened umbræ. On the sun they occupied, as already stated, a tract about 12° in length, not quite parallel to the equator; and may be compared to some of those which are most conspicuous in Mr. Carrington's plates.

From what has been observed, it appears that, in a given zone of spots, not only are the aspects of the particular spots much diversified, but further, that the changes to which they are subject offer much variety. These circumstances seem to point to particular local conditions as the cause of the diversity of appearance, though it may be possible to refer to other influences the frequency of their occurrence, if not the fact of their occurring at all.

The five spots now under review lie within an arc of longitude of 205°, leaving 155° in which as yet no spot has lately been seen. In 1864, during the months of March and April, a zone of spots, also five in number, and on the same side of the equator, was contained within an arc of longitude of 243°, leaving 117° at that time free from spots. There was then within the same arc of 243° a pair of spots in about the same latitude, but in the opposite hemisphere. Taking these into account, the average of the arcs of longitude between the spots was about 49°. In the case of the spots lately passing it was 51°. Twenty-six revolutions would have brought the middle of that zone of activity of 1864 to nearly the same place on the sun's disk, as the group now under consideration.

To whatever cause we may ascribe the fact of the breaking out of these spots, there would appear reason for expectation that spots of like character may be expected to recur again in the same parts of the sun's surface. In the great work of Mr. Carrington we find several examples of the appearance of new spots in nearly the same places as those which had been so occupied before. Those who think with M. Chacornac that sun-spots are due to volcanic eruptions, and regard their changes of appearance as effects of the displacement of solid and gaseous bodies about the region of disturbance, must naturally look for repetitions of these phenomena in the same parts of the solar surface.

The greater frequency of spots between the parallels of 10° and 30° lat. N. and S., the comparative rarity of them on the equator, and the almost entire absence of them from the circumpolar regions is well established. If we take the data from Mr. Carrington's register and suppose in all 1000 spots to be observed, 178 will be found between the parallels of 0° and 10° from the equator, 450 between 10° and 20°, 324 between 20° and 30°, and 48 above 30°.

The proportionate numbers for the northern and southern hemispheres are 450 in North latitude, 550 in South latitude.

If now we inquire, by the aid of the same invaluable book, as to the relative frequency of spots in different longitudes, we shall obtain a result of considerable interest in reference to the question of the place of the eruptions. Mr. Carrington has registered his observations through 99 rotations, and has arranged them in groups which can be tabulated for longitude. Assuming the rotation-period to be exact enough for fixing the longitudes in the course of seven years and 142 days, we may represent the relative frequency of the spots on different meridians as follows:—

Longitude	10° to 20°	320 maximum;
„	60 to 70	220 minimum;
„	100 to 110	313 maximum;
„	160 to 170	250 minimum;
„	190 to 200	366 maximum;
„	300 to 310	173 minimum;

showing three maxima at intervals of 90°, 90°, and 180°, and three minima at intervals of 100°, 140°, and 120°. Or if we take the circumference of the sun in three meridional compartments of 120° each, and suppose 1000 spots in all, we shall find

0 to 120	357
120 to 240	368
240 to 360	274

I am at present of opinion that this result may be trusted so far as to show that certain tracts of the sun's surface are more liable to eruption than other tracts, by reason of local peculiarity only.

Luminosity of the Sun.

Attentive observation shows the light of the sun to be feeblest toward the edges of the disk, strongest about the centre. By M. Chacornac's measure the ratio of the central to the marginal light is 100 to 45. Looking directly on the central regions, the light is found to be much the brightest on the apparent summits of the undulations of the photosphere ("rice-grains"); and looking to the limb, it is the faculæ which are the brightest parts. In each case it is the outermost parts of the photosphere which are the brightest, and it is the innermost parts which are the darkest. The depth of shade appears to be in direct proportion to the depth below the outer surface of the photosphere. It appears to me that such appearances would follow naturally from the hypothesis mentioned in my first communication on this subject *. The hypothesis is that the lowest parts of the umbral spaces yield the least refrangible and least luminous rays, such as belong to the space about the red end of the spectrum. These spaces may not be black, not even very dark, except by comparison with the brilliant spaces around; where the rays from them pass into the photosphere, they heat it, as the dark rays separated by sulphuret of carbon in Tyndall's experiment heat the platinum foil, and other solid bodies. Bodies thus heated emit new rays according to their nature; the solar photosphere is of such a nature as to send to us the mingled pencils which we receive; the principal effect of this kind being at a maximum on the outermost layer. The effect of this will be to cause streams of the most luminous rays from the most elevated parts of the photosphere, which will be seen directly in front, about the sun's centre, while toward the edges the sides of the undulations alone will be seen, and the rays which they yield will be less luminous—the only parts which are *there* very bright being the high ridges of the faculæ.

Another view has presented itself to me. If we admit the depressed part to be the body of the sun disclosed from below the luminous envelope, we may suppose its darkness to be due simply to radiation. For however hot the sun may be, if its composition be like that of the earth, the fused parts would be cooled and darkened at the surface where it may be uncovered—very much darkened where the exposure is complete (the umbra), partially so where the envelope is not wholly removed (the penumbra). It might be some test of this view, if the hourly changes of the umbra, immediately after its first appearance, could be accurately noted in respect of the degree of darkness as well as of the change of form. The umbra ought to grow darker and darker, never lighter and lighter, except by the overspreading of photosphere, which would be indicated independently by changes on the penumbral area.

From what I have seen in the course of these observations, I infer that the study of the physical condition of the solar spots cannot be regarded as likely to yield data of sufficient weight, if it do not include determina-

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* Proc. Roy. Soc. January 1865.

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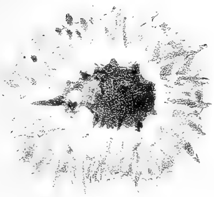

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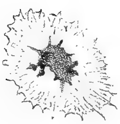

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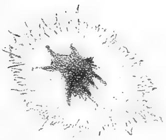

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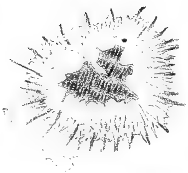

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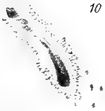

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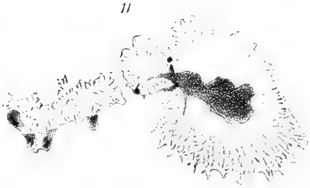

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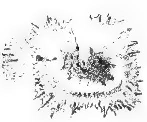

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tions at short intervals, as twice a day for the whole penumbral outline, and once an hour for selected critical parts of the umbra.

EXPLANATION OF PLATE II.

[The figures are all drawn to represent the objects as they appeared in a 6-inch achromatic furnished with the glass mirror set to reflect the rays in the equatorial plane to the westward. The motion of the spot is from left to right.]

Fig. 1. Appearance of the spot A, nearly on the central meridian, on Feb. 10th, 1866, at 11^h 15^m. The darkest part of the umbra was to the right; on the left a sharp excurrent part like a fissure; between this and the larger and darker part was a lighter brown tract. Small dots on the penumbra in the upper part to the left. Diameter 23,000 miles.

Fig. 2. First sketch of the spot A after its reappearance, March 5th, 10^h 30^m. The long excurrent parts at the extremities of the elliptical figure (elliptical by reason of the proximity of the spot to the edge of the disk) are frequent appearances in this position of the spots. This figure is rather too small in proportion to 4, 5, and 6.

Fig. 3. Spot A, further on the disk. The figure is rather too small. Note the peculiar shapes of the small fissure-like extensions on the left. March 6th, 2^h 30^m.

Fig. 4. Spot A, still further on the disk, March 7th, 10^h 0^m. The ramifications from the umbra have changed in appearance.

Fig. 5. Spot A, near the central meridian, March 8th, 12^h 30^m. The two extensions of the penumbra on the left have come into sight since yesterday. Note the little dot at their common origin, and, directed towards it, the longest of the excurrent umbral fissures.

Fig. 6. Spot A, March 9th, 12^h 0^m. Here the excurrent penumbral tracts are found to be separated from the spot, and from each other, and changed in direction. The umbra is much altered, and has a deep emargination, in place, apparently, of a small white speck, seen in fig. 5. Note also a black speck on the right upper edge of the umbra.

Fig. 7. Spot C, the largest umbral tract with its border on the right, Feb. 19th, 10^h 45^m. On account of the remarkable aspect which the umbra wore on this occasion, it was drawn repeatedly with great care. Note in particular the digitations of the large umbra, the broad facular space to the right, and the parallel very narrow, interrupted, and tegulated umbral bands, which are part of a system of interrupted small dark tracts, traceable through nearly all the length of the penumbra, which on this occasion was about 100,000 miles from end to end. The umbra itself measured about 15,000 miles between the extremities of the digitations.

Fig. 8. The same parts as they appeared on Feb. 20th, 10^h 20^m. The general figure of the umbra is changed; the long parallel narrow black space, and the broader tract on the outside of it are greatly altered; the latter, turning away from the umbra, appears to have gathered into itself the detached spots which appear in fig. 7, and to have lost the angular tegulation which then was conspicuous.

Fig. 9. In this figure the approach toward the edge of the sun is sensible in the elongation of the large umbra and its companion. Feb. 21st, 2^h 30^m.

Fig. 10 shows the reappearance of this spot C, near the edge, with a divided umbral tract, of different shades of darkness; the whole figure compressed elliptically by proximity to the edge. March 12th, 12^h 10^m.

Fig. 11. The same spot after it had proceeded on the disk, so as to allow of all parts being seen in their true proportions. March 13th, 2^h 30^m. The change of figure in the whole penumbral space, and in the darkest parts, is so great that it is difficult to be assured of the identity of any one point now visible with the appearance shown on any occasion in February. Yet, in fact, on searching carefully the neighbouring tracts of photosphere, we can discern what appear to be traces of the spaces occupied by the old penumbra. This important observation was made, not by myself only, but also by friends who were quite unaware of the interesting changes of form which had occurred.

Fig. 12. March 14th, 11^h 30^m. Here the spot is seen further modified; gathered up into a smaller and more regular shape, with a deep slit through the penumbra into the umbra; the detached penumbra on the left was lost after a further day's motion.

The Society then adjourned over the Easter Vacation to Thursday, April 12.

April 12, 1866.

Lieut-General SABINE, President, in the Chair.

The following communications were read:—

I. "On Uniform Rotation." By C. W. Siemens, F.R.S. Received March 10, 1866.

(Abstract.)

The paper sets out with an inquiry into the conditions of the conical pendulum as a means of obtaining uniform rotation. This instrument, as applied by Watt to regulate the velocity of his steam-engines, is shown to be defective,—first, because the regulated position of the valve depends upon the angular position of the pendulums, and therefore upon the velocity of rotation, which must be permanently changed in order to effect an adjustment of the valve; and secondly, because when the balance between force and resistance of the engine at a given velocity is disturbed, the angular position of the pendulums will not change until a power has been created in them, through acceleration of the engine, sufficient to overcome the mechanical resistance of the valve, giving rise to a series of fluctuations before a balance between the power and resistance of the engine is reestablished.

These defects in Watt's centrifugal governor are shown to be obviated in the chronometric governor, an instrument which was proposed by the author of the paper twenty-three years ago, and which consists of a conical pendulum proceeding at a uniform angle of rotation, and therefore at uniform speed, which is made to act upon the regulating-valve by means of a differential motion between itself and the engine to be regulated, which latter has to accommodate itself to the rotations imposed by the independent pendulum. The differential-motion wheels are taken advantage of for imparting independent driving- or sustaining-power to the pendulum; and a constancy of the angle of rotation, notwithstanding unavoidable fluctuations in the sustaining-power, is secured (within certain limits) by calling into play a break, or fluid resistance, at the moment when the angle of rotation reaches a maximum, which maximum position is perpetuated by increasing the sustaining-power beyond what is strictly necessary to overcome the ordinary resistance of the pendulum.

The chronometric governor is used by the Astronomer Royal to regulate the motion of the large equatorial telescope and recording apparatus at Greenwich, in which application a very high degree of regularity is attained; but the instrument proved to be too delicate in its adjustments for ordinary steam-engine use.

After a short allusion to M. Foucault's governor, the paper enters upon the description of a new apparatus which the writer has imagined for obtaining uniform rotation, notwithstanding great variations in the driving-power, and which consists, in the main, of a parabolic cup, open at top and

bottom and mounted upon a vertical axis, which cup dips with its smaller opening into a liquid contained within a casing completely enclosing the cup. It is shown that a certain angular velocity of the cup will raise the liquid (entering from below) in a parabolic curve to its upper edge or brim, and that a very slight increase of the velocity will cause actual overflow, in the form of a sheet of liquid, which, being raised and projected against the sides of the outer chamber, descends to the bath below, whence fresh liquid continually enters the cup. Without the overflow scarcely any power is required to maintain the cup, with the liquid it contains, in motion; but the moment an overflow ensues, a considerable amount of power is absorbed in raising and projecting a continuous stream of the liquid, whereby further acceleration is prevented, and nearly uniform velocity is the result. When absolute uniformity is required, the cup is not fixed upon the rotating axis, but is suspended from it by a spiral spring, which not only supports its weight, but also transmits the driving-power by its torsional moment. The cup is guided in the centre upon a helical surface, which arrangement has for its result that an increase of resistance or of driving-power produces an increased torsional action of the spring, and with it an automatic descent of the cup, sufficient to make up for the thickness of overflow required to effect the readjustment between power and resistance, without permanent increase of angular velocity.

It is shown that the density of the liquid exercises no influence upon the velocity of the cup, which velocity is expressed by the following formula,

$$n = \frac{\sqrt{2gh\left(1 + \frac{\rho^2}{r^2 - \cdot 293\,\rho^2}\right)}}{2r\pi}$$

in which

n signifies the number of revolutions per second,
h the height of liquid from the surface to the brim of cup,
r the radius of the brim, and
ρ the radius of lower orifice of cup;

only the rigidity of the spring must be greater when a comparatively dense liquid is employed.

In order to test the principle of action here involved, Mr. Siemens has constructed a clock consisting of a galvanic battery, an electro-magnet, and his gyrometric cup, besides the necessary reducing-wheels and hands upon a dial face, which proceeds at a uniform rate, although the driving-power may be varied between wide limits, by the introduction of artificial resistances into the electrical circuit. The instrument appears, therefore, well calculated for regulating the speed of all kinds of philosophical apparatus, and also for obtaining synchronous rotations at different places for telegraphic purposes. One of its most interesting applications is embodied in the "Gyrometric Governor" for steam-engines, of which an illustration is given. This consists of a cup of 200 millimetres diameter and the same height, which is fixed upon its vertical axis of rotation, and is enclosed in an outer chamber, containing water in such quantity that the lower extremity of the cup dips below its surface. The upper edge of the rotating cup is, in this application, surrounded by a stationary ring armed with vertical vanes, by which the overflowing liquid is arrested and directed downward, causing it to fall through a space or zone which is traversed by a number of radial and vertical blades projecting from the external surface of the rotating cup, which, in striking the falling liquid, project it with considerable force against the sides of the outer vessel, at the expense of a corresponding retarding effect on the cup, increasing its regulating-power.

The cup-spindle carries at its lower extremity a pinion, which gears into two planet-wheels at opposite points, which on their part gear into an inverted wheel surrounding the whole, which latter is fastened upon a vertical shaft in continuation of the cup-spindle, and is driven round by the engine in the opposite direction to the motion of the cup. The two intermediate or planet-wheels are attached to a rocking frame supported, but not fixed, upon the central axis, which wheels, in rotating upon their studs, are also free to follow the impulse of either the pinion or the inverted wheel to the extent of the differential motion arising between them. The rocking frame is connected to the regulating valve of the engine, and also to a weight suspended from a horizontal arm upon the valve-spindle, tending to open the valve and at the same time to accelerate the cup to the extent of the pressure produced between the teeth of the planet-wheels and the pinion, while the engine is constantly employed to raise the weight and to cut off the supply of steam. The result is that the engine has to conform absolutely to the regular motion imposed by the cup, which will be precisely the same when the engine is charged with its maximum or its minimum of resisting load.

The paper shows that the action upon the valve must take place at the moment when the balance between the power and load of the engine is disturbed, and that the readjustment will be effected notwithstanding a resistance of the valve exceeding 100 kilogrammes—a result tending towards the attainment of several important objects.

II. "On a Fluorescent Substance, resembling Quinine, in Animals; and on the Rate of Passage of Quinine into the Vascular and Non-vascular Textures of the Body." By H. Bence Jones, M.D., F.R.S., and A. Dupré, Ph.D., F.C.S. Received March 14, 1866.

Part I.

On a Fluorescent Substance, resembling Quinine, in Animals.

The term fluorescence in the last few years has found a place in physiological works because different substances that occur in the body have been said to possess the property of fluorescence. Of these the solution of bile-acids in concentrated sulphuric acid, the white of egg when kept for a short time, and the urine sometimes, are the best-known fluorescing substances.

But as long since as 1845, Professor Brücke, in Müller's 'Archiv,' stated that he had found in many and very frequently repeated experiments, that the lens absorbed the blue rays of light to a very great extent, and that the cornea and the aqueous humour did so to a less extent, but that the lens together with these other media absorbed these rays to the greatest degree. He used the eyes of oxen and of rabbits, and the lens of a pike's eye, which last, when dried with care, preserved its transparency, and allowed the light to fall on a porcelain plate covered with tincture of guaiacum and bleach a portion of the green surface.

Professor Stokes, in his well-known paper "On the Change of the Refrangibility of Light," in the Philosophical Transactions for 1852, says (p. 512), "It is found that the property of change of refrangibility in the incident light is extremely common." "To make a list of sensitive substances would be useless work; for it is very rare to meet with a white or light-coloured organic substance which is not more or less sensitive." Among others he mentions horn, bone, ivory, white shells, leather, quills, white feathers, white bristles, the skin of the hand, and the nails. And in his conclusion (p. 557), he says, "The phenomenon of change of refrangibility proves to be extremely common, especially in the case of organic substances, such as those ordinarily met with, in which it is almost always manifested to a greater or less degree."

When speaking of the fluorescence of sulphate of quinine, he says (p. 541), "When quinine was dissolved in dilute hydrochloric acid, the blue colour was not exhibited, not even when the fluid was held in the sunlight and examined by superficial projection."

In 1855 Helmholtz published a paper in Poggendorff's 'Annalen,' vol. xciv. p. 205, in which he says that, as far as quinine paper showed that the spectrum extended, so far the eye could perceive light *.

He then proposes this question, Does the retina see the rays beyond the violet directly as it sees other colours of the spectrum, or does it fluoresce under the influence of these rays? and is the blue colour of the rays beyond the violet light of less refrangibility which shows itself in the retina only under the influence of the violet rays?

To determine this question, he says, I examined for fluorescence the retina of the eye of a man who had been dead for eighteen hours. The first experiment showed that it was very feebly fluorescent. The retina was less fluorescent than paper, linen, and ivory, but more than porcelain.

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* In 1853 Prof. Donders, in a paper in Müller's 'Archiv,' p. 471, "On the Action of the Invisible Rays of high Refrangibility on the Media of the Eye," says that "most, if not all, the rays of higher refrangibility than the violet reach the retina, and are not absorbed by the different media through which the light passes;" and Dr. Kessler, in 'Archiv für Ophthalmologie,' 1854, vol. I. p. 449, says that "the crystalline lens is not the cause of the invisibility of the rays of high refrangibility; for when the lens is removed by operation these rays are not more visible.

The colour of the light dispersed through the retina is greenish white, very different from that which the direct perception of these rays usually gives.

In 1858, in the Comptes Rendus des Séances de la Société de Biologie pendant le mois de Novembre 1858, pp. 166 & 167, there is a short notice headed "Analyse et conclusions d'un travail sur la fluorescence des milieux de l'œil, par M. Jules Regnauld."

He used sunlight, and found in man and the mammifera that the cornea fluoresced in a very slight degree. In the sheep, dog, cat, and rabbit, the crystalline lens possessed in the highest degree fluorescent properties. In these animals, and also in many birds, the central part of the lens (endophaine of MM. Valenciennes and Fremy), preserved by desiccation at a low temperature, retained this property.

The central portion of the crystalline of many aquatic vertebrata and mollusca (phaconine of MM. Valenciennes and Fremy) is almost entirely without fluorescence.

The hyaloid body possesses only a very feeble fluorescence, due to the hyaline membranes; for the vitreous humour itself is not fluorescent.

The retina, as M. Helmholtz discovered, possesses a certain fluorescence which is not at all comparable in intensity to that of the crystalline lens of mammifera.

Finally, M. Regnauld concludes that, if we must attribute the accidents caused by feebly luminous radiations of the electric light to the phenomena of fluorescence, it is above all in the energetic action on the crystalline that it is natural to look for an explanation. The impression which the cornea undergoes must nevertheless not be neglected.

In 1859, in the 'Archiv für Ophthalmologie,' vol. v. part 11. p. 205, there is a paper by J. Setschenow of Moscow, "On the Fluorescence of the Transparent Media of the Eyes of Man and some other Animals." He undertook the examination at Professor Helmholtz's request, because it was possible that the phenomena of fluorescence observed by Helmholtz might have been modified by a post-mortem change in the eye.

He experimented on the eyes of oxen and rabbits. The fresh retina showed the same phenomena as the dead human retina. It diffused a greenish-white light, which, examined by a prism, gives a spectrum in which the red is wanting.

The vitreous humour in a thin glass vessel showed only traces of fluorescence. The lens, on the contrary, fluoresced very strongly: the colour of the dispersed light is white-blue, exactly like quinine; only the quinine was a little stronger. Examined by a prism, the dispersed light gave a spectrum in which the red was wanting, and in which the blue tone predominated. The fluorescence begins, as in quinine solutions, between G and H, and is strongest at the outer edge of the violet rays, and extends into the ultra-violet to the same distance in the case of the lens as in the case of the quinine solution.

When the cornea was cut out, it fluoresced much feebler than the lens; the aqueous humour did not fluoresce at all.

The appearances in the three last media, he says, can be shown with the greatest ease, even in the eye of a living man. When the eye is brought into the focus of the ultra-violet rays, immediately the cornea and the lens begin to glimmer with a white-blue light. The cornea in the living eye is much more strongly fluorescent than when dissected out, probably from the loss of transparency consequent on contraction of the texture and from evaporation.

The question how and why our eyes perceive the ultra-violet spectrum is still undetermined. The fluorescence of the lens would be rather a hindrance than a help; only the general sensibility to the light which the ultra-violet rays produce in our eyes can be explained by the fluorescence of the media lying before the retina.

In 1862, 'Zeitschrift für Rationelle Medicin,' 3rd series, vol. xiii. p. 270, Pfluger, by mixing fresh ox-gall with concentrated sulphuric acid, saw a clear dichrotic solution form. It had a deep red colour by transmitted light, and a beautiful green colour by reflected light. This was seen very beautifully when dried bile was put into sulphuric acid; and then the dichroism increased by standing. It is desirable to separate the cholesterin and the bile-fats.

The green fluorescence appeared when only blue or green light fell on the sulphuric-acid solution of the bile. On the contrary, it did not appear when the light was only yellow or red.

The sulphuric-acid solution of the bile absorbed all the rays of the spectrum except the yellow and the red.

In the 'Journal für Prakt. Chem.' vol. xcii. p. 167, Schönbein has the following remarks on the formation of a fluorescing substance in the putrefaction of human urine:—

If urine is left to stand in the air until it becomes covered on the surface with a thick layer of fungus, the alkaline fluid that filters from it shows a very strong fluorescence of a greenish colour. Small quantities of the stronger organic or inorganic acids take away this fluorescence, which, however, by alkalies can be again reproduced. This substance has a reaction like esculine, and, like this, is the opposite to quinine, the fluorescence of which is increased by those acids. The hydrobromic, hydriodic, and hydrochloric acids lessen the fluorescence of the solution of quinine almost to entire removal. Schönbein also remarked that fresh urine had a feeble fluorescence, and also that a weak solution of albumen, by standing long in the air, became fluorescent to a considerable degree.

This was the state of our knowledge of fluorescence in animals when, having traced the rate of passage of chloride of lithium and other mineral substances into and out of the textures by means of the spectrum analysis, we endeavoured to find some method of determining the rate at which organic substances passed into and out of the same structures.

Among all the delicate tests for different organic substances, the fluorescence of sulphate of quinine appeared likely to afford good results; for the following experiments on the delicacy of this test for sulphate of quinine show that this method of tracing sulphate of quinine into and out of the body, though inferior to the spectrum determinations of lithium, was superior in delicacy to the spectrum determinations of many other substances.

On the Delicacy of the Fluorescent Test of Sulphate of Quinine when a Ruhmkorf coil was used as the source of light.

One grain of sulphate of quinine was dissolved in five ounces of acidified water, and this was again and again diluted until one grain of quinine-salt was present in 1,800,000 parts of water. This, when examined in a quartz cell by the induction-spark, showed blue fluorescence distinctly in twenty grains of solution.

Another grain, dissolved in a litre and diluted until one grain of salt was present in 1,440,000 parts of water, when acidified, also showed the fluorescence distinctly in twenty grains of solution.

The same quantity, dissolved in one litre of water, was diluted to 512 litres. This was equal to one part in 7,200,000 parts of water; as the fluorescence could be seen in twenty grains of this solution, $\frac{1}{360,000}$ of a grain of sulphate of quinine gives the fluorescence.

In another experiment, $\frac{1}{25,000}$ of sulphate of quinine, in fifty minims of water acidified, showed the fluorescence strongly, and even $\frac{1}{100,000}$ of a grain of sulphate of quinine in fifty minims of water showed the fluorescence feebly. As the fluorescence could be seen in twenty grains of this solution, $\frac{1}{250,000}$ of a grain of sulphate of quinine gives the fluorescence.

In the last two sets of experiments the light of a bright induction-spark was concentrated by a small quartz lens.

One grain of disulphate of quinine, dissolved in 256 litres of water acidulated with one-eighth of sulphuric acid (1 to 8), shows fluorescence feebly in a quartz cell containing twelve grains of the solution. Hence $\frac{1}{330,000}$ grain gives the fluorescence feebly.

If one grain of disulphate of quinine is dissolved in a thousand litres, or one part of quinine to 15,440,000 of water, the fluorescence is still perceptible in one ounce of the solution.

On the Existence of an extractable Fluorescent Substance in Animals and Man.

Immediately on trying to apply this reaction to test the different textures of guineapigs, after and before they had taken quinine, we found that in health no part of any of the tissues of the guineapig was free from blue fluorescence. It became desirable, therefore, to separate the fluorescence produced by some fluorescing substance normally present in the tissues from that produced after quinine was given. After very many attempts to extract these substances separately from the tissues, and many more to separate them when they were conjointly extracted, all of which proved unsuccessful, we resorted to the plan of determining the amount of natural fluorescence by comparing it with standard solutions of sulphate of quinine; and by the same means we measured the increase that occurred in the amount of fluorescence from the same organs after quinine had been taken.

The following plan was adopted for the extraction of the fluorescent substance from the textures, both before and after quinine was taken.

The part to be examined was treated on a water-bath with very dilute sulphuric acid, either directly or after previous drying in a water-oven. This extraction was repeated again and again. The acid extracts were mixed, filtered after cooling, neutralized with caustic soda, and repeatedly shaken up with their own bulk of ether. The residue left after evaporation of the ether was taken up by dilute sulphuric acid, filtered, and tested for the amount of fluorescence after having been made up to a certain bulk, generally twenty-five minims.

When a large quantity of material, as two or three pounds of liver, was employed, the acid extract was a second time neutralized and treated with ether; the residue from the second ethereal solution was then taken up with dilute sulphuric acid and tested.

In very dilute solutions the fluorescence of the animal substance cannot be distinguished from that produced by quinine; if the solution is more concentrated, the fluorescence of the animal substance is confined much more to the surface, the fluorescence in a solution of quinine passing much further into the liquid; and in still more concentrated solutions, the colour of the light given out is of a decidedly greenish hue. The fluorescence also of the animal substance begins to appear somewhat nearer to the red end of the spectrum than is the case with quinine; but both extend to the same distance beyond the violet end.

From two to three pounds of liver, only about fifty minims of a solution was obtained showing a fluorescence equal to that produced by two or three grains of quinine to the litre of water; and when slightly acidified, the following reactions were obtained with the solution. It gives a precipitate with solution of iodine, with a solution of iodide of mercury in iodide of potassium, and also with phosphomolybdic acid, bichloride of platinum, and terchloride of gold: this last precipitate is soluble in alcohol, like that produced in solutions of quinine.

A weak solution of quinine interposed in a quartz cell before the solution of the natural fluorescing substance, did not stop the fluorescence of this latter substance entirely; but when the solution of animal substance was placed before the solution of quinine, no fluorescence whatever could be perceived in the quinine. Ether is unable to extract the animal substance from an acid solution; the acid solution may be shaken up several times with ether; but the ethereal solution on evaporation yields a residue which, when taken up by dilute sulphuric acid, gives no blue fluorescence whatever.

The fluorescence of this animal substance is much less strong in hydrochloric acid solutions by the light of the coil-spark, and it is almost destroyed by alkalies. The substance does not lose its fluorescence when treated with dilute sulphuric acid on a water-bath, nor even on the addition of a dilute solution of permanganate of potash. In an alkaline solution with permanganate of potash it is immediately destroyed. Quinine behaves in a precisely similar way.

Parts of the brain, kidney, liver, and heart, and the crystalline lens of a human subject dead for many hours, were boiled with dilute sulphuric acid, neutralized with carbonate of soda, and extracted with ether. The ethereal residue, dissolved in dilute sulphuric acid, was examined by the spark of the Ruhmkorf coil for fluorescence. From every part the extract fluoresced distinctly but very feebly. The fluorescence closely resembled that caused by a very weak solution of quinine.

The lenses from sheep's, bullocks', pikes', eagles' eyes, gave a distinctly fluorescent substance, and the extract from the sheep's liver fluoresced very strongly. Cod-liver oil also fluoresced very distinctly. The so-called pills of cod-liver oil gave no fluorescence.

The fluorescent substance could be extracted by treating the finely divided substance with alcohol, and the residue of the alcohol solution by ether, and finally dissolving the ethereal residue in water acidulated with sulphuric acid.

It follows from these experiments that there exists in the body of man and animals, fishes, and birds a substance which can be extracted from any of the tissues by the same process as quinine when present can be extracted. This substance has the same reactions with chemical agents as quinine has, and the action of light upon this substance is almost, although not altogether, identical with its action on quinine.

This substance is visible in the lens of the human eye during life. It is, from its mode of separation and reactions, an alkaloid bearing a close resemblance in its properties to quinine.

For the present we shall call this animal quinoidine. It is the cause of the blue fluorescence of weak acid extracts from any of the tissues of the body of men and animals. When concentrated, the fluorescent substance is bluish green.

On the Fluorescent Substance produced by treating Bile with strong Sulphuric Acid.

We were unable to insulate and extract the substance which causes the green fluorescence in bile when it is treated with strong sulphuric acid; nor could we separate that which forms in white of egg when a solution in water is exposed to the air.

The bile dissolved in strong sulphuric acid was transparent by transmitted light, and appeared as a brownish-yellow solution; by reflected light, even with the ordinary gaslight, it showed a strong green fluorescence, in much larger quantity than, and entirely different in appearance from, the blue fluorescent substance of the liver. Thus it was destroyed by diluting the acid with water, but returned on the addition of a larger quantity of strong sulphuric acid.

The solution of egg-albumen, exposed to the air, gradually after some days showed a strong green fluorescence, which, like esculine, disappeared on the addition of an acid, but was not destroyed by alkalies. The fluorescence gradually disappeared when the albumen began to putrefy.

PART II.

On the Increase of Fluorescence in the Textures of Animals and Man after Quinine had been taken.

Having proved that in all the different textures of an animal a natural alkaloid fluorescent substance was present when no quinine had been taken, and as no means could be found for separating the natural fluorescent substance from the quinine when it passed into the textures, it became necessary to make our analyses quantitative instead of qualitative.

For this purpose it was necessary to determine, by means of standard solutions of quinine, what was the greatest amount of naturally fluorescent substance that usually occurred and could be extracted from the tissues. Deducting this from the amount of fluorescence that could be extracted after quinine was given, we were enabled to measure the rapidity of passage of the quinine into or out of the tissues, the animals being destroyed at different periods after different quantities of sulphate of quinine had been taken.

So also by determining the amount of natural fluorescent substance in the textures, lenses, and urine of man before quinine was taken, and deducting this from the amount obtained after quinine was taken, we were enabled to show that quinine does pass into the textures and lenses, and how quickly it appeared in the urine and reached its maximum and began to disappear and entirely vanished.

First. Examinations of different textures of guineapigs when no quinine was taken, and comparison of the amount of natural fluorescent substance with standard solutions of sulphate of quinine. The amount of the different parts examined was as nearly as possible always the same:—

1. A guineapig that had taken no quinine was killed; the extract of the brain and nerves only was measured. The fluorescence of the nerves was very feeble, and about equal to one-twentieth of a grain of sulphate of quinine in a litre of water. The extract of the brain was exceedingly feeble, and it was less than one-thirtieth of a grain of sulphate of quinine in a litre of water.

2. In another guineapig the lenses and the nerves were dried, and boiled three times with dilute sulphuric acid. The acid solution was rendered alkaline by caustic potass, and it was then shaken up with ether three times. The ethereal solution was evaporated, and the residue dissolved in dilute sulphuric acid. The acid solution was made up to twenty-five minims. The solutions of the lenses and of the nerves showed some fluorescence.

These acid solutions were now again rendered alkaline and were shaken up with ether, and the ethereal solution was evaporated and the residue dissolved in acetic acid, and the excess of the acid evaporated in a water-bath; the residue was dissolved in a little water, and the solutions were divided into two parts. The half of the solution from the lenses and nerves, when tested with a solution of iodine and a solution of iodide of mercury, remained perfectly clear. The bile, brain, and liver, tested in the same way, gave no precipitates with these reagents, although they also gave slight fluorescence.

The different parts of this pig were compared with the corresponding parts of two other pigs, one of which took sixteen grains of sulphate of quinine in nine doses in four days, and the other twenty-six grains in fourteen doses in six days.

3. Another guineapig had the different organs treated in exactly the same way; the liver, lenses, kidneys, urine, blood, brain, nerves, and muscle gave a fluorescence which varied from about one-twentieth to one-thirty-second part of a grain of quinine in a litre of water. This pig was bought at the same time and place, and fed in the same way as two other pigs (18 and 23), that were given six grains of sulphate of quinine in three doses with twenty minutes' interval. One pig was killed between five and six hours after the quinine, and the other in twenty-four hours.

4. Another pig was also treated in the same way, for comparison with three other pigs which took five grains of quinine, and were killed four hours, eight hours, and thirty-two hours afterwards. The lenses, humours, brain, blood, nerves, and muscle gave a fluorescence varying from one-twenty-fifth to one-fiftieth of a grain of quinine in a litre of water.

5. Another pig had taken no quinine. The brain gave a fluorescence equal to one-thirtieth of a grain of quinine, and the nerves fluoresced equal to one-twentieth of a grain of quinine.

6. Another guineapig had equal quantities of the liver, bile, kidney, urine, brain, lenses, humours, nerves, blood, and muscle treated in the same way. The fluorescence obtained from the liver was from one-thirty-second to one-sixteenth part of a grain of quinine. The fluorescence of all other parts was less than one-thirty-second part of a grain of quinine. The fluorescence of the humours was least of all.

7. Another guineapig was given no quinine. Equal quantities of dry liver, blood, bile, kidney, brain, nerves, lenses, muscle, and humours were taken. Of the bile and humours, which were taken entire, rather less than half a grain, of the other parts half a grain was used; the liver fluoresced equal to one-sixteenth of a grain of quinine. The bile, blood, kidney, brain, nerves, lens, and muscle showed somewhat less than one-sixty-fourth part of a grain to a litre; the humours of the eye rather more than one-sixty-fourth part.

8. Another pig, bought at the same time and place as 17, was given no quinine; and the fluorescence of every part was less than one-sixty-fourth of a grain of quinine in a litre of water. The fluorescence of each solution was rendered still less by the addition of common salt.

Secondly. On the increase of fluorescence that was observed when different quantities of sulphate of quinine were given to animals at different periods before they were killed:—

9. A guineapig was given sixteen grains of sulphate of quinine, in nine doses, in the course of four days. It was found dead more than twelve hours after the last dose. Its fluorescence was compared with pig 2, which had taken no quinine; the lenses and the nerves, treated the same way, showed much more fluorescence in the pig that had taken quinine than in the other which had had no quinine. The solutions of the lenses and the nerves also gave a distinct precipitate, which was least in the solution of the lenses with solutions of iodine and of iodide of mercury; whilst the solutions of the lenses and the nerves of the pig that had taken no quinine remained clear. The bile and the brain fluoresced brightly. The solution of the liver, when diluted even to one hundred minims, fluoresced distinctly in daylight, and very strongly in the light of the spark. The brain-solution gave a distinct precipitate with iodine and iodide of mercury. The bile gave a very faint turbidity. The liver gave an abundant precipitate, and moreover gave Herapath's test for quinine very distinctly.

10. Another pig took twenty-six grains of quinine, in fourteen doses, during six days. It was killed nineteen hours after the last dose, being apparently partially paralyzed. The solutions of the different organs were prepared in the same way as 9 and 2, and they were examined at the same time. The lenses showed the fluorescence very strongly when a cone of sunlight was thrown into them with a quartz lens. The humours showed no fluorescence at all in this manner. The solutions of the bile, brain, urine, nerves, lenses, spinal marrow, liver, gave more or less distinct fluorescence. The solution of the brain gave no precipitate with iodine or iodide of mercury; nor did the bile give a precipitate, but the liver gave a slight precipitate with both reagents.

Having thus satisfied ourselves that quinine does pass into the vascular and non-vascular tissues, we proceeded to determine how quickly four, five, or six grains of sulphate of quinine given to guineapigs could be detected in the different textures of their bodies.

11. A guineapig was given four grains of sulphate of quinine, and it was killed in one quarter of an hour; all the extracts from the different parts of the body were mixed with one-eighth of their bulk of dilute sulphuric acid, and were made up to twenty-five minims. The amount of fluorescence was compared with the fluorescence obtained in pig 6, and with standard solutions of sulphate of quinine with one-eighth of dilute sulphuric acid, containing one grain, three-quarters, half, one-eighth, one-sixteenth, and one-thirty-second of a grain of sulphate of quinine. The solutions of the extracts of pigs 19, 22, 25, and 26, which had taken four grains of sulphate of quinine and were killed six hours, twenty-four hours, forty-eight hours, and seventy-two hours after the quinine was taken, were examined at the same time.

The urine and the blood gave a fluorescence equal to half a grain of quinine to a litre of water; the extract of the kidney and the liver gave a fluorescence equal to three-fourths of a grain of quinine; the extract of the muscle gave a fluorescence between half a grain and one-fourth of a grain. The extract of the bile and the brain gave a fluorescence nearly equal to one-eighth of a grain of quinine to a litre. The nerves gave rather more fluorescence than one-sixteenth of a grain of quinine, and the lenses gave between one-sixteenth and one-thirty-second of a grain.

Comparing these numbers with pig 6, in which the fluorescence of the liver was greatest, amounting to from one-thirty-second to one-sixteenth of a grain of quinine to a litre of water, whilst in all other parts, urine, blood, kidney, muscle, bile, brain, nerves, and lenses, the natural fluorescence was less than one-thirty-second of a grain, it is evident that in a quarter of an hour sulphate of quinine passes into all the vascular and non-vascular structures of the body.

12. Another guineapig was given four grains of sulphate of quinine, and it was killed in half an hour; the nerves and brain were compared with pig 1, which had taken no quinine. The extract of the liver and kidneys gave a fluorescence equal to two-fifths of a grain of sulphate of quinine in a litre; the blood, the bile, and the muscle gave a fluorescence equal to one-fifth; the urine gave a fluorescence between one-fifth and one-tenth; the brain between one-tenth and one-twentieth; the humours of the eye fluoresced feebly, but a little more than the lenses, about one-twentieth; the lenses and the nerves less than one-twentieth of a grain of quinine.

13. Another guineapig took four grains of sulphate of quinine, and was killed one hour afterwards. The blood, the kidney, and the urine gave a fluorescence equal to, or rather more than, one-fifth of a grain of quinine to a litre; the liver fluoresced equal to between one-fifth and two-fifths of a grain of quinine; the bile and the muscles gave a fluorescence equal to one-twentieth of a grain of quinine; the brain fluoresced between one-twentieth and one-thirtieth; the humours fluoresced rather more than the lenses; and the nerves gave the slightest fluorescence.

14. Another guineapig was given four grains of sulphate of quinine; after three hours it was killed. It had been strongly affected, and the brain was much congested. The liver, kidney, blood, urine, bile, brain, and muscle gave very strong fluorescence, equal to between one and two grains of quinine in a litre of water; the nerves gave a fluorescence equal to one-sixteenth of a grain of quinine; the humours gave a fluorescence between one-sixteenth and one-thirty-second part of a grain; the lenses less than one-thirty-second part of a grain of quinine to a litre of water.

15. Another guineapig was given four grains of sulphate of quinine; after three hours it was killed. Of every part half of the dry substance was taken, except the aqueous humour, bile, and urine, of which the whole was taken; but when dry each of these was less than half a grain. Half the muscle gave a fluorescence equal nearly to one-sixteenth of a grain of quinine in a litre; half the brain fluoresced a little more than one-thirty-second;

half the kidney one-eighth to one-fourth of a grain; all the urine one-eighth; half the blood fluoresced one-sixty-fourth to one-thirty-second; half the liver a little more than one-sixteenth; all the bile a little less than one-fourth; half the lenses a little less than one-sixty-fourth; all the humours a little more than one-sixteenth; half the nerves one-sixty-fourth to one-thirty-second of a grain of quinine to a litre of water.

16. Another guineapig was killed four hours after five grains of sulphate of quinine, in two doses, with half an hour's interval. The fluorescence of the different extracts of the tissues was compared with that of pig 4, that had taken no quinine, and with pigs 21 and 24, that were killed after taking a dose of five grains eight and thirty-two hours previously. The liver, the kidney, the brain, and the muscle gave a fluorescence about equal to one grain of quinine in a litre of water; the blood rather less; the nerves rather more than the blood; the lenses and the humours less than the blood.

17. Another pig was killed four hours and a half after four grains of quinine. The whole of each organ was taken; the muscle gave a fluorescence equalling from one-half to one grain of quinine to a litre of water; the brain one-sixteenth to one-eighth of a grain; the kidney fluoresced nearly equal to one grain; the urine rather more than one grain; the blood one-eighth to one-fourth; the liver nearly one; the bile a little more than one-eighth; the lenses, humours, and nerves fluoresced less than one-sixty-fourth of a grain of quinine to a litre of water.

This pig was compared with one which was given no quinine.

18. Another pig was killed five hours and a half after six grains of sulphate of quinine, given in three doses, with twenty minutes' interval. It was very much affected. The extract of its tissues was compared with pig 3, which had taken no quinine; the liver, kidney, and muscle fluoresced very strongly; the brain fluoresced strongly; the blood fluoresced between one-fifth and two-fifths of a grain to a litre of water; the nerves and the lenses fluoresced much less.

19. Another pig was killed six hours after four grains of sulphate of quinine. The liver and kidney fluoresced equal to from one-half to one-fourth of a grain of sulphate of quinine to a litre of water; the urine from one-eighth to one-quarter; the blood fluoresced about one-eighth, the muscle a little more; the brain and the humours of the eye about one-sixteenth of a grain of quinine; the lenses between one-sixteenth and one-thirty-second, and the nerves rather less than one-thirty-second part of a grain of quinine; the fluorescence was compared with pig 6, that had taken no quinine.

20. Another pig was killed six hours after four grains of sulphate of quinine. The liver gave a fluorescence equal to between one and two grains of sulphate of quinine; the kidney and the urine gave a fluorescence equal to one grain of quinine; the blood fluoresced between one grain and half a grain; the bile equalled three-quarters of a grain; the muscles and the brain one-quarter of a grain; the nerves one-sixteenth of a grain; the

humours rather more than one-thirty-second of a grain; and the lenses between one-thirty-second and one-sixty-fourth of a grain to a litre of water.

21. Another guineapig was killed in eight hours after taking five grains of sulphate of quinine, in two doses, with half an hour's interval. The extract of its tissues was compared with pig 8, which had taken no quinine, and with pig 16 and pig 24, which were killed four hours and thirty-two hours after taking five grains of quinine; the fluorescence of the liver was very strong; the muscles, the brain, and the kidneys showed the fluorescence very distinctly; the fluorescence of the urine equalled from two-fifths to three-fifths of a grain of quinine; the bile was not more than one-twentieth of a grain of quinine; the blood fluoresced more distinctly; the lenses and the humours not so much, and the nerves least of all.

This pig was with young when killed, and the fluorescence of the fœtus was distinct; the fluorescence of the liquor amnii was less than that of the fœtus.

22. Another pig was killed in twenty-four hours after taking four grains of sulphate of quinine. The fluorescence of its textures was compared with pig 6, that had taken no quinine; the fluorescence of the liver and kidney was equal to half a grain of quinine; the blood and bile fluoresced equal to one-eighth of a grain; the urine between one-eighth and one-sixteenth; the muscle the same; the brain and the humours rather less than one-sixteenth of a grain; the lenses and the nerves about one-thirty-second of a grain of quinine to a litre of water.

23. Another pig was killed twenty-four hours after taking six grains of quinine, in three doses, with 20 minutes' interval. The fluorescence was compared with pig 3, which had taken no quinine; the liver showed the most fluorescence; the muscles, the kidney, and the brain were next; the nerves and blood equalled about one-tenth of a grain of quinine, and the lenses fluoresced least of all.

24. Another pig was killed thirty-two hours after taking five grains of sulphate of quinine. The fluorescence was compared with pig 4, which had taken no quinine; the liver fluoresced scarcely more than that of the pig that had taken no quinine, about one-twenty-fifth of a grain to a litre of water; the kidney fluoresced a little more than that of the pig without quinine; the fluorescence of the urine was scarcely perceptible, less than one-fiftieth of a grain of quinine; the muscles a little less than the pig without quinine; the blood, the nerves, the brain fluoresced very slightly; the lens and the humours more than any other part.

25. Another pig was killed forty-eight hours after four grains of sulphate of quinine. The fluorescence was compared with pig 6, which had taken no quinine; the extract of the liver and of the blood had a fluorescence equal to one-sixteenth of a grain of quinine in a litre of water; the bile, the kidney, the urine, the brain, the lenses, the humours, the nerves, and the muscles had each a fluorescence less than one-thirty-second part of a grain of quinine.

26. Another pig was killed seventy-two hours after four grains of sulphate of quinine. The fluorescence was compared also with pig 6, which had taken no quinine; the extract of the liver and of the brain had a fluorescence equal to one-sixteenth of a grain of sulphate of quinine; the lenses had a fluorescence equal to one-thirty-second part of a grain; the bile, kidney, urine, the nerves, blood, and muscle had a fluorescence less than one-thirty-second part of a grain; and the humours fluoresced much less than one-thirty-second part of a grain of sulphate of quinine in a litre of water.

Fluorescence without quinine, measured by the number of grains of quinine in 100 litres of water (176 pints).

	Pig 1.	Pig 4.	Pig 5.	Pig 6.	Pig 7.	Pig 8.
Liver....	....	....	....	6 to 3	less 6·2	less 1·6
Lenses ..	....	....	....	less 3	,, 1·6	,, 1·6
Kidney ..	....	....	....	,, 3	,, 1·6	,, 1·6
Urine ..	....	....	....	,, 3	,, 1·6	,, 1·6
Bile	....	....	....	,, 3	,, 1·6	,, 1·6
Blood ..	....	4 to 2	....	,, 3	,, 1·6	,, 1·6
Brain ..	less 3	4 to 2	3	,, 3	,, 1·6	,, 1·6
Nerves ..	5	4 to 2	5	,, 3	,, 1·6	,, 1·6
Muscles .	....	4 to 2	....	3	,, 1·6	,, 1·6
Humours	....	4 to 2	....	least	more 1·6	,, 1·6

Fluorescence after quinine.

	Pig. 11. 15 min.	Pig 12. 30 min.	Pig 13. 1 hour.	Pig 14. 3 hours.	Pig 15. 3 hours.	Pig 16. 4 hrs.	Pig 17. 4½ hours.
Liver ..	75	40	20 to 40	100 to 200	6·2	100	100
Lenses .	6 to 3	5	..	3	1·6	..	1·6
Kidney .	75	40	20	100 to 200	12 to 25	100	100
Urine ..	50	20 to 10	20	100 to 200	12	..	100
Bile	12	20	5	100 to 200	25	..	12·5
Blood ..	50	20	20	100 to 200	1·6	..	12 to 25
Brain ..	12	10 to 3	5 to 3	100 to 200	3	100	6 to 12
Nerves .	6	5	least	6	1·6 to 3	..	1·6
Muscles	50 to 25	20	5	100 to 200	6·2	100	50 to 100
Humours	..	5	..	6 to 3	6·2	..	1·6

	Pig 18. 5½ hours.	Pig 19. 6 hours.	Pig 21. 6 hours.	Pig 22. 8 hours.	Pig 23. 24 hrs.	Pig 24. 32 hrs.	Pig 25. 48 hrs.	Pig 26. 72 hrs.
Liver ..	..	50 to 25	100 to 200	..	50	4	6	6
Lenses .	..	6 to 3	3 to 1	..	3	..	3	3
Kidney .	..	50 to 25	100	..	50	..	3	3
Urine. .	..	25 to 12	100	4 to 6	12 to 6	2	3	3
Bile....	..	..	75	5	12	..	3	3
Blood ..	20 to 40	12	100 to 50	..	12	..	6	3
Brain ..	..	6	25	..	6	..	..	3
Nerves.	..	3	6	..	3	..	3	3
Muscles	..	12	25	..	12 to 6	..	3	3
Humours	..	6	3	..	6	..	3	least

From these experiments it is seen that the fluorescent substance which exists naturally in the tissues, at the very highest reaches to 6 grains in 100 litres of water, and usually is between 4 and 1½ grains of quinine dissolved in this quantity of water.

When quinine has been taken, even in a quarter of an hour the fluorescence may become equal to 75 grains of quinine in 100 litres of water. It is found to be in greatest quantity in the liver and the kidney, and somewhat less in the blood, urine, and muscles; still less in the brain, nerves, and bile; and the increase is slightly perceptible even in the lenses. In three hours the maximum effect of the quinine may be reached, the fluorescence being then from 100 to 200 grains of quinine in 100 litres of water. This amount was found in the liver, kidney, urine, bile, blood, brain, and muscles. The increase was much less perceptible in the nerves and in the aqueous humour, and was least in the lenses.

In six hours the amount of fluorescence was rather less than in three hours; in twenty-four hours it was considerably less than half as much as in three hours; in forty-eight hours there was but little more fluorescent substance than naturally exists in the textures, except in the liver and the blood; and in seventy-two hours there was no increase except in the liver. Hence, in guineapigs, in fifteen minutes the quinine has passed to all the vascular and probably to the extravascular textures. In three hours the amount of quinine in the textures may be at the maximum, and for six hours it remains in excess; in twenty-four hours the quinine is much diminished, and in forty-eight hours it is scarcely perceptible anywhere.

In order, if possible, to obtain very decisive proof that quinine passed into the non-vascular texture of the lens, we gave two pigs three grains of sulphate of quinine, and half an hour afterwards three grains more; the animals were killed five hours after the first dose. One lens of each pig was put into glycerine, to be compared with the lenses of two other pigs bought at the same time and place, to which no quinine was given. In the electric light there was no apparent difference, either in the colour or in the brightness of the fluorescence, between the pigs that had taken quinine and those that had taken none. Examined by the spark of the coil, the fluorescence of all four lenses was less than one-sixty-fourth of a grain of quinine in a litre of water; and scarcely any difference was perceptible, though the fluorescence of the lenses of the pigs that had taken quinine was slightly the strongest. In all, the fluorescence was rendered less strong by the addition of a strong solution of common salt to the solution.

Two pigs were both given fifteen grains of sulphate of quinine in five doses of three grains each, with the interval of one hour between each dose. They were killed one hour after the last dose, being, however, almost dead from the effects of the quinine. One lens of each animal was examined in the usual way. The fluorescence in both was equal to one-thirty-second of a grain of quinine in a litre of water, or three grains per 100 litres.

Professor Donders has carefully investigated the time in which atropine and Old Calabar bean begin and cease to act on the iris in man.

A solution of atropine dropped upon the cornea began to act in fifteen minutes, and attained its maximum in from twenty to twenty-five minutes. In forty-two hours the pupil was rather smaller; and even after thirteen days the pupil had not returned to its natural size.

The solution of Calabar bean began to act in from five to ten minutes. It attained its maximum in from thirty to forty minutes. At the end of three hours it began to diminish, and its effect disappeared entirely in from two to four days.

After continued applications of belladonna to the eye of a rabbit, it was thoroughly washed by a full current of water. The aqueous humour was then evacuated and brought into contact for a long time with the eye of a dog (De Graefe injected the aqueous humour into the anterior chamber); then a notable dilatation of the pupil was observed. As one part in 120,000 of water acts very energetically, the quantity must be very little that produced the dilatation of the pupil.

When belladonna used internally produces the dilatation, the aqueous humour which is taken from the anterior chamber is inactive.

Thirdly. The fluorescence that naturally occurs in different parts of the human body when no quinine had been taken before death was determined, in order that the effect of quinine on the fluorescence in the same parts might be estimated:—

The different parts were dried in a water-bath, and equal quantities of the dried substance were taken, amounting to 0·6 grain, that being the weight of the dry lens. The same method of extraction was followed. The solution was in all cases made up to twenty-five grains.

Extract from	per 100 litres.
Cartilage fluoresced one 32nd of a grain of quinine to a litre	=3·1
Nerves fluoresced a little more than one 64th of a grain of quinine to a litre	=1·6
Liver fluoresced a little more than one 64th of a grain of quinine to a litre	=1·6
Kidney fluoresced a little more than one 64th of a grain of quinine to a litre	=1·6
Lens fluoresced a little less than one 64th of a grain of quinine to a litre	=1·6
Lungs fluoresced one 128th to one 64th of a grain of quinine to a litre	=0·8 to 1·6
Muscle fluoresced a little more than one 128th of a grain of quinine to a litre	=0·8
Spleen fluoresced one 128th of a grain of quinine to a litre	=0·8

In another patient, who died after a surgical operation, the same quantity of substance, treated in exactly the same way, gave—

Extract from	per 100 litres
Cartilage fluoresced a little less than one 128th of a grain of quinine to a litre	=0·8
Nerves fluoresced a little less than one 128th of a grain of quinine to a litre	=0·8
Liver fluoresced a little less than one 64th of a grain of quinine to a litre	=1·6
Kidney fluoresced a little less than one 64th of a grain of quinine to a litre	=1·6
Lungs fluoresced one 128th of a grain of quinine to a litre	=0·8
Muscle fluoresced between one 128th and one 64th of a grain of quinine to a litre	=0·8 to 1·6
Spleen fluoresced a little less than one 128th of a grain of quinine to a litre	=0·8
Heart fluoresced one 128th of a grain of quinine to a litre	=0·8

When a much larger quantity of each organ was taken (whether from loss or destruction of substance in the process of preparation), no great increase of fluorescence was obtained.

Thus, of the different parts of a man who died of apoplexy, six grains were taken and treated as in the previous cases.

Extract from	per 100 litres.
Cartilage fluoresced one 128th to one 64th of a grain of quinine to a litre	=0·8 to 1·6
Nerves fluoresced one 64th of a grain of quinine to a litre	=1·6
Liver fluoresced one 64th of a grain of quinine to a litre	=1·6
Kidney fluoresced one 64th to one 32nd of a grain of quinine to a litre	=1·6 to 3·1
Lungs fluoresced one 64th of a grain of quinine to a litre	=1·6
Muscle fluoresced a little less than one 64th of a grain of quinine to a litre	=1·6
Spleen fluoresced one 64th to one 32nd of a grain of quinine to a litre	=1·6 to 3·1
Heart fluoresced one 32nd of a grain of quinine to a litre	=3·1
Brain fluoresced one 64th to one 32nd of a grain of quinine to a litre	=1·6 to 3·1

In a precisely similar way the different parts of the tissues of a woman who had taken small doses of quinine up to twenty-four hours of her death, were examined.

The tissues were dried in a water-bath, and 0·6 grain of the dry substance was taken for examination.

Extract from	per 100 litres.
Kidney fluoresced one 32nd to one 16th of a grain of quinine to a litre	=3·1 to 6·3

Nerves fluoresced a little more than one 64th of a grain of quinine to a litre.......................... =1·6
Liver fluoresced one 64th of a grain of quinine to a litre.... =1·6
Muscle fluoresced one 64th of a grain of quinine to a litre .. =1·6
Spleen fluoresced a little less than one 32nd of a grain of quinine to a litre .... =3·1
Lungs fluoresced one 128th to one 64th of a grain of quinine to a litre .... =0·8 to 1·6

Fourthly. On the increase of fluorescent substance in the human lens after different quantities of sulphate of quinine had been taken at different periods before the operation for cataract (for the means of making these experiments we are indebted to the great kindness of Mr. Bowman):—

The fluorescence of the human lens without cataract was about equal to one 64th of a grain of quinine in a litre of water; grs. per 100 litres.
That is, natural fluorescence about......................... =1·6
Sulphate of quinine was given for many days previous to the extraction of a cataract. After the operation the fluorescence was found to be less than one 16th and more than one 32nd of a grain of quinine to a litre of water .... =6·2 to 3·1
In four patients, aged respectively 75, 60, 60, and 72:—
the lens removed 1 hour after 5 grains of quinine, fluorescence =1·6
,, $1\frac{1}{4}$,, ,, =1·6
,, 2 ,, ,, =1·6
,, $2\frac{1}{4}$,, ,, =2·1 to 1·6

Fifthly. On the rate of increase of fluorescent substance in the urine after quinine was taken, or on the rapidity of the passage of quinine, when taken by the stomach, into and out of the urine of man:—

A healthy man breakfasted at 8.30 A.M.; at 12 he emptied the bladder, and took four grains of sulphate of quinine in solution. The fluorescence of the urine at different periods after the quinine was taken was examined by rendering it alkaline with caustic potash and shaking it up three times with its own bulk of ether. Half an ounce of urine was taken for each examination.

Urine passed at gr. of quinine. water.
12 (just before taking the quinine), fluorescence=$\frac{1}{128}$ to $\frac{1}{64}$ to 1 litre.
12.10, fluorescence ....= $\frac{1}{64}$,,
12.20 ,, ...= $\frac{1}{4}$ to $\frac{1}{2}$,,
12.30 ,, ...= $\frac{1}{4}$ to $\frac{1}{2}$,,
1 ,, ...= $\frac{1}{4}$ to $\frac{1}{2}$
2 ,, ...= $\frac{1}{2}$ to 1 ,,
4 ,, ...= $\frac{1}{4}$ to ,,
8 ,, ...= $\frac{1}{8}$,,
24 hours after quinine, fluorescence..........=$\frac{1}{16}$, little less.
48 ,, ,, ...=$\frac{1}{32}$, little more.
72 ,, ,, ...about $\frac{1}{128}$,,

The same man breakfasted at 8.30 A.M.; at 12 took four grains of sulphate of quinine.

Urine passed at	gr. of quinine.	water.
12 (just before taking the quinine), fluorescence	= $\frac{1}{128}$ to $\frac{1}{64}$	to 1 litre.
12.10, fluorescence	= $\frac{1}{128}$ to $\frac{1}{64}$	,,
12.20 ,,	= $\frac{1}{16}$	,,
12.30 ,,	= $\frac{1}{8}$ to $\frac{1}{4}$	,,
1 ,,	= $\frac{1}{4}$	,,
2 ,,	= $\frac{1}{4}$, rather more.	
3 ,,	= $\frac{1}{4}$ to $\frac{1}{2}$	,,
4 ,,	= $\frac{1}{4}$ to $\frac{1}{2}$	,,
8 ,,	= $\frac{1}{4}$	,,
24 hours after quinine, fluorescence	= $\frac{1}{16}$ to $\frac{1}{8}$	,,
48 ,, ,,	= $\frac{1}{32}$	,,
72 ,, ,,	scarcely perceptible.	

The same man, at 12 at noon, took four grains of sulphate of quinine. The same quantity of urine (half an ounce) was taken for each examination.

Urine passed at	gr. of quinine.	water.
12 noon (just before taking the quinine), fluorescence	= $\frac{1}{128}$ to $\frac{1}{64}$	to 1 litre.
12.10, fluorescence	=	,,
12.20 ,,	= $\frac{1}{4}$	,,
12.30 ,,	= $\frac{1}{2}$	,,
1 ,,	= $\frac{1}{2}$ to 1	,,
2 ,,	= 1	,,
3 ,,	= $\frac{1}{2}$ to 1	,,
4 ,,	= $\frac{1}{2}$ to 1	,,
8 ,,	= $\frac{1}{4}$ to $\frac{1}{2}$	,,
24 hours after quinine, fluorescence	= $\frac{1}{8}$	,,
48 ,, ,,	= $\frac{1}{32}$ to $\frac{1}{16}$	,,
72 ,, ,,	scarcely perceptible.	

Hence in from ten to twenty minutes the quinine is detectable in the urine, and in from two to three hours after four grains have been taken it is in greatest amount in the urine; and even in three or four hours the quantity in the urine may be diminishing, and for more than forty-eight hours it will continue to pass off. Before seventy-two hours are passed not a trace will be perceptible.

A boy passed urine at 12, and immediately took four grains of sulphate of quinine. The fluorescence was observed at different periods after the quinine was taken, half an ounce being taken for each determination.

Urine passed at	gr. of quinine.	water.
12 (just before the quinine), fluorescence not perceptible.		
12.10, fluorescence below	$\frac{1}{128}$	to 1 litre.
12.20, ,,	$\frac{1}{32}$ to	,,

Urine passed at		gr. of quinine.	water.
12.30,	fluorescence below	$\frac{1}{4}$	to 1 litre.
1,	,,	$\frac{1}{4}$ to $\frac{1}{2}$	,,
2,	,,	$\frac{1}{2}$ to 1	,,
3,	,,	$\frac{1}{2}$ to 1	,,
4,	,,	$\frac{1}{2}$ to 1	,,
8,	,,	$\frac{1}{4}$	,,
24,	,,	$\frac{1}{16}$ to $\frac{1}{8}$	,,
48,	,,	$\frac{1}{32}$	,,
72,	,,	scarcely perceptible.	

The same boy breakfasted at 8.30, passed urine at 12 noon, and immediately took four grains of sulphate of quinine. The fluorescence was observed in half an ounce of the urine passed at different periods after the quinine was taken.

Urine passed at		gr. of quinine.	water.
12 (just before taking the quinine),	fluorescence	$\frac{1}{128}$, little more,	to 1 litre.
12.15,	fluorescence	$\frac{1}{16}$ to $\frac{1}{8}$	
12.30,	,,	1	,,
1,	,,	above 1	,,
2,	,,	above 1	,,
3,	,,	above 1 (was at maximum)	,,
4,	,,	above 1	,,
8,	,,	1	,,
24,	,,	$\frac{1}{16}$	,,
48,	,,	$\frac{1}{128}$ to $\frac{1}{64}$	,,
72,	,,	not perceptible.	

Hence in fifteen minutes quinine is detectable in the urine, and in three hours the maximum quantity is present in the urine. In eight hours it begins to decrease; in forty-eight hours it is much decreased; and in seventy-two hours it has entirely disappeared.

CONCLUSIONS.

Part I.

From every texture of man and of some animals a fluorescent substance can be extracted, which is identical with the fluorescent substance that for some years has been known to exist in the lenses of man and animals.

This fluorescent substance, when extracted, has a very close optical and chemical resemblance to quinine, and when mixed with quinine it cannot be separated from it; we have therefore called it "animal quinoidine."

Part II.

By quantitative determinations of the amount of fluorescent substances naturally existing in the textures, we were able to determine the rate and time of increase of fluorescent substance in the vascular and non-vascular textures of animals, and in the urine of man after quinine had been taken. By this means we have shown that in guineapigs, in fifteen minutes, the quinine has certainly passed into all the vascular, and most probably into the extra-vascular textures. In three hours the amount of quinine in the textures may be at the maximum; and for six hours it does not much diminish. In twenty-four hours the quinine sinks very considerably, and in forty-eight hours it is scarcely perceptible anywhere.

By similar experiments on cataracts in man, it appears that in two hours and a quarter traces of quinine may be found in the lens.

After quinine has been taken, it begins to appear in the urine in from ten to twenty minutes; in from two to three hours it has reached its maximum; in from three or four, or at longest eight hours, it begins to decrease; in twenty-four hours it has very much decreased; in forty-eight hours its presence is still detectable; but in seventy-two hours not a trace of it can be found.

April 19, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "Account of the Discovery of the Body of a Mammoth, in Arctic Siberia," in a Letter from Dr. CARL ERNST VON BAER, of St. Petersburgh, For. Mem. R.S. Received April 17, 1866.

A la Société Royale de Londres.

Présumant que la Société Royale de Londres prendra peut-être quelque intérêt à la découverte nouvelle d'un mammouth avec sa peau et ses poils dans le sol gelé de la Sibérie arctique, je ne veux pas manquer de lui faire cette communication.

Déjà en 1864 ce mammouth a été trouvé par un Samoïède dans les environs de la baie du Tas, bras oriental du grand golfe de l'Obi. Ce n'est que vers la fin de l'an 1865, que j'en ai reçu la nouvelle. Mais comme dans ces régions les corps des grandes bêtes se conservent longtemps, s'ils ne sont pas pleinement mis à découvert, et que ce mammouth, au moins en 1864, restait encore enchâssé dans les terres gelées, l'Académie de St. Pétersbourg a expédié, avec l'aide du gouvernement, au mois de février de l'année courante, M. Fréd. Schmidt, paléontologue distingué, pour examiner non-seulement l'animal, mais aussi sa position dans la localité. Nous espérons que M. Schmidt arrivera avant que la destruction soit trop avancée, et qu'on aura non-seulement connaissance complète de l'extérieur de l'animal, mais aussi de sa nourriture par la dissection de l'estomac. Ce serait la première fois qu'un naturaliste soit venu à temps pour ces recherches, car Adams, comme on sait, est arrivé trop tard. Il a trouvé les crins tombés de la peau et a pleinement négligé d'examiner la nourriture.

Un rapport plus détaillé sur la trouvaille de ce mammouth et sur l'expédition est sous presse et j'aurai l'honneur de la transmettre à la Société Royale. Mais les nouvelles de ce qu'a trouvé M. Schmidt ne peuvent arriver qu'après quelques mois. Dr. Ch. Ern. de Baer.

St. Pétersbourg, $\frac{\text{30 Mars}}{\text{11 Avril}}$ 1866.

II. "On the Bursa Fabricii." By John Davy, M.D., F.R.S., &c. Received March 24, 1866.

In this paper I have the honour to submit to the Society some observations which I have made on the Bursa Fabricii—an organ respecting the function of which so little has yet been determined with any certainty, some physiologists regarding it, after the manner of the author who first described it, as a receptaculum seminis, others as the analogue of Cowper's glands, others as that of the prostate; and one as that of the urinary bladder of fishes.

For the sake of order and to save some repetition, before entering into particulars it may not be amiss to state briefly that this peculiar organ, in every instance it is met with, is found to lie low in the cavity of the pelvis, behind the intestine, either directly in the median line, or a little on one side of it; that it is covered anteriorly by the reflected peritoneum; is composed mainly of two coats, one an outer muscular, the other an inner mucous, the latter in the instances of most development abounding in follicles; that it communicates with the cloaca by an opening, in the female, close to the entrance of the oviduct, in the male between and a little inferior to that of each vas deferens, in both inferior to the termination of the ureters*; and that it has over its orifice, when most perfect, a slight valvular fold, affording some, but not perfect, security against the entrance into its cavity of fæcal matter whilst passing in the act of expulsion.

What is remarkable in this organ, giving rise to much of the obscurity adverted to, is the different aspects which it exhibits in the same animal according to age, and the differences as to form and proportional size and degree of persistence which it presents in different species.

The number of birds in which I have sought for the organ, and have examined it when found, has been considerable, at least thirty different species, all of them, with the exception of the skylark, belonging to or frequenters of the Lake district.

I may further briefly premise that, when the microscope has been used, the power employed has been that of $\frac{1}{8}$th inch focal distance, and that, when

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* The ureters in those birds in which they are most easily traced, such as the common fowl, turkey, goose, I have found not to terminate in the cloaca, but just above it, near, or in the margin of the inner anal aperture (anus interne of M. Milne-Edwards), *i. e.* the orifice of the rectum into the cloaca; and, in consequence, the urinary excretion is voided adhering to the inferior portion of the fæcal mass which accumulates in the lower rectum—which is unusually capacious and glandular; accordingly, from such observations as I have made, I cannot but entertain great doubt of the cloaca being the proper place for the reception of the urine before its expulsion.

spermatozoa have been sought for, a drop of a solution of common salt of the sp. gr. 1038, has been added to the fluid to be examined.

In the descriptive part which follows, I propose to confine myself to the more striking examples illustrative of the peculiarities adverted to and likely to aid in accounting for them, passing over the several species, or very briefly noticing them, when displaying no marked difference.

I. *Common Fowl* (Gallus domesticus).—I begin with this bird, as I have had the best opportunity of examining it at different ages.

1. Of a chicken four days old, the bursa was about the size of a small pea; it communicated with the cloaca, and was empty.

2. Of another chicken, seventeen days old, found dead on the 10th of March from cold, the bursa measured ·3 by ·2 inch; it was distinctly plicated internally; it was empty.

3. Of a young cock, eleven weeks old, examined on the 16th of June, the bursa, of a globular form, was 1·1 inch in diameter; it communicated with the cloaca by a narrow neck about ·15 inch in width; internally it was strongly plicated; the projecting laminæ were of a crescentic form, about twenty in number, and their width, where widest, was about ·4 inch. It contained some turbid fluid, in which were numerous mucus-like corpuscles and a few well-defined spermatozoa. The testes were large; besides sperm-cells, they contained some spermatozoa.

4. Of another cock, hatched in July, examined when nineteen weeks and six days old, the bursa, 1·2 inch in diameter, weighed 74 grs.; it was similar to the preceding in structure, and was empty.

5. Of a third male, hatched on the 19th of September, examined when twenty-one weeks and one day old, weighing six pounds, the bursa was 1·5 inch in diameter; its plicæ like the preceding, its opening into the cloaca large; many spermatozoa were found in the little turbid fluid with which they were moistened. The testes were large; the left weighed 144 grs., the right 130 grs.; the vasa deferentia were small.

6. Of a fourth, hatched on the 18th of October, examined on the 20th of March, when seventeen weeks and seven days old, weight five pounds, the bursa, compared with the preceding, was of diminished size; its diameter only ·6 inch, its plicæ few, short and thick, and bloodshot; its opening into the cloaca large and exposed, without any valvular protection. It contained a little thick mucus, in which there was commingled an appearance of spermatozoa. The testes were large, and abounded in sperm-cells and spermatozoa; and the vasa deferentia were well developed, and contained a cream-like fluid rich in delicate spermatozoa*.

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* The examination was made whilst the fowl was still warm. The fluid of the testes had a distinct alkaline reaction. In other instances I have obtained the same result, showing thus a marked difference when compared with the ovum, the yelk of which, when fresh, I have always found to exhibit an acid reaction, proper precautions being taken to avoid contact with the alkaline white. See the author's 'Physiological Researches' (1863), p. 426.

7. In a fifth, a cock of about six years old, weighing four pounds and a half, no traces of a bursa could be found. The vasa deferentia were large, and were distended with a cream-like fluid abounding in spermatozoa. They terminated well apart in the cloaca, and had neither of them a visible papilla*. The right testis weighed 93·7 grs., the left 119·7 grs.

8. Of a young hen, hatched on the 17th of May, examined when eleven weeks old, the bursa was more flask-like than globular; it measured 1·7 by 1·5 inch. Its plicæ were large, and their glandular structure so well developed that the orifices of the follicles, as puncta, were seen with the naked eye. The bursa was empty, merely moistened with mucous fluid.

9. Of a hen hatched on the 17th of March, examined when seventeen weeks and five days old, the bursa was about the same size and form as that of the preceding; it contained a small quantity of turbid mucous fluid, in which were seen delicate filaments bearing a resemblance to spermatozoa.

10. Of another, hatched in May, and which, like the preceding, had never laid, examined when nineteen weeks old, the bursa, which was empty, measured 1·7 by 1·5 inch.

11. Of a fourth, hatched on the 19th of September, said to have laid and known to have been trod, examined when twenty-four weeks old, the bursa was of shrunken appearance; it was ·5 inch in diameter; its parietes thick (·2 inch thick); there were no plicæ; it communicated freely with the cloaca, and contained a little mucous fluid in which were seen filaments like spermatozoa, but not unmistakeably such. There was a large ovum nearly ready to be detached from the ovary. A very few tolerably distinct spermatozoa were found in the oviduct, which was well developed.

12. In a fifth, hatched on the 19th of July, examined when twenty weeks and five days old, after having laid about twenty-five eggs, no vestige of a bursa could be detected †.

13. Of another, about eight months old, examined on the 17th of March, after having laid three or four eggs, the bursa was of a tubular form, ·9 inch long by ·1 wide; its walls were exceedingly thin, and it did not communicate with the cloaca. In the little turbid mucous fluid it contained, a single spermatozoon was detected. There was a fully formed egg in the oviduct, the incrustation of which had begun. Nearest the infundibulum many spermatozoa were found.

14. In a laying hen about three years old the bursa was reduced to a small hard mass, hardly equal to a pea in size. It contained a minute cavity without an opening into the cloaca.

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* After immersion in water for forty-eight hours, then from distention the papillæ appeared, each about ·24 inch in length.

† This was a clucking hen, and the day before had been trod. The ova in the ovary were small, the largest ·2 inch in diameter. There was no egg in the oviduct; spermatozoa were found in different parts of it.

15. In another, about three years and a half old, no traces of a bursa could be detected. Its cloaca and oviduct were very large, as were also those of the preceding.

II. *Pheasant* (Phasianus colchicus).—In ten examined (seven males, three females), with the exception of three (inferred to be old birds), the organ in question was found. It resembled in structure that of the common fowl of from four to eight months old. In each instance it was empty, merely wet with mucous fluid. These birds were shot in November, December, January, and February. Not knowing their precise age, but supposing them to have been hatched in the spring, their bursa as to size was somewhat less than that of the common fowl. The smallest, that of a hen shot in February, measured ·3 inch by ·2; it retained its plicated structure, and freely communicated with the cloaca.

III. *Partridge* (Perdix cinerea).—Of this bird three specimens have been examined. In one, apparently old, no trace could be found of a bursa. In the other two, in which it occurred, it resembled in form and structure that of the common fowl; it measured ·4 inch by ·3. These were young birds which had taken wing.

IV. *Turkey* (Meleagris gallopavo).—In three instances of this bird, all hatched in spring, one examined in October, one in December, one in January, the bursa was found similar to that of the common fowl, and in each nearly of the same size, about 1·5 inch by ·7.

V. *Grouse* (Tetrao scoticus).—In a young bird, not fully fledged, just capable of a short flight, shot in the island of Lewis on the 12th of August, expressly for the purpose of examination, the bursa was found small, about the size of a pea. Air was found in its humeri, but only partially in its femora. In two, both from Scotland, later in the season, no bursa could be detected. Their femora contained air as well as their humeri.

VI. *Pigeon* (Columba domestica).—In two full-grown males examined in September, no trace was found of a bursa; in other two (these younger birds) the bursa was pretty large.

VII. *Buzzard* (Falco buteo).—Of a young one taken from its nest on the 9th of June, when about a fortnight old, the bursa was globular and comparatively large, about ·8 inch in diameter, non-plicated, and empty. This nestling weighed 5193 grs.; it was covered, except at the umbilicus, where bare, with plush-like yellow feathers, thick, very closely set, equal in weight to 647 grs.

VIII. *Sparrow-hawk* (Falco nisus).—Of a young bird, examined on the 31st of July, just capable of flight, weighing 3686 grs., its sternum still cartilaginous, its humeri only partially filled with air, the bursa was small. In another, a male, apparently an old bird, shot on the 8th of March, no traces of bursa were found. It weighed 2836 grs.

IX. *Tawny Owl* (Strix stridula).—Of a young one taken from its nest on the 21st of June, when well fledged, but its quill-feathers not fully formed, weight 4496 grs., the bursa was globular and comparatively large, ·7 inch in diameter. It contained a good deal of turbid urinary fluid abounding in lithate of ammonia. It had no air in any of its bones.

In an old bird, the parent of the preceding, no bursa was found.

X. *Cuckoo* (Cuculus canorus).—Of a young one shot on the 25th of August, weight 1310 grs. (judging from its plumage, a bird of this season), the bursa was very small. In another, a male*, an older bird, shot on the 6th of June, weight 1768 grs., no traces could be found of a bursa.

XI. *Common Goose.*—Of one hatched in the spring, examined when about six months old, the bursa, of an ovoid form, measured 1·2 by ·7 inch. It was plicated, like that of the common fowl. Of two others, one four months old, one of about eight months, the bursa was about the same size as the preceding.

XII. *Common Duck.*—Of one, a male hatched in March, the bursa was of a cylindrical form, 1·6 inch long by ·4 inch wide. Its lining membrane was without plicæ; the apertures of the mucous follicles were conspicuous, and arranged in parallel lines. It contained some dark fæcal matter similar to that in the intestine.

Of another male, about three months old, the bursa was of a flask-like form, 2·6 inches in length, ·6 inch in width where widest; in structure like the preceding. It contained a turbid greyish fluid, in which were suspended granules and small globules. It was coagulated by nitric acid.

Of a female, about a month older, of the same brood, the bursa resembled the last.

Of another female, a little more than a year old, the bursa was very small; its cavity was not quite obliterated, nor was its opening into the cloaca closed.

Of a male about two years old, no traces remained of a bursa.

XIII. *Water-hen* (Gallinula chloropus).—Of a male shot in November, the bursa, of a flask-like form, was ·6 inch by ·4; its cavity was small and without plicæ.

XIV. *Common Coot* (Fulica atra).—In a male shot on the 8th of March no bursa was found.

XV. *Common Gull* (Larus canus).—The same remark applies to one examined in January.

XVI. *Woodcock* (Scolopax rusticola).—In two examined, one in December, the other in February, no traces of a bursa were found.

XVII. *Rook* (Corvus frugilegus).—Of this bird thirteen specimens have been examined. In eight no traces of a bursa were detected; from their appearance and the quality of their bones, it was inferred that they were a year or more old. In the remaining five a bursa was met with; three were examined in May, two in August; all had the marks of young birds. Of one, which weighed 6132 grs., caught when not quite capable of flight,

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* One testis weighed ·7 gr., and measured ·24 inch by ·26; the other 1 gr., and measured ·28 by ·20 inch; they were of a rich yellow colour, and contained sperm-cells and spermatozoa, the latter of uniform thickness, about ·002 inch in length, nowise spiral.

the bursa was nearly globular and about ·7 by ·6 inch; it was distended, as was also the cloaca, with a turbid fluid abounding in lithate of ammonia. Its inner surface was not plicated, but slightly pitted. Of the others, the bursa differed little from the last; it was somewhat smaller. In the bursa of one of these some flakes were found, consisting chiefly of lithate of ammonia.

XVIII. *Carrion-crow* (C. corone).—Of a young male shot on the 21st of June the bursa was globular, very like that of the rook, about ·7 inch in diameter; it contained some flakes of lithate of ammonia.

Of another, shot on the 30th of June, weight 6163 grs., the bursa was somewhat different in form; it was broadest at its base; it measured ·9 by ·8 inch.

Of one killed on the 16th of July, weight 5653 grs., the bursa was smaller; it contained some flakes of lithate of ammonia.

Of a fourth, killed in February, which, judging from the smallness of the oviduct, was not an old bird, the bursa, had it not been for its opening into the cloaca, might have escaped observation, not on account of its smallness, for it was little less than that of the preceding, but from the extreme thinness of its coats and adherence to the adjoining tissues.

XIX. *Jackdaw* (C. monedula).—Three specimens have been examined. Two of these were old; in neither of them was there a bursa: one was young; it was shot on the 11th of July, and was fully fledged; its bursa was pretty large, similar to that of the rook, and empty.

XX. *Jay* (C. glandarius).—Of one, judging from the state of its bones, hatched in spring, the bursa was comparatively large, heart-shaped, measuring ·6 by ·4 inch. Its cavity was small; its inner surface smooth, without plicæ.

XXI. *Blackbird* (Turdus merula).—Six different specimens have been examined. In an old bird shot in March, a male, no trace of a bursa was found.

In an unfledged nestling, weighing 112 grs., found dead, the bursa was so thin as to be translucent; it was proportionally large, and contained some flakes of lithate of ammonia.

In the others, which were examined between the middle of June and the beginning of October, none of them, it was inferred, more than five months old, the bursa, nearly globular, was from about ·4 to ·5 inch in diameter; its lining membrane was smooth; its parietes proportionally thick. In each instance it was empty.

XXII. *Song-thrush* (T. musicus).—In an old male examined on the 28th of June no bursa was found.

Of two young ones obtained on the 15th of the same month, the bursa was about the size of a large pea; one was empty, the other contained some lithate of ammonia.

XXIII. *Water-ousel* (T. cinclus).—In one, a male, probably a young one, a small bursa was found. It was shot on the 11th of November.

In another, an older bird (judging from its appearance), shot at the same time, there was no trace of a bursa*; and the same remark applies to a third examined in January.

XXIV. *Common Starling* (Sturnus vulgaris).—Of a young one shot on the 29th of June, the bursa was about the size of that of the young thrush. In two old birds shot on the 7th of March, not a trace of the organ was found.

XXV. *Skylark* (Alauda arvensis).—In two examined in January there was the like deficiency.

XXVI. *Chiffchaff* (Trochilus minor).—Of one, a young bird, examined on the 14th of July, the bursa, of moderate size, contained a little fæcal matter. In another, an older bird, shot in March no bursa could be found.

XXVII. *Robin* (Sylvia rubecula).—Of a young one found dead on the 16th of June, still warm, weight 255 grs., the bursa was of moderate size.

Of another, nearly fully fledged, found dead on the 25th of June, weight 285 grs., the bursa was comparatively large, exceeding a little in size that of the preceding.

XXVIII. *Yellow Ammer* (Emberiza citrinella).—In one, a male, its testes abounding in spermatozoa, examined on the 16th of June, there was no trace of a bursa.

XXIX. *Blue Tit* (Parus cæruleus).—Of two young ones examined on the 8th of June, when just able to fly, one weighing 177·5 grs., the other 162 grs., the bursa was comparatively large; in each it was empty.

XXX. *Cole Tit* (P. ater).—In one examined in February no trace of the organ could be found.

XXXI. *Martin* (Hirundo urbica).—Of three young ones taken from the nest on the 10th of July, the bursa was comparatively large; in each it was empty. One nestling weighed 414 grs., another 398 grs., the third 423·5 grs. The parent birds were both found of less weight; that of the male was 263 grs., of the female 287·4 grs. In the latter a distinct bursa was found, little less than that of the young birds. Search was made for spermatozoa, but none were found in it. Of the male, the pelvis was so injured by shot that it was useless for examination. In a male shot on the 1st of August, the testes of which contained spermatozoa, no trace of a bursa was found.

From the preceding results may not the following conclusions be drawn?—

1st. That in some birds, as in the common fowl, and probably in all the gallinaceous family, and that of the Anatidæ, the bursa increases in size and in completeness of organization up to a certain age, beyond which it gradually diminishes equally in both sexes, and eventually disappears.

2nd. That in other birds, those of rapid growth, which take wing as

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* It was shot near the river Brathy, in which charr were then spawning. In its gizzard and œsophagus nine ova of this fish were found; they were transparent when extracted, but immersed in water they soon became opaque.

soon as they are capable of flight, the bursa is comparatively large whilst they are nestlings, does not increase conspicuously, if at all, with their growth, but rather diminishes, and after a certain age disappears, and probably sooner than in the first mentioned. The buzzard and owl are examples, and probably all birds of the same family, all of the Corvinæ, all of the thrush kind, and all the smaller birds, with the exception perhaps of the female martin.

Of the uses of the organ, I venture to conjecture, founding my conjectures on what I have observed, that they may be provisional and various; that in some birds, whilst nestlings, it may act the part of a urinary bladder, as witnessed in the instance of the young owl, and in some of the young rooks, crows, and thrushes, thereby tending to prevent the fouling of the nests; that in others it may serve as a seminal reservoir at an early period, and in both male and female in the instances mentioned, in which it has been found most completely formed before the attainment of full size—in the male before the vasa deferentia are fully developed, in the female so long as the oviduct is still small and unexpanded; and that generally, as the organ is more or less amply supplied with mucous follicles, it may serve, by the secretion it yields, to lubricate the cloaca with which it is connected, and to aid in its functions.

These conjectures, or inferences, if deserving of being so considered, might be supported by what we know of the structure of the part and its position in relation to the termination of the ureters, of the spermatic vessels, and of the oviduct; but I think it better to rest them on the facts observed—the urinary matter detected in the organ in some instances, the spermatozoa in others*, and the mucous fluid generally.

But granting even that the bursa may be useful, and in the female as well as in the male, in aid of fecundation, as Fabricius supposed, yet his extreme view that that aid, in the stored-up spermatic fluid in the bursa of the hen bird, might suffice for a year, as stated in the subjoined passage†, for which and for other extracts I am indebted to the kindness of Professor Sharpey, is both highly improbable, and is opposed by the fact of the decrease of the bursa with the advancing age of the fowl, and the enlargement of the ovi-

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* As in no instance I have yet found unquestionable spermatozoa in the bursa of the young hen, I cannot fairly infer that the bursa in the female is a receptaculum seminis; but as in two instances there were detected in it filaments which were very like these bodies, and in one a single pretty distinct spermatozoon, and further, as the bursa seems to diminish rapidly as the oviduct becomes developed and not till then, is it not probable that for a short time it may perform the part assigned, as conjectured above? It need hardly be remarked that the mucous secretion of the bursa adds to the difficulty of demonstrating the presence of spermatozoa.

† "Semen autem Galli ad podicem immittitur, et in vesica reponitur et conservatur, quousque pullus conformetur; immo vero per totum integrum anni tempus inibi servatur, postea quam semel admisso Gallo, ova omnia per totum illud anni tempus fœcunda redduntur, tanquam vesica unicum ob id foramen habente, ut in concluso loco semen Galli diutius ut in proprio et congruo loco servetur."—*Opp. Omnia,* Lugd. Bat. 1738, p. 21.

duct and of the vasa deferentia. Harvey, who was opposed to the notion of Fabricius, expresses the opinion that an intercourse once or twice repeated might suffice to impregnate a whole bunch of yelks, he having found that an egg laid on the 20th day of seclusion of a hen produced a chick*. This fact is an interesting one, however explained. It might be adduced in favour of the opinion of Fabricius; but inasmuch as the passage of the fully-formed egg in the act of expulsion does not necessarily secure the expulsion of any spermatozoa previously received into the oviduct, it is of little value in the argument: and here I may mention that I have detected spermatozoa in the oviduct, even in that portion in which the egg was receiving its calcareous incrustation.

III. "Researches on Gun-cotton.—Memoir I. Manufacture and Composition of Gun-cotton." By F. A. Abel, F.R.S., V.P.C.S.

Received April 10, 1866.

(Abstract.)

A review of the researches on the production, properties, and composition of gun-cotton hitherto published, and a brief examination into the probable causes of the discrepancies exhibited between the results and conclusions of different experimenters, are followed in this paper by a criticism of the several steps in the system of manufacture of gun-cotton, as prescribed by Baron v. Lenk.

The conclusions arrived at on this subject are founded upon carefully conducted laboratory-experiments, and upon extensive manufacturing operations carried on during the last three years at the Royal Gunpowder Works, Waltham Abbey. In some of these operations v. Lenk's system of manufacture, as originally communicated to the English Government by that of Austria, was strictly followed; in others, various modifications were introduced in different stages of the manufacture—such as in the composition of the acids used, in the proportion borne by the cotton to the acids in which it remained immersed, in the duration of the treatment of cotton with the acids, and in the methods of purification to which the gun-cotton was submitted.

Exception is taken to one or two points in the general system of manufacture, and directions are indicated in which they may be advantageously modified; but the general conclusion arrived at is that, although Baron v. Lenk cannot be said to have initiated any new principle as applied to the production of gun-cotton, he has succeeded in so greatly perfecting the process of converting cotton into the most explosive form of pyroxyline or gun-cotton, and also the methods of purification, as to render a simple attention to his clear and definite regulations alone necessary to ensure the

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* Opera Omnia, a Col. Med. Lond. ed. 1766, p. 206.

manufacture of very uniform products, which are unquestionably much more perfect in their nature than those obtained in the earlier days of the history of gun-cotton. Great stress is laid upon the fact that deviations from the prescribed process, which at first sight may appear trivial (such as a slight modification in the strength of the acids used, the neglect of proper cooling-arrangements), are certain to lead to variations in the products of manufacture, affecting their explosive characters, or their permanence, or both. A considerable deviation from the normal composition, due evidently to some accidental irregularities in the course of manufacture pursued, has been exhibited occasionally by gun-cotton obtained from the manufactories at Hirtenberg and Stowmarket.

The composition of gun-cotton has been made the subject of a very extensive series of experiments, both analytical and synthetical. The material employed in the analytical researches consisted of ordinary products of manufacture, prepared at Waltham Abbey, and obtained from Hirtenberg and Stowmarket. The general analytical results are as follows:—

Air-dry gun-cotton contains very uniformly about two per cent. of water, which proportion it reabsorbs rapidly from the atmosphere after desiccation. If exposed to a moist confined atmosphere, it will gradually absorb as much as six per cent. of water; but it rarely retains more than two per cent. upon re-exposure to open air.

The mineral constituents of gun-cotton vary according to the quality of the water employed in its purification. The average proportion of ash furnished by gun-cotton prepared at Waltham Abbey, where the water used is hard, amounts to one per cent. It should be observed that the process of "silicating" the gun-cotton, which is prescribed by Von Lenk, but the value of which is not admitted, has been applied at Waltham Abbey only in special experimental operations. Its use naturally adds to the mineral constituents contained in the finished products.

The proportions of matters soluble in alcohol alone, and in mixtures of alcohol and ether, were found to be remarkably uniform in products of manufacture obtained by strictly following Von Lenk's directions. In the ordinary products from Waltham Abbey, the matter extractable by alcohol amounted to between 0·75 and 1 per cent., and consisted of a yellowish nitrogenized substance possessed of acid characters, and evidently produced from matters foreign to cellulose (which are retained by cotton fibre after its purification), and the products of oxidation which escape complete removal when the gun-cotton is submitted to purification in an alkaline bath. The average proportion of matter extractable by ether and alcohol after the alcoholic treatment is from 1 to 1·5 per cent. This consists of one or more of the lower products obtained by the action of nitric acid upon cotton-wool, the existence of which was established by Hadow. The causes of the invariable production of small proportions of these substances in the ordinary manufacturing operations, and of their existence in larger quantities in exceptional instances, have been carefully examined into.

Their absolute removal from specimens of gun-cotton, purified for analytical purposes, was found to be almost impossible.

The methods employed for determining the proportions of carbon, hydrogen, and nitrogen in gun-cotton, and the relative proportions of carbonic acid and nitrogen furnished by its combustion, have been very carefully tested. Four different methods of determining the carbon were employed, and forty-nine successful estimations of that element have been accomplished in a variety of products of manufacture. A number of very concordant hydrogen-determinations, and eighteen direct estimations of the volumes of nitrogen furnished by the complete oxidation of gun-cotton, have been made. The individual as well as the mean results obtained in these analytical experiments correspond much more closely to the requirements of the formula $\text{Ȼ}_6\,H_7\,N_3\,\Theta_{11}=\text{Ȼ}_6\left\{\begin{matrix}H_7\\ 3\,N\Theta_2\end{matrix}\right\}\Theta_5$, *trinitro-cellulose,* or $\text{Ȼ}_{12}\,H_{14}\,\Theta_7, 3N_2\,\Theta_5$, *trinitric cellulose*) than to the formula recently assigned for gun-cotton by Pelouze and Maury, $\text{Ȼ}_{24}\,H_{36}\,\Theta_{18}, 5\,N_2\,\Theta_5$. The determinations of the comparative volumes of carbonic acid and nitrogen have furnished results closely in accordance with those of the direct determination of nitrogen.

Since the specimens of gun-cotton analyzed always retained small quantities of the products soluble in ether and alcohol, it was to be expected that the proportion of nitrogen found would be slightly below, and consequently that the carbon-results would be somewhat above, those which the chemically pure substance should furnish. The variations exhibited by the analytical results do not exceed such as are ascribable to the above cause.

A number of experiments were instituted with Hadow's method of determining the composition of gun-cotton, which consists in reducing the latter to cotton by means of potassic sulphydride. The results show that, although the method is useful for controlling the results obtained, by determining the increase of weight which cotton sustains by treatment with nitric acid, it does not afford sufficiently definite and trustworthy data to render it applicable as a method of ascertaining the degree of perfection of manufacturing products, *i. e.* the extent of freedom of a specimen of the most explosive gun-cotton from admixture with the soluble varieties.

The treatment of cotton with nitric and sulphuric acids has been varied in many ways in laboratory experiments, with the view to examine fully into the increase in weight sustained by the former, upon its conversion into the most explosive gun-cotton, and to determine what circumstances may exert an influence upon the amount of increase,—an acid mixture of uniform strength being employed throughout the experiments (3 parts by weight of sulphuric acid of spec. grav. 1·84, and 1 part of nitric acid of spec. grav. 1·52). The results arrived at may be briefly summed up as follows :—

Finely carded and carefully purified cotton-wool will sustain an increase of weight varying between 81·8 and 82·5 upon 100 parts of cotton, if submitted for 24–48 hours to treatment with a very considerable excess (about 50 parts to 1 of cotton) of the acid mixture. Similar results may also be obtained by repeatedly treating the same sample of cotton for comparatively brief periods with fresh quantities of acid, provided this treatment be not too greatly prolonged. Lower results (somewhat above or below 78 upon 100 parts of cotton) are obtained if the cotton be submitted to treatment with a large excess of acid for only brief or for very protracted periods, or if it be left for about 24 hours in contact with a comparatively limited proportion of acid (10 or 15 to 1 of cotton). The increase of weight which 100 *parts of pure cellulose* should sustain by *complete* conversion into a substance of the formula $\Theta_6 H_7 N_3 \Theta_{11}$, is 83·3; if converted completely into a substance of the composition $\Theta_{24} H_{36} \Theta_{18}, 5 N_2 \Theta_5$, it should sustain an increase in weight of 77·78.

There is strong evidence that the differences between the highest results furnished by carefully purified cotton-wool, and the number 83·3, are to be principally ascribed to the small proportions of foreign matter still existing in the fibre at the time of its conversion.

The maximum increase of weight sustained by cotton of ordinary quality, such as is used in gun-cotton-manufacture, is, as might have been anticipated, below the result obtained, under similar conditions, with cotton of finer quality and more thoroughly purified. The highest numbers obtained by treatment of such cotton, in small quantities, with a considerable excess of acid, were somewhat below 181, from 100 of cotton. The increase of weight which this quality of cotton sustains is, however, more generally about 78 per cent.

Experiments are quoted which show that the attainment of lower results with cotton of ordinary quality is ascribable to the existence of higher proportions of foreign matters in the cotton under treatment.

Some quantitative manufacturing experiments yielded results considerably below those obtained with some of the same cotton in laboratory operations (171 and 176 of gun-cotton having been produced from 100 of cotton). The causes of these differences are investigated and explained.

The identity in their characters, and close resemblance in composition, of the most perfect results of laboratory experiments, and of the purified products of manufacture, the close approximation frequently exhibited by the weight of the former to the theoretical demands of the formula $\Theta_6 H_7 N_3 \Theta_{11}$ (which may be expressed as

$$\Theta_6 \left\{ \begin{matrix} H_7 \\ 3\,N\Theta_2 \end{matrix} \right\} \Theta_5, \text{or } \Theta_{12} H_{14} \Theta_7, 3 N_2 \Theta_5),$$

and the satisfactory manner in which the unavoidable production of somewhat lower results in the manufacturing operations admits of practical demonstration, appear to afford conclusive evidence of the correctness of either of the above formulæ, as representing the composition of the most explosive gun-cotton, and demonstrate satisfactorily that the material, prepared strictly according to the system of manufacture perfected by Von Lenk, consists uniformly of the substance now generally known as trinitro-cellulose, in a nearly pure condition.

IV. "On the Mysteries of Numbers alluded to by Fermat." By the Rt. Hon. Sir Frederick Pollock, Lord Chief Baron, F.R.S., &c. Received March 19, 1866. [See page 115.]

April 26, 1866.

J. P. GASSIOT, Vice-President, in the Chair.

The following communications were read:—

I. "On the Dentition of *Rhinoceros leptorhinus* (Owen)." By W. Boyd Dawkins, M.A., Oxon., F.G.S. Communicated by Prof. J. Phillips, F.R.S. Received March 28, 1866.

(Abstract.)

The fossil remains of the genus *Rhinoceros* found in Pleistocene deposits in Great Britain indicate four well-defined species. Of these the *R. tichorhinus*, or the common fossil species, ranged throughout France, Germany, and Northern Russia, and, like its congener the Mammoth, was defended from the intense winter cold by a thick clothing of hair and wool. Its southern limit in the Europæo-Asiatic continent was a line passing through the Pyrenees, the Alps, the northern shore of the Caspian, and the Altai Mountains. It has not yet been proved to have existed in Europe anterior to the deposit of the Boulder Clay. The second species, the *R. megarhinus* of M. de Christol, characterized by its slender limbs and the absence of the "cloison," has been determined by the author among remains from the brick-earths occupying the lower part of the Thames valley, and from the Preglacial forest-bed of Cromer. The species ranged from the Norfolk shore southwards through Central France into Italy. In France and Italy it characterizes the Pliocene deposits, being found in the former country in association with *Mastodon brevirostris* and *Halitherium Serresii*, in the latter with *M. Arvernensis*. From its southern range we may infer that the megarhine species was fitted to inhabit the warm and temperate zones of Europe, just as the tichorhine was peculiarly fitted for the endurance of an Arctic winter.

The third species is the *R. etruscus* of Dr. Falconer, confined to the forest-bed of the Norfolk shore, and, like the *R. megarhinus*, found in the Pliocenes of France and Italy; it ranged across the Pyrenees as far as Malaga, and is the only species known to occur in Spain.

The fourth, the *R. leptorhinus* of Professor Owen, is the equivalent of

the *R. hemitœchus* of Dr. Falconer. It is defined as "R. à narines demi-cloisonnées," and is probably not the same animal as the *R. leptorhinus* or "*R. à narines non-cloisonnées*" of Baron Cuvier, the evidence as to the absence or presence of the cloison in the type of the species being of the most conflicting nature. In Central France it is identical with *R. mesotropus* and *R. velaunus* of M. Aymard, the *R. Aymardi* of M. Pomel, and the *R. leptorhinus* (du Puy) of M. Gervaise. Its dentition is characterized by the presence of the *third costa* in the upper molar series, coupled with the stoutness of the cingulum, the suppression of the *anterior combing plate,* the smoothness of the enamel, and the extent to which the upper molars overhang the lower, which causes the enamel on the outer side of the latter to be worn obliquely. The lower molars can be determined by the flattening of the *anterior area,* coupled with the fine sculpturing of the enamel-surface. In common with the other fossil British Rhinoceroses, it possessed a molar series of six only on either side, and was bicorn. It ranged through England, from the Hyæna-den of Kirkdale in Yorkshire in the north, as far south as the plains of Somersetshire, and as far to the West as Pembrokeshire. It is very generally found in association with *Elephas antiquus* and *Hippopotamus major,* both species which lived in Pliocene times. The association in Wookey Hole Hyæna-den with *Elephas primigenius* and *R. tichorhinus* and other characteristic Postglacial mammals proves that it coexisted with the tichorhine species, to which it probably bore the same geographical relation as the Elk does to the Reindeer in the high northern latitudes. The sum of the evidence proves that it was coeval with the Mammoth and tichorhine Rhinoceros, and does not characterize deposits of an earlier epoch in the Pleistocene. It has not as yet been found in Preglacial formations. The *R. leptorhinus* is more closely allied to the bicorn Rhinoceros of Sumatra than to any other living species.

II. "Experimental Researches in Magnetism and Electricity."—Part I. By H. Wilde, Esq. Communicated by Mr. Faraday. Received March 26, 1866.

(Abstract.)

This paper is divided into two sections,—the first being on some new and paradoxical phenomena in electro-magnetic induction, and its relation to the principle of the conservation of physical force; the second on a new and powerful generator of dynamic electricity.

The author defines the principle of the conservation of force to be the definite quantitative relation existing between all phenomena whatsoever; and in the particular application of the principle to the advancement of physical science and the mechanical arts, certain problems are pointed out which, in their solution, bring out results as surprising as they are paradoxical. Although, when rightly interpreted, the results obtained are in

strict accordance with the principle of conservation, yet they are, at the same time, contrary to the inferences which are generally drawn from analogical reasonings, and to some of those maxims which philosophers propound for the consideration of others.

The author directs attention to some new and paradoxical phenomena arising out of Faraday's important discovery of magneto-electric induction, the close consideration of which has resulted in the discovery of a means of producing dynamic electricity in quantities unattainable by any apparatus hitherto constructed. He has found that an indefinitely small amount of magnetism, or of dynamic electricity, is capable of inducing an indefinitely large amount of magnetism,—and again, that an indefinitely small amount of dynamic electricity, or of magnetism, is capable of evolving an indefinitely large amount of dynamic electricity.

The apparatus with which the experiments were made consisted of a compound hollow cylinder of brass and iron, termed by the author a magnet-cylinder, the internal diameter of which was $1\frac{5}{8}$ inch. On this cylinder could be placed, at pleasure, one or more permanent horseshoe magnets. Each of these permanent magnets weighed about 1 lb., and would sustain a weight of about 10 lbs. An armature was made to revolve rapidly in the interior of the cylinder, in close proximity to its sides, but without touching. Around this armature 163 feet of insulated copper wire was coiled, 0·03 of an inch in diameter, and the free ends of the wire were connected with a commutator fixed upon the armature-axis, for the purpose of taking the alternating waves of electricity from the machine in one direction only. The direct current of electricity was then transmitted through the coils of a tangent galvanometer; and as each additional magnet was placed upon the magnet-cylinder, it was found that the quantity of electricity generated in the coils of the armature was very nearly in direct proportion to the number of magnets on the cylinder.

Experiments were then made for the purpose of ascertaining what relation existed between the sustaining-power of the permanent magnets on the magnet-cylinder, and that of an electro-magnet excited by the electricity derived from the armature.

When four permanent magnets capable of sustaining collectively a weight of 40 lbs. were placed upon the cylinder, and when the submagnet was placed in metallic contact with the poles of the electro-magnet, a weight of 178 lbs. was required to separate them. With a larger electro-magnet a weight of not less than 1080 lbs. was required to overcome the attractive force of the electro-magnet, or twenty-seven times the weight which the four permanent magnets used in exciting it were collectively able to sustain. It was further found that this great difference between the power of a permanent magnet and that of an electro-magnet excited through its agency might be indefinitely increased.

Experiments were then made with electro-magnets of various sizes, for the purpose of ascertaining the cause of these paradoxical results.

When the wires forming the polar terminals of the magneto-electric machine were connected for a short time with those of a very large electro-magnet, a bright spark could be obtained from the electro-helices twenty-five seconds after all connexion with the magneto-electric machine had been broken. Hence it is inferred that an electro-magnet possesses the power of accumulating and retaining a charge of electricity in a manner analogous to, but not identical with, that in which it is retained in insulated submarine cables, and in the Leyden jar. It was also found that the electro-helices offered a temporary resistance to the passage of the current from the magneto-electric machine. When four magnets were placed on the cylinder, the current from the machine did not attain a permanent degree of intensity until an interval of fifteen seconds had elapsed; but when a more powerful machine was used for exciting the electro-helices, the current attained a permanent degree of intensity after an interval of four seconds had elapsed.

The general conclusion which is drawn by the author from a consideration of these experiments is, that when an electro-magnet is excited through the agency of a permanent magnet, the large amount of magnetism manifested in the electro-magnet, simultaneously with the small amount manifested in the permanent magnet, is the constant accompaniment of a correlative amount of electricity evolved from the magneto-electric machine, either all at once, in a large quantity, or by a continuous succession of small quantities,—the power which the metals (but more particularly iron) possess of accumulating and retaining a temporary charge of electricity, or of magnetism, or of both together (according to the mode in which these forces are viewed by physicists), giving rise to the paradoxical phenomena which form the subject of this part of the investigation.

Having established the fact that a large amount of magnetism can be developed in an electro-magnet by means of a permanent magnet of much smaller power, it appeared reasonable to the author to suppose that a large electro-magnet excited by means of a small magneto-electric machine could, by suitable arrangements, be made instrumental in evolving a proportionately large amount of dynamic electricity.

Two magnet-cylinders were therefore made, having a bore of 2½ inches, and a length of 12½ inches or five times the diameter of the bore.

As frequent mention is made of the different-sized machines employed in these investigations, they are distinguished by the calibre, or bore of the magnet-cylinders.

Each cylinder was fitted with an armature, round which was coiled an insulated strand of copper wire 67 feet in length, and 0·15 of an inch in diameter. Upon one of the magnet-cylinders sixteen permanent magnets were fixed, and to the sides of the other magnet-cylinder was bolted an electro-magnet formed of two rectangular pieces of boiler-plate enveloped with coils of insulated copper wire. The armatures of the 2½-inch magneto-electric and electro-magnetic machines were driven simultaneously at an equal velocity of 2500 revolutions per minute. When the electricity from the magneto-electric machine was transmitted through a piece of No. 20 iron wire 0·04 of an inch in diameter, a length of 3 inches of this wire was made red-hot. When the direct current from the magneto-electric machine was transmitted through the coils of the electro-magnet of the electro-magnetic machine, the electricity from the latter melted 8 inches of the same-sized iron wire as was used in the preceding experiment, and a length of 24 inches was made red-hot.

When the electro-magnet of a 5-inch machine was excited by the $2\frac{1}{2}$-inch magneto-electric machine, the electricity from the 5-inch electro-magnetic machine melted 15 inches of No. 15 iron wire 0·075 of an inch in diameter.

The author having found that an increase in the dimensions of the machines was accompanied by a proportionate and satisfactory increase of the magnetic and electric forces, a 10-inch electro-magnetic machine was constructed: the weight of its electro-magnet is nearly 3 tons, and the total weight of the machine is about $4\frac{1}{2}$ tons. The machine is furnished with two armatures—one for the production of "intensity"-, and the other for the production of "quantity"-effects.

The intensity armature is coiled with an insulated conductor consisting of a bundle of thirteen No. 11 copper wires, each 0·125 of an inch in diameter. The coil is 376 feet in length, and weighs 232 lbs.

The quantity armature is enveloped with the folds of an insulated copper-plate conductor 67 feet in length, the weight of which is 344 lbs. These armatures are driven at a uniform velocity of 1500 revolutions per minute, by means of a broad leather belt of the strongest description.

When the direct current from the $1\frac{5}{8}$-inch magneto-electric machine, having on its cylinder six permanent magnets, was transmitted through the coils of the electro-magnet of the 5-inch electro-magnetic machine, and when the direct current from the latter was simultaneously, and in like manner, transmitted through the coils of the electro-magnet of the 10-inch machine, an amount of magnetic force was developed in the large electro-magnet far exceeding anything which has hitherto been produced, accompanied by the evolution of an amount of dynamic electricity from the quantity armature so enormous as to melt pieces of cylindrical iron rod 15 inches in length, and fully one-quarter of an inch in diameter. With the same arrangement, the electricity from the quantity armature also melted 15 inches of No. 11 copper wire 0·125 of an inch in diameter.

When the intensity armature was placed in the magnet cylinder, the electricity from it melted 7 feet of No. 16 iron wire 0·065 of an inch in diameter, and made a length of 21 feet of the same wire red-hot.

The illuminating power of the electricity from the intensity armature is, as might be expected, of the most splendid description. When an electric lamp, furnished with rods of gas-carbon half an inch square, was placed at the top of a lofty building, the light evolved from it was sufficient to cast the shadows from the flames of the street-lamps a quarter of a mile distant upon the neighbouring walls. When viewed from that distance, the rays proceeding from the reflector have all the rich effulgence of sunshine.

A piece of the ordinary sensitized paper, such as is used for photographic printing, when exposed to the action of the light for twenty seconds, at a distance of 2 feet from the reflector, was darkened to the same degree as was a piece of the same sheet of paper when exposed for a period of one minute to the direct rays of the sun, at noon, on a very clear day in the month of March.

The extraordinary calorific and illuminating powers of the 10-inch machine are all the more remarkable from the fact that they have their origin in six small permanent magnets, weighing only 1 lb. each, and only capable, at most, of sustaining collectively a weight of 60 lbs.; while the electricity from the magneto-electric machine employed in exciting the electro-magnet was of itself incapable of heating to redness the shortest length of iron wire of the smallest size manufactured.

The production of so large an amount of electricity was only obtained (as might have been anticipated by the physicist) by a correspondingly large amount of mechanical force; for it was found that the large electro-magnet could be excited to such a degree that the strong leather belt was scarcely able to drive the machine.

When the electro-magnet of the 10-inch machine was excited by means of the 2½-inch magneto-electric machine alone, about two-thirds of the maximum amount of power from the 10-inch machine was obtained.

From a consideration of the combined action of the magneto-electric and electro-magnetic machines, the author points out a remarkable analogy, subsisting between the operation of the static forces of magnetism and of cohesion in modifying dynamical phenomena, which throws additional light upon the nature of the magnetic force.

On reviewing and comparing the whole of the analogous phenomena manifested in the operation of the magnetic and cohesive forces under the varied conditions to which the author invites attention, it appears to him that magnetism is a mode of the force of cohesion, or is, if the term be allowed, polar cohesion acting at sensible distances, the equivalent of magnetic force being obtained at the expense of an equivalent of ordinary cohesive force (in an axial direction) so long as the iron continues to be magnetized.

III. "Extract of a Letter from Charles Chambers, Esq., Acting Superintendent of the Bombay Magnetic Observatory, to the President. Dated March 28, 1866." Communicated by the President. Received April 26, 1866.

You will probably have heard from Mr. Stewart that the opportunity of applying usefully the experience which I acquired at Kew has been temporarily accorded to me by the Bombay Government, by my appointment to the superintendence of this Observatory. The confirmation of my present appointment will probably depend upon the sanction of the scheme of improvements for the Observatory which I have just sent in for the consideration of Government. Meanwhile I have arranged the working power of the establishment so as to take up the reduction of the old observations, and I am sure you will be interested to learn that there is a probability of their turning out trustworthy and valuable. The separation of seven years of declination-disturbances has already been effected, with the results shown in the enclosed Tables and Curves; but as the whole series of observations (from 1845 to 1865) will include two complete cycles of the decennial period, and as the reductions have already been so long delayed, I propose completing the twenty-one years before discussing the connected questions and publishing the whole; it is, however, a *little* doubtful whether the opportunity of doing this will be afforded me, as the Indian Government, in sanctioning my appointment, have limited its duration to the end of next month; and though I am hopeful that, partly in consequence of a representation that I have made to the Government, of the wide scope for usefulness that is open to me here, and of what has been effected and has been engaged upon since my arrival six months ago, they may be induced to extend their approval of the appointment until the improvements suggested in my Report shall have been considered, yet it seems right, as there are some interesting points about the results already arrived at, that I should inform you of them whilst I may, especially as in case of a second reference of the matter to the home Government, you will, I believe, consider them good grounds upon which to recommend the continuance of the reductions of the twenty-one consecutive years of the Bombay observations.

Referring to page 283 of your paper in the Philosophical Transactions, 1863, it will be seen that these results supply the required knowledge of the laws of the disturbances at a station intermediate in longitude between Kew and Nertschinsk. The general characteristics of the westerly disturbance-diurnal-variation curve are the same as you describe for Pekin and Nertschinsk. The curve is remarkably regular, and the ordinates between 8 P.M. and 4 A.M. have scarcely appreciable values, being in the latter respect like the westerly curve for Hobarton, and the easterly for Kew and St. Helena. The maximum occurs at 11 A.M., which corresponds to about 18^h Kew astronomical time, implying, by comparison with the corresponding hours of maximum at the other two eastern stations (Pekin and Nertschinsk), a rather slow propagation of the disturbing action from north to south of the eastern part of the northern hemisphere. The other curve (of easterly disturbance) presents a less systematic appearance; and the ratios are at no part of the day smaller than 0·64, or greater than 1·83.

The Table of aggregate values of disturbance in the several years points very distinctly to a minimum as occurring early in 1864, thus adding another to the determinations of this turning-point in three former cycles given by you in the third volume of the 'St. Helena Observations,' and confirming the conclusions you arrived at as to the propriety of the appellation "decennial" to the period in question. The Table does not extend far enough backwards to fix the time of maximum distinctly, but it suffices to place it with probability in the year 1859.

The principal requests that I have made in my Report are for a set of Kew magnetographs, and for a suitable room, and for a body of computers to work up the old observations.

Astronomical hours.		Ratios of the aggregate values of the declination-disturbances exceeding 1′·4 in amount at the several hours in the seven years from 1859 to 1865 inclusive.	
Bombay.	Kew (approximate).	Westerly disturbances.	Easterly disturbances.
12	7	·05	·70
13	8	·17	·68
14	9	·16	·64
15	10	·14	·84
16	11	·18	·69
17	12	·36	·71
18	13	·80	·92
19	14	·96	1·17
20	15	1·64	1·00
21	16	2·35	1·08
22	17	2·73	1·61
23	18	2·76	1·83
0	19	2·84	1·68
1	20	2·57	1·56
2	21	1·86	1·29
3	22	1·41	·99
4	23	1·06	·91
5	0	·61	·85
6	1	·40	·85
7	2	·45	·80
8	3	·16	1·00
9	4	·11	·79
10	5	·16	·70
11	6	·08	·72
Aggregate values in the seven years		2801·1	4538·7
		7339·8	

Aggregate values of all disturbances exceeding 1′·4 in the several years from 1859 to 1865 inclusive.	
Years.	Aggregate values.
1859	1532·1
1860	1421·6
1861	951·8
1862	1240·5
1863	691·1
1864	595·9
1865	906·8

Ratios of disturbance in the several years from 1859 to 1864 inclusive to the mean aggregate disturbance in the six years taken as unity.	
Years.	Ratios.
1859	1·43
1860	1·33
1861	0·89
1862	1·16
1863	0·64
1864	0·56
The ratio of the maximum in 1859 to the minimum in 1864 is as 2·6 to 1.	

IV. "On the Tides of the Arctic Seas.—Part III. On the Semi-diurnal Tides of Frederiksdal, near Cape Farewell, in Greenland. By the Rev. S. Haughton, F.R.S. Received April 12, 1866.

(Abstract.)

The observations discussed in this paper by the Rev. Dr. Haughton were made for him (in 1863–64), at the request of Admiral Irminger of the Royal Danish Navy, by Missionary Asboe, at Frederiksdal, near Cape Farewell in Greenland.

They proved amply sufficient for the complete discussion of the semi-diurnal tide of that interesting locality; and the following results were obtained:—

1.	Eccentricity of lunar orbit	0·06786
2.	Mass of earth as compared with mass of moon	64·638
3.	Depth of Atlantic deduced from heights	10·03 miles.
4.	Depth of Atlantic deduced from times	3·30 miles.

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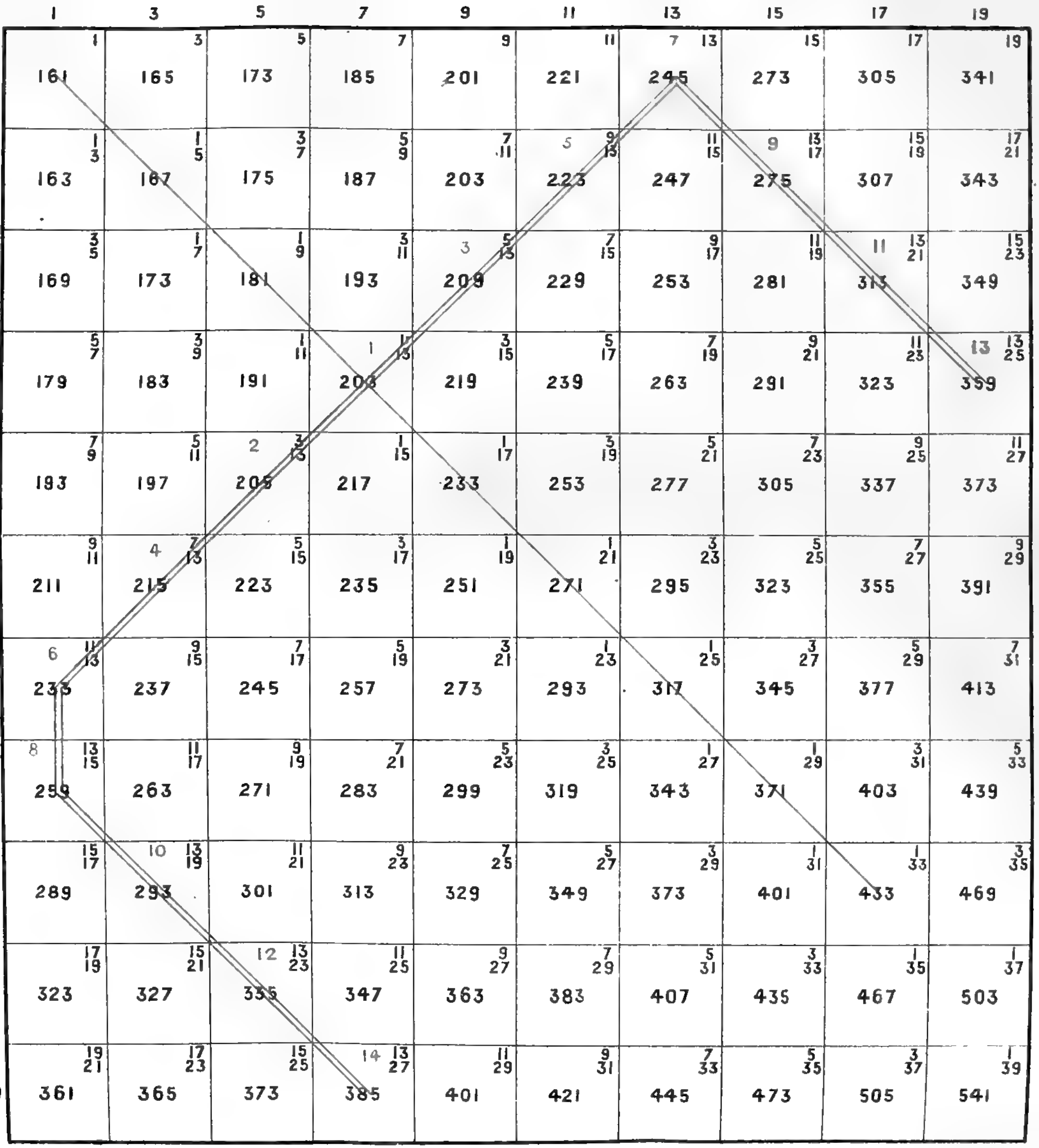
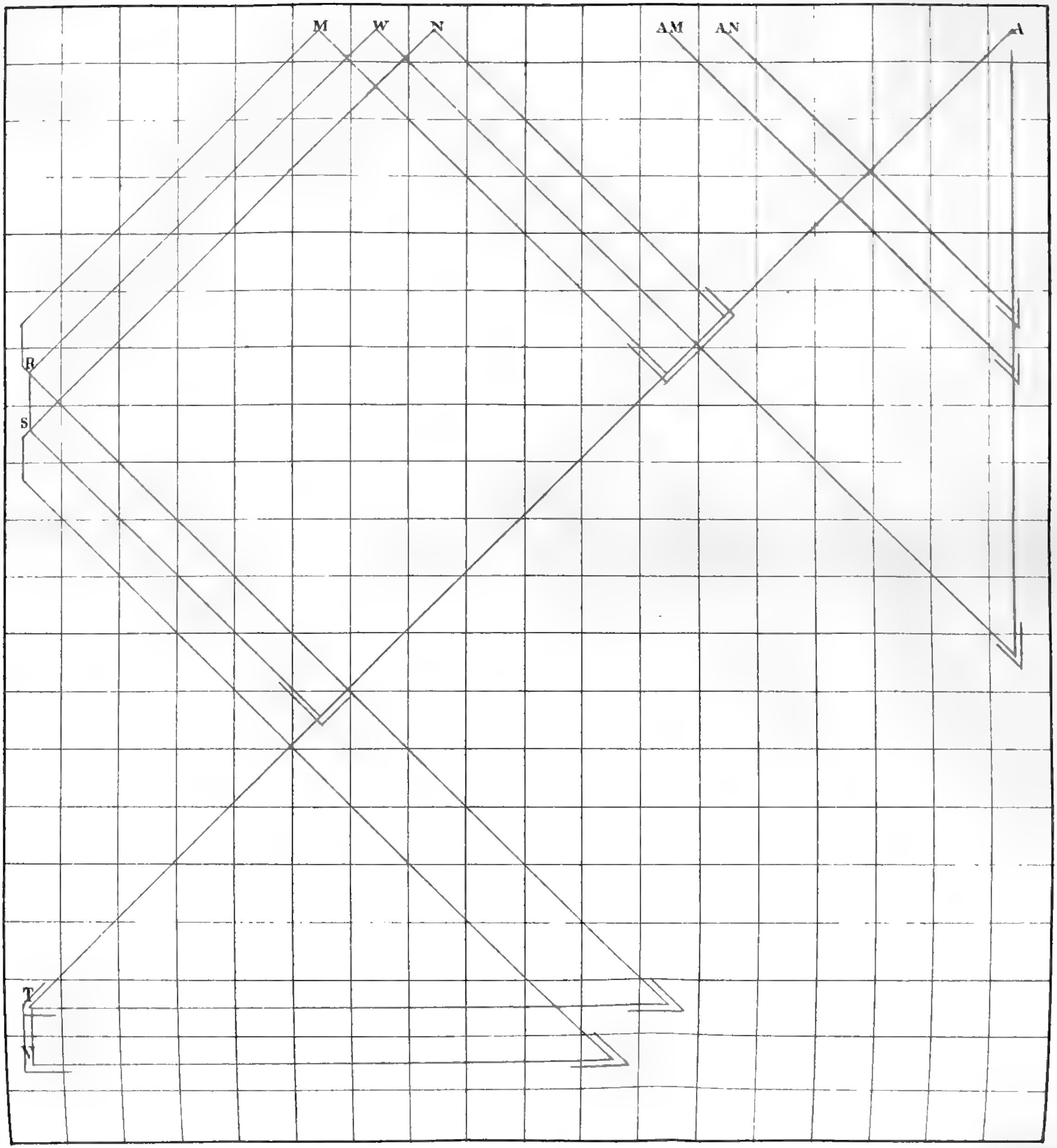
Diagram No. 1
10 n + 1
n = 16
P 2



	1	3	5	7	9	11	13	15	17	19
0	1 161	3 165	5 173	7 185	9 201	11 221	7 13 245	15 273	17 305	19 341
2	$\frac{1}{3}$ 163	$\frac{1}{5}$ 167	$\frac{3}{7}$ 175	$\frac{5}{9}$ 187	$\frac{7}{11}$ 203	5 $\frac{9}{13}$ 223	$\frac{11}{15}$ 247	9 $\frac{13}{17}$ 275	$\frac{15}{19}$ 307	$\frac{17}{21}$ 343
4	$\frac{3}{5}$ 169	$\frac{1}{7}$ 173	$\frac{1}{9}$ 181	$\frac{3}{11}$ 193	3 $\frac{5}{13}$ 209	$\frac{7}{15}$ 229	$\frac{9}{17}$ 253	$\frac{11}{19}$ 281	11 $\frac{13}{21}$ 313	$\frac{15}{23}$ 349
6	$\frac{5}{7}$ 179	$\frac{3}{9}$ 183	$\frac{1}{11}$ 191	1 $\frac{1}{13}$ 203	$\frac{3}{15}$ 219	$\frac{5}{17}$ 239	$\frac{7}{19}$ 263	$\frac{9}{21}$ 291	$\frac{11}{23}$ 323	13 $\frac{13}{25}$ 359
8	$\frac{7}{9}$ 193	$\frac{5}{11}$ 197	2 $\frac{3}{13}$ 205	$\frac{1}{15}$ 217	$\frac{1}{17}$ 233	$\frac{3}{19}$ 253	$\frac{5}{21}$ 277	$\frac{7}{23}$ 305	$\frac{9}{25}$ 337	$\frac{11}{27}$ 373
10	$\frac{9}{11}$ 211	4 $\frac{7}{13}$ 215	$\frac{5}{15}$ 223	$\frac{3}{17}$ 235	$\frac{1}{19}$ 251	$\frac{1}{21}$ 271	$\frac{3}{23}$ 295	$\frac{5}{25}$ 323	$\frac{7}{27}$ 355	$\frac{9}{29}$ 391
12	6 $\frac{11}{13}$ 233	$\frac{9}{15}$ 237	$\frac{7}{17}$ 245	$\frac{5}{19}$ 257	$\frac{3}{21}$ 273	$\frac{1}{23}$ 293	$\frac{1}{25}$ 317	$\frac{3}{27}$ 345	$\frac{5}{29}$ 377	$\frac{7}{31}$ 413
14	8 $\frac{13}{15}$ 259	$\frac{11}{17}$ 263	$\frac{9}{19}$ 271	$\frac{7}{21}$ 283	$\frac{5}{23}$ 299	$\frac{3}{25}$ 319	$\frac{1}{27}$ 343	$\frac{1}{29}$ 371	$\frac{3}{31}$ 403	$\frac{5}{33}$ 439
16	$\frac{15}{17}$ 289	10 $\frac{13}{19}$ 293	$\frac{11}{21}$ 301	$\frac{9}{23}$ 313	$\frac{7}{25}$ 329	$\frac{5}{27}$ 349	$\frac{3}{29}$ 373	$\frac{1}{31}$ 401	$\frac{1}{33}$ 433	$\frac{3}{35}$ 469
18	$\frac{17}{19}$ 323	$\frac{15}{21}$ 327	12 $\frac{13}{23}$ 355	$\frac{11}{25}$ 347	$\frac{9}{27}$ 363	$\frac{7}{29}$ 383	$\frac{5}{31}$ 407	$\frac{3}{33}$ 435	$\frac{1}{35}$ 467	$\frac{1}{37}$ 503
20	$\frac{19}{21}$ 361	$\frac{17}{23}$ 365	$\frac{15}{25}$ 373	14 $\frac{13}{27}$ 385	$\frac{11}{29}$ 401	$\frac{9}{31}$ 421	$\frac{7}{33}$ 445	$\frac{5}{35}$ 473	$\frac{3}{37}$ 505	$\frac{1}{39}$ 541

Diagram Nº 2.



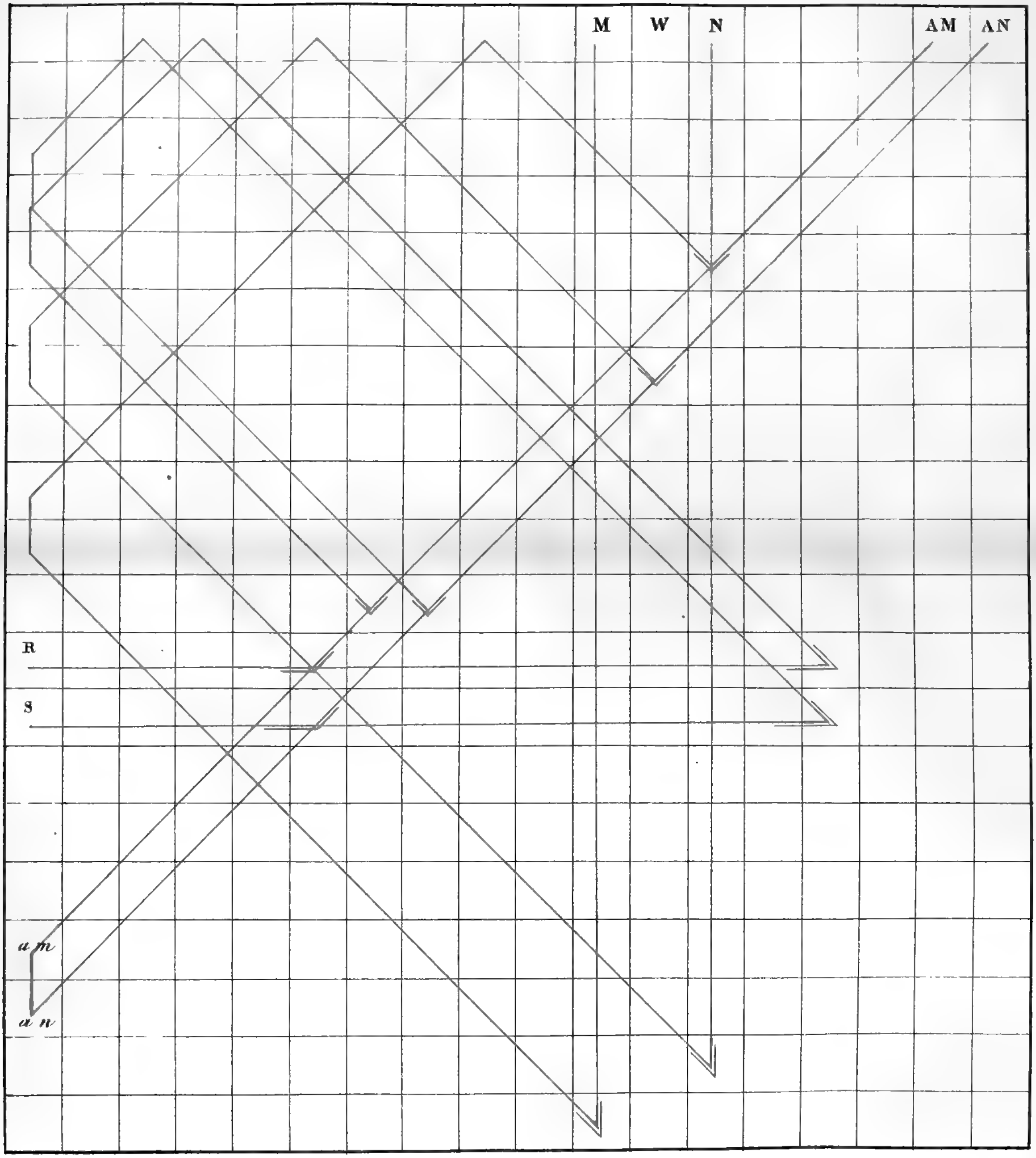
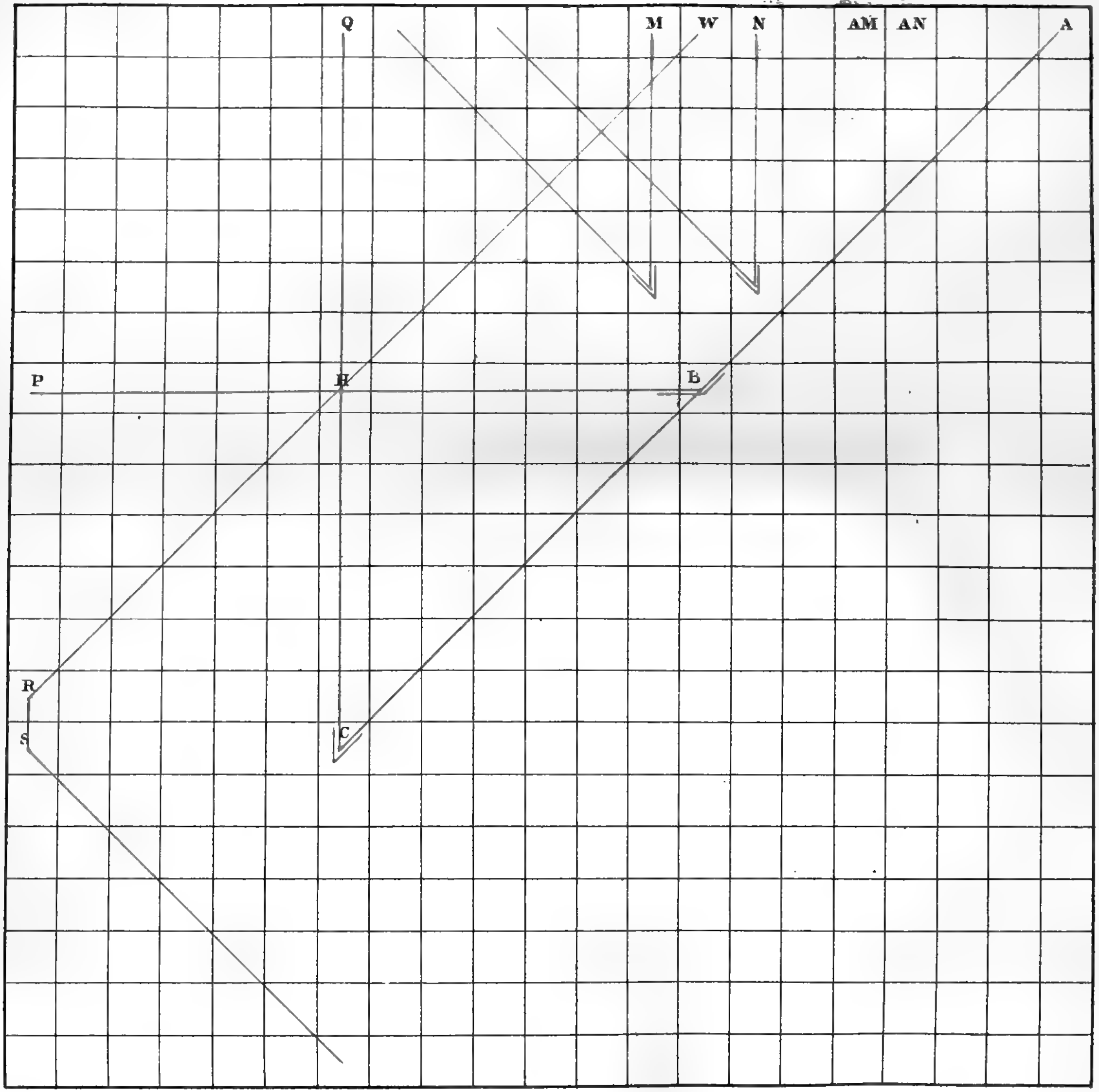

M
W
N
AM
AN
R
S
a m
a n

Diagram N° 4.



"On the Mysteries of Numbers alluded to by Fermat." By the Rt. Hon. Sir Frederick Pollock, Lord Chief Baron, F.R.S., &c.*

(Abstract.)

This paper is presented as a continuation of one which appeared in the Philosophical Transactions of the Royal Society for 1861, vol. cli. p. 409, and the object of it is to call attention to certain properties of odd numbers when placed in a square, according to an arrangement to be explained below.

It appears to me probable that these properties are connected with (if, indeed, they be not actually some form of) the mysterious properties of numbers, to which Fermat alludes in the announcement of his theorem (as furnishing the proof of it); for in point of fact these properties give *a method* by which every odd number can be divided into four square numbers, and every number (odd or even) can be divided into not exceeding three triangular numbers.

I am not prepared to say whether or not the method affords a *demonstration*, and proves that it *can* always be done; but it always *does* it, and the cause of its success may be distinctly shown. The properties I allude to are scarcely less interesting and curious than the theorem itself, and present results for which I can find no name more appropriate than the *geometry of numbers*, for relations appear to be established between various numbers in the square, which relations are not founded on any arithmetical connexion between them, but on the positions they respectively occupy in the square of which they form a part.

The arrangement of the numbers is as shown in diagram No. 1. Any odd number (which may be the subject of inquiry) is made the first term of a series, increasing from left to right by the numbers 4, 8, 12, 16, . . . ($4n$) This series forms the horizontal line at the top; each term of the series so formed is made the first term of a series, increasing downwards by the numbers 2, 6, 10, 14 . . . . ($4n-2$). A square of indefinite magnitude is thus formed, consisting of two sets of series, one set all horizontal, the other set all vertical. 161 is the first number in diagram No. 1.

The result of the arrangement of the two series in the manner above mentioned is the formation of a third set of series, which may be found in the diagonal lines of the square.

If from the first number in the square (161) a diagonal line be drawn towards the opposite corner, it will pass through the first terms of one portion of the third set of series; the second, third, and following terms are taken alternately from each side of the diagonal line. These series increase

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* Read April 19. (See p. 106.)

by 2, 4, 6, 8, &c. . . . ($2n$), and with the exception of one term they are all in the diagonal lines (see diagram No. 1), in which the single red line passes through the first terms, and the double red line shows where the terms of the series (the second, third, &c.) belonging to *that* (as a first term) are to be found; the red ink numbers indicate their order.

$$\overset{1}{203},\ \overset{2}{205},\ \overset{3}{209},\ \overset{4}{215},\ \overset{5}{223},\ \overset{6}{233},\ \overset{7}{245},\ \overset{8}{259}$$

The Nos. 203, 205, 209, 215, 223, 233, 245, 259, &c., &c., compose the series; the terms increase by 2, 4, 6, 8, 10, &c. . . . $2n$. Any number in the diagonal from 161 may be the first term of a similar series.

The diagonal from 163 will give all the other first terms of the third set of series.

In order to explain the indices which appear in the diagram No. 1, and to show in what manner the series are connected with, and pass into each other, it is necessary to point out the properties of the two series which compose the square, and of the third series, which is a necessary result.

All of them have this property in common,—that if you can discover the roots of the squares which compose any term of the series with reference to the nature of the series, and the order in the series of that term, then you know the roots of every term in the series, both before and after that term. The first series increases by 4, 8, 12, &c. The indices of the terms of a series so increasing I have made 1, 3, 5, 7, &c., and they are put at the top as being common to all the series that are horizontal.

For this reason, if two numbers differ by 1, as n, $n+1$, and the larger be increased by 1, and the smaller be diminished by 1, and the process be continued, the result will be

$$\begin{array}{ll} n, & n+1, \\ n-1, & n+2, \\ n-2, & n+3, \\ n-3, & n+4, \\ \cdot & \cdot \\ \&c. & \&c. \\ n-(p-1) & n+p. \end{array}$$

If these be treated as roots, the sums of the squares will be

$$\begin{array}{l} 2n^2+2n+1, \\ 2n^2+2n+5, \\ 2n^2+2n+13, \\ 2n^2+2n+25, \\ \quad \&c. \quad \&c. \\ 2n^2+2n+2p^2-2p+1. \end{array}$$

The sums of the squares increase by 4, 8, 12, &c.

If therefore the roots of the square, into which any odd number may be divided, be p, q, n, $n+1$, and the number be increased by 4, 8, 12, &c. ... $(4n)$, the mth term in the series will be composed of squares whose roots will be p, q, $n-(m-1)$, $n+m$; two of the roots will be constant, the others will vary, and their differences in the successive terms will be 1, 3, 5, 7, &c. $(2n-1)$; and if you discover the roots of any term in the series, you can find the roots of all the terms.

The second series resembles the first in having two roots constant, and two variable; the differences between the variable roots are, in the first term 0, in the second term 2, in the third term 4, &c., and the indices of the terms are therefore 0, 2, 4, 6, 8, 10, &c., which are placed vertically by the side of the square. For if two numbers are equal, as n, n, and one of them be increased by 1, and the other diminished by 1, and the process be continued, the result will be

$$\left.\begin{array}{ll} n & n \\ n-1, & n+1 \\ n-2, & n+2 \\ n-3, & n+3 \\ \&c. & \&c. \end{array}\right\} \begin{array}{c} \text{If these be treated as} \\ \text{roots, the sums of} \\ \text{their squares will be} \end{array} \left\{\begin{array}{l} 2n^2 \\ 2n^2+2 \\ 2n^2+8 \\ 2n^2+18 \\ \&c.\ \&c. \end{array}\right.$$

The sums of the squares increase by 2, 6, 10, 14 .... $(4n-2)$, and the successive differences of the variable roots are 0, 2, 4, 6, &c.; and if the roots of the squares into which any odd number may be divided be p, q, n, n, and the number be increased by 2, 6, 10, &c., the roots of the mth term will be p, q, $n-(m-1)$, $n+(m-1)$, and if the roots of any one term be known, the roots of all the others may be found.

The small figures in the upper right-hand corner of each division or small square are the indices of the third set of series. In this set all the roots are variable. The character of the first set of series is, that two roots in every term differ by an odd number; the character of the second set of series is, that two roots in every term differ by an even number; but in the third set of series, the algebraic sum of all the roots of the squares into which the successive terms may be divided is successively 1, 3, 5, 7, 9, &c. (an odd number): the sum of the roots of the squares into which an odd number can be divided cannot be an even number.

The following Table will explain in what manner the series is formed from the roots of the squares into which any odd number may be divided, so as to make the algebraic sum equal to 1. I have preferred to use figures instead of algebraic symbols, as being more readily understood and more easily dealt with; but the result is the same whatever figures or symbols may be used. The series begins from the centre.

Let -7, -3, 2, 9, which are the roots of the squares into which the odd number 143 may be divided, be placed in the centre, and let the positive roots be increased downwards and decreased upwards, and the negative roots increased upwards and decreased downwards, the result will be as in the Table below:—

	Order of of terms.	Roots.	Algebraic sums of roots.	Sums of squares of roots.	Order of terms.
	12	4 5 7 −13−9−4 3	−23	275	12
	10	4 5 7 −12−8−3 4	−19	233	10
	8	4 5 7 −11−7−2 5	−15	199	8
	6	4 5 7 −10−6−1 6	−11	173	6
	4	4 5 7 − 9−5 0 7	− 7	155	4
	2	4 5 7 − 8−4 1 8	− 3	145	2
Centre	1	4 5 7 − 7−3 2 9	1	143	1
	3	4 5 7 − 6−2 3 10	5	149	3
	5	4 5 7 − 5−1 4 11	9	163	5
	7	4 5 7 − 4 0 5 12	13	185	7
	9	4 5 7 − 3 1 6 13	17	215	9
	11	4 5 7 − 2 2 7 14	21	253	11
	13	4 5 7 − 1−3 8 15	25	299	13
		&c. &c.	&c.		

It will be observed that the terms of the series 143, 145, 149, 155, &c. increase by 2, 4, 6, 8, . . (2 n). In the column of the sums of the roots 1, 5, 9, 13, &c. increase by 4.

1, −3, −7, −11 decrease by 4; the differences of the roots, if arranged in the order of their numerical value, is always the same throughout the series.

Note.—In the remainder of this paper every odd number that becomes a term in any of the series is expressed by the roots the sum of whose squares form the number itself; $\boxed{2,\ 3,\ 6,\ 8}$ means that the number occupying that division or small square is $113 = (2^2 + 3^2 + 6^2 + 8^2)$; a figure (or collection of figures) representing merely the arithmetical value is put into a circle, thus: $\boxed{\begin{array}{c} \textcircled{113} \\ 2,\ 3,\ 6,\ 8 \end{array}}$.

Having explained the construction of the square and the indices which belong to the series of which it is composed, I propose to point out the properties which discover the roots of the squares into which the odd numbers (which are found in the different parts of the square) may be divided.

If the first odd number in the square be of the form $(4p+2) . n+1$ (or $2n+1$, $6n+1$, $10n+1$, $14n+1$, &c.), n being of any value whatever, the $(n-p)$th term will be $\boxed{-(p+1), -p, n, n}$; these are the roots (the number itself would be $2n^2+2p^2+2p+1$); and as the index of the nth term is $n+(n-1)$, or $2n-1$, the index of the $(n-p)$th will be $2n-(2p+1)$, and the algebraic sum of the roots may be made equal to the index; it is therefore a term in a diagonal series [that the $(n-p)$th term is $2n^2+2p^2+2p+1$, will appear by finding in the usual way the $(n-p)$th term of a series whose first term is $(4p+2) . n+1$, and the terms of which increase by 4, 8, 12, 16, &c. . . . $4n$]; but as two of the roots are equal, n, n, it is the first term of a vertical series descending, thus:

$$\begin{array}{l} p+1, p, n, n \\ p+1, p, (n-1), n+1 \\ p+1, p, (n-2), n+2 \\ p+1, p, (n-3), (n+3). \end{array}$$

I call this term (A)* and indicate it by that letter, and from this term the roots of many others may be derived (which are indicated by other letters connected with A in an invariable manner), whose squares will compose the number that belongs to that term. For example, counting $2p+1$ squares backwards from A is a term I have distinguished as W; the roots of the number belonging to it are

$$\boxed{\overline{p+1}, p, (n-\overline{4p+2}), n}$$

These roots may, of course, be either positive or negative; arranging them thus, $-\overline{p+1}, -p, n, -\overline{4p+2}, n$, we have the algebraic sum of their roots equal to $2n-\overline{6p+3}$, which is the index of the square in which W is found going backwards from A, the index of which is $2n-\overline{2p+1}$ (as already stated); immediately adjoining W are two squares which I have called M and N respectively.

M is the term next before W, and is composed of the roots

$$\boxed{n-\overline{2p+2}, n-\overline{2p+2}, \overline{3p+2}, \overline{p+1}}$$

N is the term next after W, and consists of the roots

$$\boxed{\overline{n-2p}, \overline{n-2p}, \overline{3p+1}, p}$$

Each of these will traverse the square diagonally, the algebraic sums of

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* See Diagrams Nos. 2, 3, and 4.

their roots being equal to the index; the roots of W may also be obtained from A by taking its roots down vertically, making n, n successively $\overline{n-1}$, $\overline{n+1}$, $\overline{n-2}$, $\overline{n+2}$, &c., until the square is reached, in which the index is $2n+2p+1$, and $2n+2p+1$ being the arithmetical sum of all the roots of A; A here becomes a term in a series which moves diagonally, and on being carried up towards the left will give the roots of W. When the indices of the squares through which this vertical series passes equal $2n\pm1$, by making $\overline{p+1}$, p, one positive and the other negative, the term becomes a term in another series which moves diagonally upwards towards the left. This must occur both when the index equals $2n-1$ and when it equals $2n+1$; and as the indices increase downwards uniformly by 2, it follows that $2n-1$ and $2n+1$ will be the indices of contiguous terms of the vertical series, and therefore two contiguous terms will become terms of series moving diagonally upwards to the left; and as these two series are contiguous to each other, their terms found in the first series (that is, the series in the top line) will also be contiguous.

These two terms I have designated as AM and AN. AM comes from the term where the index equals $2n+1$, and AN from the term where the index equals $2n-1$; the roots of AM are

$$\boxed{0,\ \overline{2p+1},\ n-\overline{2p+2},\ n,}$$

those of AN are

$$\boxed{0,\ \overline{2p+1},\ n-2p,\ n,}$$

and being arranged thus,

$$\boxed{-\overline{2p+1},\ 0,\ n-\overline{2p+2},\ n,}\boxed{-\overline{2p+1},\ 0,\ \overline{n-2p},\ n,}$$

will move diagonally downwards to the left; and as each of these have two roots that differ in one case by $2p$, in the other by $2p+2$, the terms in these series that are parallel to those terms in the first vertical series which have their external indices respectively $2p$ and $2p+2$, the terms of AM and AN will (I say) in these places become terms in series moving vertically, and on being followed up to the series in the top row will be found to give the roots of M and N. The roots of M and N may be obtained by another method as follows:—As the algebraic sum of the roots of A equals its index, therefore A is a term in a diagonal series moving downwards towards the left; and as two of its roots, p, $p+1$, differ by 1, it follows that whenever the value of n has been so altered that $2n\pm1$ equals the index by making $\overline{p+1}$, p, one positive and the other negative, the term becomes a term in another series, which series will move at right angles to the series last mentioned. Now taking the series which moves to the left of A, it is clear that this result will obtain in two places; first, when the altered value of n makes $2n-1$ equal the index, and secondly, when the altered value of n makes $2n+1$ equal the index.

The first of these going up gives the roots of N, the second gives those of M, and these roots are identical with those obtained from AM and AN.

The index of A is $2\,n-\overline{2\,p+1}$, therefore when n in moving downwards becomes $\overline{n+1}$, $\overline{n+2}$, $\overline{n+3}$, ...

$$\begin{array}{|c|c|}\hline 2\,n-\overline{3\,p+2}, & 2\,n-\overline{3\,p+1} \\ \hline\end{array},$$

$\overline{p+1}$, p being two of the roots of each term in this series; by adding $p+1$ to the first-mentioned root and p to the other, these two terms will be found to be terms in two horizontal series, of which the first terms are in the first vertical series, and these terms both of them make the diagonal index, and therefore are terms in a diagonal series which, rising towards the right, give the roots of W.

A descends diagonally to the left, and on each side of the line which leads to W changes to cross diagonals which lead to M and N. W leads diagonally to the left to R and S, and where it crosses AT changes and goes up to A. M goes down into R, and then diagonally to where it meets the horizontal series from T; its roots there correspond with the series from T; it returns to T and up to A. In like manner N goes down through S to the line from V, and so to V and up to A. R and S go each of them across to the vertical line from A, and so up to A: every term through which these lines pass has the four roots indicated whose squares would make the term.

The number of terms in the whole square, whose four roots may be expressed in term of p and n, is very considerable; and it may be well now to present some skeleton diagrams of the many ways in which certain members of the square are *invariably* connected.

I propose to exhibit several (to avoid confusion, which would arise from putting all in one diagram); these do not by any means include all the connexions that exist; but whatever may be the value of n or p, a number of the form $(4p+2).\,n+1$, whether it be $2n+1$, $6n+1$, $10n+1$, &c., and whatever be the value of n, gives the following results. See diagram No. 2. At the $(n-p)$th term there will be A, which descends vertically till the index is $2n-1$, then $2n+1$, and from these rise up in diagonals AN and AM, as already mentioned. A then further descends till the index is equal to $2n+2p+1$, when it rises in a diagonal to W.

Diagram No. 3 shows certain connexions between AM and AN and other terms in the square. AM is always $-\overline{2\,p+1}$, 0, $n-\overline{2\,p+2}$, n, AN is always $-\overline{2p+1}$, 0, $\overline{n-2p}$, n.

If the series in which AM is a term be carried down diagonally till 0 becomes $2p+1$ (that is $2p+1$ places), it becomes a term in a diagonal series that intesects it and rises to the top, then goes down to the horizontal series from R, where it becomes a term in that series and passes to where it is below M and rises vertically up to it. AN does the same with respect to the series from S, and rises up to N.

If the term AM descends till $n-\overline{2p+2}=2p+1$, it rises to the left in another diagonal series and goes on till it crosses M, where it is found that the roots are always the same as those which arise from M, descending by means of its two equal roots. The term AN does the same with respect to N.

If AM descend to the margin at *am*, and one step further into *an*, and AN descends to *an*, they will be found to have the same roots, and they will be

from AM

$-\overline{2p+1},\ 1,\ n-\overline{2p+1},\ n$

from AN

$-\overline{2p+1},\ -1,\ n-\overline{2p+1},\ n$

The only difference being that in the one case 1 is positive, in the other it is negative; but whatever be the value of p or n, in this portion or term of the square 1 is *always* one of the roots.

In crossing the two horizontal series from R and S, it will always be found that at the points of intersection the roots of AM correspond with the roots of the series from R, and the roots from AN correspond with those from S.

Diagram No. 4 exhibits the way in which B, C, P, and Q are connected together. The roots of A at the $(n-p)$th square will always be $-(p+1)$, $-p$, n, n, and $p+1$, p must be one of them odd, the other even; therefore, whether n be odd or even, an odd number will be formed by $-(p+1)$, n, or $-p$, n. The series from A descending diagonally has the roots n, n decreasing, but the negative roots increasing. The differences will continue the same; and when the series arrives under that index which corresponds to the odd number $-(p+1)$ n, or $-p$, n, it becomes a term in a horizontal series which goes to P, two terms of which are always p, $p+1$. W, in descending diagonally, has its index on reaching PB 1. When A has descended so as to reach the even number of the two, $(-(p+1), n, -p, n)$, it rises in a vertical series to Q; and the three series, WR, BP, CQ, always intersect in the same point or small square, H.

The paper then exhibits in a Diagram (No. 5) all the roots which arise from applying the *method* to the odd No. 161, and shows that the roots

of A would be	3,	2,	16,	16
of W ,,	3,	2,	6,	16
of M ,,	8,	3,	10,	10
of N ,,	7,	2,	12,	12
of AM ,,	5,	0,	10,	16
of AN ,,	5,	0,	12,	16

It shows the roots of the squares marked B, C, P, Q, R, S, and many others; and, finally, it shows that 161 would be composed of the squares of the following roots, either 3, 12, 2, 2, or 10, 0, 5, 6; but to set forth the roots of the numbers which are in Diagram No. 1, would require a diagram larger than could conveniently be put into a publication of the size of the Proceedings. The roots 10, 0, 5, 6, if arranged thus, −10, 0, 5, 6, have 1 for their sum; but it was proved in the former paper (see p. 410, Trans. Roy. Soc. for 1861) that if the algebraic sum of the roots be 1, then the number is the double +1 of a number composed of 3 triangular numbers, 161=80×2+1, and 80 is composed of the 3 triangular numbers 55, 15, 10. If therefore any number be doubled, and 1 be added, an odd number will be obtained, to which the same process may be applied as is here applied to 161.

I have stated that the cause of the success of "*the method*" (though it does not at present amount to a demonstration) may be easily shown. It arises, first, from "*the method*" requiring every odd number that is a term in any of the series to be represented by the roots of the square numbers that compose it; and secondly, and more particularly, from every series being connected with at least six others of a different kind which intersect it, each of which is again connected with at least five others, so that when the whole network has been pursued, and the roots which in succession form every term have been recorded, it will be found that many different modes of dividing each term into four squares or less will be discovered, *i. e.* if the numbers be large. I propose to show the manner in which the series are apparently interwoven by an example from each kind.

Let the first term in a horizontal series be 22 $\boxed{\begin{array}{r}21\\23\\3,\ 7,\ 13,\ 14\end{array}}$ with 22 as the index in the margin, and 21 and 23 being the indices of the diagonal series which pass through this square; for, except at the top line, two diagonal series pass through every square. 13 and 14 are the variable roots, which become 12, 15, | 11, 16, | 10, 17, | 9, 18, | &c. | in the successive terms; when in the second term 14 becomes 15 (15+7=22), and the roots are 3, 7, 12, 15, a term in the series which would come down from the second square; the roots in that square will therefore be 3, 12, 4, 4; when 15 becomes 19 the roots will be 3, 7, 8, 19, and the roots at the top will be 7, 8, 8, 8; so when 15 becomes 25 at the tenth term the roots are 3, 7, 2, 25; and as −3, 25=22, another series rises up with roots of the first term, 7, 2, 14, 14.

13 diminishes to 0, and then increases, giving 2 more when it becomes 15 or 19; but the series is also crossed by diagonal series; and when the index in the two rows from the first term $\left\{\begin{array}{llllr}21 & 19 & 17 & 15 & 13,\ \&c.\\ 23 & 25 & 27 & 29 & 31,\ \&c.\end{array}\right\}$ is 37 (the sum of all the roots), or 31 (the sum of the variable plus the difference of the constant roots), or 17 (the sum of the variable roots minus the sum of the constant roots), or 23 (the sum of the constant roots minus the difference of the variable), a diagonal series arises. Here are no less

than 10 other series by which this is crossed and associated and connected, and the number cannot in any case be less than **6**. A series of the second kind gives rise to other series crossing it in the same manner *mutatis mutandis,* which result is so obvious that it is not necessary further to dwell upon it. A series of the third kind has all the differences of its roots the same in each term. Let $\boxed{\begin{array}{r} 13 \\ -7, 2, 4, 14 \end{array}}$ be a term in a diagonal series at the top row of the system.

Thus:

0	11 8, 13, 2, 2	13 9 2 10 −7, 2, 4, 14	15 6, 15, 4, 4
2	9 9 2 10 −8, 1, 3, 13		17 9 2 10 −6, 3, 5, 15

When it reaches to the left and to the right, the second place, as $3-1=2$ and $5-3=2$, it furnishes two vertical series; at the tenth row it furnishes two more; at the twelfth row two more. When it gets into the column whose index is 11 and then 9, before it reaches the margin or outer edge, it furnishes two horizontal series; and after it has passed the margin at 9 and 11 it furnishes two more, and at 21 it furnishes another. Here are six new vertical and five new horizontal series; besides which it furnishes at least two other diagonal series which cross it.

Having stated the properties which belong to the square, if the first odd number in it be of the form $(4p+2).n+1$, and that whether it be $2n+1$, $6n+1$, $10n+1$, &c., or whatever be the value of n, certain squares may be found which I distinguish as A, B, C, H, P, Q, W, M, N, AM, AN, R, S, T, V, &c., which are connected together by a community of roots where the series cross each other in a manner that is invariable.

The Diagram No. 3 is another example of the manner in which certain of the terms in the different series communicate with each other, by the roots being common to both, at the point where they cross. AM passes diagonally to *am,* down to *an,* and up to AN. AN, in like manner, goes down to *an* and up to *am* and AM. If the first term of the square be an odd number of the form $(4p+2)\ n+1$, the roots from AM are in *an* $-(2p+1)$, 1, $(n-2p+1)$, n. The roots from AN are $-(2p+1)$, -1, $(n-2p+1)$, n. The indices of the diagonal series are $\begin{pmatrix} 2n-(4p+3) \\ 2n-(4p+1) \end{pmatrix}$, and the algebraic sum of the roots is the one or the other, according as 1 is + or −; but AM also passes to the series from R, and AN to the series from S, and go to R and S, and thus go up to W. AM also reaches the vertical line from M, and passes up to M, as AN does to N. Lastly, AM goes to N thus, and AN to W in a similar manner. Whatever be the values of p and n, these connexions occur, but the form of them varies with the values of p and n.

I have pointed out some of the results of having as the first term in the square an odd number of the form $(4p+2), n+1$; the forms will be $2n+1$, $6n+1$, $10n+1$, $14n+1$, &c., in which p may be 0, 1, 2, 3, &c. But there is *another* form which gives similar results, viz. $4pn+(2p+1)$, which is in many respects the "*converse*" of the other; the forms will be $4n+3$, $8n+5$, $12n+7$, $16n+9$, &c. These are the *first terms* to which this system gives rise. The $(n-\overline{p-1})$th term is always $\boxed{p, p, n, n+1}$. In the former system n produced the equal roots and p the unequal; *here*, p produces the equal and n the unequal roots, and I call the $(n-\overline{p-1})$th term A as in the other. This system has also W and M and N on each side of it, and other squares similar to the other, but the roots of which they are composed are differently formed. M and N come from below. W, instead of being

$$\boxed{-\overline{p+1}, -p, n-\overline{4p+2}, n,} \text{ will be } \boxed{-3p,\ p,\ (n-2p),\ (n+1-2p),}$$

and other terms are similarly altered, but the general result is the same. A specimen of a square of this form is given in Diagram No. 8, but which cannot be reduced to the size of the Proceedings, where $p=3$ and $n=16$; the first number is (199), and the square is completed so far as to show that the roots of the 4 squares whose sum is (199) = $\boxed{\begin{array}{rrrr} 5, & 13, & 1, & 2 \\ -9, & -3, & 3, & 10 \\ 1, & 10, & 7, & 7 \end{array}}$ but neither in this Diagram nor in Diagram No. 5 is the whole square completed (to avoid confusion); but if the series be traced in succession, the entire Diagram 6 would be filled up, and every term would disclose the roots whose squares compose it. In this manner every odd number in all the series is divided into the squares that compose it (not exceeding 4), the squares being indicated by their roots.

The two systems of $(4p+2)\times n+1$ and $4\,p.n+2p+1$ include every possible odd number; $4n+3$ includes every alternate odd number from 3; $8n+5$ every fourth number from 5, and so on; $2n+1$ includes every odd number; $6n+1$ every third odd number. Many odd numbers belong to both systems, and to more than one in each. 151 is an example; it is either $10n+1$ $(n=15)$, or it is $12n+7$ $(n=12)$. The paper contains a Diagram (No. 9) exhibiting the odd number 151 as belonging to both systems; but the Diagram cannot be reduced. The roots of the squares that compose 151 are (10. 1. 5. 5), or (3. 9. 5. 6).

Diagram No. 6.

$2n-\overline{6p+5}$	$2n-\overline{6p+3}$	$2n-\overline{6p+1}$		$2n-\overline{4p+3}$	$2n-\overline{4p+1}$		$2n-\overline{2p+1}$
M	W	N		AM	AN		A
$-\overline{3p+2}$, $p+1$, $n-\overline{2p+2}$, $n-\overline{2p+2}$,	$-\overline{p+1}$, $-p$, $n-\overline{4p+2}, n$,	$-3p+1$, $p, n-2p$, $n-2p$,	an interval of $\overline{p-2}$ squares.	$-2p+1, 0$, $n-\overline{2p+2}, n$,	$-\overline{2p+1}, 0$, $n-\overline{2p}, n$,	an interval of $\overline{p-1}$ squares.	$-\overline{p+1}$, $-p, n, n$

This Diagram (No. 6) is introduced to show in terms of n and p what roots A, AN, AM, N, W, and M contain, and the intervals between them. Each of these in passing downwards to the left is crossed by other series, with which they amalgamate; N may be derived from AN, M from AM: N, W, and M each furnish two others, and these again each two more. When n is small compared with the coefficient $(4p+2)$, W may be on the right of A; for although the series begins at A, and according to the law of the series reaches the 1st square, the same law enables it to continue, with terms whose indices become negative.

Diagram No. 7.

There is an interval of $n-\overline{3p+3}$ squares from the 1st term.

*$2n-\overline{6p+4}$ R	$2n-\overline{6p+5}$ $-\overline{3p+2}, p$ $n-\overline{2p+2}, n-\overline{2p+1}$
$2n-\overline{6p+4}$ S	$2n-\overline{6p+1}$ $-\overline{3p+1}, p+1$ $n-\overline{2p+1}, n-2p$

There is here an interval of $(p-1)$ squares from S to *an*.

$2n-\overline{4p+2}$ *an*	$2n-\overline{4p+3}$ $2n-\overline{4p+1}$ $-\overline{2p+1}, 1$ $n-\overline{2p+1}, n$

There is here an interval of $(p-1)$ squares from *an* to T.

$2n-\overline{2p+2}$ T	$2n-\overline{2p+3}$ $p+1, p+1$ $\overline{n-1}, n$
$2n-2p$ V	$2n-\overline{2p-1}$ $p, p, n, n+1$

This Diagram (No. 7) shows in terms of n and p the roots of R, S, *an* (which comes down diagonally from AN), T, and V, and the number of squares between them; the relative positions of these terms depend entirely on p, and are always the same for the same value of p.

Terms similar to these increase indefinitely as n increases. The value of certain roots is independent of n, and therefore is the same for every value of n.

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* [The numbers above the letters are the indices of the vertical series.]

May 3, 1866.

Lieut.-General SABINE, President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

John Charles Bucknill, M.D.
Rev. Frederick William Farrar.
William Augustus Guy, M.B.
James Hector, M.D.
John William Kaye, Esq.
Hugo Müller, Ph.D.
Charles Murchison, M.D.
William Henry Perkin, Esq.
The Ven. John Henry Pratt, M.A.
Capt. George Henry Richards, R.N.
Thomas Richardson, Esq., M.A.
William Henry Leighton Russell, Esq.
Rev. William Selwyn, D.D.
Rev. Richard Townsend, M.A.
Henry Watts, B.A.

The following communications were read:—

I. “Report on the Levelling from the Mediterranean to the Dead Sea.” By Colonel Sir HENRY JAMES, R.E., F.R.S. Received April 5, 1866.

The instructions for levelling from the Mediterranean to the Dead Sea having been received after the party had arrived at Jerusalem, it was thought best to level in the first place from Jerusalem to the Dead Sea during the cool months, and to complete the line to the Mediterranean at Jaffa, when the party were on their way home.

But in describing the line levelled, we may assume that it was made direct from Jaffa to the Dead Sea.

The line selected was that which runs across the maritime plain direct from Jaffa to Lydda, three miles beyond which, on the road to Beth Horon, the line turns to the right by Jimsu, Birfileeah, and Beit Sira; and from thence up the Wady Suleiman to El Jib, where it again joins the old Roman Road from Lydda, by Beth Horon to Jerusalem. But at about $1\frac{1}{2}$ mile on the north road from the Damascus Gate, the line turns to the eastward over Mount Scopus, where it reached the altitude of 2724 feet, the height of the top of the large cairn on it. This was the highest point crossed between the Mediterranean and the Dead Sea.

From Mount Scopus the line follows the high ground to the Mount of Olives, and thence takes the road down to Bethany, and, following the road by Khan Hadhur to near Jericho, the line turns to the right within about a mile of the latter place, and was carried thence across the plain bordering the Dead Sea to a point opposite a small island in the sea itself.

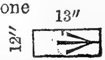
Throughout the entire length of this line bench marks (⤒) have been cut at intervals, wherever it was practicable, on the fixed rocks, or on permanent objects.

The following is a list of the bench marks, with the distances between them :—

List of the Bench marks made in levelling the line from the Mediterranean to the Dead Sea in March, May, and June 1865.

No.	Place.	Distance, in Miles and Links.	Altitude.	Where cut.	Remarks.
1.	Jaffa	0 0	feet 3·800	castle wall	On the wall, 3800 feet above level of the Mediterranean.
2.	"	0 1490	31·080	town wall	On gate at entrance to town.
3.	"	0 7972	31·225	on fountain	On the fountain near Jaffa.
4.	"	1 5831	55·735	on a well	At west side of road.
5.	Yazur	3 7656	85·405	on a well	At the east end of the village.
6.	Bethdagon	5 5843	91·435	on a well	At the west end of the village.
7.	Sephoneya	7 3157	126·540	on a wall	In Sephoneya village, near lone tree.
8.	"	9 6495	143·630	on a tree	On a lone tree on maritime plain.
9.	Lydda	11 5922	164·770	garden wall.........	At east end of Lydda village.
10.	"	14 0358	248·630	west angle of well..	Half a mile west of Jimzu village.
11.	Jimzu	14 5194	411·605	on rock	In road at thrashing-floor, Jimzu.
12.	"	15 1714	478·725	on rock	On side of road, top of hill.
13.	"	15 6495	549·000	on rock	On road, top of hill.
14.	"	16 4284	573·035	on rock	West side of road.
15.	"	17 0548	517·520	on rock	Side of road, and junction of Wady.
16.	"	18 0479	650·075	on rock	At junction of road to Birfilleya.
17.	"	18 7527	819·180	on rock	East side of road.
18.	"	19 5196	747·865	on rock	On side of road.
19.	"	21 7893	751·105	on rock	In centre of road.
20.	"	25 7849	1,157·530	on rock	On side of road.
21.	"	26 5121	1,251·470	on rock	On side of road.
22.	"	27 3392	1,423·015	on wall	Corner of garden wall in Wady.
23.	"	30 3428	2,064·945	on rock	South side of stream.
24.	"	30 7617	2,258·250	on rock	Entrance to Wady Sulieman.
25.	"	33 5560	2,419·505	on rock	East side of road.
26.	"	37 1670	2,681·915	on stone	Near trigonometrical station, Jerusalem Survey.

TABLE (*continued*).

No.	Place.	Distance, in Miles and Links.	Altitude.	Where cut.	Remarks.
			feet		
27.	„	37 4078	2,685·390	on stone	In centre of road.
28.	Mount Scopus	37 6345	2,648·545	on rock	In centre of road.
29.	„	38 0612	2,688·700	on cistern	East side of road.
30.	„	38 1896	2,715·795	on rock	Near Gustam Pole.
31.	„	38 5086	2,662·905	on pillar	On east side of road.
32.	„	38 6530	2,603·875	on stone	West side of road, near junction.
33.	Mount Olivet	39 0236	2,623·790	on wall	West side of road.
34.	„	39 1721	2,662·500	on rock	Near summit of Mount Olivet.
35.	„	39 1731	2,665·080	surface..............	At trigonometrical station.
36.	Ascension	39 2794	2,643·220	on house	South side of road.
37.	Bethany	40 2409	2,281·825	on rock	Trigonometrical line, Bethany to Scorpion.
38.	„	40 4225	2,208·755	on rock	Near well, Bethany village.
39.	„	41 0148	2,018·350	on rock	North side of road at junction of fences.
40.	Well of the Apostles..	41 6063	1,519·615	on wall	Base of Bethany Hill.
41.	„ ...	42 2457	1,351·845	on rock	South side of road.
42.	„ ...	43 1606	1,163·060	on rock	South side of road.
43.	„ ...	44 0032	1,039·145	on rock	North side of road.
44.	„ ...	45 0375	902·515	on rock	Near junction of Nebi Musa.
45.	„ ...	47 0498	654·190	on stone	Opposite trees in valley.
46.	„ ...	47 3127	776·130	on rock	At the top of the hill.
47.	Khan Hadhur	48 5296	870·590	on rock	At entrance to cave opposite Khan.
48.	„	50 2545	537·010	on rock	On side of road.
49.	„	51 0706	451·510	on rock	At top of hill.
50.	Old Aqueduct	52 7866	− 89·715	on rock	On side of road.
51.	„	53 5174	− 99·035	on rock	On side of road.
52.	„	54 0339	− 209·890	on rock	North side of road.
53.	„	54 4465	− 477·045	on rock	In centre of road opposite house.
54.	„	62 2514	−1,273·215	on stone 	Sunk on the beach of the Dead Sea opposite island.
55.	„	62 2965	−1,292·135	level of sea	Height of Dead Sea on the 12th March, 1865.

At the distance of 3¾ miles beyond Khan Hadhur, on the road to Jericho, the level of the Mediterranean was crossed, and from thence towards the Dead Sea the levels are marked with the negative sign.

On the 12th of March, 1865, the party reached the Dead Sea, when its level was found to be 1292 feet below the level of the Mediterranean; but from an examination of the drift wood on the shore, it was ascertained that at some time of the year, probably after the winter freshets, the water rises 2½ feet higher, which would make the least depression 1289·5.

From inquiry amongst the Bedouins and European residents in Palestine, it was ascertained that during the early summer the level of the sea falls at least 6 feet below the level at which it stood on the day the levelling was taken, which would make the depression 1298 feet; and we may conclude that the maximum depression at no time exceeds 1300 feet. Lieut. Symonds, R.E., in 1841, made the depression 1312·2 feet.

The soundings in the Dead Sea by Lieut. Vignes of the French Navy, gave a maximum depth of 1148 feet, making the depression of the bottom of the Dead Sea 2446 feet below the level of the Mediterranean. The soundings in the Mediterranean, midway between Malta and Candia, by Captain Spratt, R.N., gave a depth of 13,020 feet, or a depression of the bottom five times greater than that of the bottom of the Dead Sea.

The levelling was executed by two independent observers, and from a comparison of the two sets of levelling, it is certain that the levels have been obtained with absolute accuracy to within 3 or 4 inches.

The establishment of a chain of levels across the country with bench marks cut on so many points, cannot but prove of the utmost importance for any future investigations, or for any more extended surveys in Palestine, such as are contemplated by the Society which has been formed since this survey was made, "for the accurate and systematic investigation of the archæology, the topography, the geology, and physical geography, &c. of the Holy Land, for Biblical illustration."

For the survey of Jerusalem itself, it was of the utmost importance, as it enabled us to connect all the levels in and about the city with the level of the Mediterranean, and to harmonize, so to speak, all the levels which have been taken.

II. "Note on the Amyl-Compounds derived from Petroleum." By C. Schorlemmer. Communicated by Professor Roscoe. Received April 26, 1866.

In a former communication I have shown that the hydride of heptyl obtained from petroleum has a higher specific gravity than its isomers ethyl-amyl, and hydride of heptyl from azelaic acid. The same is the case with their derivatives, and some of these isomeric compounds also show considerable differences in their boiling-points*. I could not compare the different heptyl-compounds which I prepared with those of heptyl-alcohol formed by fermentation, as the latter substance is very little known, and I therefore considered it interesting to compare the amyl-compounds from fusel-oil with those obtained from petroleum. From the latter substance I prepared a considerable quantity of pure hydride of amyl, which boiled constantly at 33°–35° C.; and I did not succeed in lowering the boiling-point any further. From this hydride other amyl-compounds were obtained in exactly the same way as the heptyl-com-

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* Proc. Roy. Soc. vol. xiv. p. 464.

pounds. Pure amyl-compounds from fusel-oil were also prepared with the greatest care, and their specific gravities and boiling-points compared, under exactly the same circumstances, with the compounds prepared from petroleum. The results of this investigation are contained in the following Table:—

Amyl-Compounds.

	From fusel-oil.		*From petroleum.*	
	Boiling-point.	Specific gravity.	Boiling-point.	Specific gravity.
C_5H_{12}	..	..	34° C.	0·6263 at 17°
$C_5H_{11}Cl$	101° C.	0·8750 at 20°	101° C.	0·8777 at 20°
$\left.\begin{matrix}C_5H_{11}\\C_2H_3O\end{matrix}\right\}O$	140° C.*	0·8733 at 15°	140° C.	0·8752 at 15°
$C_5H_{12}O$	132° C.	0·8148 at 14°	132° C.	0·8199 at 14°.

It appears from this Table that the boiling-points of the same compounds agree perfectly, and that the specific gravities show only very small differences, those of the substances obtained from petroleum being a little higher. This is easily accounted for by an admixture of higher boiling compounds, which towards the end of the distillation raise the boiling-points a little, and which cannot be removed completely, even by long-continued rectifications. The amyl-compounds from petroleum and those from fusel-oil are therefore identical.

III. "On a New Series of Hydrocarbons derived from Coal-tar." By C. Schorlemmer. Communicated by Professor Roscoe. Received April 26, 1866.

The light oils obtained by the destructive distillation of Cannel-coal at a low temperature, contain, besides the hydrocarbons of the marsh-gas and benzol series, other substances, which are attacked by concentrated sulphuric acid. If the oil, which has been repeatedly shaken with this acid, be subjected to distillation, the hydrocarbons which are unacted upon volatilize first, and a black tarry liquid, equal in bulk to about half the crude oil, remains behind†. On heating this residue more strongly, a brown oil, having an unpleasant smell, comes over at about 200° C.; the temperature rises gradually up to 300° C., and at last a black pitchy mass is left in the retort. Even after repeated rectifications the oil always leaves a solid black residue behind, and it was only by continued fractional distillations over solid caustic potash and metallic sodium, that I succeeded in isolating substances possessing nearly a constant boiling-point and volatilizing almost completely. The compounds which I thus obtained from Cannel-coal oil, boiling below 120°C., are hydrocarbons of the general formula $(C_nH_{2n-2})_2$, as the following analyses and determinations of the vapour-densities show:—

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* The boiling-point of acetate of amyl is given very differently by different observers (Cahours found 125°, Landolt 133–134°; Pogg. Ann. vol. cxxii. p. 554). My observation agrees perfectly with that of Wanklyn (Chem. Soc. Journ. (2.) iii. p. 30).

† Journ. Chem. Soc. vol. xv. p. 420.

(1) $C_{12}H_{20}$ boiling-point 210° C.

(*a*) 0·262 substance gave 0·840 carbonic acid and 0·290 water.

(*b*) 0·1978 substance gave 0·635 carbonic acid and 0·2195 water.

	Calculated.		Found.	
			a.	*b.*
C_{12}	144	87·8	87·44	87·55
H_{20}	20	12·2	12·30	12·32
	164	100·0	99·74	99·87

Globe with air	5·547
Temperature of air	17° C.
Globe with vapour	5·730
Temperature on sealing	250° C.
Capacity of globe	65·0 cubic centims.

Calculated.	Found.
6·68	6·98

The residue in the globe had a brown colour, the oil not being completely volatile; this accounts for the difference between the calculated and found vapour-densities.

(2) $C_{14}H_{24}$, boiling-point 240°.

0·107 substance gave 0·343 carbonic acid and 0·1195 water.

	Calculated.		Found.
C_{14}	168	87·5	87·42
H_{42}	24	12·5	12·40
	192	100·0	99·82

(*a*)	Weight of globe	3·354
	Temperature of air	10°·5 C.
	Globe with vapour	3·5745
	Temperature on sealing	280° C.
	Capacity of globe	67·6 cub. centims.
(*b*)	Weight of globe	3·286
	Temperature of air	7° C.
	Globe with vapour	3·4665
	Temperature on sealing	270° C.
	Capacity of globe	55·2 cub. centims.

Calculated.	Found.	
	a.	*b.*
6·65	7·06	7·02

The liquid remaining in the globe had also in both cases a brown colour.

(3) $C_{16}H_{28}$, boiling-point 280° C.

0·152 substance gave 0·4885 carbonic acid and 0·174 water.

	Calculated.		Found.
C_{16}	192	87·27	87·11
H_{28}	28	12·73	12·72
	220	100·00	99·83

These hydrocarbons are colourless, oily, strongly refracting liquids, lighter than water, and possessing a faint peculiar smell, resembling that of the roots of *Daucus carota* or *Pastinaca sativa.* I have obtained them in small quantities only, and could study their reactions therefore only incompletely. They combine with bromine with a hissing noise, and if the reaction is not moderated, the liquid blackens and hydrobromic acid is evolved; but by keeping the substance well cooled, and by adding the bromine very carefully, nearly colourless, heavy, oily, sweet-smelling bromine-compounds are obtained, without the formation of hydrobromic acid. These are very easily decomposed by heating; charry matter separates out, and hydrobromic acid is given off even below the boiling-point of water. From the hydrocarbon $C_{14} H_{24}$ alone I obtained a sufficient quantity of the bromide for analysis.

0·3715 substance gave 0·3605 bromide of silver and 0·0123 metallic silver.

Calculated for $C_{14} H_{24} Br_2$.	Found.
45·45 per cent. Br.	43·7 per cent. Br.

As it was impossible to purify the small quantity of bromide, the difference between the found and calculated quantities is easily accounted for.

Concentrated nitric acid dissolves these hydrocarbons, much heat being evolved; on diluting the acid solution with water, yellow, heavy, thick oily nitro-compounds separate, which have a faint but peculiarly unpleasant smell. By heating these nitro-compounds with tin and hydrochloric acid, a portion is converted into a black tarry mass, and the solution contains a considerable quantity of chloride of ammonium, and a small quantity of a hydrochlorate, which can be obtained as a crystalline deliquescent mass by evaporating *in vacuo.* On concentrating the solution in the air, decomposition takes place, a violet substance being formed. By adding caustic potash to the solution of the hydrochlorate, a dark oily base separates, which quickly oxidizes into a black tarry mass. Platinic chloride produces at first no precipitate in the concentrated solution of the hydrochlorate, but after a few minutes a dark violet tar separates.

I could not succeed in obtaining crystallized double chlorides of tin or zinc.

If these hydrocarbons are heated with a concentrated solution of bichromate of potassium and sulphuric acid, carbonic acid is evolved, a strongly acid liquid, on which an oily layer swims, distils over, a resinous substance remaining in the retort. As I did not obtain any of the pure hydrocarbons in sufficient quantity to study their separate products of oxidation, I took all that remained, together with the intermediate distillates, and the oil boiling above 280° C., which had been previously well purified by rectification over sodium. After oxidation, the distillate was neutralized with carbonate of sodium, the oil being left undissolved. This neutral oil, which has an ethereal smell, and boils between 200° and 300° C., gave on analysis 84·9 per cent. C and 11·8 per cent. H; it consists, therefore, of non-oxidized hydrocarbons, containing a small quantity of an oxygen-compound. The solution of the sodium-salt was evaporated on the water-bath, the residue distilled with diluted sulphuric acid, and the distillate rectified. It smelt strongly of acetic acid, and also slightly of butyric acid. By neutralization with carbonate of sodium, a crop of crystals of acetate of sodium was obtained, which were converted into the crystallized silver-salt.

0·1335 of this salt gave 0·861 of silver.

Calculated for $C_2 H_3 Ag O_2$.	Found.
64·67 per cent. Ag.	64·50 per cent. Ag.

The syrupy mother-liquor of the sodium-salt gave, with nitrate of silver, a white precipitate, which, on boiling the liquid, decomposed with effervescence and separation of metallic silver, showing the presence of formic acid; from the filtered liquid small warty crystals of a silver-salt separated.

0·1314 of this salt gave 0·842 of silver, or 64·1 per cent. Ag.

The mother-liquor gave on evaporation again crystals of acetate of silver.

0·2196 gave 0·1418 silver, or 64·56 per cent.

The volatile acids produced by the oxidation of the hydrocarbons are therefore carbonic acid, acetic acid, formic acid, and perhaps a trace of an acid richer in carbon.

The resinous substance left in the retort is an acid which dissolves in caustic potash, and is precipitated from this solution as a brown greasy substance, easily soluble in alcohol. The alcoholic solution, neutralized with ammonia, gave, with nitrate of silver, a white flocculent precipitate of a silver-salt, which dried into a brown resinous mass, not fit for analysis.

As these hydrocarbons were obtained by the action of sulphuric acid on coal-tar oils boiling below 120°, and as they differ by $C_2 H_4$, it appears to me almost certain that they are polymers of the hydrocarbons of the acetylene series, $C_n H_{2n-2}$, formed in the same way as diamylene is formed, by treating amylene with sulphuric acid. The products of oxidation are also in accordance with this view.

In order to test this theory, I have made some experiments with the two isomers $C_6 H_{10}$, namely, diallyl and hexoylene. By acting with sulphuric acid on these compounds, I obtained, besides large quantities of tarry matter, polymeric modifications boiling above 200°, having a smell similar to the hydrocarbons described above, giving also similar nitro-compounds; but the quantities which I got were not large enough for a more exact examination.

The sulphuric acid which was used to purify the coal-tar oils contains an organic substance in solution, which can be isolated by neutralizing the acid liquid with carbonate of calcium, filtering, evaporating to dryness in the water-bath, extracting the residue with alcohol, and evaporating the alcoholic solution. It forms a yellow amorphous mass, which has a faint, bitter, and astringent taste. A substance with exactly the same properties was obtained from the acid which was used to act upon the hydrocarbons $C_6 H_{10}$.

I am at present engaged upon experiments to isolate the hydrocarbons $C_n H_{2n-2}$ contained in coal-tar.

IV. "The Calculus of Chemical Operations; being a Method for the Investigation, by means of Symbols, of the Laws of the Distribution of Weight in Chemical Change. Part I.—On the Construction of Chemical Symbols." By Sir B. C. Brodie, Bart., F.R.S., Professor of Chemistry in the University of Oxford. Received April 25, 1866.

(Abstract.)

In chemical transformations the absolute weight of matter is unaltered, and every chemical change, as regards weight, is a change in its arrangement and distribution. Now this distribution of weight is subject to numerical laws, and the object of the present method is to facilitate the study of these laws, by the aid of symbolic processes. The data of the chemical calculus, as indeed of every other application of symbols to the investigation of natural phenomena, are supplied by observation and experiment; and its aim is simply to deduce from these data the various consequences which may be inferred from them. The province of such a method commences where that of experiment terminates.

This part comprises the consideration of the fundamental principles of symbolic expression in chemistry, and also the application of the method to the solution of perhaps the most important of all chemical problems, namely, the question of the true composition, as regards weight, of the units of chemical substances.

Section I. In the first section certain definitions are given of those weights and relations of weight, of which the symbols are subsequently considered. It may be regarded as containing an analysis of the subject of chemical investigation. The definitions are of "a chemical substance," "a weight," "a single weight," "a group of weights," "identical weights," "a compound weight," "a simple weight," and "an integral compound weight."

The unit of a chemical substance is defined as that weight of the substance which at 0° Centigrade, and 760 millims. pressure and in the condition of a perfect gas, occupies the volume of 1000 cubic centimetres. This volume is termed the unit of space.

Section II. The second section treats of symbolic expression in chemistry. A "chemical operation" is defined as an operation of which the result is a weight. These operations are symbolized by letters, x, y, &c. An interpretation is assigned to the symbols $+$ and $-$, as the symbols of aggregation and segregation, that is, of the mental operations by which groups are formed. The symbol $=$ is selected as the symbol of chemical identity; the symbol

0 as the symbol of the absence of a weight, this symbol being identical with $x-x$. The symbol $(x+x)$ is the symbol of two weights collectively considered, and as constituting a whole.

The symbols xy and $\frac{x}{y}$ are selected as the symbols of compound weights, and it is proved that with this interpretation these symbols are subject to the commutative and distributive laws,

$$xy=yx,$$

$$x(y+y_1)=xy+xy_1,$$

and also to the index law,

$$x^p x^q=x^{p+q}.$$

Section III. treats of the properties and interpretation of the chemical symbol 1, which is selected as the symbol of the subject of chemical operations, namely, the unit of space. With this interpretation the chemical symbol 1 has the property of the numerical symbol 1 given in the equation $x1=x$.

Section IV. Chemical symbols are here shown to be subject to a special symbolic law, given in the equation

$$xy=x+y.$$

This property, by which chemical symbols are distinguished from the symbols employed in other symbolic methods, is termed the "logarithmic" property of these symbols. A consequence of this property is that $0=1$, and that any number of numerical symbols may be added to a chemical function without affecting its interpretation as regards weight.

Section V. relates to the special properties of the symbols of simple weights, which are termed prime factors, from their analogy to the prime factors of numbers. These symbols differ, however, from these factors in that, like the numerical symbol 1, they are incapable of partition as well as of division, which is a consequence of the condition $xy=x+y$.

The symbol of the unit of a chemical substance, expressed as a function of the simple weights of which it consists, is identical with the symbol of a whole number expressed by means of its prime factor, a^n, b^{n_1}, c^{n_2}. . . . A general method is given for discovering the prime factors of chemical symbols.

Section VI. is on the construction of chemical equations from experimental data.

Section VII. On the expression of chemical symbols by means of prime factors in the actual system of chemical equations. The object of this section is to prove that the units of weight of chemical substances are integral compound weights, and to discover the simplest expression for the symbols which is consistent with this assumption.

Such an expression cannot be effected unless some one symbol be determined from external considerations. The unit of hydrogen, therefore, is assumed to consist of one simple weight, its symbol being expressed by one prime factor, α, which is termed the *modulus* of the symbolic system.

This assertion is the expression of a hypothesis which may be proved or disproved by facts, and the consequences of which are here traced.

The symbols of the elements are considered in three groups. 1. The symbols of the elements of which the density in the gaseous condition can be experimentally determined, and which form with one another gaseous combinations. 2. The symbols of carbon, boron, and silicon. 3. The symbols of other elements, which are determined with a certain probability by the aid of the law of Dulong.

For the method of constructing these symbols, which depends upon the solution in whole numbers of certain simple indeterminate equations, we must refer to the memoir itself.

The following symbols may serve as an illustration of the general results:—

Name of substance.	Prime factor.	Absolute weight in grammes.	Relative weight.
	α	0·089	1
	ξ	0·715	8
	χ	1·542	17·25
	ν	0·581	6·5
	ϕ	1·305	15
	κ	0·536	6
	Symbol.		
Hydrogen	α	0·089	1
Oxygen	ξ^2	1·430	16
Water	$\alpha\xi$	0·804	9
Chlorine	$\alpha\chi^2$	3·173	35·5
Hydrochloric acid	$\alpha\chi$	1·631	18·25
Oxide of chlorine	$\alpha\chi^2\xi$	3·888	43·5
Hypochlorous acid	$\alpha\chi\xi^3$	2·346	26·25
Teroxide of chlorine	$\alpha\chi^2\xi$	5·319	59·5
Chlorous acid	$\alpha\chi\xi^2$	3·062	34·25
Chloric acid	$\alpha\chi\xi^3$	3·777	42·25
Nitrogen	$\alpha\nu^2$	1·251	14
Ammonia	$\alpha^2\nu$	0·760	8·5
Protoxide of nitrogen	$\alpha\nu^2\xi$	1·966	22
Nitrite of ammonium	$\alpha^3\nu^2\xi^2$	2·860	32
Chloride of ammonium	$\alpha^3\nu\chi$	2·391	26·75
Phosphorus	$\alpha^2\phi^4$	5·541	62
Phosphide of hydrogen	$\alpha^2\phi$	1·519	17
Pentachloride of phosphorus	$\alpha^3\phi\chi^5$	9·319	104·25
Terchloride of phosphorus	$\alpha^2\phi\chi^3$	6·145	68·75
Oxychloride of phosphorus	$\alpha^2\phi\chi^3\xi$	6·869	76·75
Carbon	κ^y	0·536+y	6+y
Acetylene	$\alpha\kappa^2$	1·161	13
Marsh-gas	$\alpha^2\kappa$	0·704	8
Alcohol	$\alpha^3\kappa^2\xi$	2·056	23
Ether	$\alpha^5\kappa^4\xi$	3·308	37
Acetic anhydride	$\alpha^3\kappa^4\xi^3$	4·559	51
Acetic acid	$\alpha^2\kappa^2\xi^2$	2·682	30
Trichloracetic acid	$\alpha^2\chi^3\kappa^2\xi^2$	6·146	68·75
Hydrocyanic acid	$\alpha\nu\kappa$	1·207	13·5
Cyanogen	$\alpha\nu^2\kappa^2$	2·324	26

Section VIII. Certain apparent exceptions are considered, in which it is not found possible to express the symbols of chemical substances by means of an integral number of prime factors, consistently with the assumption of the modulus a.

The Society then adjourned to Thursday, May 17.

May 17, 1866.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

I. "On the Motion of a Rigid Body moving freely about a Fixed Point." By J. J. SYLVESTER, LL.D., F.R.S. Received April 26, 1866.

Received April 26, 1866.

(Abstract.)

The nature of the present brief memoir will be best conveyed by my giving a succinct account of the principal results which it embodies, in the order in which they occur. The direct solution, in its present form, of the important problem of the motion of a rigid body acted on by no external forces, originating in the admirable labours of Euler, has received the last degree of finish and completeness of which it is susceptible from the powerful analysis of Jacobi; in one sense, therefore, it may be said that the discussion is closed and the question at an end. Notwithstanding this, in the mode of conceiving and representing the general character of the motion, there are certain circumstances which merit attention, and which may be expressed without reference to the formulæ in which the analytical solution is contained.

Poinsot's method of representing the motion by means of his so-called "central ellipsoid" has passed into the every-day language of geometers, and may be assumed to be familiar to all. The centre of this ellipsoid is supposed to be stationary at the point round which any given solid body is turning; its form is determined when the principal moments of inertia of that body are given, and it is supposed accurately to roll without sliding on a fixed plane whose position depends on the initial circumstances of the motion. The associated free body is conceived as being carried along by the ellipsoid, so that its path in space, its continuous succession of changes of position, is thereby completely represented; but no image is thus presented to the mind of the time in which the change of position is effected. I show how this defect in the representation may be remedied, and the time, like the law of displacement, reduced to observation by a slight modification of the apparatus of the central ellipsoid or representative nucleus, as it will for the moment be more convenient to call it. To steady the ideas, imagine the fixed invariable plane of contact with the nucleus to be horizontal and situated under it; now conceive a portion of its upper surface, say the upper

half, to be pared away until it assumes the form of a semiellipsoid confocal to the original surface, and that an indefinitely rough plate always remaining horizontal, but capable of turning in its own plane round a vertical axis, which, if produced, would pass through the centre of the ellipsoid, is placed in contact with this upper portion; as the nucleus is made to roll with the under part of its surface upon the fixed plane below, the friction between the upper surface and the plate will cause the latter to rotate round its axis, for the nucleus will not only roll upon the plate above, but at the same time have a swinging motion round the vertical, which will be communicated to the plate. I prove, by an easy application of the theory of confocal ellipsoids, that the time of the free body passing from one position to another will be in a constant ratio to this motion of rotation, which may be measured off upon an absolutely fixed dial face immediately over the rotating-plate; and furthermore I show that the relation between the angular divisions of this dial and the time depends only upon the spinning force which may be supposed to set the free body originally in motion, so that it will hold the same, at whatever distance, by a preliminary adjustment, the rotating-plate may be supposed to be set from the fixed horizontal plane.

Thus, then, we may realize a complete *kinematical* image of all the circumstances of the motion of a free rotating body, and reduce to a purely mechanical measurement the determination of an element hitherto unrepresented, but in reality the most important of all, viz. the *time*.

I then proceed to point out a very singular and hitherto unnoticed *dynamical* relation between the free rotating body and the ellipsoidal top, as I shall now prefer to call Poinsot's central ellipsoid, because I imagine it set spinning like a top upon the invariable plane in contact with it and left to roll of its own accord, the friction between it and the plane being supposed adequate to prevent all sliding. I start with supposing that the density of the top follows any law whatever, and call its principal moments of inertia A, B, C, its semiaxes a, b, c, the relations between these six quantities being left arbitrary.

It is easy to establish that, if a rotating body be acted on by any forces which always meet the axis about which it is at any instant turning, the *vis viva* will remain unaffected by their action; this will be the case in the present instance with the pressure and friction of the invariable plane, the only forces concerned, as we may either leave gravity out of account altogether or suppose the centre of gravity of the top to be at the centre of the ellipsoid, which will come to the same thing. By aid of this principle, conjoined with the two conditions to which the angular velocities of the associated free body are known to be subject, it is easy to infer that the velocities of this body and its representative top will, throughout the motion, remain in a constant ratio, or, if we please, equal to one another, provided that

$$\mathrm{A}:\mathrm{B}:\mathrm{C}::\frac{1}{a^2}+\frac{\lambda}{a^4}:\frac{1}{b^2}+\frac{\lambda}{b^4}:\frac{1}{c^2}+\frac{\lambda}{c^4},$$

where λ is an arbitrary constant. If, now, we revert to the natural supposition of the top being of uniform density, it is well known that

$$A : B : C :: b^2+c^2 : c^2+a^2 : a^2+b^2,$$

and these ratios may be identified with those above (although this would not at the first blush be supposed to be the case) by giving a suitable value to the arbitrary constant λ.

Thus, then, Poinsot's central ellipsoid supposed of uniform density and set spinning upon a roughened invariable plane will represent the motion of a free rotating solid, not in space only but also in time; the body and the top may be conceived as continually moving round the same axis and at the same rate at each moment of time*.

The problem of the top is completed in the memoir by applying the general Eulerian equations to determine the friction and pressure, a process which involves some rather operose but successfully executed algebraical calculations.

I next proceed to account analytically for the kinematical theory established at the outset of the memoir, and in doing so am necessarily led to give greater completeness to it, and at the same time an extension to the existing theory of confocal surfaces of the second order, by introducing the complementary motion of surfaces that I call contrafocal to one another: confocal ellipsoids are those the differences between the squares of whose corresponding principal arcs are all three the same; contrafocal ellipsoids I define to be those the sums of the squares of whose corresponding arcs are the same. Any two bodies whose central ellipsoids are either confocal or contrafocal I term related—correlated in the one case, contrarelated in the other, and I show that the kinematical construction in question is only another rendering of the first of the propositions herein subjoined concerning bodies so related.

1st. If two correlated bodies be placed with their principal axes respectively parallel and be set spinning by the same impulsive couple, they will move so that the corresponding axes of the one and the other body will continue always equally inclined to the axis of the couple, and their original parallelism at any instant may be restored by turning one of the bodies about this last-named axis through an angle proportional to the time elapsed since the commencement of the motion. Virtually, this amounts to saying that the difference between the displacement of two correlated bodies subject to the same initial impulse is equivalent to a simple uniform motion about the invariable line.

2nd. So, in like manner, if the bodies be contrarelated, the *sum* of their displacements is equivalent to a simple uniform motion about such line.

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* Accordingly, if we conceive any body as lying wholly in the interior of the ellipsoidal top, which is its kinematical exponent, such body will move precisely as if it were free, and consequently its density may be uniformly increased or diminished in any ratio, or it may be entirely removed without affecting the law of the motion of the surrounding crust in relation to space or time.

3rd. In either case alike, the difference between the squared angular velocities of the related bodies is constant throughout the motion.

From these propositions it follows that for all practical intents and purposes the motion of any body is sufficiently represented by the motion of any other one correlated or contrarelated to it. To a spectator on the invariable plane the apparent motion of one rotating body may be made identical with that of any other *related* one by merely making the plane on which he stands turn uniformly round a perpendicular axis. It becomes natural, then, to ascertain whether there is not always some one or more simplest form or forms which may be selected out of the whole couple of infinite series of related bodies, which may conveniently be adopted as the *exemplar* or type of all the rest. Obviously, the best suited for such purpose will be a body reduced to only two dimensions, in other words an indefinitely flattened disk, provided that it be possible in all cases to find a disk correlated or contrarelated to any given solid*.

The algebraical investigation for ascertaining the existence of such disk is the same whichever species of relation is made the subject of inquiry, and leads to the construction of three quadratic equations corresponding to the respective suppositions of the original body becoming indefinitely flattened in the direction of each of its three principal axes in turn; so that for a moment it might be supposed that the number of disks fulfilling the required condition could, according to circumstances, be zero, two, four, or six. But on closer examination, and bearing in mind that negative equally with imaginary moments of inertia are inadmissible, it turns out that there are always two such disks, and no more (except in the case of two of the moments of inertia being equal when the solution becomes unique). Of these two disks, one will be correlated and the other contrarelated to the given body, and they will be respectively perpendicular to the axes of greatest and least moments of inertia. We have thus the choice between two methods of reduction to the type form, and this choice is not a matter of unimportance (in nature nothing exists in vain); for by means thereof the motion of any given body subject to any initial conditions can be made to depend upon either at will of the two comprehensive cases (Legendre's 1st and 3rd) to which the motion of a free rotating body is usually referred, so that the distinction between these two cases (corresponding to the two species of Polhodes on either side of the "Dividing Polhode," according to Poinsot's method of exposition) is virtually abrogated.

From the preceding theory, it follows (as also may be made to appear alike from an attentive synoptic view of the commonly received analytical formulæ as from Poinsot's theory of the associated "sliding and rolling

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* The peculiar feature in the absolute motion of a disk is, that whilst it is turning in its own plane with a variable velocity, the rate at which it turns about itself is constant, as will at once become evident from eliminating r between the two equations

$$Ap^2 + Bq^2 + (A+B)r^2 = M,$$
$$A^2p^2 + B^2q^2 + (A+B)^2r^2 = L^2.$$

cone") that in the problem of the motion of a free body, of whatever form and subject to whatever initial conditions one pleases, there enter but two arbitrary parameters. Calling A, B, A+B the three moments of inertia of one or the other equivalent disks, L the magnitude of the impulsive couple, M the *vis viva*, these two parameters (say p, q) will be $\frac{AL}{M^2}$, $\frac{BL}{M^2}$; if to them we add a quantity $\frac{Mt}{L}$, say τ (where t is the terms reckoned, as it may be, from an *intrinsic* epoch as explained in the memoir), all other elements of the motion, as the total or partial velocities and the angles, whatever they may be, selected to determine the position of the rotating body become known functions of these three quantities, p, q, τ, and may be reduced to tables of triple entry, or be graphically represented by a few charts of curves; and it should be noticed that p, the smaller of the two parameters p, q, will be always necessarily included between o and 1, and that the other parameter q may, by a due choice of the species of reduction adopted, be *forcibly retained* within the same limits. The five quantities o, p, q, 1, $p+q$ will then form an ascending series of magnitudes subject only to the liability of the middle term q to become equal to 1 on the one hand, or to p on the other: q becoming unity corresponds to the case of the so-called "Dividing Polhode," Legendre's 2nd case ("*cas très remarquable*"); and q becoming equal to p is of course the case of the body itself, or its "central ellipsoid," becoming a figure of revolution, in which case the motion is practically the same as that of a uniform circular plate.

Besides these two exceptional cases, the only singular cases properly so called, the quinary scale of magnitudes just exhibited serves to indicate all the more remarkable cases (requiring or inviting particular methods of treatment) which can present themselves in the theory. These may be distinguished into special cases, which arise from any two consecutive terms becoming (to use Prof. De Morgan's expressive term) subequal, *i. e.* differing from one another by a quantity whose square may be neglected, and double special cases, which arise when any three consecutive terms become subequal; all of which, together with peculiar subcases appertaining to the double special class, perhaps deserve more thorough examination than may have been hitherto accorded to them. I conclude the memoir with pointing out the place which this problem of Rotation appears to me to occupy in dynamical theory, as belonging to a natural and perfectly well defined group of questions, of which the motion of a body attracted to two fixed centres and the renowned problem of three bodies acted on by their mutual attractions are conspicuous instances. This group is characterized by the feature that, as regards them, equations of motion admit of being constructed, from which not only the element of time, as in ordinary mechanical problems, but also an element of absolute space is shut out; supposing the equations thus reduced by two in the number of the variables to have been integrated, Jacobi's theory of the last multiplier serves to reduce both the excluded elements to quadratures and thus to complete the solution. I notice that whilst the time may fairly be said to be eliminated, the space element may be more properly said to undergo the negative process, if it may be so called, of ab-limination; it is not introduced into and then expelled from, but prevented from ever making its appearance at all in the resolving system of differential equations. It is from the study of one of these allied but more difficult questions that the present memoir has taken its rise as a collateral inquiry and elucidatory digression.

II. "On Appold's Apparatus for regulating Temperature and keeping the Air in a Building at any desired degree of Moisture." By J. P. Gassiot, Esq., V.P.R.S. Received May 3, 1866.

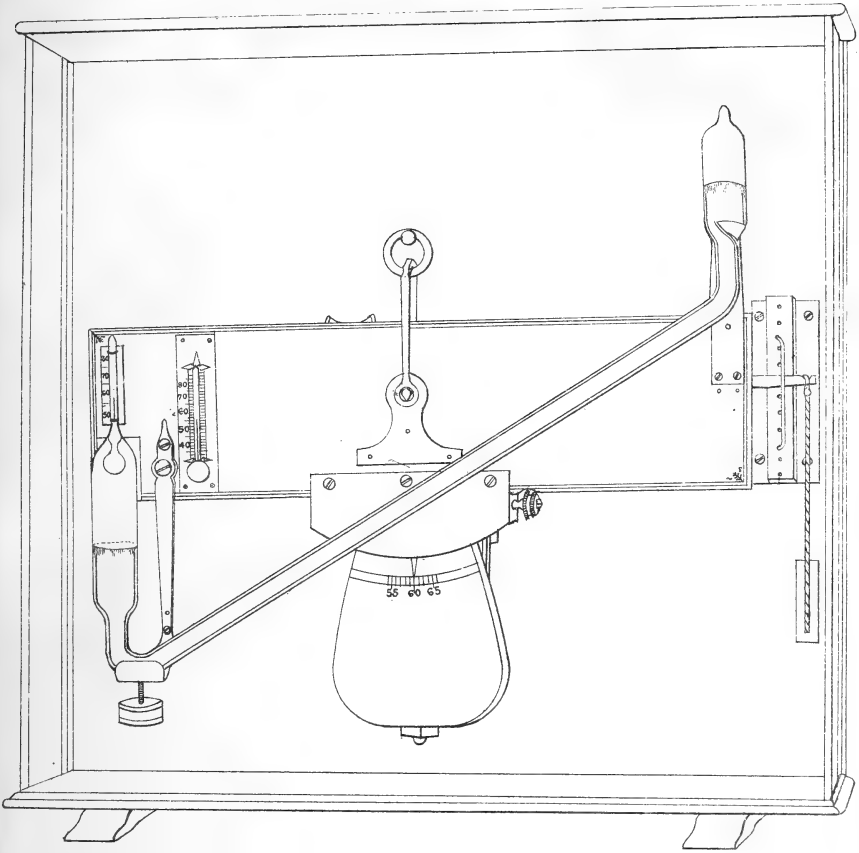
Those Fellows of the Royal Society who were acquainted with the late Mr. John George Appold, have often expressed their admiration at the various scientific arrangements which he from time to time adapted to his dwelling-house in Wilson Street, Finsbury Square. However intense might be the frost of winter or the heat of summer, or the brilliancy of the gas with which his rooms were lighted, when once under his hospitable roof you enjoyed a pure and refreshing atmosphere. Much of this was undoubtedly due to the steam-power he always had at command connected with his business premises immediately adjacent to his dwelling-house, by which he could at any time force a current of fresh air at a given temperature into any of his rooms; indeed Mr. Appold always contended that houses could not be made thoroughly comfortable as habitations without the aid of steam-power. But among the many of his arrangements to obtain equable temperature in rooms, there were also those that do not require the aid of steam-power, so seldom applicable in private dwellings, and which, being easy of adaptation, might be used in private houses with much advantage as regards the health and comfort of the inmates. I allude to his Automatic Temperature regulator, and to his Automatic Hygrometer; and these instruments, as originally constructed by her late husband, and used for many years in their house, but now repaired and placed in perfect working order by Mr. Browning, Mrs. Appold has requested me to offer in her name to the President and Council of the Royal Society. She desires me to express a hope that they will oblige her by retaining them among the other scientific apparatus belonging to the Royal Society, as a mark of respect to the memory of one who always highly esteemed the honour he received when he was elected into that body in June 1853.

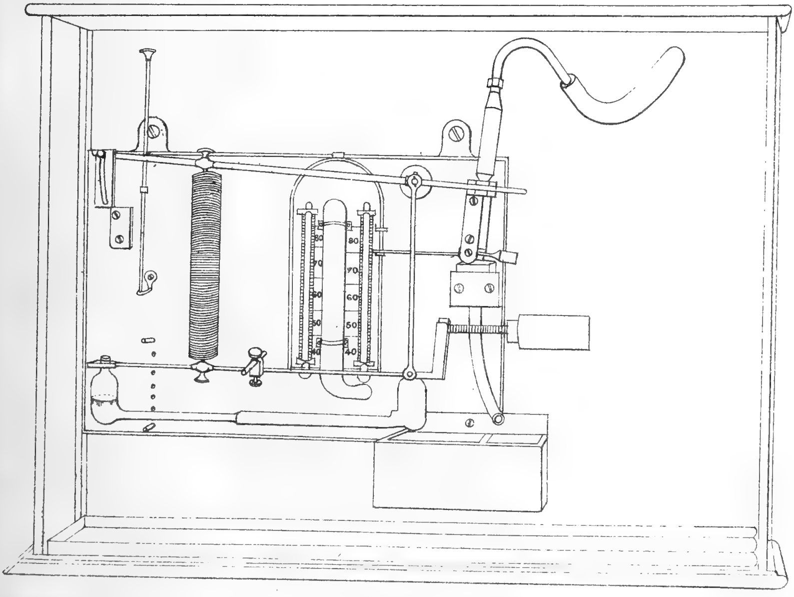
I annex a description and drawing of both instruments (Plate VII).

Appold's apparatus for regulating the temperature of buildings automatically.

This instrument consists of a glass tube having bulbs at each end. The

APPOLD'S AUTOMATIC TEMPERATURE REGULATOR.





APPOLD'S AUTOMATIC HYGROMETER.

J. Browning, del.

J. Basire, lith.

tube is filled, as also about half of each bulb, with mercury; the lower bulb containing ether to the depth of half an inch, which floats on the mercury. The tube is secured to a plate of boxwood, and supported on knife-edges, on which it turns freely. At the end of the plate, underneath the highest bulb, is a lever, to which a string is attached. This string is carried, by means of bell-cranks, to the supply-valve of a gas-stove, or the damper of a furnace.

The instrument acts in the following manner:—Supposing the stove to be lighted and to have raised the temperature more than is required, the heat will convert a portion of the ether in the lower bulb into vapour. The expansion of this vapour drives a quantity of the mercury out of the bulb underneath it through the tube into the upper bulb. The end to which the mercury has been driven being thus rendered the heaviest falls, and motion being communicated by the lever to the string, this closes the supply-valve or damper of the stove or furnace. Of course, if this should be carried beyond the required extent the reverse action will take place.

A weight in the centre of the plate, the position of which is regulated by a milled-head screw shown at the side, serves to alter the centre of gravity of the whole apparatus. The value of the motion of this weight being carefully ascertained, a scale is engraved upon it. By moving this weight, according to a scale engraved on it, the instrument may be set so as to maintain any desired temperature in the building in which it is fixed.

The range of action of the instrument is from 54° to 66° Fahr., and with a change of temperature of one degree it has the power to raise one ounce three inches.

Appolds automatic Hygrometer for keeping the Air in a Building at any desired degree of moisture.

This instrument, both in principle and construction, is very similar to the automatic regulator just described. The acting portion consists also of a tube with a bulb at each end. This tube contains mercury to about half the height of each bulb, and a portion of ether floating on the mercury at each end. One half of the tube and one bulb is covered with bibulous paper, which is always kept wet, and the tube is suspended and turns freely on knife-edges placed just above the covered bulb. The action of the apparatus is as follows:—

A deficiency of moisture in the air increases the evaporation from the bibulous paper. This evaporation produces cold, which condenses the vapour of the ether in the covered bulb, and the mercury being pressed on by the vapour of the ether in the naked bulb is forced into the covered bulb. The uncovered bulb, being now much the lightest, rises, and raises a lever, which in its turn opens a valve at the end of a small tube. This tube communicates with a cistern kept full of water. The water which is thus admitted is suffered to trickle over heated pipes which are covered with bibulous paper; upon the desired dew-point being attained, the action ceases.

The range of the instrument is regulated by means of a spiral spring at one end of the tube, and an adjustable weight at the other.

By means of a pencil attached to one of the levers, the instrument may be made self-registering.

An ordinary Mason's hygrometer is attached to the instrument for regulation and comparison.

With a variation of one degree in the moisture of the atmosphere, the instrument is capable of supplying ten quarts of water per hour to the surface of the pipes from which it evaporated.

III. "On the Spectrum of a New Star in Corona Borealis"*. By William Huggins, F.R.S., and W. A. Miller, M.D., Treas. R.S. Received May 17, 1866.

Yesterday, May the 16th, one of us received a note from Mr. John Birmingham of Tuam, stating that he had observed on the night of May 12 a new star in the constellation of Corona Borealis. He describes the star as "very brilliant, of about the 2nd magnitude." Also Mr. Baxendell of Manchester wrote to one of us giving the observations which follow of the new star, as seen by him on the night of the 15th instant.

"A new star has suddenly burst forth in Corona. It is somewhat less than a degree distant from ϵ of that constellation in a south-easterly direction, and last night was fully equal in brilliancy to β Serpentis or ν Herculis, both stars of about the 3rd magnitude."

Last night, May 16, we observed this remarkable object. The star appeared to us considerably below the 3rd magnitude, but brighter than ϵ Coronæ. In the telescope it was surrounded with a faint nebulous haze, extending to a considerable distance, and gradually fading away at the boundary†. A comparative examination of neighbouring stars showed

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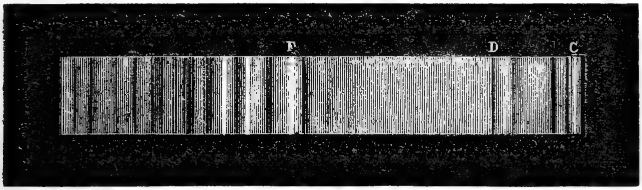
* The Astronomer Royal wrote to one of us on the 18th, "Last night we got a meridian observation of it; on a rough reduction its elements are—

R.A. 1866, May 17........................ $15^h\ 53^m\ 56^s{\cdot}08$,
N.P.D.. 63° 41′ 53″,

agreeing precisely with Argelander, No. 2765 of "Bonner Sternverzeichniss," declination + 26°, magnitude 9·5." Mr. Baxendell writes on the 21st, "It is probable that this star will turn out to be a variable of long or irregular period, and it may be conveniently at once designated T Coronæ." Sir John Herschel informs one of us that on June 9, 1842, he saw a star of the 6th magnitude in Corona very nearly in the place of this strange star. As Sir John Herschel's position was laid down merely by naked eye allineations, the star seen by him may have been possibly a former temporary outburst of light in this remarkable object.

† On the 17th this nebulosity was suspected only; on the 19th and 21st it was not seen.

that this nebulosity really existed about the star. When the spectroscope was placed on the telescope, the light of this new star formed a spectrum unlike that of any celestial body which we have hitherto examined. The light of the star is compound, and has emanated from two different sources. Each light forms its own spectrum. In the instrument these spectra appear superposed. The principal spectrum is analogous to that of the sun, and is evidently formed by the light of an incandescent solid or liquid photosphere, which has suffered absorption by the vapours of an envelope cooler than itself. The second spectrum consists of a few bright lines, which indicate that the light by which it is formed was emitted by matter in the state of luminous gas*. These spectra are represented with considerable approximative accuracy in a diagram which accompanies this paper.



Spectrum of Absorption and Spectrum of Bright Lines forming the Compound Spectrum of a New Star near ε Coronæ Borealis.

Description of the spectrum of absorption.—In the red a little more refrangible than Fraunhofer's C are two strong dark lines. The interval between these and a line a little less refrangible than D is shaded by a number of fine lines very near each other. A less strongly marked line is seen about the place of solar D. Between D and a portion of the spectrum about the place of *b* of the solar spectrum, the lines of absorption are numerous, but very thin and faint. A little beyond *b* commences a series of close groups of strong lines; these follow each other at small intervals, as far as the spectrum can be traced.

Description of the gaseous spectrum.—A bright line, much more brilliant than the part of the continuous spectrum upon which it falls, occupies a position which several measures make to be coincident with Fraunhofer's F†. At rather more than one-fourth of the distance which

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* The position of the groups of dark lines shows that the light of the photosphere, after passing through the absorbent atmosphere, is yellow. The light, however, of the green and blue bright lines makes up to some extent for the green and blue rays (of other refrangibilities) which have been stopped by absorption. To the eye, therefore, the star appears nearly white. However, as the star flickers, there may be noticed an occasional preponderance of yellow or blue. Mr. Baxendell, without knowing the results of prismatic analysis, describes the impression he received to be "as if the yellow of the star were seen through an overlying film of a blue tint."

† On the 17th, the lines of hydrogen, produced by taking the induction-spark through the vapour of water, were compared in the instrument simultaneously with the bright lines of the star. The brightest line coincided with the middle of the expanded line of hydrogen which corresponds to Fraunhofer's F. On account of the faintness of the red

separates F from G, a second and less brilliant line was seen. Both these lines were narrow and sharply defined. Beyond these lines, and at a distance a little more than one-third of that which separates the second bright line from the strongest bright one, a third bright line was observed. The appearance of this line suggested that it was either double or undefined at the edges. In the more refrangible part of the spectrum, probably not far from G of the solar spectrum, glimpses were obtained of a fourth and faint bright line. At the extreme end of the visible part of the less refrangible end of the spectrum, about C, appeared a line brighter than the normal relative brilliancy of this part of the spectrum. The brightness of this line, however, was not nearly so marked in proportion to that of the part of the spectrum where it occurs, as was that of the lines in the green and blue *.

General Conclusions.—It is difficult to imagine the present physical constitution of this remarkable object. There must be a photosphere of matter in the solid or liquid state emitting light of all refrangibilities. Surrounding this must exist also an atmosphere of cooler vapours, which give rise by absorption to the groups of dark lines.

Besides this constitution, which it possesses in common with the sun and the stars, there must exist the source of the gaseous spectrum. That this is not produced by the faint nebulosity seen about the star is evident by the brightness of the lines, and the circumstance that they do not extend in the instrument beyond the boundaries of the continuous spectrum. The gaseous mass from which this light emanates must be at a much higher temperature than the photosphere of the star; otherwise it would appear impossible to explain the great brilliancy of the lines compared with the corresponding parts of the continuous spectrum of the photosphere. The position of two of the bright lines suggests that this gas may consist chiefly of hydrogen.

If, however, hydrogen be really the source of some of the bright lines, the conditions under which the gas emits the light must be different from those to which it has been submitted in terrestrial observations; for it is well known that the line of hydrogen in the green is always fainter and more expanded than the brilliant red line which characterizes the spectrum of this gas. On the other hand, the strong absorption indicated by the

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end of the spectrum, when the amount of dispersion necessary for these observations was employed, the exact coincidence of the line in this part of the spectrum with the red line of hydrogen, though extremely probable, was not determined with equal certainty.

* The spectra of the star were observed again on the 17th, the 19th, the 21st, and the 23rd. On these evenings no important alteration had taken place. On the 17th and succeeding evenings, though the spectrum of the waning star was fainter than on the 16th, the red bright line appeared a little brighter relatively to the green and blue bright lines. On the 19th and 21st the absorption lines about *b* were stronger than on the 16th. From the 16th the continuous spectrum diminished in brightness more rapidly than the gaseous spectrum, so that on the 23rd, though the spectrum as a whole was faint, the bright lines were brilliant when compared with the continuous spectrum.

line F of the solar spectrum, and the still stronger corresponding lines in some stars, would indicate that under suitable conditions hydrogen may emit a strong luminous radiation of this refrangibility*.

The character of the spectrum of this star, taken together with its sudden outburst in brilliancy and its rapid decline in brightness, suggest to us the rather bold speculation that, in consequence of some vast convulsion taking place in this object, large quantities of gas have been evolved from it, that the hydrogen present is burning by combination with some other element and furnishes the light represented by the bright lines, also that the flaming gas has heated to vivid incandescence the solid matter of the photosphere. As the hydrogen becomes exhausted, all the phenomena diminish in intensity, and the star rapidly wanes.

In connexion with this star, the observations which we made upon the spectra of α Orionis and β Pegasi, that they contain no absorption lines of hydrogen, appear to have some new interest. The spectra of these stars agree in their general characters with the absorption spectrum of the new star. The whole class of white stars are distinguished by having hydrogen lines of extraordinary force. It may also be mentioned here that we have found that the spectra of several of the more remarkable of the variable stars, namely those distinguished by an orange or ruddy tint, possess a close general accordance with those of α Orionis, β Pegasi, and the absorption spectrum of the remarkable object described in this paper. The purely speculative idea presents itself from these observations, that hydrogen probably plays an important part in the differences of physical constitution which apparently separate the stars into groups, and possibly also in the changes by which these differences may be brought about†.

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* On the dependence of the relative characters of the bright lines of hydrogen upon conditions of pressure and temperature see Plücker and Hittorf, Phil. Trans. 1865, p. 21

† Mr. Baxendell sends us the following Table of magnitudes:—

		h	m			
May 15	at	12	0	G.M.T.,	T Coronæ	= 3·6 or 3·7 magnitude.
,, 16	,,	10	30	,,	,,	= 4·2
,, 17	,,	11	0	,,	,,	= 4·9
,, 18	,,	12	30	,,	,,	= 5·3
,, 19	,,	12	15	,,	,,	= 5·7
,, 20	,,	12	30	,,	,,	= 6·2
,, 21	,,	12	0	,,	,,	= 7·3
,, 22	,,	11	15	,,	,,	= 7·7
,, 23	,,	10	30	,,	,,	= 7·9
,, 24	,,	10	30	,,	,,	= 8·1

IV. "Condensation of Determinants, being a new and brief Method for computing their arithmetical values." By the Rev. C. L. Dodgson, M.A., Student of Christ Church, Oxford. Communicated by the Rev. Bartholomew Price, M.A., F.R.S. Received May 15, 1866.

If it be proposed to solve a set of n simultaneous linear equations, not being all homogeneous, involving n unknowns, or to test their compatibility when all are homogeneous, by the method of determinants, in these, as well as in other cases of common occurrence, it is necessary to compute the arithmetical values of one or more determinants—such, for example, as

$$\begin{vmatrix} 1, & 3, & -2 \\ 2, & 1, & 4 \\ 3, & 5, & -1 \end{vmatrix}.$$

Now the only method, so far as I am aware, that has been hitherto employed for such a purpose, is that of multiplying each term of the first row or column by the determinant of its complemental minor, and affecting the products with the signs + and − alternately, the determinants required in the process being, in their turn, broken up in the same manner until determinants are finally arrived at sufficiently small for mental computation.

This process, in the above instance, would run thus:—

$$\begin{vmatrix} 1, & 3, & -2 \\ 2, & 1, & 4 \\ 3, & 5, & -1 \end{vmatrix} = 1 \times \begin{vmatrix} 1, & 4 \\ 5, & -1 \end{vmatrix} - 2 \times \begin{vmatrix} 3, & -2 \\ 5, & -1 \end{vmatrix} + 3 \times \begin{vmatrix} 3, & -2 \\ 1, & 4 \end{vmatrix}$$
$$= -21 - 14 + 42 = 7.$$

But such a process, when the block consists of 16, 25, or more terms, is so tedious that the old method of elimination is much to be preferred for solving simultaneous equations; so that the new method, excepting for equations containing 2 or 3 unknowns, is practically useless.

The new method of computation, which I now proceed to explain, and for which "Condensation" appears to be an appropriate name, will be found, I believe, to be far shorter and simpler than any hitherto employed.

In the following remarks I shall use the word "Block" to denote any number of terms arranged in rows and columns, and "interior of a block" to denote the block which remains when the first and last rows and columns are erased.

The process of "Condensation" is exhibited in the following rules, in which the given block is supposed to consist of n rows and n columns:—

(1) Arrange the given block, if necessary, so that no ciphers occur in its interior. This may be done either by transposing rows or columns, or by adding to certain rows the several terms of other rows multiplied by certain multipliers.

(2) Compute the determinant of every minor consisting of four adjacent

terms. These values will constitute a second block, consisting of $\overline{n-1}$ rows and $\overline{n-1}$ columns.

(3) Condense this second block in the same manner, dividing each term, when found, by the corresponding term in the interior of the first block.

(4) Repeat this process as often as may be necessary (observing that in condensing any block of the series, the rth for example, the terms so found must be divided by the corresponding terms in the interior of the $\overline{r-1}$th block), until the block is condensed to a single term, which will be the required value.

As an instance of the foregoing rules, let us take the block

$$\begin{vmatrix} -2 & -1 & -1 & -4 \\ -1 & -2 & -1 & -6 \\ -1 & -1 & 2 & 4 \\ 2 & 1 & -3 & -8 \end{vmatrix}.$$

By rule (2) this is condensed into $\begin{vmatrix} 3 & -1 & 2 \\ -1 & -5 & 8 \\ 1 & 1 & -4 \end{vmatrix}$; this, again, by rule (3), is condensed into $\begin{vmatrix} 8 & -2 \\ -4 & 6 \end{vmatrix}$; and this, by rule (4), into -8, which is the required value.

The simplest method of working this rule appears to be to arrange the series of blocks one under another, as here exhibited; it will then be found very easy to pick out the divisors required in rules (3) and (4).

$$\begin{vmatrix} -2 & -1 & -1 & -4 \\ -1 & -2 & -1 & -6 \\ -1 & -1 & 2 & 4 \\ 2 & 1 & -3 & -8 \end{vmatrix}$$

$$\begin{vmatrix} 3 & -1 & 2 \\ -1 & -5 & 8 \\ 1 & 1 & -4 \end{vmatrix}$$

$$\begin{vmatrix} 8 & -2 \\ -4 & 6 \end{vmatrix}$$

$$-8.$$

This process cannot be continued when ciphers occur in the interior of any one of the blocks, since infinite values would be introduced by employing them as divisors. When they occur in the given block itself, it may be rearranged as has been already mentioned; but this cannot be done when they occur in any one of the derived blocks; in such a case the given block must be rearranged as circumstances require, and the operation commenced anew.

The best way of doing this is as follows:—

Suppose a cipher to occur in the hth row and kth column of one of the derived blocks (reckoning both row and column from the *nearest* corner of the block); find the term in the hth row and kth column of the given block (reckoning from the corresponding corner), and transpose rows or columns cyclically until it is left in an outside row or column. When the necessary alterations have been made in the derived blocks, it will be found that the cipher now occurs in an outside row or column, and therefore need no longer be used as a divisor.

The advantage of *cyclical* transposition is, that most of the terms in the new blocks will have been computed already, and need only be copied; in no case will it be necessary to compute more than *one* new row or column for each block of the series.

In the following instance it will be seen that in the first series of blocks a cipher occurs in the interior of the third. We therefore abandon the process at that point and begin again, rearranging the given block by transferring the top row to the bottom; and the cipher, when it occurs, is now found in an exterior row. It will be observed that in each block of the new series, there is only *one* new row to be computed; the other rows are simply copied from the work already done.

$$\begin{vmatrix} 2 & -1 & 2 & 1 & -3 \\ 1 & 2 & 1 & -1 & 2 \\ 1 & -1 & -2 & -1 & -1 \\ 2 & 1 & -1 & -2 & -1 \\ 1 & -2 & -1 & -1 & 2 \end{vmatrix}$$

$$\begin{vmatrix} 5 & -5 & -3 & -1 \\ -3 & -3 & -3 & 3 \\ 3 & 3 & 3 & -1 \\ -5 & -3 & -1 & -5 \end{vmatrix}$$

$$\begin{vmatrix} -30 & 6 & -12 \\ 0 & 0 & 6 \\ 6 & -6 & 8 \end{vmatrix}$$

$$\begin{vmatrix} 1 & 2 & 1 & -1 & 2 \\ 1 & -1 & -2 & -1 & -1 \\ 2 & 1 & -1 & -2 & -1 \\ 1 & -2 & -1 & -1 & 2 \\ 2 & -1 & 2 & 1 & -3 \end{vmatrix}$$

$$\begin{vmatrix} -3 & -3 & -3 & 3 \\ 3 & 3 & 3 & -1 \\ -5 & -3 & -1 & -5 \\ 3 & -5 & 1 & 1 \end{vmatrix}$$

$$\begin{vmatrix} 0 & 0 & 6 \\ 6 & -6 & 8 \\ -17 & 8 & -4 \end{vmatrix}$$

$$\begin{vmatrix} 0 & 12 \\ 18 & 40 \end{vmatrix}$$

$$36.$$

The fact that, whenever ciphers occur in the interior of a derived block, it is necessary to recommence the operation, may be thought a great obstacle to the use of this method; but I believe it will be found in practice that, even though this should occur several times in the course of one operation, the whole amount of labour will still be much less than that involved in the old process of computation.

I now proceed to give a proof of the validity of this process, deduced from a well-known theorem in determinants; and in doing so, I shall use the word "adjugate" in the following sense:—if there be a square block, and if a new block be formed, such that each of its terms is the determinant of the complemental minor of the corresponding term of the first block, the second block is said to be *adjugate* to the first.

The theorem referred to is the following :—

"If the determinant of a block $=$R, the determinant of any minor of the mth degree of the adjugate block is the product of R^{m-1} and the coefficient which, in R, multiplies the determinant of the corresponding minor."

Let us first take a block of 9 terms,

$$\begin{vmatrix} a_{1,1} & a_{1,2} & a_{1,3} \\ a_{2,1} & a_{2,2} & a_{2,3} \\ a_{3,1} & a_{3,2} & a_{3,3} \end{vmatrix} = \mathrm{R};$$

and let $\alpha_{1,1}$ represent the determinant of the complemental minor of $a_{1,1}$, and so on.

If we "condense" this, by the method already given, we get the block $\left\{\begin{matrix} \alpha_{3,3} & \alpha_{3,1} \\ \alpha_{1,3} & \alpha_{1,1} \end{matrix}\right\}$, and, by the theorem above cited, the determinant of this,

viz. $$\begin{vmatrix} \alpha_{3,3} & \alpha_{3,1} \\ \alpha_{1,3} & \alpha_{1,1} \end{vmatrix} = \mathrm{R} \times a_{2,2}.$$

Hence $$\mathrm{R} = \frac{\begin{vmatrix} \alpha_{3,3} & \alpha_{3,1} \\ \alpha_{1,3} & \alpha_{1,1} \end{vmatrix}}{a_{2,2}},$$ which proves the rule.

Secondly, let us take a block of 16 terms:

$$\begin{vmatrix} a_{1,1} \cdots & a_{1,4} \\ \vdots & \vdots \\ a_{4,1} \cdots & a_{4,4} \end{vmatrix} = \mathrm{R}.$$

If we "condense" this, we get a block of 9 terms; let us denote it by $\left\{\begin{matrix} b_{1,1} \cdot\cdot & b_{1,3} \\ \vdots & \vdots \\ b_{3,1} \cdot\cdot & b_{3,3} \end{matrix}\right\}$, in which $b_{1,1} = \begin{vmatrix} a_{1,1} & a_{1,2} \\ a_{2,1} & a_{2,2} \end{vmatrix}$, &c.

If we "condense" this block again, we get a block of 4 terms, each of which, by the preceding paragraph, is the determinant of 9 terms of the original block; that is to say, we get the block $\left\{\begin{matrix} \alpha_{4,4} & \alpha_{4,1} \\ \alpha_{1,4} & a_{1,1} \end{matrix}\right\}$; but, by the theorem already quoted, $\begin{vmatrix} \alpha_{4,4} & \alpha_{4,1} \\ \alpha_{1,4} & \alpha_{1,1} \end{vmatrix} = \mathrm{R} \times b_{2,2}$; therefore $\mathrm{R} = \dfrac{\begin{vmatrix} \alpha_{4,4} & \alpha_{4,1} \\ \alpha_{1,4} & \alpha_{1,1} \end{vmatrix}}{b_{2,2}}$; that is, R may be obtained by "condensing" the block $\left\{\begin{matrix} \alpha_{4,4} & \alpha_{4,1} \\ \alpha_{1,4} & \alpha_{1,1} \end{matrix}\right\}$.

This proves the rule for a block of 16 terms; and similar proofs might be given for larger blocks.

I shall conclude by showing how this process may be applied to the solution of simultaneous linear equations.

If we take a block consisting of n rows and $\overline{n+1}$ columns, and "condense" it, we reduce it at last to 2 terms, the first of which is the determinant of the first n columns, the other of the last n columns.

Hence, if we take the n simultaneous equations,

$$\begin{array}{l} a_{1,1}x_1+a_{1,2}x_2+\,\ldots\ldots+a_{1,n}x_n+a_{1,n+1}=0, \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ a_{n,1}x_1+\,\ldots\ldots\ldots\ldots\ldots\ldots+a_{n,n+1}=0\,; \end{array}$$

and if we condense the whole block of coefficients and constants, viz.

$$\left\{\begin{array}{ccc} a_{1,1} & \ldots & a_{1,n+1} \\ \vdots & & \vdots \\ a_{n,1} & \ldots & a_{n,n+1} \end{array}\right\},$$

we reduce it at last to 2 terms: let us denote them by S, T, so that

$$S=\left|\begin{array}{ccc} a_{1,1} & \ldots & a_{1,n} \\ \vdots & & \vdots \\ a_{n,1} & \ldots & a_{n,n} \end{array}\right|, \text{ and } T=\left|\begin{array}{ccc} a_{1,1} & \ldots & a_{1,n+1} \\ \vdots & & \vdots \\ a_{n,2} & \ldots & a_{n,n+1} \end{array}\right|.$$

Now we know that $x_1=(-)^n\dfrac{T}{S}$, which may be written in the form $(-)^n S\,.\,x_1=T$.

Hence the 2 terms obtained by the process of condensation may be converted into an equation for x_1, by multiplying the first of them by x_1, affected with + or −, according as n is even or odd. The latter part of the rule may be simply expressed thus:—"place the signs + and − alternately over the several columns, beginning with the last, and the sign which occurs over the column containing x_1 is the sign with which x_1 is to be affected."

When the value of x_1 has been thus found, it may be substituted in the first $\overline{n-1}$ equations, and the same operation repeated on the new block, which will now consist of $\overline{n-1}$ rows and n columns. But in calculating the second series of blocks, it will be found that most of the work has been already done; in fact, of the 2 determinants required in the new block, one has been already computed correctly, and the other so nearly so that it only requires the *last* column in each of the derived blocks to be corrected.

In the example given opposite, after writing + and − alternately over the columns, beginning with the last, we first condense the whole block, and thus obtain the 2 terms 36 and − 72. Observing that the x-column has the sign − placed over it, we multiply the 36 by $-x$, and so form the equation $-36x=-72$, which gives $x=2$.

Hence the x-terms in the first four equations become respectively 2, 2, 4, and 2; adding these values to the constant terms in the same equations, we obtain a block of which we need only write down the last two columns, viz.

$$\left|\begin{array}{rr} 2 & 4 \\ -1 & -2 \\ -1 & -2 \\ 2 & 6 \end{array}\right|.$$

We then condense these into the column $\begin{vmatrix} 0 \\ 0 \\ 2 \end{vmatrix}$, and, supplying from the second block of the first series the column $\begin{vmatrix} 3 \\ -1 \\ -5 \end{vmatrix}$, we obtain $\begin{vmatrix} 3 & 0 \\ -1 & 0 \\ -5 & 2 \end{vmatrix}$ as the last two columns of the *second* block of the new series; and proceeding thus we ultimately obtain the two terms 12, 12. Observing that the y-column has the sign $+$ placed over it, we multiply the first 12 by $+y$, and so form the equation $12y=12$, which gives $y=1$. The values of z, u, and v are similarly found.

It will be seen that when once the given block has been successfully condensed, and the value of the first unknown obtained, there is no further danger of the operation being interrupted by the occurrence of ciphers.

$$\begin{array}{cccccccc}
- & + & - & + & - & + & & \\
x & +2y & +\ z & -\ u & +2v & +2 & =0 \\
x & -\ y & -2z & -\ u & -\ v & -4 & =0 \\
2x & +\ y & -\ z & -2u & -\ v & -6 & =0 \\
x & -2y & -\ z & -\ u & +2v & +4 & =0 \\
2x & -\ y & +2z & +\ u & -3v & -8 & =0
\end{array}$$

$$\begin{vmatrix} 1 & 2 & 1 & -1 & 2 & 2 \\ 1 & -1 & -2 & -1 & -1 & -4 \\ 2 & 1 & -1 & -2 & -1 & -6 \\ 1 & -2 & -1 & -1 & 2 & 4 \\ 2 & -1 & 2 & 1 & -3 & -8 \end{vmatrix} \quad \begin{vmatrix} 2 & 4 \\ -1 & -2 \\ -1 & -2 \\ 2 & 6 \end{vmatrix} \quad \begin{vmatrix} 2 & 6 \\ -1 & -3 \\ -1 & -1 \end{vmatrix} \quad \begin{vmatrix} 2 & 5 \\ -1 & -1 \end{vmatrix} \quad \begin{vmatrix} 2 & 4 \end{vmatrix} \therefore -2v=4 \ldots v=-2$$

$$\begin{vmatrix} 3 & 3 \end{vmatrix} \therefore 3u=3 \ldots\ldots\ldots\ldots\ldots \quad u=\ \ 1$$

$$\begin{vmatrix} 3 & 0 \\ -1 & -2 \end{vmatrix}$$

$$\begin{vmatrix} -3 & -3 & -3 & 3 & -6 \\ 3 & 3 & 3 & -1 & 2 \\ -5 & -3 & -1 & -5 & 8 \\ 3 & -5 & 1 & 1 & -4 \end{vmatrix} \quad \begin{vmatrix} 3 & 0 \\ -1 & 0 \\ -5 & -2 \end{vmatrix} \quad \begin{vmatrix} 6 & 6 \end{vmatrix} \therefore -6z=6 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad z=-1$$

$$\begin{vmatrix} 6 & 0 \\ 8 & -2 \end{vmatrix}$$

$$\begin{vmatrix} 0 & 0 & 6 & 0 \\ 6 & -6 & 8 & -2 \\ -17 & 8 & -4 & 6 \end{vmatrix} \quad \begin{vmatrix} 12 & 12 \end{vmatrix} \therefore 12y=12 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad y=\ \ 1$$

$$\begin{vmatrix} 0 & 12 & 12 \\ 18 & 40 & -8 \end{vmatrix}$$

$$\begin{vmatrix} 36 & -72 \end{vmatrix} \therefore -36x=-72 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad x=\ \ 2$$

$$\begin{array}{cccccc}
- & + & - & + & & \\
5x & +2y & -3z & +\ 3 & =0 \\
3x & -\ y & -2z & +\ 7 & =0 \\
2x & +3y & +\ z & -12 & =0
\end{array}$$

$$\begin{vmatrix} 5 & 2 & -3 & 3 \\ 3 & -1 & -2 & 7 \\ 2 & 3 & 1 & -12 \end{vmatrix} \quad \begin{vmatrix} -3 & 8 \\ -2 & 10 \end{vmatrix} \quad \begin{vmatrix} -3 & 12 \end{vmatrix} \therefore 3z=12 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad z=\ \ 4$$

$$\begin{vmatrix} -7 & -14 \end{vmatrix} \therefore -7y=-14 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad y=\ \ 2$$

$$\begin{vmatrix} -11 & -7 & -15 \\ 11 & 5 & 17 \end{vmatrix}$$

$$\begin{vmatrix} -22 & 22 \end{vmatrix} \therefore 22x=22 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad x=\ \ 1$$

The Society then adjourned over the Whitsuntide Recess to Thursday, May 31.

May 31, 1866.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "An Account of Experiments in some of which Electroscopic Indications of Animal Electricity were detected for the first time by a new method of experimenting." By CHARLES BLAND RADCLIFFE, M.D., Fellow of the Royal College of Physicians in London, Physician to the Westminster Hospital and to the National Hospital for Paralysis and Epilepsy, &c. Communicated by CHARLES BROOKE, M.A. Received March 15, 1866.

Very soon after the discovery of animal electricity by Galvani, Hemmer ascertained that electroscopic indications of electricity might be obtained at the surface of the human body. The instruments used in these investigations were the electroscope of Saussure and the condenser of Volta: the broad result arrived at was—that the indications in question might be sometimes present and sometimes absent; that they pointed sometimes to positive electricity, and sometimes to negative; and that they did not depend (except, perhaps, in a very small degree) upon the friction of the hair, or skin, or clothes, or carpet.

Upwards of sixty years have passed since Hemmer published the account of his labours. During the first half of this period not a little good work was done in this branch of scientific inquiry, especially by Gardini in 1792, by Ahrens in 1817, and by Nasse in 1834; and what was done is in the main confirmatory of the genuineness of the work done by Hemmer. During the last thirty years, on the contrary, little or nothing has been done*. It seems, indeed, as if the discovery of the galvanometer, now a little more than thirty years ago, had diverted attention from the electroscope: at any rate it is the fact that it has been the fashion since the discovery of the galvanometer to forget the static, and to think only of the current phenomena of animal electricity. Nor is this altogether to be wondered at; for it must be allowed that the facilities for detecting the current phenomena of animal electricity are far, very far greater than those for detecting the static phenomena of this agent. Be this as it may, however, my own experience amounts to this—that I found it difficult to

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* One exception to this statement must be made in favour of some recent investigations by Dr. Meissner, of which an account is given in an article entitled "Ueber das electrische Verhalten der Oberfläche des menschlichen Körpers," in Henle and Pfeufer's 'Zeitschrift für rationelle Medicin.' Dritte Reihe, Band xii. 1861. These investigations seem to be very deserving of careful study, and I much regret that my attention was only for the first time directed to them in some remarks which followed the reading of this paper at the Meeting of the Royal Society.

detect these latter phenomena easily and satisfactorily until I hit upon the method of investigation employed in the experiments about to be described—a method which dispenses with the use of the condenser, which appears to be as delicate as it is certain and simple, and which I now proceed to describe without any further preamble.

I. *An account of the method of experimenting employed in the experiments which have to be related presently.*

In order to carry out this method of experimenting, the instruments necessary are two small electroscopes, two insulating stands upon which to fix these electroscopes, and a conducting rod with an insulating handle. Each electroscope is provided in the usual way with a pair of gold leaves, and with slips of tinfoil in the interior of the glass bell of the instrument, and it has, in addition, an opening underneath the wooden base, by which it may be screwed on the top of the insulating stand. Each insulating stand is a piece of glass rod 9 or 10 inches in length, fixed by its lower end into a suitable foot, and having at the upper end a screw which fits into the opening underneath the wooden base of the electroscope. In one stand (for reasons which will appear presently) the glass stem is varnished; in the other it is left unvarnished. The conducting rod may be of any form. For the rest, all that need now be said is that, in order to avoid the chance of electricity being developed by the friction of lackered surfaces, the caps of the electroscopes, and the end of the conducting rod which has to be brought into contact with the caps at certain times, are left unlackered, and that, in order to secure as good insulation as possible, the exterior of the electroscopes are well varnished whenever practicable.

In preparing for an experiment, the electroscopes are screwed on the insulating stands, and then charged in a particular way with free electricity—the one with free positive electricity, the other with free negative electricity. This charge is obtained by gently rubbing the glass stem of the insulating stands between the finger and thumb—the positive electricity from the unvarnished glass stem, the negative from that which is varnished. The electricity thus obtained is communicated, not to the cap, which is in direct communication with the gold leaves, but through the wooden base to the tinfoil slips which run halfway up the interior of the glass bell of the electroscope; and thus, instead of being charged directly, the gold leaves become charged *inductively* with the opposite kind of electricity to that which is communicated to the tinfoil slips. The result of doing this is that the gold leaves take up a given degree of divergence, and that they remain divergent so long as the tinfoil slips retain their charge of electricity. Charged in this manner, in fact, the gold leaves cannot be brought together by placing a conductor between the cap of the electroscope and the earth; indeed, so far from this being possible, the effect of placing a conductor in this position, under these circumstances, is (as may easily be understood) to increase the divergence of the gold leaves.

Now what has to be done in preparing for an experiment is to get the gold leaves in that second position of divergence into which they pass when the discharging rod is applied to the cap of the charged electroscope. First of all, the gold leaves are made to diverge to a given degree by charging the electroscope in the manner which has been described; then the conducting rod is placed in position and the gold leaves are made to take up the full degree of divergence by so doing; and when this is done the electroscopes are ready for use.

The electroscopes being "set" in this manner, the experiment which has to be performed consists in bringing the body, whose electrical condition has to be examined, to the cap of each electroscope in turn, and in noting the movements of the gold leaves. The experiment is simple, and the results are these. When the body is electrified positively, it causes increased divergence of the gold leaves in the electroscope in which these leaves are electrified with positive electricity; and *vice versâ,* when the body is electrified negatively, it causes increased divergence of the gold leaves in the electroscope in which these leaves are electrified with negative electricity, and diminished divergence in the electroscope in which these leaves are electrified with positive electricity. These are, as will be easily understood, the movements of the gold leaves which must take place under these circumstances. Moreover the charge of electricity in the electroscope reacts upon the body which is brought to the cap of the instrument, and produces, in a way which is intelligible enough, a certain small amount of increased divergence of the gold leaves of *both* electroscopes. Now this slightly increased divergence of the gold leaves in *both* electroscopes is of little or no moment when bodies electrified with comparatively large amounts of free electricity are made to act upon the caps of the instruments, but it is of great moment when these bodies are electrified with minute amounts of free electricity; for in this case the movements of the gold leaves arising from the action of the free electricity will be exaggerated or masked according as they happen to be in the same direction or in the opposite direction to the movements produced by the reaction of the charge in the electroscopes. Thus, if the degree of increased divergence in the gold leaves of *both* electroscopes arising from the reaction of the charge in the instruments be $=2$, and if the alteration in the divergence of the gold leaves produced by the action of free electricity be of the same value, that is $=2$, the result of this latter action will be, not increased divergence of the gold leaves $=2$ in one electroscope, and diminished divergence of these leaves $=2$ in the other instrument, but increased divergence $=4$, in the electroscope in which it causes increased divergence (for in this case it is the action of the free electricity *plus* that of the reaction of the charge in the instrument, $2+2=4$), and no alteration of divergence in the electroscope in which it causes diminished divergence (for in this case it is the converging action of the free electricity *minus* that of the diverging action of the charge in the instrument, that is, $2-2=0$),

—and so also in similar cases, the movement of increased divergence of the gold leaves in both electroscopes arising from the reaction of the charge in the instrument being added to or subtracted from the movement of the gold leaves produced by the action of free electricity, according as these movements happen to be in the same or in opposite directions.

These, then, being the facts, it is easy to see how, by using two electroscopes, it is possible to eliminate the reaction of the charge of electricity in the instruments upon the gold leaves, and to make this reaction tell in making more obvious the action of very minute quantities of electricity. It is easy to eliminate the reaction of the charge of electricity in the electroscopes upon the divergence of the gold leaves; for this reaction causes slightly increased divergence of these leaves in *both* instruments. It is easy to make the reaction of the charge of electricity in the electroscopes upon the divergence of the gold leaves tell in making more obvious the action of free electricity upon the divergence of these leaves; for it is plain that in the electroscope in which the action of positive electricity causes increased divergence of the gold leaves, this movement will be aided by the increased divergence of these leaves arising from the reaction of the charge of electricity in the electroscopes, and, *vice versâ,* that in the electroscope in which negative electricity causes increased divergence of the gold leaves this movement will be aided by the increased divergence of these leaves arising from the reaction of the electricity in the instrument. Nor does it follow that one of the electroscopes is in reality superfluous. *A priori,* indeed, it might be supposed that one electroscope would be sufficient. It might appear enough to take the movements of increased divergence of the gold leaves as evidence of the action of one kind of electricity, and the movements of diminished divergence as evidence of the other kind of electricity. But when dealing with minute quantities of electricity, it is found practically that the movements of diminished divergence are not so easily produced as those of increased divergence, and that there are impediments to free movement in this direction, arising from the clashing of the movement of diminished divergence due to the action of free electricity with the movement of increased divergence due to the reaction of the charge of electricity in the electroscope. In short, the plain truth appears to be, not only that the two electroscopes act as a check upon each other, and show the same thing from two different points of view, but that they furnish evidence which in itself is far more conclusive, when dealing with minute quantities of electricity, than can be got from either instrument singly.

In the description of the experiments upon which I am now about to enter, it is necessary to be able to distinguish the two electroscopes the one from the other; and I propose, therefore, to speak of the instrument in which the gold leaves are charged with positive electricity, and in which positive electricity causes increased divergence of these leaves, as the *Positive Electroscope,* and of the instrument in which the gold leaves are charged with negative electricity, and in which negative electricity produces increased divergence of these leaves, as the *Negative Electroscope.*

II. *An account of experiments in some of which electroscopic indications of animal electricity were detected by the method of experimenting which has just been described.*

In this account the degree of movement of the gold leaves is indicated by arbitrary figures. It is assumed also that the increased divergence of the gold leaves in *both* electroscopes, which movement has been seen to arise from the reaction of the charge of electricity in the electroscope, is $=2$; and in each experiment this figure is added to the movement produced by the action of free electricity in the case where this action causes increased divergence of the gold leaves, and subtracted in the case where this action causes diminished divergence of these leaves. Thus, in the case where the movement of the gold leaves arising from the action of free electricity is $=3$, the actual movement of the gold leaves in the instrument in which there is increased divergence of these leaves will be $=5$, for $3+2=5$, and in the instrument in which there is diminished divergence of these leaves will be $=1$, for $3-2=1$. Or, in the case where the movement due to free electricity is $=1$, there will be a different degree of *increased* divergence of the gold leaves in *both* electroscopes; for in the instrument in which the action of the free electricity causes increased divergence, the actual movement of the gold leaves will be $=3$, for $1+2=3$; and in the instrument in which this action tends to cause diminished divergence, this tendency will be overpowered by the increased divergence due to the reaction of the charge of electricity in the electroscope, and the actual movement which results will be one of increased divergence $=1$; for the movement of increased divergence arising from the reaction of the charge in the electroscope, $=2$, *minus* the movement of diminished divergence, $=1$, arising from the action of the free electricity, must be increased divergence $=1$, for $2-1=1$.

In the account of these experiments, also, certain abbreviations are made use of: thus i. d. stands for increased divergence of the gold leaves, d. d. for diminished divergence of those leaves.

For the rest, I have only to add that these experiments, which I leave to tell their own story, with the aid only of a few short comments at the end of each series, supply the first electroscopic indications of electricity in living blood and in living nerve-tissue, and that, to say the least, they clear up all uncertainty as to the presence in the living human body and in living muscular tissue of electricity capable of supplying like indications.

First Series.—Experiments which furnish electroscopic indications of electricity in the living human body.

In the first four of these experiments all that was done was to apply the palm of my hand or the tips of my fingers to the cap of each electroscope in turn, and to note the movements of the gold leaves produced by so doing. In the fifth experiment, the part experimented upon was brought to the caps of the electroscopes by taking hold of a loop of silk braid which had been previously attached to it.

Exp. 1.—In this case the result was d. d. =4 in the negative electroscope, and i. d. =8 in the positive electroscope,—a result showing, as has been already explained, that the electroscopes were acted upon by positive electricity =6.

Exp. 2.—Here the movements of the gold leaves indicated the action of positive electricity =2, the facts being—no alteration of the divergence of the gold leaves in the negative electroscope, and i. d. =4 in the positive electroscope.

Exp. 3.—In this case there was i. d. =2 in *both* electroscopes—a state of things showing that the hand was electrically neutral at the time, seeing that the movements of the gold leaves were only those which were due to the reaction of the charge of electricity in the electroscopes.

Exp. 4.—Here there was no alteration of the divergence of the gold leaves in the positive electroscope, and i. d. =4 in the negative electroscope—a state of things (the reverse of what happened in Exp. 2) signifying that the electroscopes were being acted upon by negative electricity =2.

Exp. 5.—A part of a foot which had been removed by amputation from a patient in the Westminster Hospital twenty-four hours previously was the part experimented upon in this instance; and the result was the same as in Exp. 3, namely, i. d. =2, in *both* electroscopes equally—the result which is brought about when a body which is electrically neutral is brought to the caps of the electroscopes.

⁂ I have performed experiments like the first four many hundred times, and I have repeatedly tested in the same way the electrical condition of other persons. In the great majority of instances the electroscopic indications were those of positive electricity. Only now and then all electroscopic indications were absent. I have also on several occasions repeated the experiment on portions of the dead body, and always without finding any signs of electricity. More than once, on the same occasion, I have found strong indications of positive electricity in one person, and very feeble indications, or no indications whatever, in another person. More than once also I have been able to detect electricity in my own body, or in the bodies of other persons at an elevation of some feet above the ground, when it was impossible to do so on the ground itself. In fact I have obtained proof of the existence of great variations in the electricity of myself and others at various times and under various circumstances, and I have begun a systematic series of observations with a view to ascertain the electrical condition of the human body at different times and under different circumstances as to health and disease. I have already in this way arrived at some curious results, and I have certainly seen enough to make me hope that a knowledge of the electrical changes in human bodies may shed much light upon the changes which are continually taking place in the vital condition of the human body, especially when the electrical changes within the body are taken in connexion with electrical and other changes without the body.

Second Series.—Experiments which furnish electroscopic indications of electricity in living blood.

The blood used in this series of experiments was collected in a wide-mouthed glass bottle capable of holding about 2 oz. Immersed in the blood and projecting to a convenient distance from the neck of the bottle, was a piece of platinum wire. The bottle was provided with a loop of silk braid, which loop was fastened in such a way as to allow the bottle to be lifted up by it without spilling the contents; and the necessary communication with each electroscope in turn was made by taking hold of the loop and by bringing the platinum wire projecting from the mouth of the vessel into connexion with the cap of the instrument.

Exp. 6.—In this case the blood used was from the internal jugular vein of a donkey, and the electroscopic indications obtained were those of negative electricity = 6,—the actual movements of the gold leaves being d. d. = 4 in the positive electroscope, and i. d. = 8 in the negative electroscope.

Exp. 7.—Here the blood was from the internal carotid artery of the same donkey which had furnished the venous blood used in the last experiment; and the result was also the same, namely, d. d. = 4 in the positive electroscope, and i. d. = 8 in the negative electroscope,—a result denoting the action of negative electricity = 6 upon the instruments.

Exp. 8.—In this experiment blood from the carotid artery of a sheep was examined; and the movements of the gold leaves were those of positive electricity = 2, there being no alteration of the divergence of the gold leaves in the negative electroscope, and i. d. = 4 in the positive electroscope.

Exp. 9.—Here the blood used was from the carotid artery of a dog. The examination was made without loss of time, and the animal seemed to be in good health; but all signs of electricity in the blood were absent, the movements of the gold leaves being those of i. d. = 2 in both electroscopes equally.

Exp. 10.—The blood experimented upon in this case was that which had been already used in Exp. 6, an interval of an hour and a half having elapsed between the two experiments. In the former experiment the blood gave electroscopic indications of negative electricity = 6; in this instance these indications had disappeared altogether, for there was i. d. = 2 in *both* electroscopes equally.

⁂ I have repeated experiments like these many times upon the blood of various animals—oxen, sheep, dogs, rabbits, and so on—sometimes upon pure arterial blood, sometimes upon pure venous blood, more frequently upon the mixed stream which follows the knife of the butcher in the ordinary process of slaughtering sheep and oxen. As a rule, I have found decided electroscopic indications of negative electricity indifferently in arterial, venous, or mixed blood; not unfrequently I have failed to find any such signs. Now and then I have found comparatively feeble signs of positive electricity. In every case also where I have examined blood after an interval of an hour or so from the time when it had flowed fresh from the vessel, I failed to detect any sign of electricity, negative or positive. These experiments no doubt leave much to be discovered in the same direction, but this at least they do,—they furnish the first electroscopic proof of the presence of electricity in living blood. Nay, it is perhaps not too much to say that they supply the first unequivocal proof of electricity in blood, for the current electricity recently obtained from blood by M. Scoutteten and Dr. Shettle *may* in reality be nothing more than the result of chemical and other changes produced by the blood upon the terminal wire of the galvanometer used in these experiments.

Third Series.—Experiments which furnish electroscopic indications of electricity in living nerve-tissue.

The plan adopted in this series of experiments was to tie a loop of silk braid to the part to be experimented on, and to use this loop as the means for bringing this part to the cap of each electroscope in turn.

Exp. 11.—The medulla oblongata of an ox obtained a few minutes after the animal had been killed in the ordinary way in the shambles, was the part used in this experiment. At one time the cut surface exposing the transverse section of the fibres and the internal grey matter was brought to the caps of the electroscopes; at another time the uncut surface corresponding to the longitudinal surface of the fibres was treated in this manner; and in each case the movements of the gold leaves were indicative of the action of positive electricity = 6, or thereabouts, the only difference perceptible being a slight one of degree. The average movements obtained were those of d. d. = 4 in the negative electroscope, and i. d. = 8 in the positive electroscope.

Exp. 12.—The brachial enlargement of the spinal cord of an ox, taken out of the canal when the carcass was being split into two lateral halves at the usual time, that is, about half an hour from the moment when the animal had been felled with the pole-axe, was used in this experiment, and the result was d. d. = 4 in the negative electroscope, and i. d. = 8 in the positive electroscope. It was found also that this result was the same, except in some trifling difference in degree, in the case where the transverse sectional surface of the fibres was brought to the caps of the electroscopes, and in the case where the longitudinal surface of these fibres, natural or artificial, was examined in this manner.

Exp. 13.—Here the part examined was the posterior lobe of the cerebrum of a sheep which had been killed in the usual way a few minutes previously in the shambles. No time was lost in making the necessary preparations, but not the slightest indications of electricity were obtainable, the movements being only those of i. d. = 2 in both electroscopes equally.

Exp. 14.—The cerebellum of a sheep was examined in this experiment, and the result was—no alteration in the divergence of the gold leaves in the negative electroscopes, and i. d. = 4 in the positive electroscopes, a state of things indicating the action of positive electricity = 2 upon the instruments. It was ascertained also that all parts of the cerebellum indifferently behaved in the same manner.

Exp. 15.—In this experiment the brain of a donkey just killed by loss of blood was examined, and it was found that all parts of the surface indifferently, natural or artificial, gave similar indications of negative electricity = 4, there being d. d. = 2 in the positive electroscope, and i. d. = 6 in the negative electroscope.

Exp. 16.—The brain of the donkey used in the last experiment was used also in this instance, an interval of an hour, or thereabouts, having elapsed between the two experiments. When first examined this organ gave indications of negative electricity = 4; now it was found to have lost all traces of electrical activity everywhere, for the movements of the gold leaves were simply those of i. d. = 2 in *both* electroscopes equally.

⁂ These experiments, as I believe, bring to light a new fact, inasmuch as they furnish the first electroscopic proof of the presence of electricity in living nerve-tissue. Judging from these and several other experiments of the same kind, in which dogs and rabbits, as well as oxen, sheep, and donkeys, were put under contribution, it would seem that living nerve-tissue, as a rule, furnishes electroscopic signs of electricity, sometimes positive and sometimes negative in character, and that these signs are always absent when the nerve-tissue may be supposed to have lost all traces of vitality. And this also would seem to be a conclusion deducible from the same evidence—that all parts of the nerve-tissue present signs of the *same kind* of electricity. It would seem, in fact, as if these experiments suggested a conclusion which is at variance with a conclusion drawn by Professor Du Bois Reymond from some of his experiments. Watching the direction of the "nerve-current" which passes through the galvanometer between the longitudinal surface, natural or artificial, of the nerve-fibres, and the transverse sectional surface of these fibres, Professor Du Bois Reymond comes to the conclusion that these two surfaces are in opposite electrical conditions, the one being positive, the other negative. Because the current passes in a particular direction, he infers that these surfaces must be electrified with different kinds of electricity. But it is plain that the current might pass between parts electrified with *different degrees* of the *same* electricity; and indeed M. Du Bois Reymond himself explains the current passing between two points of the same surface in this manner; and therefore, even on his own showing, there is no necessity to suppose that the longitudinal surface of the nerve-fibres is electrified with one kind of electricity and the transverse sectional surface of the fibres with the other kind. At any rate, the facts revealed by the electroscope do not appear to be of doubtful significance, and the only inference which I can deduce from them is that every part of the surface of living nerve-tissue, natural or artificial, furnishes signs of the same kind of electricity, and that the only electrical differences between one part and another are nothing more than differences of degree.

Fourth Series.—Experiments which furnish electroscopic indications of electricity in living muscular tissue.

In this series of experiments the mode of proceeding was the same as that adopted in the last series.

Exp. 17.—The piece of muscle examined in this experiment was cut out of the sterno-mastoid of an ox a few moments after the animal had been killed in the shambles, and every part of the surface was tested in turn, and, except in some trifling difference in degree, the movements of the gold leaves were in all cases indicative of the action of positive electricity = 6, these movements being those of d. d. = 4 in the negative electroscope, and those of i. d. = 8 in the positive electroscope.

Exp. 18.—Here the sterno-mastoid of a sheep just killed in the ordinary way in the shambles, furnished the material for experiment, and the result was—no alteration in the divergence of the gold leaves in the negative electroscope, and i. d. = 4 in the positive electroscope—a result showing the action of positive electricity = 2 upon the electroscopes.

Exp. 19.—In this instance the portion of muscle examined was taken from the glutæus maximus of a donkey which had just been killed by hæmorrhage, and it was found that all parts of the surface indifferently supplied indications of negative electricity = 1, the movements of the gold leaves being those of i. d. = 1 in the positive electroscope, and i. d. = 3 in the negative electroscope.

Exp. 20.—A piece of the left ventricle of the heart of a dog just dead from hæmorrhage was examined in this instance, and the only movements of the gold leaves were those which are produced by the action of a body electrically neutral, namely, i. d. = 2 in both electroscopes equally.

Exp. 21.—Here the piece of muscle experimented upon was that used in Exp. 17. Twelve hours had elapsed between the two experiments, and *rigor mortis* had now fully set in, and the result showed that the positive electricity which was present formerly was no longer present, the movements of the gold leaves being simply those of i. d. = 2 in both electroscopes equally.

⁂ With the exception of an experiment in which Professor Matteucci incidentally states that he obtained "signes de tension avec un condensateur délicat" from one of his "muscular piles," these experiments furnish, so far as I know, the only electroscopic indications of the presence of electricity in living muscular tissue—perhaps the very first really *distinct* proofs of this fact. I have repeated these experiments several times on the muscles, living and dead, of various animals, oxen, sheep, donkeys, dogs, and rabbits, and I have found in the great majority of instances that all parts of the surface of living muscle furnished indications of the same kind of electricity, that this electricity was sometimes positive, sometimes negative, and that these signs were invariably absent in muscle which had passed into the state of *rigor mortis*. These experiments, moreover, make it difficult to agree with Professor Du Bois Reymond in thinking that the longitudinal surface, natural or artificial, of the muscular fibres, and the transverse sectional surface of these fibres, are electrified with different *kinds* of electricity. With respect to the electricity of muscular tissue, indeed, it seems to be precisely as it is with respect to the electricity of nerve-tissue, namely this, that all parts of the surface are electrified with the same kind of electricity, positive or negative, as the case may be, the only difference between one part and another being one of degree; and the comments upon M. Du Bois Reymond's conclusions, when speaking upon the condition of nerve-tissue as to electricity, are equally applicable to the present case, if only the words muscular tissue and muscular current be substituted for nerve-tissue and nerve-current.

In conclusion, it only remains for me to direct attention to one bearing of the facts recorded in this paper. These facts, one and all, exhibit animal electricity, not in the form of a feeble nerve-current, or of a feeble muscular current, or of the still feebler currents of less definite character, but as endowed with a considerable amount of tension. They bring to light a property of animal electricity which is more intelligible on the supposition that the *primary* condition of this electricity is not current but statical. It is easy to account for these phenomena of tension if the primary condition of animal electricity be statical, for tension is the characteristic property of statical electricity; it is by no means easy to account for these phenomena of tension if the primary condition of animal electricity be that of the current revealed by the galvanometer, for the currents so revealed are far too feeble to allow one to suppose that they can be endowed with an appreciable amount of tension. In a word, with the phenomena of tension to account for which are revealed in the experiments recorded in this paper, the natural inference, as it seems to me, is that the primary condition of animal electricity is, not current, but statical, and that the currents made known by the galvanometer to which so much attention has been paid of late—the muscular current, the nerve-current, and the rest—are *secondary* phenomena developed accidentally by placing the ends of the coil of the galvanometer so as to include points in which the electricity is different in degree. Nay, it would seem that these currents may in reality be a retarded discharge of statical electricity, for it is a fact that they cannot be detected without a coil of which the wire is so long and so fine as to be capable of giving sufficient resistance to bridle a discharge into the quieter pace of ordinary currents*. Many important consequences in physiology and in pathology, as I think, result directly or indirectly from this view of the matter, of which some are set forth in some Lectures which I gave at the College of Physicians in London three years ago, and which have since appeared in print; but it is no part of my present task to consider these consequences. Indeed, what I proposed to do in this paper I have now done; and this was simply to direct attention to certain facts as facts, and to offer certain passing comments suggested naturally by these facts.

II. "On the Dynamical Theory of Gases." By J. Clerk Maxwell, F.R.S. L. & E. Received May 16, 1866.

(Abstract.)

Gases in this theory are supposed to consist of molecules in motion, acting on one another with forces which are insensible, except at distances which are small in comparison with the average distance of the molecules. The path of each molecule is therefore sensibly rectilinear, except when two molecules come within a certain distance of each other, in which case the direction of motion is rapidly changed, and the path becomes again sensibly rectilinear as soon as the molecules have separated beyond the distance of mutual action.

Each molecule is supposed to be a small body consisting in general of parts capable of being set into various kinds of motion relative to each other, such as rotation, oscillation, or vibration, the amount of energy existing in this form bearing a certain relation to that which exists in the form of the agitation of the molecules among each other.

The mass of a molecule is different in different gases, but in the same gas all the molecules are equal.

The pressure of the gas is on this theory due to the impact of the molecules on the sides of the vessel, and the temperature of the gas depends on the velocity of the molecules.

The theory as thus stated is that which has been conceived, with various degrees of clearness, by D. Bernoulli, Le Sage and Prevost, Herapath, Joule, and Krönig, and which owes its principal developments to Professor Clausius. The action of the molecules on each other has been generally assimilated to that of hard elastic bodies, and I have given some applica-

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* It is to be supposed that certain molecules in living animal bodies are, under certain given conditions, a constant source of electricity—are so, perhaps, in the way in which certain molecules of the electrophorus are such a source. The idea is that this electricity is so supplied as to admit of a series of frequent discharges, or to keep up a constant current if these discharges are retarded sufficiently. At any rate, it does not follow that this constancy of the current of animal electricity detected by the galvanometer is an objection in itself to the idea that the primary condition of animal electricity may be, not current, but statical.

tion of this form of the theory to the phenomena of viscosity, diffusion, and conduction of heat in the Philosophical Magazine for 1860. M. Clausius has since pointed out several errors in the part relating to conduction of heat, and the part relating to diffusion also contains errors. The dynamical theory of viscosity in this form has been reinvestigated by M. O. E. Meyer, whose experimental researches on the viscosity of fluids have been very extensive.

In the present paper the action between the molecules is supposed to be that of bodies repelling each other at a distance, rather than of hard elastic bodies acting by impact; and the law of force is deduced from experiments on the viscosity of gases to be that of the inverse fifth power of the distance, any other law of force being at variance with the observed fact that the viscosity is proportional to the absolute temperature. In the mathematical application of the theory, it appears that the assumption of this law of force leads to a great simplification of the results, so that the whole subject can be treated in a more general way than has hitherto been done.

I have therefore begun by considering, first, the mutual action of two molecules; next that of two systems of molecules, the motion of all the molecules in each system being originally the same. In this way I have determined the rate of variation of the mean values of the following functions of the velocity of molecules of the first system:—

α, the resolved part of the velocity in a given direction.

β, the square of this resolved velocity.

γ, the resolved velocity multiplied by the square of the whole velocity.

It is afterwards shown that the velocity of translation of the gas depends on α, the pressure on β, and the conduction of heat on γ.

The final distribution of velocities among the molecules is then considered, and it is shown that they are distributed according to the same law as the errors are distributed among the observations in the theory of "Least Squares;" and that if several systems of molecules act on one another, the average *vis viva* of each molecule is the same, whatever be the mass of the molecule. The demonstration is of a more strict kind than that which I formerly gave, and this is the more necessary, as the "Law of Equivalent Volumes," so important in the chemistry of gases, is deduced from it.

The rate of variation of the quantities α, β, γ in an element of the gas is then considered, and the following conclusions are arrived at.

(α) 1st. In a mixture of gases left to itself for a sufficient time under the action of gravity, the density of each gas at any point will be the same as if the other gases had not been present.

2nd. When this condition is not fulfilled, the gases will pass through each other by diffusion. When the composition of the mixed gases varies slowly from one point to another, the velocity of each gas will be so small that the effects due to inertia may be neglected. In the quiet diffusion of two gases, the volume of either gas diffused through unit of area in unit of time is equal to the rate of diminution of pressure of that gas as we pass in the direction of the normal to the plane, multiplied by a certain coefficient, called the coefficient of interdiffusion of these two gases. This coefficient must be determined experimentally for each pair of gases. It varies directly as the square of the absolute temperature, and inversely as the total pressure of the mixture. Its value for carbonic acid and air, as deduced from experiments given by Mr. Graham in his paper on the Mobility of Gases,* is

$$D=0{\cdot}0235,$$

the inch, the grain, and the second being units. Since, however, air is itself a mixture, this result cannot be considered as final, and we have no experiments from which the coefficient of interdiffusion of two pure gases can be found.

3rd. When two gases are separated by a thin plate containing a small hole, the rate at which the composition of the mixture varies in and near the hole will depend on the thickness of the plate and the size of the hole. As the thickness of the plate and the diameter of the hole are diminished, the rate of variation will increase, and the effect of the mutual action of the molecules of the gases in impeding each other's motion will diminish relatively to the moving force due to the variation of pressure. In the limit, when the dimensions of the hole are indefinitely small, the velocity of either gas will be the same as if the other gas were absent. Hence the volumes diffused under equal pressures will be inversely as the square roots of the specific gravities of the gases, as was first established by Graham†; and the quantity of a gas which passes through a thin plug into another gas will be nearly the same as that which passes into a vacuum in the same time.

(β) By considering the variation of the total energy of motion of the molecules, it is shown that,

1st. In a mixture of two gases the mean energy of translation will become the same for a molecule of either gas. From this follows the law of Equivalent Volumes, discovered by Gay-Lussac from chemical considerations; namely, that equal volumes of two gases at equal pressures and temperatures contain equal numbers of molecules.

2nd. The law of cooling by expansion is determined.

3rd. The specific heats at constant volume and at constant pressure are determined and compared. This is done merely to determine the value of a constant in the dynamical theory for the agreement between theory and experiment with respect to the values of the two specific heats, and their ratio is a consequence of the general theory of thermodynamics, and does not depend on the mechanical theory which we adopt.

4th. In quiet diffusion the heat produced by the interpenetration of the

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* Philosophical Transactions, 1863.

† "On the Law of the Diffusion of Gases," Transactions of the Royal Society of Edinburgh, vol. xii. (1831).

gases is exactly neutralized by the cooling of each gas as it passes from a dense to a rare state in its progress through the mixture.

5th. By considering the variation of the difference of pressures in different directions, the coefficient of viscosity or internal friction is determined, and the equations of motion of the gas are formed. These are of the same form as those obtained by Poisson by conceiving an elastic solid the strain on which is continually relaxed at a rate proportional to the strain itself.

As an illustration of this view of the theory, it is shown that any strain existing in air at rest would diminish according to the values of an exponential term the modulus of which is $\frac{1}{5,100,000,000}$ second, an excessively small time, so that the equations are applicable, even to the case of the most acute audible sounds, without any modification on account of the rapid change of motion.

This relaxation is due to the mutual deflection of the molecules from their paths. It is then shown that if the displacements are instantaneous, so that no time is allowed for the relaxation, the gas would have an elasticity of form, or "rigidity," whose coefficient is equal to the pressure.

It is also shown that if the molecules were mere points, not having any mutual action, there would be no such relaxation, and that the equations of motion would be those of an elastic solid, in which the coefficient of cubical and linear elasticity have the same ratio as that deduced by Poisson from the theory of molecules at rest acting by central forces on one another. This coincidence of the results of two theories so opposite in their assumptions is remarkable.

6th. The coefficient of viscosity of a mixture of two gases is then deduced from the viscosity of the pure gases, and the coefficient of interdiffusion of the two gases. The latter quantity has not as yet been ascertained for any pair of pure gases, but it is shown that sufficiently probable values may be assumed, which being inserted in the formula agree very well with some of the most remarkable of Mr. Graham's experiments on the Transpiration of Mixed Gases*. The remarkable experimental result that the viscosity is independent of the pressure and proportional to the absolute temperature is a necessary consequence of the theory.

(γ) The rate of conduction of heat is next determined, and it is shown

1st. That the final state of a quantity of gas in a vessel will be such that the temperature will increase according to a certain law from the bottom to the top. The atmosphere, as we know, is colder above. This state would be produced by winds alone, and is no doubt greatly increased by the effects of radiation. A perfectly calm and sunless atmosphere would be coldest below.

2nd. The conductivity of a gas for heat is then deduced from its viscosity, and found to be

$$\frac{5}{3}\,\frac{1}{\gamma-1}\,\frac{p_0}{\rho_0\theta_0}\,\frac{\mu}{S},$$

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* Philosophical Transactions, 1846.

where γ is the ratio of the two specific heats, p_0 the pressure, and ρ_0 the density of the standard gas at absolute temperature θ_0. S the specific gravity of the gas in question, and μ its viscosity. The conductivity is, like the viscosity, independent of the pressure and proportional to the absolute temperature. Its value for air is about 3500 times less than that of wrought iron, as determined by Principal Forbes. Specific gravity is ·0069.

For oxygen, nitrogen, and carbonic oxide, the theory gives the conductivity equal to that of air. Hydrogen according to the theory should have a conductivity seven times that of air, and carbonic acid about $\frac{7}{9}$ of air.

III. "On the means of increasing the Quantity of Electricity given by Induction-Machines." By the Rev. T. Romney Robinson, D.D. Received May 10, 1866.

Among the remarkable results obtained by studying the spectra of electric discharges, is the change exhibited by certain substances when the nature of the discharge is varied. In general the mere spark shows fewer and fainter lines than when a Leyden jar is in connexion, though the amount of electricity supplied by the machine is the same. In the latter case, however, the discharge passes almost instantaneously, and therefore its concentrated action will be more powerful. But, as far as I know, much has not been attempted towards increasing the power of the jar: this cannot be done by increasing its surface (unless indeed that be too small to condense all the electricity supplied); the supply itself must be increased.

This may be done in three ways:—

First, the power of the exciting battery may be increased. This, however, is limited by the risk of destroying the acting surfaces of the rheotome; and by the decreasing rate at which the magnetism of the iron core increases with the primary current. In some investigations on the electromagnet (Trans. Irish Academy, vol. xxiii. p. 529) I have shown that its lifting power L is approximately given by the equation

$$L=\frac{A\Psi}{B+\Psi},$$

in which Ψ is the product of the current and number of spires, A the maximum lift of the magnet, and B the Ψ which would excite it to half A. The rate of change $\frac{dL}{d\Psi}$ is therefore inversely as $(B+\Psi)^2$. The results obtained with two of the magnets which I used will illustrate this. Their A's are 781 lbs. and 278. The first 1000 of Ψ make their lifts 576 and 235; the second 1000 adds to these 87 and 19; the third 35 and 8; and the fourth only 19 and 3. With a primary of 180 spires, $\Psi=4000$ implies a current which can evolve in a voltameter 34·7 cubic inches of gases per minute, and of course has great deflagrating power. There is therefore not much to be gained in this direction.

Secondly, the secondary helix may be made of longer or thicker wire. It will be shown immediately that the length does not increase the quantity of the current at all, and that the effect of the thickness is limited.

Thirdly, the analogy of the voltaic battery suggests the plan of combining several helices collaterally, as is done when cells are arranged for quantity; and this I think may avail to a very great extent.

In spectral work, as in most other applications of the inductorium (as the Germans have named it), the breaking current alone is of importance: the other, though equal in quantity, is so much inferior in tension that it is stopped by a thin film of highly rarefied air*. This current proceeds from two causes. When the circuit is broken, the current in the primary ceases; not instantaneously†, but in a time which is very small; according to Edlund less than $\frac{1}{200}$ of a second. During its decline it induces a current in the secondary. But while it was passing it had magnetized the iron core of the apparatus: this magnetism now passes away, and in doing so it also induces a current in the secondary, which lasts longer and is more powerful than the other. I compared the two by measuring those given when the core was removed from a primary, and when it was in its place: they were as 1 : 8·62 when the rheotome made 17 discharges in a second; so that in round numbers the electric induction was only a tenth of the whole effect. I shall therefore in what follows confine myself to the magnetic induction.

If y be the magnetism at any time t, M its maximum, P the potential of the magnet on the secondary helix, Π the potential of that helix on itself, r the resistance of the secondary circuit, ϕ the secondary current at the time t, we have, as is well known,

$$\phi=\frac{P}{r}\times-\frac{dy}{dt}-\frac{\Pi}{r}\times\frac{d\phi}{dt}, \quad \ldots\ldots\ldots\ldots \quad (a)$$

the last term being the counter current produced by the reaction of ϕ on the helix. If the inductive coefficient of electric action be different from that of magnetic, Π should be multiplied by a factor e, which in (b) will multiply μ in the exponent and denominator. I see no reason why they should differ unless some work be lost by molecular changes in the core when excited. The great difference between the two currents which I have just mentioned arises most probably from the different values of t in the exponentials.

To integrate (a) we must assume some relation between dy and dt. The

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* This is not quite correct. Mr. Gassiot showed from the reversed curvature of the strata in an exhausted tube that some of the closing current does pass when the action is powerful. The same conclusion follows from a fact which I observed last year with Mr. Atkinson's magnificent Ruhmkorff. In general when a discharge is made between platinum points, the negative one only is ignited; in this case the positive one was so also, though to a far less degree and for half the length.

† As the tension at the opening of the rheotome decreases, so must also the velocity of discharge there; and time is required for the passage of the electricity from the centre of the circuit to its extremities.

most probable is that the loss of magnetism is as the magnetism, which gives $\frac{dy}{y}=-\mu dt$, whence $y=\mathrm{M}e^{-\mu t}$.

Putting $b=\frac{r}{\Pi}$, and supposing ϕ to vanish with t,

$$\phi=\frac{\mathrm{P}}{r}\mathrm{M}\times\frac{\mu b}{\mu-b}\left\{\left(\frac{y}{\mathrm{M}}\right)^{\frac{b}{\mu}}-\frac{y}{\mathrm{M}}\right\}. \quad \ldots\ldots\ldots \quad (b)$$

The total current in the time $t=\Phi=\int_0^t\phi dt=\int-\frac{\mathrm{P}}{r}dy-\frac{1}{b}\int d\phi$, and hence putting F for $-\frac{\mathrm{P}}{r}(\mathrm{M}-y)$ and c for $\frac{y}{\mathrm{M}}$, we obtain

$$\Phi=\frac{\mathrm{F}}{1-c}\left\{1+\frac{bc}{\mu-b}-\frac{\mu c^{\frac{b}{\mu}}}{\mu-b}\right\} \quad \ldots\ldots\ldots \quad (c)$$

The expression for the current due to the electric induction will be similar to this, so far as having the factor F and the exponential. If, as is not unlikely, the relation of $d\mathrm{E}$ to dt be the same as for the magnetism, they would only differ in the values of μ and c.

The quantity F is the current which would be produced were it not for the inductive reaction of the current on itself. It is as P directly and r inversely. The first of these, P, is as n the number of spires in the helix* multiplied by a rather complex function of its length and diameter, which is constant when they are given. The second, r, may be assumed proportional to the length and section of the wire of the helix, for in general the other resistances of the circuit are comparatively small. Hence it follows that

If two equipotential helices equally excited be placed in series, the tension will be doubled; but the current will be intermediate between that of each, for $\mathrm{F}=\frac{2\mathrm{P}}{r+r'}$, and if $r=r'$ it will not be changed. If they be used collaterally (their homonymous terminals connected), P remains unchanged, and therefore the currents of the helices are simply added. If there be an external resistance, allowance must be made for it. This may be extended to any number of helices; for calling the external resistance ρ, we find

$$\mathrm{F}_n=\frac{\frac{\mathrm{P}}{r}+\frac{\mathrm{P}'}{r'}+\ldots\frac{\mathrm{P}_n}{r_n}}{1+\rho\left(\frac{1}{r}+\frac{1}{r'}\ldots+\frac{1}{r_n}\right)}. \quad \ldots\ldots\ldots \quad (d)$$

The constant c must be a small fraction, for in any ordinary work of the inductorium the residual magnetism of the core is very feeble. As $it=e^{-\mu t}$, μt must be large; and as t for wire cores does not exceed a few hundredths of a second†, μ must be very large.

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* Not as the mere length of wire, as is sometimes loosely stated.

† I have been informed that with the inductorium which Mr. Whitehouse constructed for the first Atlantic telegraph, the cores of which were massive iron cylinders, the discharge lasted some seconds. If it be still in existence, it would be interesting to examine the spectra which it would give.

The potential Π is as n^2 multiplied by another function of the length and diameters of the helix (see Maxwell's valuable paper "On the Electromagnetic Field," Phil. Trans. 1865); and the term b is always less than unity. When helices are consecutive their Π's are added, not when collateral.

From this it follows that $\frac{bc}{\mu-b}$ may be neglected; that $\frac{\mu}{\mu-b}$ is nearly unity; and that the difference between F and Φ increases as b diminishes.

When equal helices are consecutive, b as well as F are unchanged; therefore so is Φ.

When they are collateral, each separate b remains unchanged (unless they be so close that they react on each other); and therefore, as with F, the resultant Φ is the sum of its components.

If the resistance of the wire be diminished by increasing its section without making much change in the dimensions of the helix, b is diminished, and therefore the coefficient of F. It is evident from the form of equation (c) that Φ has a maximum for r, and that beyond this there is actual loss of power in increasing the thickness of the wire.

It remained to test these views by experiment, but the task has some difficulties. A single discharge of inductive electricity is usually determined by the swing which it causes in a galvanometer needle; but it is scarcely possible to get two discharges exactly equal. The slightest variation in the manner of breaking the circuit, the least oxidation or roughening of the surfaces where the break is made, change the result; and therefore it seemed best to take the actual working of the inductorium, in hopes that the average of some thousand discharges must be near the real value of the current.

The rheometer which I used is Weber's (for the use of which I am indebted to the kindness of Mr. Gassiot), and it showed an amount of fluctuation even greater than I expected. With every precaution as to the action of the rheotome, the mirror of the Weber never becomes stationary, and the oscillations are irregular; twelve of them were taken for each set, of course read at each end and reduced by the usual formula; yet the sets differ so much, that I only offer their results as tolerable approximations. Two facts illustrating this uncertainty may be mentioned. With a mechanical rheotome driven at a uniform speed, and its acting surfaces platinum, the ratios of the current were—

When set so that the point rises but little from the anvil ..	1·0000
Rise greater	1·7894
Rise still greater, tension of spring greater	1·9685
Rise still greater, tension further increased	1·8371

Here a slight change of the adjustment nearly doubles the action of the inductorium.

Another cause of uncertainty is the variable speed of the rheotome. In general it is worked by the primary current, and therefore is affected by fluctuation of the battery and the extra current of the primary. The mechanical rheotome which I have mentioned is driven by clockwork, and its speed can be exactly regulated. With it I obtained

		s		
1.	Time of rheotome's stroke	=0·2865.	Current	=1·0000
2.	„ „	0·2020.	„	0·9298
3.	„ „	0·1862.	„	0·8741
4.	„ „	0·1381.	„	0·7235
5.	„ „	0·1219.	„	0·5771
6.	„ „	0·1201.	„	0·5588
7.	„ „	0·0983.	„	0·3788
8.	„ „	0·0883.	„	0·2823
9.	„ „	0·0671.	„	0·2478
10.	„ „	0·0476.	„	0·2007
11.	„ „	0·0278.	„	0·1946
12.	„ „	0·0196.	„	0·1273
13.	„ „	0·0098.	„	0·0470

No. 10 was taken with a mercurial rheotome; and the remaining three with a spring one, such as Mr. Ladd applies to his inductoria*.

This decrease of power is due to the core requiring time to be magnetized. Suppose the current $=A+Bt$, A being that caused by the electric induction, B by the magnetic, I get from the above by minimum squares $A=0·0802$; $B=4·1713$; $\frac{B}{A}=508$; which values represent the observations pretty fairly, the probable error being $\pm 0·0491$. Three cells were used here: on another trial with five I had $\frac{B}{A}=129$, confirming a previous remark that the electric induction increases faster than the magnetic. Hence much power is lost by working at too high a speed.

The inductorium which I use consists of a strong oak table, on which are fixed vertically four primary helices, their axes being 12 and 18 inches apart; at which distance the mutual action of the secondary helices is scarcely sensible in the Weber. I denote these primaries by P', P'', &c. Their wire is No. 12; P' and P'' are 12·5 inches long; they have, in four layers, the first 383 spires, the second 343†. Their cores of iron wire, No. 18, are 1·12 diameter. P''' and P^{iv} are 13·5 inches long; they have each 181 spires in two layers, and their cores are 1·60 diameter. They are all insulated by strong glass jars, and their connectors are so arranged that the current can be sent through any one separately, or through all at once. The normal arrangement is that the battery-current passes through P''', then through P' and P'' collateral, and lastly through P^{iv}. Thus the Ψ or exciting power of each primary is nearly the same. On a shelf below stands a Fizeau's condenser, each of whose coatings is 120 feet divided into

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* For the first nine of these the time was given by the clockwork of the rheotome; for the rest by dropping sparks on a slip of prepared paper, which was made to travel at the rate of 12 inches per second.

† The difference arose from the cotton lapping being thicker in P''.

five sections. This, though so potent in respect of sparks, does not affect the quantity, which with it I found as 1·0000, without it 0·9948, a difference not worth noting. The case, however, would be different if there were a gaseous interval in the circuit*. Beside this shelf is a bracket which supports rheotomes of various kinds.

Over the jars can be put any of the secondary helices, the constants of which are given in the following Table:—

The potential P and resistance of the first one, which I take as a standard, are assumed =1.

TABLE I.

Name.	Feet of wire.	Diameter of wire.	Layer.	Spires.	Entire diameter.	Height.	Potential. P.	Resistance. *r*.
		in.			in.	in.		
G	17,070	0·0092	73	13,655	6·84	4·0	1·00000	1·00000
H	17,070	0·0092	73	13,655	6·84	4·0	1·00000	0·85270
I	8,110	0·0153	55	6,570	6·72	4·0	0·48114	0·10079
K........	8,110	0·0153	55	6,570	6·72	4·0	0·48114	0·10079
M	8,130	0·0192	29	6,524	6·21	6·0	0·47520	0·08073
N........	8,130	0·0192	29	6,524	6·56	5·9	0·47520	0·08282
A........	7,000	0·0107	...	6,189	5·93	3·5	0·44673	} 0·51808
B........	9,200	0·0107	...	8,135	5·93	4·0	0·60272	

The first six were made by Mr. Ladd, who also determined for me the length and number of layers. The thickness of the wires was measured by me with a fühlhebel which read to 0·0001: each is a mean of ten measures at different places, for no wire that I have ever tried is quite uniform. The two last are experimental, their wire not being lapped, but merely insulated by a varnish of wax and rosin, as proposed by the late Dr. Callan: this plan does well for quantity, but cannot be trusted for any high tension.

The potentials were computed, supposing the helices at the middle of tke primary P''' (where they are a maximum). For G I computed them in four other positions, and had the curiosity also to measure the currents.

Distance of G from centre =0, potential 1·0000, current 1·0000
" " 1, " 0·9842, " 0·9856
" " 2, " 0·9790, " 0·9746
" " 3, " 0·9488, " 0·9488
" " 4, " 0·8798, " 0·9181

All but the last agree tolerably. For positions of M and N, which were not central, they were specially computed.

The resistances were obtained by including in the circuit of a small Grove's

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* Three of the combinations described in Table II. have tensions nearly as 1, 2, and 3. Their quantities, with a circuit entirely metallic, and with one in which there was an interval of $\frac{1}{8}$ inch of air at 0·01 in. pressure, are as

G .... entire 1·0000, interval 1·0000
G+H............... " 1·0348, " 1·5580
V+V′ .... " 0·9994, " 1·8844

The first set are nearly equal; the second increase, though at a decreasing rate, with the tension.

cell and a tangent rheometer of 950 spires, first the helix G, then that to be determined, and lastly the sum of it and G. Assuming G=1, we have three equations to determine—the r of the helix in question, the remaining r of the circuit, and the electromotive force of the cell. In deducing the currents, the term involving $\sin^2\theta$ was used with a coefficient obtained by integrating through the length and breadth of the rheometer's coil; and as its mean diameter is 6·4 times the length of its needle, and θ never passed 54°, I think the numbers of the Table are true to the last decimal.

For brevity I symbolize the combinations, when in series, as a sum, G+H; when collateral, as a product G.H; and for a reason which will soon appear, when two are on the same primary I denote them by a new letter, as V=G+I. At first I used I and G on P'''; K and H on P^{iv}. To prevent disruptive discharge, they were kept 1·5 inch asunder by disks of baked wood. The results are given in

Table II.

Name.	Φ.	F.*	Spark.	Sets.	$\frac{\Phi}{F}$.	Sum of components.	Π.	*b.*
			in.					
G…………	1·0000	1·0000	3·95	…	1·00000	……	1·00000	1·00779
H …………	…	1·1000	3·37	…	…	……	1·00000	0·985385
G+H……	1·0348	1·0096+	…	3	1·02497	……	2·00000	0·92306
G.H ……	1·9988	2·0223	…	4	0·98837	2·0855	1·00000	
I …………	2·5343	4·3635	1·23	1	0·59167	……	0·23149	0·46542
I+K ……	2·7654	4·5358	2·49	3	0·60969	……	0·46298	0·44872
G.H (I+K) ..	3·0552	6·5756	0·80	4	0·46462	4·6220		
V+V′……	0·9996	1·3776	9·35	3	0·72562	……	2·46298	0·83075
V.V′ ……	1·9434	2·7319	4·39	1	0·71138			

F is computed with a correction for the place of the helix on the primary, and with a resistance which includes that of the Weber =0·00779. The Weber must have a considerable Π of its own; but as I did not know its constants, and as this Π must vary with the deflection, I did not compute it. Possibly some of the discrepancies may be owing to this. The sparks show the difference of tension; they were taken with platinum points, and when the machine was excited by four Groves. The column, sum of components, gives for the collateral combinations the values which arise from adding the Φ of each helix, taking into account the Weber's resistance.

1. It will be observed that G+H with twice as many spires, and V+V′ with thrice as many as G, give the same current; so also that of I+K is near that of I.

2. On the other hand, G.H is twice G, and V.V′ twice V+V′.

3. It is also manifest that the effect of I is not proportional to its diminished resistance: its F is 4·4 times greater than that of G, but its actual

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* These values of F should be multiplied by a factor representing the F of G, which must be greater than its Φ, here assumed as unity. As, however, it belongs to all, its omission does not affect the comparison.

current is only 2·5. This is at once explained by its *b* being so much less. So also the ratio of the theoretic to the effective current is nearly unity in G+H, while it is only 0·73 in V+V′, and for the same reason. In G . H . (I+K) the ratio is still smaller; but I shall recur to this.

I now used the four primaries I+K=L on P^{iv}, single helices on the others. I had some doubt whether the difference of the cores might not influence the results, and tried this with P', P''', and one P^{v}, whose core =1·90 inch in diameter and 15·5 inches long. It has 349 spires of No. 15 in two layers. G was put on each of them, and currents transmitted, which made their Ψ's nearly equal.

Table III.

Name.	Sect. of core as	Φ.	Sets.	Spark.	Ψ.
P''	1·00	1·0414*	3·5	2·225	1169
P^{iv}	2·04	1·0000	2·	3·237	1168
P^{v}	3·00	1·0054	2·	2·662	1156

It follows from this that the least of these cores is large enough for the excitation produced by four cells: the size does seem to increase the spark, though this increase may be owing to better insulation of the core-wires in P''' and P^{v}†.

The following results were obtained with the mechanical rheotome worked at a uniform speed of 13 discharges per second.

Table IV.

	Φ.	F.	Sets.	$\frac{\Phi}{F}$.	Sum of components.	Π.	*b*.
G...............	1·0000	1·0000	...	1·00000	......	1·00000	1·00779
L=I+K ...	1·8832	4·5106	19	0·41752	......	0·6+	0·34624−
G+L	1·3149	1·6143	2	0·81451	......	1·6+	0·75002−
G . L	3·0402	5·3109	11	0·57159	2·8324		
H	1·4196	1·3578	2	1·04551	......	1·00000	0·85385
C=A+B ...	1·9882	2·0112	2	0·99855	......	0·56286+	0·92704
N...............	2·5535	5·4100	1	0·47202	......	0·31646	0·27756
I	2·5580	4·3635	2	0·58623	......	0·23149	0·46542
K...............	2·6465	4·3635	5	0·60651	......	0·23149	0·46542
G . K	3·5558	5·2464	1	0·67776	3·8594		
G . L . H	4·3179	6·5756	3	0·65666	4·1799		
G . L . C	5·1460	6·9400	3	0·74153	4·6932		
G . L . C . H...	6·0758	8·2081	3	0·74030	6·0488		
M+N.........	2·5207	5·4586	2	0·46179	......	0·63292	0·26862
=O...........	1·7616	5·4586	4	0·32271	......	0·73130	0·23248
S=M+N ...	1·6687	4·9644	1	0·33614	......	0·65810	0·28543
L . O	3·4596	9·5774	1	0·36124	3·4996		
M N I K ...	8·1939	15·7398	1	0·52058	8·2736		

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* 2·5 sets taken on the first day were very unsteady. The one taken on the second was close, and gave 1·0267.

† At the same time I tried two helices similar to A and B, except that their wire is varnished *iron*, which were given to me by Dr. Callan. Their Φ is 0·6887, and their spark only 0·524 in. The Φ of A + B is 2·9138 times as great, a difference caused solely by the greater resistance of the iron.

1. As before, two helices in series give no increase of quantity; M+N is the same as N, G+L nearly the mean of G and L.

2. The quantity of collateral helices is seen in column 6 (as in Table II.) to be the sum of the separate actions of each. The discrepancies are not greater than what can be explained by the uncertainties inherent in these measures, which I have already described. One apparent exception is a strong confirmation of this rule—the case of G . H (I+K). Its observed $\Phi=3{\cdot}0552$, while the sum $=4{\cdot}6220$. G and I being on the same primary are excited together; but in measuring either, as there is no current in the other (but merely a state of tension), their Π is not changed. When, however, they are connected collaterally, the currents react on each other, their Π's are increased: the Φ's are thus diminished, and therefore their resultant is the sum of quantities less than those used in the computation. The *b*'s in this case become—for G 0·94289, for H 0·79885, and for I+K 0·34588, which are quite sufficient to account for the difference.

3. As with I in Table II., so here it will be observed that N has less relative power than either I or K; its actual power is even less, though its theoretical force exceeds theirs in the proportion of 5 : 4. This is explained by its *b* being so much smaller; but it gives this important information, that, at least in helices of these dimensions, nothing is gained by using wire thicker than that of I, or $\frac{1}{65}$ of an inch*.

4. The effect of L is far less than that of I+K. In the first the helices are on the same primary, in the other on separate ones. In the former case the Π is larger, for it is the sum of the Π of each on itself, and those of each on the other; *b* therefore is less. Besides, the potential of the core on the helices is less than when each of them is central on it.

The difference is even more remarkable in O as compared to its elements M+N, its effect being only 0·7 of the other, and 0·3 of the theoretic power. The same disparity of course prevails in their combinations; O . L giving only 3·46, while the same four helices arranged on separate primaries give 8·19. The combinations G . H (I+K) and G . L . H have the same helices; but in the first two were on the same primaries. As, however, they were 1·5 inch instead of 0·5 apart, the Π was not so much increased as in the other cases, and therefore there is not quite so great a decrease of power.

The following practical maxims may be deduced from the experiments and reasoning which I have related.

The attention of instrument-makers has been chiefly directed towards increasing the length of spark given by these machines, and in this they have succeeded to a surprising degree; but in doing so they have not added to the quantity of electricity which is produced by them. This, however, is by far the most important object; for in most applications of

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* This is between 27 and 28 of the Birmingham wire-gauge. I believe Ruhmkorff uses 28.

the inductorium all tension above what is necessary to force the necessary quantity of current through the circuit is useless, nay sometimes injurious*. I am inclined to think that a tension which gives sparks of 4 inches will be found quite sufficient in ordinary cases, and this will be given by about 20,000 spires; all beyond only adding to the weight of the instrument, its cost, and the difficulty of insuring its insulation. It must be kept in mind that the mere quantity is independent of the length of wire: I actually found it the same for a flat spiral of 21 spires and for a helix of 13,655.

It is not, I believe, ascertained what is the best proportion of height and diameter for a secondary helix of a given number of spires. It is generally made as long as its primary, though perhaps not on any definite principle. The magnetic potential P is in this form a little greater than in that which I used, but so also is Π: the length of wire is less, which increases F, but also decreases b; and *à priori* it is not easy to decide which way the balance inclines. The Π is something less if the spires be in separate sections than if they be in one continuous coil.

The dimensions of the core do not seem to be of importance as to quantity within the limits which I tried; their length seems to increase the tension.

The quantity is greatly diminished when the rheotome works rapidly; and in spectral work the probable limit of its slowness is that the impression on the eye shall be continuous.

The quantity increases with the diameter of the wire up to a maximum, which is attained when this is about the sixty-fifth of an inch.

Helices may be combined either for tension or quantity without much loss of these respective powers†.

If for the first, they are combined in series; the general tension is the sum of the individual ones, and in this way we can obtain sparks of a length limited only by the strength of the insulator which is interposed between the primary and secondary helices. If the latter be all of the same wire, the quantity remains unchanged; if they differ in this respect, it will be intermediate between the weakest and strongest.

If they are combined for quantity, they must be set collaterally, *i. e.* all their positive terminals connected, and all their negative. The resulting current will be the sum of all the separate ones, but the tension is not increased; the sparks seem even a few hundredths of an inch shorter, but are much denser, and in the higher combinations approach to the character of a jar discharge. Hence there is no risk to the apparatus by extending this mode of combination to any extent.

It deserves notice that the helices need not be equal in tension or resistance; thus the arrangement G . K gives little less than the sum of its

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* It has often been remarked that intense discharges will not show strata well in an exhausted tube.

† The connectors add some resistance and some counter-induction.

components, though K has only half as many spires as G and but a tenth of its resistance.

In combining these instruments, the primaries should not be consecutive if of large numbers, for so the action of their extra-current would be very destructive to the rheotome; with $P'+P''$ containing 726 spires in series the spark in the mercurial one is almost explosive, but when they are collateral it works quietly. Were, however, ten or twelve to be so combined, it would require a battery of very large cells to maintain the current, and it is better to have a separate battery for each pair of primaries. In this I find no difficulty; the negative* poles of all the batteries are connected with the mercury of the rheotome; from its platinum point, separate wires go to the entering bind-screw of each primary, other wires go from their exit bind-screws to the positive poles of their respective batteries, and thus their action is perfectly simultaneous. Of course, if many batteries were used, the current in the rheotome might be too powerful, but then there would be no difficulty in having separate rheotomes worked by one electromagnet, and (at least with the mercurial form) adjusting them by a revolving mirror to perfect synchronism.

In this way I feel sure that we can attain an amount of electric power which has not yet been approached by the inductorium, and which may be expected to be a most powerful means of research in those inquiries to which I referred at the commencement of this paper. At the head of these stands the palmary discovery of Mr. Huggins, that there are nebulæ and comets whose matter possesses spectral attributes not corresponding to that of the sun, the stars, or our own earthly elements. Is that difference an indication of some body *sui generis*, or a mere result of peculiar temperature or other molecular conditions? Is, for instance, the bright line, corresponding to one of nitrogen, which occurs, we may say, normally, produced by nitrogen as such? If so, what has blotted out the other bright lines of that magnificent spectrum? Is it due to an *element* of nitrogen, dissociated by some enormous temperature from other elements, perhaps from hydrogen, one line of which is also present? And the third line, elsewhere unknown—is it the herald of a new body, or merely a derivative from another spectrum? We cannot even hope for an answer to these questions till the spectra of at least those elements which seem cosmical have been examined through a range of temperature extending from the lowest that developes in them luminous lines, to the highest that is excited by the most potent electric discharges which we can produce and control. Now, to obtain such a graduated range, the plan of combination which I have been describing seems well fitted. It, of course, cannot be expected to equal, under any extension, the wonderful voltaic battery of Mr. Gassiot (at least its arc-discharge); but how few can avail themselves

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* If the mercury be made positive, each discharge makes a sharp report and blows about the metal and alcohol in most unpleasant profusion.

of such an instrument as that! But if, as seems probable, we can without much difficulty increase the heating-power of the induction-discharge an hundredfold, we shall have made a very great step, and by means which are everywhere accessible. Inductoria are common; there are few situations where a physicist cannot obtain access to several, and combine them as Despretz did the voltaic batteries of Paris to make the experiments which have thrown such splendour on his name.

IV. "On the Stability of Domes." By E. Wyndham Tarn, M.A. Communicated by George Godwin, Esq. Received May 5, 1866.

The few writers who have attempted to treat the subject of the Equilibrium of Domes mathematically, have entirely failed to obtain results that are of any practical use to the architect. Their failure has arisen from taking a too theoretical view of the subject, and endeavouring by mathematical reasoning to find the *form* which a dome ought to have in order that it might stand safely. Such a question is of no practical utility, as domes of various sizes and forms have been erected for centuries past, and the question for the architect is,—given a dome of certain form and size, what are the conditions which must obtain between it and the wall of the building it is intended to cover, in order that the whole structure may be in a condition of stability?

The object, therefore, of this paper is to find a solution to the following problem:—

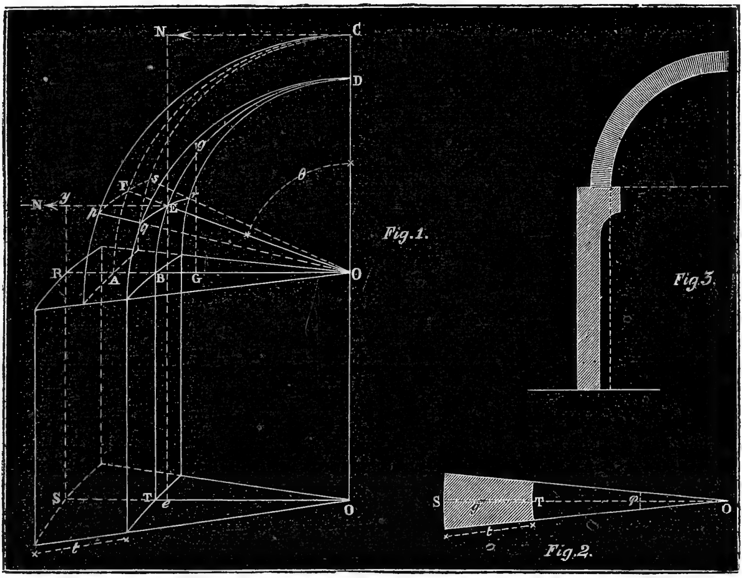
Given a spherical dome, built of stone or brick, of any radius and thickness, and standing on a "drum" or walls of any height; to find the thrust of the dome on the "drum," and the thickness that must be given to the walls in order to insure the stability of the structure.

I take the case of the dome having a spherical section, as being the form most commonly used; but the same method of investigation will apply to domes of any form.

In this investigation I shall consider the dome as made up of a large number of arched ribs, of which the bases resting on the top of the "drum" subtend a small angle (ϕ) at the centre, and the vertices have no thickness; each rib having the form of a wedge cut out of the spherical shell by two planes intersecting in a vertical line through the centre, and making the small angle ϕ with each other. I shall then consider that the two wedges thus formed on opposite sides of the dome, thrust against each other at the vertex, as in the case of an ordinary semicircular arch, and by this means keep each other in equilibrium. Of course no arch of this form if built alone could stand for a moment, as it would give way laterally; but in the dome this is prevented by the parts on each side of the rib under consideration.

This method is adopted by Venturoli in treating on the equilibrium of domes.

Fig. 1 represents a rib of the form above described cut out of the dome, and the corresponding part of the "drum" which sustains it. The arch will have a tendency to fall in at the crown C D, and open outwards at the haunches E F, causing the joint C D to open at D, and that at E F to open at F.



We may therefore suppose that N, the thrust of the corresponding rib on the opposite side, acts at C.

We shall now find the effect of the force N upon a given joint EF. Let P be the weight of the portion of rib above EF; x the perpendicular distance from E of a vertical from the centre of gravity of P; y the vertical distance of CN from E. Then the moments of these forces about E are Px and Ny; and in order that there may be equilibrium, we must have N equal to the greatest value of $P . \frac{x}{y}$. We have therefore to express $P\frac{x}{y}$ in terms of θ, the angle which FE makes with the vertical, and find what value of θ makes $P\frac{x}{y}$ a maximum.

Let r be the internal and R the external radius of the sphere, δ the weight of a cubic foot of the material of which it is composed. Then the volume of any portion of the rib ($p\,q\,r\,s$ CD) is

$$V=\iiint r^2 \sin\theta . d\theta . dr . d\phi ; \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (A)$$

so that $P=\delta . V=\delta . \phi\,(1-\cos\theta)\frac{R^3-r^3}{3}$, taking limits from $\theta=0$.

If g is the centre of gravity of the portion of the rib whose weight is P, and G its projection on OA; then if $OG=z$, we find the value of z from

$$z=\frac{\iiint r^3 \,.\, \sin^2\theta \,.\, \cos\phi \,.\, dr \,.\, d\theta \,.\, d\phi}{\iiint r^2 \,.\, \sin\theta \,.\, dr \,.\, d\theta \,.\, d\phi}$$

$$=\frac{\sin\frac{1}{2}\phi}{\frac{1}{2}\phi}\cdot\frac{\iint r^3\,\frac{1}{2}\,(1-\cos 2\theta)\,dr\,.\,d\theta}{\iint r^2 \,.\, \sin\theta \,.\, dr \,.\, d\theta}. \quad \ldots\ldots \quad (B)$$

And since ϕ is small, we may consider $\frac{\sin\frac{1}{2}\phi}{\frac{1}{2}\phi}=1$, nearly; in which case

$$z=\frac{3}{8}\,\frac{R^4-r^4}{R^3-r^3}\,\frac{\theta-\frac{1}{2}\sin 2\theta}{1-\cos\theta}.$$

Now x is the perpendicular distance of gG from E; therefore

$$x=r\sin\theta-z.$$

Therefore $Px=\delta \,.\, \phi\,(1-\cos\theta)\frac{R^3-{}^3}{3}\times\left\{r\sin\theta-\frac{3}{8}\,\frac{R^4-r^4}{R^3-r^3}\,\frac{\theta-\frac{1}{2}\sin 2\theta}{1-\cos\theta}\right\}$,

or

$$Px=\frac{\delta\,.\,\phi}{24}\left\{8r\,(R^3-r^3)\sin\theta\,(1-\cos\theta)-3\,(R^4-r^4)\,(\theta-\tfrac{1}{2}\sin 2\theta)\right\}.$$

And since $y=R-r\cos\theta$, and $N=P\frac{x}{y}$,

$$N=\frac{\delta\,.\,\phi}{24}\,\frac{8r\,(R^3-r^3)\sin\theta\,(1-\cos\theta)-3\,(R^4-r^4)\,(\theta-\frac{1}{2}\sin 2\theta)}{R-r\cos\theta}.$$

I will take ϕ as the circular measure of an angle of 2°, or $\phi=\frac{\pi}{90}=\cdot 0349066$, in which case we have

$$N=\cdot 001454\,\delta\,\frac{8r(R^3-r^3)\sin\theta\,(1-\cos\theta)-3\,(R^4-r^4)\,(\theta-\frac{1}{2}\sin 2\theta)}{R-r\cos\theta}$$

$$=\cdot 001454\,\delta\times N', \text{ say.}$$

We have now to find what value of θ will make N' a maximum, and as the ordinary rules for maxima will not apply to this expression, we must find it by calculating N' for different values of θ. I have done this for the case when $r=10$, $R=11$, and find that N is greatest when $\theta=70°$; thus

$\theta=69°$ .... $N'=506{\cdot}034$
$\theta=70°$ .... $N'=506{\cdot}241$
$\theta=71°$ .... $N'=506{\cdot}018$.

We have now to substitute in N the values of θ, $\sin\theta$, &c. when the angle is 70°, or when $\theta=35\,\frac{\pi}{90}=1{\cdot}22173$, $\sin\theta=\cdot 93969$, $\cos\theta=\cdot 34202$, $\sin 2\theta=\cdot 64279$.

This reduces the expression for N to

$$N=\delta\frac{\cdot 007192\,r\,(R^3-r^3)-\cdot 0039273\,(R^4-r^4)}{R-\cdot 34202\,r} \quad \ldots \quad (1)$$

We can now transpose N and P to the point E; and in order to find the thickness of the pier, we proceed to take their moments, together with those of the part of the rib below EF, and the pier itself, about the outer bottom edge S of the pier.

If we call $y\mathrm{S}=b$, the perpendicular distance from S of the direction of N, then

$$b=\mathrm{H}+r\,.\cos\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where H is the height RS of the pier or "drum."

Let $\mathrm{S}e=a$, the distance from S of a vertical dropped from E; F=the weight of the portion of the rib below EF; c=the perpendicular distance from S of a vertical from the centre of gravity of the lower portion of the rib whose weight is F; Q=the weight of the portion of the "drum" on which the rib stands, and q=the distance from S of a vertical from its centre of gravity; we have then (in equilibrium) the equation

$$\mathrm{N}\,.\,b=\mathrm{P}\,.\,a+\mathrm{Q}\,.\,q+\mathrm{F}\,.\,c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which I will call the *Equation for Equilibrium.* From this equation we can find the thickness (t) of the pier necessary to produce equilibrium.

In order, however, that we may have stability in the structure, we must multiply Nb by the *coefficient of stability,* which we may take as 2. The equation from which to find the value of t then becomes

$$2\mathrm{N}\,.\,b=\mathrm{P}\,.\,a+\mathrm{Q}\,.\,q+\mathrm{F}\,.\,c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which I will call the *Equation of Stability.*

I now proceed to find the values of P . a, Q . q, and F . c.

The value of P was previously found to be

$$\mathrm{P}=\delta\,.\,\phi\;\;1-\cos\theta)\frac{\mathrm{R}^3-r^3}{3};$$

and if we substitute for ϕ and θ their values,

$$\mathrm{P}=\cdot007656\,\delta\,(\mathrm{R}^3-r^3).$$

Also $a=\mathrm{S}e=t+r\,(1-\sin\theta)=t+\cdot06031\,r$; so that we find for the value of P . a,

$$\mathrm{P}a=\cdot007656\,\delta\,(\mathrm{R}^3-r^3)\,t+\cdot0004618\,\delta\,.\,(\mathrm{R}^3-r^3). \quad . \quad . \quad . \quad . \quad (5)$$

The value of F is found by means of the integral (A), taking the limits from $\theta=0$ to $\theta=\frac{\pi}{2}$; which gives us

$$\mathrm{F}=\delta\,.\,\phi\,.\cos\theta\frac{\mathrm{R}^3-r^3}{3}$$

$$=\cdot00398\,\delta\,(\mathrm{R}^3-r^3).$$

Let z_1 be the distance from O on OA of the perpendicular dropped from the centre of gravity of the portion of the rib whose weight is F. Then the integral (B) gives us, by taking the same limits of θ as for F,

$$z_1=\frac{3}{8}\,\frac{\mathrm{R}^4-r^4}{\mathrm{R}^3-r^3}\,\frac{\frac{\pi}{2}-\theta+\frac{1}{2}\sin 2\theta}{\cos\theta}\,.$$

And if we substitute for θ its value,

$$z_1 = \cdot 7351 \frac{R^4 - r^4}{R^3 - r^3}.$$

And $c = OR - z_1 = t + r - z_1$.

Hence we find

$$F.c = \cdot 00398\,\delta\,(R^3 - r^3)\,t + \cdot 00398\,\delta\,\{r\,(R^3 - r^3) - \cdot 7351\,(R^4 - r^4)\} \quad . \ (6)$$

We have now to find the moment about S of the weight of the pier.

Let Q be the weight of the pier, δ_1 the weight of a cubic foot of its material; then

$$Q = \tfrac{1}{2}\phi \,.\, \delta_1 \left((r+t)^2 - r^2\right) . H = \tfrac{1}{2}\phi\,\delta_1\,(2r+t)\,t\,.\,H$$
$$= \cdot 017453\,\delta_1\,(2r+t)\,t\,.\,H.$$

Let figure 2 represent the plan of the pier, g its centre of gravity, $ST = t$ its thickness. Then by the integral calculus we find

$$Og = \frac{4}{3}\frac{(r+t)^3 - r^3}{(r+t)^2 - r^2} \times \frac{\sin\frac{1}{2}\phi}{2\,.\,\frac{\phi}{2}}; \text{ or, putting } \frac{\sin\frac{1}{2}\phi}{\frac{1}{2}\phi} = 1,$$

$$= \frac{2}{3}\frac{(r+t)^3 - r^3}{(r+t)^2 - r^2} = \frac{2\,(3r^2 + 3rt + t^2)}{3\,(2r+t)};$$

$$q = Sg = OS - Og = r + t - Og$$

$$= 3\,\frac{(r+t)\,(2r+t) - 2\,(3r^2 + 3rt + t^2)}{3\,(2r+t)}$$

$$= \frac{3rt + t^2}{3\,(2r+t)}.$$

We therefore obtain for the value of Qq,

$$Qq = \cdot 017453\,\delta_1\,(2r+t)\,t\,.\,H\,\frac{3r+t}{3\,(2r+t)}\,t$$
$$= \cdot 0058177\,\delta_1\,.\,t^2\,(3r+t)\,.\,H. \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ (7)$$

We can now, by adding together the several quantities (5), (6), (7), and equating to the value of Nb as found from (1) and (2), form the equation for *equilibrium* (3), which will be a cubic equation in respect of t. We shall thence find the value of t necessary to produce *equilibrium* in the structure, and, by equating the same quantities to 2Nb, form the equation for *stability* (4), from which we get the value of t necessary to produce *stability* in the structure.

The following examples will serve to show how readily these formulæ can be applied, and the use which the practical architect may make of them.

Example 1 :—

Let $r = 10$ ft., $R = 11$ ft., $\delta = 125$ lbs., $\delta_1 = 150$ lbs., $H = 50$ ft.

From (1) . . . $N = 92\cdot 0092$;

„ (2) . . . $b = 53\cdot 4202$;

therefore

$$Nb = 4915,$$

and

$$2Nb = 9830.$$

From (5) . . . $P\,.\,a = 316{\cdot}767\ t + 191{\cdot}06975$;
„ (6) . . . $F\,.\,c = 164{\cdot}6725\ t - 50{\cdot}54$;
„ (7) . . . $Q\,.\,q = 1308{\cdot}98\ t^2 + 43{\cdot}63275\ t^3$.

Hence the equation for *equilibrium* (3) is

$$4915 = 43{\cdot}63275\ t^3 + 1308{\cdot}98\ t^2 + 481{\cdot}4395\ t + 140{\cdot}52975,$$

which reduces to

$$t^3 + 30t^2 + 11{\cdot}03t - 109{\cdot}42 = 0.$$

We can solve this by means of Horner's process, and find $t = 1{\cdot}7$ ft. for *equilibrium.*

The equation for *stability* (4) becomes

$$9830 = 43{\cdot}63275t^3 + 1308{\cdot}98t^2 + 481{\cdot}4395t + 140{\cdot}52975,$$

which reduces to

$$t^3 + 30t^2 + 11{\cdot}03t - 222{\cdot}069 = 0,$$

from which we obtain, by Horner's process,

$$t = 2{\cdot}45 \text{ ft. for } stability.$$

Example 2 :—

Let $r = 30$ ft., $R = 33$ ft., $H = 80$ ft., δ and δ_1 as before.

From (1) . . . $N = 2483{\cdot}84$;
„ (2) . . . $b = 90{\cdot}2606$;

therefore

$$Nb = 224192{\cdot}8,$$

and

$$2Nb = 448385{\cdot}6.$$

From (5) . . . $P\,.\,a = 8552{\cdot}71\ t + 15476{\cdot}65$;
„ (6) . . . $F\,.\,c = 4446{\cdot}16\ t + 4094{\cdot}17$;
„ (7) . . . $Q\,.\,q = 69{\cdot}8124\ t^3 + 6283{\cdot}116\ t^2$.

Adding and reducing, the equation for *equilibrium* becomes

$$t^3 + 90t^2 + 186{\cdot}2t - 3048{\cdot}31 = 0,$$

whence $t = 4{\cdot}83$ ft. for *equilibrium.*

And the equation for *stability* is

$$t^3 + 90t^2 + 186{\cdot}2t - 6159{\cdot}7 = 0,$$

whence $t = 7{\cdot}07$ ft. for *stability.*

Example 3 :—

I will apply the formulæ to the case of the dome of Sultan Mohammed's tomb at Beejapore, described in Mr. Fergusson's 'Handbook of Architecture.' One peculiarity of this structure is that the inner face of the dome is set 5 ft. 6 in. *within* the inner face of the wall on which it rests, as shown in fig. 3.; so that in calculating the values of a, &c., we must add 5·5 ft. to the thickness t; hence we shall get a proportionately less value for t from the resulting equations.

In this dome, $r = 62$ ft., $R = 72$ ft., $H = 116$ ft.; and we will suppose, as before, that $\delta = 125$ lbs., $\delta_1 = 150$ lbs.

From (1) . . . . . $N = 31132$;
„ (2) . . . . . $b = 137{\cdot}20524$;

therefore

$$\mathrm{N}b = 4271473{\cdot}522,$$

and

$$2\mathrm{N}b = 8542947{\cdot}044.$$

From (5) . . . $\mathrm{P}.a = {\cdot}957\,(134920)\,(t+5{\cdot}5) + 3{\cdot}56745\,(134920)$
$= 129118{\cdot}44\,t + 1191511{\cdot}78;$

„ (6) . . . $\mathrm{F}.c = {\cdot}4975\,(134920)\,(t+5{\cdot}5) - {\cdot}4975\,(527854{\cdot}4)$
$= 67122{\cdot}7t + 106567{\cdot}28;$

„ (7) . . . $\mathrm{Q}.q = 101{\cdot}22798\,t^3 + 186 \times 101{\cdot}22798t^2.$

Adding and reducing, the equation for *equilibrium* (3) becomes

$$t^3 + 186t^2 + 1938{\cdot}6t - 29373{\cdot}24 = 0,$$

whence $t = 8{\cdot}2$ ft. for *equilibrium*.

And the equation for *stability* (4) becomes

$$t^3 + 186t^2 + 1938{\cdot}6t - 71570 = 0,$$

whence $t = 14{\cdot}66$ ft. for *stability*.

In the construction of this particular dome, the values obtained above for t will only apply to four points on the walls which carry the dome; for instead of being built on a circular "drum," this drum is made to cover a square chamber by means of "pendentives," which are so arranged as to throw the thrust principally on the angles of the building, where the values of a and c will become very great, and it is only at the centre of each of the four sides that the above equations can be employed to compare the thrust of the dome with the strength of the walls. At these four points the walls are about 11 ft. thick, so as to be considerably more than sufficient to produce *equilibrium*; and the *coefficient of stability* at these points is 1·374 instead of 2. Had the dome therefore been built on a circular drum of 11 feet thickness, in all probability the edifice would not have stood for any length of time.

Much of the thrust of a dome may be counteracted by means of an iron belt placed round it at the point where the thrust is greatest. This point I have shown to be at 70° from the crown, or 20° from the springing.

June 7, 1866.

The Annual Meeting for the election of Fellows was held this day.

Lieut.-General SABINE, President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Professor Brayley and Mr. Toynbee were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the Lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society:—

John Charles Bucknill, M.D.
Rev. Frederick William Farrar.
William Augustus Guy, M.B.
James Hector, M.D.
John William Kaye, Esq.
Hugo Müller, Ph.D.
Charles Murchison, M.D.
William Henry Perkin, Esq.
The Ven. John Henry Pratt, M.A.
Capt. George Henry Richards, R.N.
Thomas Richardson, Esq., M.A.
William Henry Leighton Russell, Esq.
Rev. William Selwyn, D.D.
Rev. Richard Townsend, M.A.
Henry Watts, B.A.

June 14, 1866.

Lieut.-General SABINE, President, in the Chair.

The Rev. F. W. Farrar, Dr. Charles Murchison, Captain Richards, R.N., Mr. W. H. L. Russell, and Mr. Henry Watts, were admitted into the Society.

Pursuant to notice given at the last Ordinary Meeting, Franz Cornelius Donders, George Friedrich Bernhard Riemann, and Gustav Rose, were balloted for and elected Foreign Members of the Society.

The following papers were read:—

I. "On the Anatomy of the Fovea centralis of the Human Retina." By J. W. Hulke, F.R.C.S., Assistant Surgeon to the Middlesex and Royal London Ophthalmic Hospitals. Communicated by Wm. Bowman, Esq. Received May 26, 1866.

(Abstract).

1. The Fovea centralis is a minute circular pit in the inner surface of the retina, made by the radial divergence of the cone-fibres from a central point, by the thinning and the outward curving of the inner retinal layers towards this point, and by the peripheral location of the outer granules belonging to the central cones.

2. The inner surface of the retina declines in a rapid uniform curve from the edge to the centre of the fovea, and very gradually from the edge towards the ora retinæ; so that the edge of the fovea is the most raised part in the macula lutea, where the retina is thickest, and the centre of the fovea the most depressed part in the macula, where the retina is thinnest.

3. At the centre of the fovea, proceeding from the outer to the inner surface of the retina, we meet with the following structures in succession:—the bacillary layer and the outer limiting membrane, a small quantity of finely areolated connective tissue, the inner granule-layer and the ganglionic layer very attenuated, a thin granular band containing optic nerve-fibres, and the membrana limitans interna.

4. The bacillary layer contains only cones in the fovea, but rods in addition towards the periphery of the macula lutea.

5. The cones and rods in the macula are longer and more slender than at a distance from it. Although the greater slenderness is more apparent in the cones, yet the most slender cones are stouter than the rods.

6. In both cones and rods an inner and an outer segment are discernible, and both segments consist of a sheathing membrane and contents. In the outer segment the contents do not exhibit any indication of structure. The inner segment, where its dimensions permit, contains an "outer granule," and is always produced in the form of a fibre, which connects the cones and rods with the inner layers. These I have called the *primitive cone- and rod-fibres,* collectively *primitive bacillary fibres.*

(Kölliker, in the last edition of his 'Handbuch der Gewebelehre,' restricts to these the term "*Müller's fibres,*" which was originally exclusively, and is still very generally given to the vertically radial connective-tissue fibres first described by H. Müller.)

7. An outer granule is intercalated in each primitive bacillary fibre, 1st, where the inner bacillary segment is too slender to include the granule, and, 2nd, where the segment is associated with a distant granule. This kind of connexion always obtains with the rods, and with the cones at the centre of the fovea.

The outer granules which belong to the rods are not distinguished from those which belong to the cones by any constant characters.

8. The primitive bacillary fibres run obliquely from the outer towards the inner surface of the retina, and radially from the centre of the fovea towards the periphery of the macula lutea.

9. They form a very conspicuous obliquely fibrillated band, lying between the outer and the inner granule-layers, in which the primitive fibres combine in bundles which have a plexiform arrangement. This band corresponds to that band which in the chameleon's retina I called the cone-fibre plexus. Müller and Kölliker call it the intergranule-layer.

10. Between the inner surface of this layer and the inner granule-layer, there is a thin granular band of finely areolated connective tissue, through which the bacillary fibres pass into the inner granule-layer. It is in part derived from the terminal divisions of the vertically radial connective-tissue fibres, and it answers to the band which in the chameleon I termed the intergranule-layer.

11. In the inner granule-layer two kinds of granules are distinguishable, nuclei and nucleated cells. Both are in connexion with the oblique bacillary fibres, which in this situation are exceedingly delicate, and require a high magnifying power and a good section for their demonstration.

12. The connective-tissue fibres (generally known as Müller's radial fibres), which traverse the retina in a vertically radial direction from the membrana limitans interna towards the outer surface, are very conspicuous in the inner layers, where in sections transverse to the direction of the

optic nerve-fibres they are arranged in arcades, which contain the last-named fibres and the ganglion-cells imbedded in a granular matrix of finely woven connective tissue. They are less visible in the outer layers, apparently because many of their terminal divisions lose themselves in the (connective tissue) intergranule-layer; and hence the decussation of the oblique bacillary and the vertically radial connective-tissue fibres within the cone-fibre plexus, so conspicuous in the chameleon, is scarcely noticeable in the human retina.

Deductions.

1. Since the total of the effects of light upon living tissue will be greater as the extent of tissue traversed by it is greater, and since the relative common sensitiveness of a surface varies with the number of distinct sentient elements it contains, it follows that the greater length of the cones and rods, and their greater slenderness, which allows a larger number of them to the superficial unit, are in harmony with the greater sensitiveness of the retina at the macula lutea. Inasmuch, however, as the foveal cones are stouter than the rods, a superficial unit at the centre of the fovea contains fewer sentient (*i.e.* percipient) elements than the same unit near the periphery of the macula lutea; and on this ground the sensitiveness of the retina at the fovea should be less than that of the retina near the periphery of the macula. On the other hand, the extreme thinness of the inner layers of the retina at the centre of the fovea, places the bacillary layer here most favourably for receiving incident light.

2. The division of the rods and cones into an outer and an inner segment is natural. The facts in support of this are, the presence of the division in perfectly fresh specimens; its sharpness and constant occurrence at a definite place; the constantly rectilinear figure of the outer, and the curvilinear figure of the inner segment; the different refractive powers of the segments; and their different behaviour towards staining and chemical solutions.

3. From these structural differences it is a fair inference that the segments have different physiological meanings.

The higher refractive power, straight sides, and slender cylindrical or prismatic figure of the outer segment may be adaptations for confining within the segment light incident upon its ends, and for preventing the lateral escape of light through the sides of the segment into neighbouring cones and rods. These considerations incline me to adopt the opinion that this segment has an optical function, an opinion which derives further support from the fact that, in those animals in which the segment is so wide a cylinder that a ray might be incident upon the inner surface of its sides at a small enough angle not to be reflected but to pass out, the segment is insulated by a sheath of black pigment.

The inner segments of the cones and rods are the specially modified peripheral terminations of the optic nerve-fibres; and at their junction with the outer segment the conversion of light into nerve-force may take place.

4. The outer granules being the nuclei of the inner cone- and rod-segments, probably maintain the integrity of these as living tissues, and are not directly concerned in their specific functions as organs of perception.

5. The primitive bacillary fibres are the link by which the cones and rods communicate through the inner granules and ganglion-cells with the optic nerve-fibres.

6. The smaller inner granules are nuclei of the oblique bacillary fibres in the inner granule-layer; or they may be small bipolar ganglion-cells, and act specifically on the forces transmitted through the oblique fibres from the cones and rods. The larger inner granules not being distinguishable by any definite structural characters from the smaller cells of the ganglionic layer, may agree with these latter cells in function.

7. Since the ganglion-cells (of the ganglionic layer) are fewer than the inner granules, and much fewer than the cones and rods, and since it is probable that these latter communicate with the optic nerve-fibres only through the ganglion-cells, it follows that one ganglion-cell probably is in correspondence with more than one inner granule and with several cones and rods. From this it is not an improbable conjecture that the cones and rods are disposed in groups, each of which is represented by one or more ganglion-cells thef unction of which is to connect or coordinate the individual action of the separate bacillary elements in their groups in a manner analogous to that attributed to the ganglion-cells of the spinal cord by Van der Kolk*.

8. There is a close general resemblance between the human fovea and that of the chameleon†.

II. Second Memoir "On Plane Stigmatics." By Alexander J. Ellis, F.R.S., F.C.P.S. Received May 26, 1866.

(Abstract.)

Let there be two groups of points upon a plane, termed, for distinction, *indices* and *stigmata* respectively, bearing such relations to each other that any one index determines the position of n stigmata, and any one stigma determines the position of m indices. The theory of these relations between indices and stigmata constitutes *plane stigmatics.* Each related *pair* of index X and stigma Y constitutes a *stigmatic point,* henceforth written "the s. point (xy)." The straight lines joining any index with each of its corresponding stigmata are termed *ordinates.* If, when

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* I have an impression that I have seen this in a German author, but have not been able to find the passage again.

† H. Müller, "Ueber das Auge des Chamäleon," Wurzb. naturw. Zeitschr. Bd. iii. S. 36.

the index moves upon a straight line, the ordinate remains parallel to some other straight line, the relation between index and stigma is that expressed by the relation between abscissa and ordinate in the coordinate geometry of Descartes. When only one index corresponds to one stigma and conversely, and both indices and stigmata lie always on one and the same straight line, or the indices upon one and the stigmata upon another, the relations between indices and stigmata are those between homologous points in the homographic geometry of Chasles.

The general expression of the *stigmatic relation* is obtained by a generalization of Chasles's fundamental lemma in his theory of characteristics (*Comptes Rendus,* June 27, 1864, vol. lviii. p. 1175), clinants being substituted for scalars*. It results that in certain forms of the *law of coordination,* which "coordinates" the stigmata with the indices, there may be *solitary indices* which have no corresponding stigmata, and *solitary stigmata* which have no corresponding indices, and also *double points* in which the index coincides with its stigma (76)†. The particular case in which one index corresponds to one stigma and conversely, and no solitary index or stigma occurs, is termed a *stigmatic line* (henceforth written "s. line"), because the Cartesian case is that of a Cartesian straight line in ordinary coordinate geometry, but in the general s. line the figures described by index and stigma may be any directly similar plane figures (77). The investigation of this particular case occupies almost the whole of the *Introductory Memoir.* When one index corresponds to one stigma and conversely, but there is one solitary index and one solitary stigma, we have s. *homography,* provided the solitary index is distinct from the solitary stigma (79), and s. *involution* when the solitary index coincides with the solitary stigma (78), so called because they generalize the relations treated of under these names by Chasles.

When the relations between index and stigma are expressed by an equation of the second order, the s. curves are termed s. *conics,* because they include Cartesian conics as a particular case (80). Generally two stigmata correspond to each index, and two indices to each stigma, and there is no solitary index or stigma.

Two s. curves *intersect* when they have a common ordinate, and there-

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* If OI be any fixed axis of reference, and AB any line upon a fixed plane passing through OI, we may consider that a line equal in magnitude and direction to AB can be formed from OI by the operation of turning OI through the $\angle$ (OI, AB), and altering its length in the ratio of the length of OI to that of AB. When all such lines AB lie upon the same plane, this operation is termed a *clinant* and represented by *ab* to distinguish it from the line AB. If the lines AB lay on different planes, the operation would be in general a *quaternion.* But if AB is parallel to OI, whatever be the plane on which it lies, the operation *ab* is termed a scalar. Clinants and scalars obey the laws of ordinary commutative algebra. Quaternions do not.

† The figures in parentheses refer to the articles of this "Second Memoir," which are numbered consecutively to those in the *Introductory Memoir* (Proceedings, April 6, 1865, vol. xiv. p. 176).

fore a common s. point. They *touch* when a slight alteration in the constants of one equation will make one of their common s. points into several, having their stigmata very near. When they have no common ordinate or s. point, they are *parallel* or *asymptotic,* according as the distances between the stigmata corresponding to a common index remain unchanged or, under certain circumstances, diminish infinitely (81).

It has been shown in the *Introductory Memoir* that a s. line can be determined by two stigmata corresponding to known indices, or by two points called the *direction-point* and *original stigma**. Taking either the two first-named or the two last-named points as the index and stigma in a stigmatic relation, we are able to make its expression denote a *linear relation,* which leads to the conception of enveloped s. curves, and therefore of *classes* of s. curves (82, 83).

In the above relations two ordinary geometrical points ("g. points") in a plane form the index and stigma. If we now take two s. points (that is, two relations of index to stigma), terming one *primary* and the other *secondary,* we are able, by means of two equations, to express a relation between these s. points, so that one primary shall correspond to n secondaries, and one secondary to m primaries. This is the *bipunctual relation* (84), and includes the whole subject of related s. curves, of which related Cartesian curves form a particular case. The most important and elementary cases are, first, those in which the related s. points lie respectively on known s. lines, s. involutions, s. homographies, or s. circles, and there is a stigmatic relation between the stigmata considered as forming two groups, one of indices and one of stigmata; and secondly, those in which one primary s. point corresponds to one secondary and conversely. In the latter case the relations between one index and stigma may be inferred from known relations between another index and stigma, or from the same index and a new stigma, or, as is most usual, from the same stigma and a new index. Hence follows the general theory of change of coordination (85). Homographic and homologic s. figures (86) are another example, in which one primary corresponds to one secondary s. point and conversely; but there is generally one *solitary s. line* in each s. figure such that no s. point taken upon it will have any corresponding s. point in the other figure.

Corresponding to the bipunctual relation there is a *bilinear relation* (87) in which one primary s. line corresponds to several secondary s. lines, and one secondary to several primaries, by means of the elements chosen to represent linear relations. To this belongs the *biradial relation* (87 *b*), where two pencils of s. rays, being drawn from known s. points, are determined generally by s. points having a bipunctual relation, and exceptionally

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* The original stigma is the stigma of which the origin is the index. If through the s. point (*ii*) (that is, a s. point of which both index and stigma are the point I at the further extremity of the axis of reference OI) a s. line be drawn s. parallel to any given s. line, the original stigma of this parallel s. line is the direction-point of the given s. line. Direction-points are of great use in explaining "imaginary" lines and "imaginary" angles.

by their direction-points only, so that the relation between the rays can be expressed by a stigmatic relation between the direction-points considered as indices and stigmata. When this s. relation between the direction-points is a s. homography, we have pencils of ***homographic s. rays.*** The *reciprocal relation,* in which a s. point corresponds to several s. lines, and a s. line to several s. points, follows immediately (88).

Finally, the *multindicial relation* (89) shows how several indices may be made to correspond as one group to a single stigma or a group of stigmata, and thus leads to the geometrical expression of the algebraical theories of several independent variables. The means by which a theory of *solid stigmatics,* where the indices and stigmata are taken anywhere in space, may be expressed by quaternion equations is briefly pointed out (89 *e*).

The systematic explanation and precise expression for all these relations, together with some convenient notations relating especially to s. angles [*] (90), and to anharmonic ratios (91), occupy the first part of the present memoir. The remaining two divisions are devoted to a somewhat fuller consideration of s. homography (including s. involution) and s. conics.

It results (92) from the conception of *s. involution,* already given, that if S be the coincident solitary index and solitary stigma, and X, Y a corresponding pair of index and stigma, that is, if (xy) is a s. point on the s. involution, there will be two points, E, F (called *double stigmata*), in the same straight line with S, in which index and stigma coincide, forming the *double points* (ee), (ff), and then $se^2=sf^2=sx\,.\,sy$, which implies that $\angle XSE=\angle ESY$, and $\angle XSF=\angle FSY$, and also that each of the lengths of SE, SF is a mean proportional between the lengths of SX and SY. It is readily shown that the two cases of involution of points in a line, usually acknowledged, are but the particular cases of XY lying on the line EF, or on a line through S perpendicular to EF. In the former case the double points lying on the line containing XY were readily found, and hence termed "real;" in the latter, as the method of investigation did not suffice to determine the double points, they were termed "imaginary." If A, B, C, D be any four indices, and A′, B′, C′, D′ their corresponding stigmata in a s. involution, then

$$\frac{ab\,.\,cd}{ad\,.\,cb}=\frac{a'b'\,.\,c'd'}{a'd'\,.\,c'b'},\text{ and }\frac{ab\,.\,cc'}{ac'\,.\,cb}=\frac{a'b'\,.\,c'c}{a'c\,.\,c'b'};$$

that is, *the anharmonic ratio*[†] *of any four indices is the same as that of*

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[*] If A, B be the direction-points of two s. lines, and O the origin, then $\frac{oa-ob}{1-oa\,.\,ob}$ is defined to be the s. tangent of the s. angle between these s. lines, and written tan *aib*.

[†] The equality of *anharmonic* ratios is here taken to imply, not only the same relation of magnitude which would be included in the term if the four points A, B, C, D lay upon one straight line, and their corresponding points A′, B′, C′, D′ upon another straight line, but also an angular relation which is expressed by the notation of clinants, and which in the first of these equations is $\angle BAD+\angle DCB=\angle B'A'D'+\angle D'C'B'$. It is readily seen that this relation reduces to the usual relation of direction when the points lie on straight lines.

their corresponding four stigmata, and *the anharmonic ratio of any three indices and stigma is the same as that of the corresponding three stigmata and index in a s. involution.* If the index move on the *characteristic circle* whose centre is S and radius SE, the corresponding stigma moves in the opposite segment of the same circle, and in an opposite direction of rotation. Generally if the index move on any circle passing through E, F, the stigma moves on the segment of the same circle lying on the other side of EF, and in the opposite direction of rotation. If the index moves on a straight line passing through S, the stigma moves on another straight line also passing through S. If the index move on a straight line not passing through S, the stigma moves on a circle passing through S and conversely. If the index moves on a circle not passing through S, the stigma moves on another circle also not passing through S, and in an opposite direction of rotation.

If the whole group of stigmata be removed without disturbing their mutual relations, and so that the solitary index S no longer corresponds with the solitary stigma Z′, a s. *homography* results (93), in which the relations of index and stigma may be deduced from those in a s. involution. These relations are expressed by the equation $sx\,.\,z'y = sa\,.\,z'a'$, where (xy), (aa') are s. points in the s. homography. When three s. points are given, the solitary index S, and solitary stigma Z′, and double stigmata E, F can be determined by an elementary geometrical construction in all cases. These double stigmata possess important *angular properties* with respect to the indices and stigmata of the system (94). Let (aa'), (bb'), (cc') be any three s. points in a s. homography. Then if ABC, A′B′C′ are straight lines (in which case the former contains S, and the latter Z′),

$$\tan AEA' = \tan BEB';$$

that is, *if two intersecting straight lines* EA, EA′ *revolve about their point of intersection* E, *and cut two given straight lines* AB, A′B′, *determining an index* A *upon one, and a stigma* A′ *upon the other, so that the ordinate* AA′ *is seen under an angle whose tangent is constant**, *the s. points will form part of a s. homography, of which the given point* E *is one double stigma, the solitary index* S *lying upon the index line* AB, *and the solitary stigma* Z′ *upon the stigma line* A′B′. *If* O *be the middle point of* SZ′ *and* F *the other double stigma*, FO=OE. If the two lines AB, A′B′ coalesce, then *the two points* E, F, *from which the ordinates* AA′, BB′, CC′ *of three or more s. points, of which both indices and stigmata lie upon one straight line, are seen under an angle whose tangent is constant, are the double stigmata of the s. homography determined by these three s. points.* This shows the nature of the points named in G. S. 171†, and completes it, so far as "real rays" are concerned. It is further generalized hereafter. The

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* It is customary, but erroneous, to say in such cases that the angles are equal and in the same direction. It is evident that one angle may be the supplement of another measured in the opposite direction.

† Chasles's *Géométrie Supérieure* and *Sections Coniquee* are referred to by the letters G. S. and S. C., followed by the number of the article.

two lines AB, A′B′ may be so placed that the s. homography has only one double stigma O coinciding with the middle point of SZ′, and O will then be the centre of a circle of which AB, A′B′ are tangents; the ordinates of the s. points in the s. homography (each of which contains an index and a stigma) will then be tangents to the circle of which the centre (being a double stigma) will also contain an index and stigma. Hence is immediately obtained the relation in G. S. 664, and an explanation of the remark there made, that "le centre du cercle tient lieu en quelque sorte d'une tangente."

The tangent of the angle subtended by an ordinate at the double stigma may be constantly zero, that is, the ordinates may converge to the double stigma. Then, *if through any point* E *two straight lines be drawn, and through any other point* F *rays be drawn intersecting the first line in a series of indices* A, B, C, *and the second in a series of stigmata* A′, B′, C′ *respectively, the resulting s. points* (aa′), (bb′), (cc′) *will form part of a s. homography of which* E, F *are the double stigmata.* This shows the nature of the points of issue and intersection in the fundamental proposition of anharmonic ratios, G. S. 14, and completes it so far as "real" rays are concerned. It will be further generalized hereafter.

Hence ABB′A′ is a quadrilateral, of which E, F are the intersections of opposite sides. Consequently *if* A, B′ *and* A′, B *are opposite vertices of a quadrilateral, and* E, F *the intersections of the opposite sides* (ab′), (a′b), (ef) *are s. points in a s. involution.* This completes G. S. 350, for "real" rays *.

The above considerations determine all the relations of s. homography, but do not suffice to give an explanation of those "imaginary" rays which play so important a part in Chasles's geometry. The requisite theory is very simply obtained by considering the direction-points of two sets of (primary and secondary) s. rays as indices and stigmata in a s. homography or s. involution (95). In particular it is shown that *if the s. tangent of the s. angle between two primary s. rays is equal to that between the two corresponding secondary s. rays, there are two pairs of parallel s. rays, which are parallel to the asymptotes of a s. circle*; and all the other relations for pencils of ordinary homographic rays are similarly generalized. In this case, if the direction-points of all the rays lie upon the same straight line passing through the origin, the s. rays correspond to those usually termed "real;" and if any do not, the s. rays belong to the class usually termed "imaginary," and were supposed not to have any geometrical existence.

When the two s. points of issue of the two pencils are distinct, there will be generally two primary rays which are parallel to their secondaries;

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* Some of these *results* of s. involution and s. homography have been previously obtained by Möbius (cited in the *Introductory Memoir*, 59 *t*); but his method and conception are perfectly different, and do not admit of the extension which the present propositions immediately receive, to include the case of "imaginary" rays, a subject never previously treated as strictly geometrical.

and if the two s. points of issue coincide (96), these parallel rays will become double rays. Both are found by finding the double points of the homography of direction-points. When the double rays are s. perpendicular*, the sum or difference of the s. tangents of the two s. angles made by the primary and its corresponding secondary with the double rays will be zero. If the direction-points form a s. involution, the s. rays form an involution, and there will be always two s. rays s. perpendicular and easily constructed.

The stigmata of the s. points in which the primary and secondary s. rays of two pencils, making the s. tangent of the s. angle between such rays constant, intersect two given s. lines, are indices and stigmata respectively of s. points in a s. homography, of which the stigma of the point of issue is a double stigma (97). Whence it follows that, *in any s. homography, if the indices be considered as the stigmata of s. points on a known s. line, and the stigmata as the stigmata of s. points on some other known s. line, and a second index be given to one of the double stigmata so as to form a s. point which shall lie upon neither of those given s. lines, and pairs of s. rays be drawn from the s. point thus formed to pairs of corresponding s. points in the two s. lines, the s. tangent of the s. angle between any two corresponding rays will be constant.*

Supposing each primary s. ray to coincide with its secondary, and OI to be the axis of reference (98), *the stigma of the s. point of issue of a pencil of s. rays, and the stigmata of the intersection of these s. rays with a given s. line, form the indices, and the extremity* I *of the axis of reference* OI, *and the direction-points of the same s. rays, respectively form the stigmata of a series of s. points in a s. homography.* Whence it follows that *the anharmonic ratio of any four stigmata of intersection is the same as that of the four corresponding direction-points.* This completely generalizes the fundamental proposition of anharmonic ratios, including the case where all the lines are "imaginary," and can be made the starting-point of a series of propositions precisely analogous to those in G. S., but with a much wider signification. Thus the property of the complete quadrilateral shows, on being generalized, that *if* E, A, B, C, D, E′, A′, B′ *be any eight points upon a plane, and two other points* C′, D′ *be assumed so that the triangles* E′A′C′, E′B′D′ *shall be directly similar to the triangles* EAC, EBD *respectively, and then two additional points* F′, F *be determined so that the triangles* F′A′B′, F′C′D′ *shall be directly similar to the triangles* FAB, FCD, *the point* F *will be constant, if the first five points* E, A, B, C, D *remain unchanged, whatever the next three,* E′, A′, B′, *may be, and will lie so that if* SM *be drawn bisecting the angles* ASD, BSC *and in length a mean proportional between the lengths of* SA, SD, *and also of* SB, SC, *then* SM *will also bisect the angle* ESF, *and be in length a mean proportional between* SE *and* SF.

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* If A and B are the direction-points of two s. lines, the latter are stigmatically perpendicular when $oa \, . \, ob = 1$, and the direction-points are then harmonically situate with respect to I, I′, where OI′ = IO.

When pairs of separate s. rays cutting one s. line are considered, it is shown (99) that *if from any point pairs of ordinary rays be drawn which are the two limbs of a constant angle cutting any straight line in points forming the indices and stigmata of s. points in a s. homography of which the point of issue is a double stigma, and we furnish all the points lying on the given straight line, and the point of issue, with Cartesian indices, and hence consider all the s. rays as Cartesian, they will form part of an homography of s. rays, of which the double rays drawn from the Cartesian point of issue will be two non-Cartesian s. lines parallel to the asymptotes of a s. circle, and therefore independent of the magnitude of the constant angle.* This furnishes a complete geometrical explanation of G. S. 171, 172, 181, 651, S. C. 293, &c.

Generally the whole of the propositions in G. S. respecting the homography of ordinary points and rays may be transferred to the homography of the stigmata of s. points and of s. rays, forming a series of propositions of much greater generality, in which all the s. points and s. rays will be perfectly real, and can be constructed by means of elementary geometry.

The last division of this memoir is devoted to *s. conics*, which embrace the ordinary Cartesian conics as particular cases, together with all their so-called "imaginary" points and lines. The general equation is assumed (100) in the form

$$o\alpha \,.\, ox^2 + 2o\beta \,.\, ox \,.\, oy + o\gamma \,.\, oy^2 + 2o\delta \,.\, ox + 2o\epsilon \,.\, oy + o\zeta = 0\,; \quad . \quad (a)$$

and it is shown that if $o\beta^2 = o\alpha \,.\, o\gamma$ the equation may represent a single s. line, or two parallel s. lines, or a s. *acentric*, which, by changing the origin and index, may be made to depend on the equation $x'y^2 = 4s'o' \,.\, o'x'$; but if $o\beta^2$ is not $= o\alpha \,.\, o\gamma$, the equation may represent two intersecting s. lines, or a s. *centric*—that is, a s. curve which when cut by any s. line passing through a given s. point (the s. *centre*) has the index and stigma of that s. point in the middle of the lines joining the two indices and two stigmata of the s. points of intersection respectively. It is also shown that if the origin and index are changed, the s. centric may be represented by the equations

$$\frac{o'x'^2}{o'e^2} + \frac{x'y^2}{o'g^2} = 1, \text{ or } o'x'' \,.\, x''y = o'h^2, \quad . \quad . \quad . \quad . \quad (b)$$

in which case the s. lines $o'x' = 0$, $x'y = 0$ are indeterminate, but form *pairs of s. rays in involution*, which are all *conjugate diametrics**, with the exception of the double rays $o'x'' = 0$, $x''y = 0$, which are *asymptotes*. In the particular case that $o\alpha = 0$, the general equation can, by changing origin and index, be made to depend on the equation $o'x'^2 - x'y^2 = o'e^2$. Such curves are called s. *cyclics*. In the cyclic proper, the values of $o\beta$, $o\gamma$ may be any whatever; in the s. *circle* $o\beta + o\gamma = 0$, in which case *all the conjugate diametrics are s. perpendicular, and the direction-points of the asy*

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* A *diametric* is a *s. line* passing through the s. centre and intersecting the s. curve in two s. points. A *diameter* is the *straight line* joining the two stigmata of these last s. points. A *radius* is half a diameter. These distinctions are important.

ptotes are I, I′. But if both $o\alpha$ and $o\chi$ become $=0$, the s. cyclic is a s. homography or s. involution.

The general homographic properties of all s. conics are then deduced (101). First, *a s. conic is the locus of the s. points of intersection of the primary and secondary s. rays of two pencils of homographic s. rays with different s. points of issue, and passes through these points,* which, with slight modifications, holds when a band of parallel s. lines is substituted for either or both of the pencils. Secondly, *the anharmonic ratio of four s. chords in a s. conic drawn from a variable s. point to four fixed s. points, bears a constant ratio to the anharmonic ratio of the stigmata of those four fixed s. points; and this constant ratio is one of equality when the s. conic is a s. circle.* In order to establish the latter part of this proposition, it is shown that *in a s. circle, the s. tangent of the s. angle formed by any two s. rays drawn from a variable s. point to any two fixed s. points in the s. circle is constant* (which is a generalization of Euc. iii. 21), whence is deduced the condition that four s. points should lie upon a s. circle. The classification of s. centrics is founded (102) on the first of the equations (b). After showing how the two stigmata can be generally found from the index, the name of the s. centric is made to depend on the name of the locus of the stigma when the index describes the principal diameter in whole or in part. This locus is called the *characteristic* curve. According as $o'e^2 \div o'g^2$ is a negative scalar, -1, a positive scalar, $+1$, or any clinant, the s. centric is a s. *ellipse, circle, hyperbola, equiradial* (equilateral hyperbola), or *hyperellipse* (for which the characteristic is a confocal ellipse and hyperbola). The mode of finding the two indices corresponding to each stigma is then given (103), and the circumstances under which a s. line and a s. centric may have common points investigated (104). The mode of determining s. points of intersection, when there is no intersection in Cartesian geometry ("imaginary" intersections) is illustrated, and shown to lead to the same results as the homographic method.

The *radical axis* (105) *is* shown to be *the Cartesian portion of a s. line which is the common s. chord of a series of s. circles, so situated that the extremities of their principal diameters are the indices and stigmata of a s. involution,* of which the "real" and "imaginary" circles of Chasles are two particular cases.

The properties of *asymptotes,* and the nature of those in a s. ellipse, are then considered (106), and their geometrical properties examined. *Conjugate Diametrics* and their relations to the asymptotes are next investigated, (107). In especial it is shown that if $(u'e')$, $(v'g')$ are two of the s. points in which two conjugate diametrics intersect a s. centric, and if the s. line drawn from $(u'e')$ s. perpendicular to the diametric connecting $(o'o')$ and $(v'g')$ meet the latter in a s. point whose stigma is D, we shall have

$$o'u' \,.\, u'e' + o'v' \,.\, v'g' = 0, \quad \ldots \ldots \ldots \quad (c)$$

$$o'u'^2 + o'v'^2 = o'e'^2, \qquad u'e'^2 + v'g'^2 = o'g^2, \quad \ldots \ldots \quad (d)$$

whence $$o'e'^2 + o'g'^2 = o'e^2 + o'\,g^2, \quad \ldots \ldots \ldots \quad (e)$$

and $$de' \,.\, (o'v - v'g') = \pm o'e \,.\, o'g \quad \ldots \ldots \ldots \quad (f)$$

From these relations, which are much more general than any hitherto enunciated, the ordinary relations in Cartesian geometry respecting the lengths of conjugate diameters and the areas they contain are readily deduced.

By referring s. centrics to conjugate diametrics as new coordinate axes (108), a simple general method for constructing the intersections of a s. line with a s. centric, and for drawing two s. tangents (109) from any s. point (except the s. centre) is obtained. This is illustrated by actual geometrical constructions corresponding to "imaginary" cases in Cartesian geometry. It is also shown how the *chord of contact* or *polar* may have a Cartesian portion when both the s. points of contact are non-Cartesian and hence "imaginary."

It is now possible (110) to demonstrate the fundamental anharmonic properties of tangents and asymptotes to a s. centric. *If through four s. points in a s. centric, four tangents be drawn, intersecting any fifth tangent, and also four s. chords meeting in any fifth s. point in the s. centric, the anharmonic ratio of the four stigmata of the s. points of intersection of the four tangents with the fifth will be equal to the anharmonic ratio of the four s. chords* (which generalizes S. C. 2, 5, the demonstration being here direct and therefore widely differing from Chasles's), whence, *if two fixed tangents are cut by a moveable tangent the stigmata of its s. points of intersection with them will be the indices and stigmata of s. points in a s. homography.* It is now possible to generalize the whole of Chasles's S. C., and make the theories applicable, *mutatis mutandis,* to s. conics.

The *s. foci* of a s. centric (111) *are defined as the s. points of intersection of any two tangents which are parallel to the asymptotes of a s. circle.* Hence it is shown that there are *four* s. foci to a s. centric; so that if $o's^2 = o'e^2 + o'g^2 = o'z^2$, the *two principal* s. foci are (ss) (zz), and the *two transverse* s. foci are $(o's)$, $(o'z)$. When the s. centric is Cartesian, the two principal s. foci are those usually recognized, and the only two recognized by Chasles, S. C. 275, 294. The other two transverse s. foci have, however, been recognized as "imaginary" foci by Plücker (*System der a. Geometrie,* p. 106, l. 6), and by Salmon (*Conics,* 4th ed. p. 242).

Besides the usual properties of the focal stigmata in Cartesian curves, the following entirely new correlative properties of the principal and transverse s. foci are demonstrated. *Let there be two conjugate diametrics, and through any s. point in one draw a s. line parallel to the other, and let the stigmata of the s. points of intersection of the conjugate diametrics and the chord, with the s. centric, be respectively* E′, F′; G′, H′; Y_1, Y_2. *From* E′, F′ *draw straight lines* PARALLEL *to the bisectors of the angles* Y_1SY_2, Y_1ZY_2 *respectively, forming two sides of a triangle of which* E′F′ *is the base. Then the lengths of these sides will be mean proportionals between the lengths of* SY_1, SY_2, *and* ZY_1, ZY_2 *respectively.* This is a generalization of the usual property that the sum or difference of the focal distances is the axis major in the ellipse or hyperbola. *Preserving the same notation, take* X$'$ *the middle point of* Y_1Y_2, *set off* $X'X_3=X_4X'$ $=O'S$, *and make* $X_3Y''=Y_2X_3$, *and* $X_4Y'''=Y_2X_4$. *From the extremities* G$'$,H$'$ *of the diameter* CONJUGATE *to that used in the last proposition, draw straight lines* PERPENDICULAR *to the bisectors of the angles* $Y''SY_2$,$Y'''ZY_2$ *respectively, forming the two sides of a triangle of which* G$'$H$'$ *is the base. Then the lengths of these sides will be mean proportionals between the lengths of* SY$''$, SY_2 *and* ZY$'''$, ZY_2 *respectively.* This is the correlative property of the transverse s. foci, which has no Cartesian analogue. The deduction and expression of these properties is extremely simple, and they are illustrated by a geometrical construction. Finally, *the s. tangent of the s. angle which the s. rays drawn from any s. point in a s. centric to the two principal or the two transverse s. foci make with the s. normal at the s. point are equal and opposite,* which is a generalization, *for both* pairs of s. foci, of the property from which the ordinary foci derived their name.

The following problems are then solved (112). First. *Given two conjugate radii* O$'$E$'$, O$'$G$'$ *to find the principal and transverse axes of the Cartesian centric.* The focal stigmata S, Z are found from (*e*) thus: from E$'$ draw E$'$M, E$'$M$'$ perpendicular to O$'$G$'$, and equal to it in length; join O$'$M, O$'$M$'$, bisect the angle MO$'$M$'$ by O$'$S, O$'$Z, which are in length mean proportionals between O$'$M, O$'$M$'$. Then there are three solutions, giving an ellipse or two hyperbolas, according as both or only one of the conjugate radii meet the characteristic curve of the Cartesian centric. Second. *Given any three s. points representing the s. centre, and the s. points of intersection of two conjugate diametrics with the s. centric, to find its principal and transverse axes.* Third. *Given three s. points, to find the axis of a s. circle passing through them.* Fourth. *Given any five s. points, to determine whether they lie on a s. centric; and if so, to find its principal and transverse s. axes.*

Equation (*e*) is then applied (113) to obtain a more general conception of confocal s. centrics, and Carnot's theory of transversals (as generalized in the *Introductory Memoir*) is applied to finding a new expression for the s. radius of curvature, applicable for any s. point of contact. The ordinary expression gives length and not direction, and applies only to Cartesian points. Similar investigations are very briefly given for s. *acentrics* (115–118).

In these memoirs on Plane Stigmatics, the writer has endeavoured to confine himself strictly to such a development of his theory as would suffice to establish, with geometrical severity, the following results, which he believes to be entirely new, and of great scientific importance to mathematics.

First. *The problem of the geometrical signification of imaginaries in plane geometry is completely solved; so that the terms "real" and "imaginary" are no longer required, and an unbroken agreement exists between plane geometry and ordinary commutative algebra.*

Imaginaries arise whenever an algebra is used which is more general than the geometry to which it is referred. Ordinary commutative algebra is an algebra of clinants, which corresponds to a geometry where not only the relative lengths, but the relative angular positions of straight lines are regarded. Now the geometry hitherto associated with it has been scalar, that is, has considered relative lengths, but only identity or opposition of direction. Thus in Cartesian geometry the abscissæ and ordinates were necessarily supposed to be parallel to given lines; and in homographic geometry, where the ordinates were not taken into account, their extremities were supposed to lie on one or two given lines. But the algebra employed in either case took no notice of these restrictions, and hence led to results which could not be interpreted—that is, "imaginaries." The solution of the problem of imaginaries has, therefore, consisted in that stigmatic conception which removes these restrictions, freely introduces relative angular position, and makes the geometry coextensive with the algebra. In the course of these memoirs every fundamental case in which imaginaries occur has been separately examined and shown to be fully explained by this conception.

Second. *The coordinate geometry of Descartes, and the homographic geometry of Chasles, are only particular cases of the writer's stigmatic geometry, identical in nature, and expressible by identical equations*; for both Cartesian and homographic geometry consist in the relation of index to stigma with various restrictions, which may or may not be regarded in stigmatic geometry.

Third. *The general theories of plane curves, as treated by ordinary coordinate geometry, by the systems of coordination introduced by Plücker, or the modes of investigation more recently developed by Chasles, and all the theories derivable from these, hold,* IN THEIR INTEGRITY, *for stigmatic curves* ALONE.

For in all these theories clinant algebra is associated with scalar geometry, for which the stigmatic conception enables us to substitute that clinant geometry which perfectly agrees with the algebra employed; and the fundamental propositions of these theories have been extended accordingly in the course of these memoirs.

The instrument by which the writer has been enabled to bring these investigations to a successful issue, has proved so serviceable and manageable, embracing old and new properties in a single equation, often rendering a general investigation more easy to conduct than the former special researches, and keeping the geometrical operations indicated clearly before the mind, that he would add as a subsidiary result of his theory:—

Fourth. *A calculus and a notation have been invented and developed, which enable magnitude and direction upon a plane to be expressed by a single symbol, obeying the laws of ordinary algebra, closely resembling in appearance (not in theory) the notation employed by Chasles to represent magnitude and direction upon a straight line, and as easy of manipulation.*

III. "Fundamental Views regarding Mechanics." By Professor Julius Plücker, of Bonn, For. Mem. R.S. Received May 29, 1866.

(Abstract.)

Being encouraged by the friendly interest expressed by English geometricians, I have resumed my former researches, which had been entirely abandoned by me since 1846. While the details had escaped from my memory, two leading questions have remained dormant in my mind. The first question was to introduce right lines as elements of space, instead of points and planes, hitherto employed; the second question, to connect, in mechanics, both translatory and rotatory movements by a principle in geometry analogous to that of reciprocity. I proposed a solution of the first question in the geometrical paper presented to the Royal Society. I met a solution of the second question, which in vain I sought for in Poinsot's ingenious theory of coupled forces, by pursuing the geometrical way. The indications regarding complexes of forces, given at the end of the "Additional Notes," involves it. I now take the liberty of presenting a new paper, intended to give to these indications the developments they demand, reserving for another communication a succinct abstract of the curious properties of complexes of right lines represented by equations of the *second* degree, and the simple analytical way of deriving them.

1. We usually represent a force geometrically by a limited line, *i. e.* by means of two points (x', y', z') and (x, y, z), one of which (x', y', z') is the point acted upon by the force, while the right line passing through both points indicates its direction, and the distance between the points its intensity. We may regard the six quantities

$$x-x', \quad y-y', \quad z-z', \quad yz'-y'z, \quad zx'-z'x, \quad xy'-x'y \quad \ldots\ldots \quad (1)$$

as the *six coordinates of the force.* The six coordinates of a force represent its three projections on the three axes of coordinates OX, OY, OZ, and its three moments with regard to the same axes. Accordingly we may, as far as we do not regard the point acted upon by the force, replace its coordinates by

$$X, \quad Y, \quad Z, \quad L, \quad M, \quad N \quad \ldots\ldots\ldots\ldots\ldots \quad (2)$$

in admitting the equation of condition

$$LX+MY+NZ=0, \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

which indicates that the axis of the resulting moment is perpendicular to the direction of the force. In making use of the primitive coordinates this condition is involved in the form given to them.

When the last condition is not satisfied, the coordinates X, Y, Z, L, M, N represent no longer a mere force; they are the coordinates of what I proposed to call a *dyname.*

In the general case such a dyname results when any numbers of given forces act upon any given points. Here the six coordinates of the dyname

are the sums of the six coordinates of the given forces (x', y', z', x, y, z). If between the six sums thus obtained,

$$\left.\begin{array}{lll} \Sigma(x-x'), & \Sigma(y-y'), & \Sigma(z-z'), \\ \Sigma(yz'-y'z), & \Sigma(zx'-z'x), & \Sigma(xy'-x'y), \end{array}\right\} \ldots\ldots\ldots \quad (4)$$

an equation analogous to (3) takes place, there is a resulting force. In the case of equilibrium the six sums become equal to zero.

2. In quite an analogous way as we have determined an ordinary force by means of two points in space, one of which is the point acted upon, we may represent a rotation, or the *rotatory force* producing it, by means of two planes,

$$t'x+u'y+v'z=1, \quad tx+uy+vz=1,$$

the coordinates of which are t', u', v' and t, u, v, one of the two planes (t', u', v') being the plane acted upon. The right line along which both planes meet is the axis of rotation. The plane acted upon may in a double way turn round the axis of rotation in order to coincide with the second plane (t, u, v); but there is no ambiguity in admitting that during the rotation the rotating plane does not pass through the origin, and consequently its coordinates do not become infinite.

Let us regard the six quantities

$$t-t', \quad u-u', \quad v-v', \quad uv'-u'v, \quad vt'-v't, \quad tu'-t'u \ldots\ldots \quad (5)$$

as the six coordinates of the rotatory force. As far as we do not regard the plane acted upon, we may replace them by

$$\mathfrak{X}, \quad \mathfrak{Y}, \quad \mathfrak{Z}, \quad \mathfrak{L}, \quad \mathfrak{M}, \quad \mathfrak{N} \ldots\ldots\ldots\ldots\ldots \quad (6)$$

in admitting the equation of condition,

$$\mathfrak{L}\mathfrak{X}+\mathfrak{M}\mathfrak{Y}+\mathfrak{N}\mathfrak{Z}=0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (7)$$

(For the geometrical signification of these new coordinates I refer to the original paper.)

When the last equation of condition is not satisfied, the coordinates $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$, $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ no longer represent a mere rotatory force; they are the six independent coordinates of what I called a *rotatory dyname*.

In the general case, such a rotatory dyname results when any number of given rotatory forces acts on any given planes. Here the six coordinates of the resulting rotatory dyname are the six sums of the six coordinates of the given rotatory forces.

$$\left.\begin{array}{lll} \Sigma(t-t'), & \Sigma(u-u'), & \Sigma(v-v'), \\ \Sigma(uv'-u'v), & \Sigma(vt'-v't), & \Sigma(tu'-t'u). \end{array}\right\} \ldots\ldots\ldots \quad (8)$$

If between these six coordinates the equation of condition subsists, there is a resulting rotatory force. In the case of equilibrium the six sums become equal to zero.

Any movement whatever may be referred as well to ordinary as to rotatory forces; consequently an ordinary dyname and a rotatory dyname mean the same.

3. From the notions developed we immediately obtain two general

theorems constituting the base of statics. In a similar way as d'Alembert's principle is derived from the "principe des vitesses virtuelles," both theorems may be transformed into fundamental theorems of mechanics.

In starting from the coordinates of ordinary forces, the equations of equilibrium are

$$\left.\begin{array}{lll}\Sigma(x-x')\quad=0, & \Sigma(y-y')\quad=0, & \Sigma(z-z')\quad=0,\\ \Sigma(yz'-y'z)=0, & \Sigma(zx'-z'x)=0, & \Sigma(xy'-x'y)=0,\end{array}\right\}\ldots\ldots \quad (9)$$

which, by replacing these forces by rotating ones, become

$$\left.\begin{array}{lll}\Sigma(t-t')\quad=0, & \Sigma(u-u')\quad=0, & \Sigma(v-v')\quad=0,\\ \Sigma(uv'-u'v)=0, & \Sigma(vt'-v't)=0, & \Sigma(tu'-t'u)\ =0.\end{array}\right\}\ldots\ldots \quad (10)$$

We likewise may express the conditions of equilibrium by the following six equations—

$$\left.\begin{array}{lll}\Sigma(x-x')=0, & \Sigma(y-y')=0, & \Sigma(z-z')=0,\\ \Sigma(t-t')\ =0, & \Sigma(u-u')=0, & \Sigma(v-v')=0,\end{array}\right\}\ldots\ldots \quad (11)$$

three of which are taken amongst (9), three amongst (10)—and expand these equations in the analytical way, starting either from the consideration of ordinary or rotatory forces. The interpretation of these equations immediately gives the following two theorems. In the case of equilibrium—

I. *The centre of gravity of the points acted upon by the forces coincides with the centre of gravity of the second points, by means of which the forces are determined* (No. 1).

II. *The central plane of the planes acted upon by the rotatory forces coincides with the central plane of the second planes, by means of which the rotatory forces are determined* (No. 2)*.

If we introduce the notion of *masses,* both theorems hold good, only the definition of both kinds of forces and therefore their unity is changed. The points acted upon become centres of gravity corresponding to masses; the planes acted upon, central planes corresponding to moments of inertia.

If equilibrium does not exist, we get, in the general case, one resulting force, determined by the two centres of gravity, and one resulting rotatory force determined by the two central planes. We easily obtain the six differential equations of the movement produced.

I shall think it suitable further to develope the principles merely indicated in the paper presented. A Treatise on Mechanics, reconstructed on them, will assume quite a new aspect.

4. In making use, within a plane, of point- or line-coordinates, we represent, by means of an equation between the two coordinates, a plane curve described by a point, or enveloped by a right line. In making use, in space, of point- or plane-coordinates, by means of an equation between the three coordinates, a surface is represented which may be regarded as a

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* The coordinates of the central plane are obtained in the same way by means of the coordinates of the given planes as the coordinates of the centre of gravity are obtained by means of the coordinates of the given points.

locus of points, or an envelope by planes. In my geometrical paper I introduced the right line, depending on four constants, as the element of space. An equation between these constants, if regarded as variables, represents a *complex of lines*. Each line of the complex may be regarded either as a ray described by a point, or as an *axis* enveloped by planes. In order to render the developments symmetrical, I adopted as coordinates of the right line the same expressions (1) and (4) (connected by an equation of condition) made use of in the determination of ordinary and rotatory forces. At the same time, general equations between the six coordinates were replaced by homogeneous ones. In doing so, A, B, C, D, E, F denoting any six constants,

$$\left.\begin{array}{l} \mathrm{A}(x-x')+\mathrm{B}(y-y')+\mathrm{C}(z-z')+\mathrm{D}(yz'-y'z) \\ \quad +\mathrm{E}(yx'-y'x)+\mathrm{F}(xy'+x'y')\equiv\Omega=0, \end{array}\right\} \ldots\ldots\ldots\ldots \quad (12)$$

$$\left.\begin{array}{l} \mathrm{A}(uv'-u'v)+\mathrm{B}(vt'-v't)+\mathrm{C}(tu'-t'u) \\ \quad +\mathrm{D}(t-t')+\mathrm{E}(u-u')+\mathrm{F}(v-v')\equiv\Phi=0 \end{array}\right\} \ldots\ldots\ldots\ldots \quad (13)$$

represent the *same* linear complex of lines, which may be regarded as a complex of rays as well as a complex of axes.

When general equations between the same six coordinates are admitted, the complex of rays becomes a *complex of ordinary forces*, the complex of axes a *complex of rotations* or *rotatory forces*, both ordinary and rotatory forces depending upon five constants. Here we meet a reciprocity between both kind of forces corresponding to the reciprocity between point and plane.

Finally, in omitting the equation of condition hitherto made use of, we pass from forces to dynames, depending upon the six independent constants

$$\mathrm{X},\quad \mathrm{Y},\quad \mathrm{Z},\quad \mathrm{L},\quad \mathrm{M},\quad \mathrm{N}, \ldots\ldots\ldots\ldots\ldots \quad (14)$$

or

$$\mathfrak{X},\quad \mathfrak{Y},\quad \mathfrak{Z},\quad \mathfrak{L},\quad \mathfrak{M},\quad \mathfrak{N}. \ldots\ldots\ldots\ldots\ldots \quad (15)$$

A complex of dynames is represented by an equation between six variables. Here again, as in the case of right lines, the same complex may be represented in a double way by means of the two sets of coordinates.

In order to complete these general considerations, we add the following. A dyname may be resolved into *two* variable forces, either ordinary or rotatory. These variable forces constitute a linear complex, either of ordinary or extraordinary forces. A homogeneous equation between the six *independent* coordinates (14) or (15) represents a *complex of two coupled variable right lines.*

5. I will conclude by giving some details regarding complexes of the first degree.

A linear complex of ordinary forces is represented by the linear equation

$$\Omega=1,$$

which may be expanded thus:

$$\left.\begin{array}{l} \quad\ (\mathrm{A}+\mathrm{F}y'-\mathrm{E}z')x \\ +\ \ (\mathrm{B}-\mathrm{F}x'-\mathrm{D}z')y \\ +\ \ (\mathrm{C}+\mathrm{E}x'-\mathrm{D}y')z \\ =\mathrm{A}x'+\mathrm{B}y'+\mathrm{C}z'. \end{array}\right\} \ldots\ldots\ldots\ldots\ldots\ldots \quad (16)$$

In putting x', y', z' as constants, those forces of the complex are obtained which pass through the given point (x', y', z'). In this supposition the equation of the complex becomes the equation of a plane. This plane is the locus of the second points, by means of which the forces of the complex are determined. Hence in a linear complex there are acting on each point of space forces in all directions, the intensity of each force being the segment on its direction between the point acted upon and its corresponding plane (16).

Hence we derive the following theorem:—

In a complex of rotatory forces any given plane of space is acted upon by an infinite number of forces, each line within the plane being an axis of rotation. The rotations round all axes are determined by second planes passing through a fixed point, the position of which depends upon the given point.

I showed in the geometrical paper the way of discussing linear complexes of right lines. The properties of linear complexes of forces, either ordinary or rotatory, may be developed in a similar way.

6. Right lines belonging simultaneously to two complexes constitute a single congruency, and accordingly intersect two fixed lines; if belonging to three complexes, they constitute a double congruency, *i. e.* one generation of a hyperbolic or parabolic hyperboloid. Forces, either ordinary or rotatory, belonging simultaneously to two, three, four complexes, constitute a single, double, or threefold congruency of forces. In admitting these denominations, the following results are immediately obtained.

In a linear congruency of ordinary or rotatory forces, the directions of all ordinary, or the axes of all rotatory forces, constitute a linear complex of lines. All ordinary forces acting on any given point of space are confined within the same plane; the intensity of each force is equal to the distance between the point acted upon and the point where its direction meets a fixed line within the mentioned plane. The axes of all rotatory forces, acting upon any given plane of space, meet in a fixed point within that plane. There is a fixed line passing through the fixed point, round which the second planes of all forces turn when their axes turn round the fixed point in the given plane acted upon.

In a double congruency of ordinary forces there is one force of given direction and intensity acting upon any point of space, as there is in a double congruency of rotatory forces one force acting upon any given plane.

In a threefold congruency of ordinary forces the *directions*, in a threefold congruency of rotatory forces the *axes* of all forces constitute one of the two generations of a hyperboloid.

These few indications may be sufficient here; but before concluding I must, in referring to the original paper, make a last remark. Forces acting along a given right line may either be regarded as the same, whatever may be the point of the line acted upon, or they may be regarded as varying in intensity according to the position of the point. There is an analogous distinction to be made with regard to rotatory forces. Accordingly two different kinds of complexes must be distinguished.

IV. "Contributions to Terrestrial Magnetism.—No. X." By Lieut.-General Edward Sabine, R.A., President of the Royal Society. Received June 7, 1866.

(Abstract.)

In this number of the Contributions to Terrestrial Magnetism the author resumes the discussion of the results obtained in the Magnetic Survey of the Southern Ocean by the Expedition under Sir James Clark Ross, R.N., and Captain Francis Rawdon Crozier, R.N., between the years 1839 and 1843. The proceedings during the two first years of this Survey have been the subjects of two preceding numbers of the Contributions, viz. of Nos. V. and VI., in the Philosophical Transactions for 1843 and 1844. The present number contains a similarly detailed exposition of the operations of the third year of the Survey, comprehending the Southern Atlantic between Cape Horn and the Cape of Good Hope, and completing the circumnavigation of the southern hemisphere, from the departure of the expedition from the Cape of Good Hope in March 1840, to the return to the same station in April 1843. In a subsequent memoir, which will be presented to the Royal Society early in the ensuing Session, the author proposes to connect and thoroughly coordinate the three portions of the Survey, and to supply from them the numerical data at equidistant points on each of the three parallels of 50°, 60°, and 70° of south latitude, of the three magnetic elements, which will be required for a revision of the 'Allgemeine Theorie des Erdmagnetismus' of M. Gauss—the 40th parallel having been the most southern available at the epoch of the publication of the original work.

The instruments employed in this Survey, as well as the methods of employing them, and of eliminating the disturbing influence of the iron in the equipment of the vessels, having been in a great measure of a novel character, a discussion of considerable length bearing on all such points is prefixed to a full detail of the observations themselves, arranged in Tables, showing in every instance both the immediate results of the observations, and the corrections which have been applied in conformity with the principles contained in the preliminary discussion. Tabular abstracts are also furnished, exhibiting the results of the determinations of each day, with the appropriate geographical positions.

June 21, 1866.

Lieut.-General SABINE, President, in the Chair.

Dr. Hugo Müller, Mr. Perkin, the Rev. Canon Selwyn, and the Rev. R. Townsend, were admitted into the Society.

I. "On the Relation of Rosaniline to Rosolic Acid." By H. CARO, and J. ALFRED WANKLYN, Professor of Chemistry at the London Institution. Communicated by Dr. FRANKLAND. Received June 5, 1866.

At the Meeting of the British Association held in Bath in the year 1864, it was pointed out by one of us that rosaniline and rosolic acid might be represented as ethylene which had undergone substitution:—

$$\begin{array}{ccc} \text{Type.} & \text{Rosaniline.} & \text{Rosolic Acid.} \\ C_2\left\{\begin{array}{l}H\\H\\H\\H\end{array}\right.; & C_2\left\{\begin{array}{l}N\,C_6\,H_5\,H\\N\,C_6\,H_5\,H\\N\,C_6\,H_5\,H\\H\end{array}\right.; & C_2\left\{\begin{array}{l}O\,C_6\,H_5\\O\,C_6\,H_5\\O\,C_6\,H_5\\O\,H\end{array}\right. \end{array}$$

Rosaniline and rosolic acid became members of the same family, the former being an ethylene containing $N\left\{\begin{array}{l}C_6H_5\\H\end{array}\right.$ in place of hydrogen, the latter also an ethylene, but containing $O\,C_6\,H_5$ and $O\,H$ in place of hydrogen. Some of the reasons for assigning these formulæ were given in the communication made to the Bath Meeting.

The relationship between rosaniline and rosolic acid is very well brought out by the facts which will presently be brought forward.

Griess has shown that aniline and nitrous acid yield water and diazobenzol:—

$$\underset{\text{Aniline.}}{C_6\,H_5\,N\,H_2} + H\,N\,O_2 \quad = \quad \underset{\text{Diazobenzol.}}{C_6\,H_4\,N_2} + 2H_2\,O.$$

Diazobenzol is a most remarkable compound, forming salts which are very explosive, and which undergo certain very interesting transformations under the influence of reagents. It is moreover the representative of a numerous class of compounds derived similarly by the action of nitrous acid on different bases, and for the most part resembling itself in the explosive character of the salts. One of the most remarkable reactions presented by diazobenzol is that with water, wherein the whole of the nitrogen of the diazobenzol is evolved, and its place supplied by an atom of water:—

$$\underset{\text{Nitrate of diazobenzol.}}{C_6\,H_4\,N_2\,H\,N\,O_3} \quad + \quad H_2\,O \quad = \quad \underset{\text{Carbolic acid.}}{C_6\,H_6\,O} \quad + \quad H\,N\,O_3 + N_2.$$

This elegant form of reaction appears to be characteristic of the class to which diazobenzol belongs; and Griess has resorted to a measurement of the quantity of nitrogen set free during the reaction, as a means of arriving at the composition of the azo-compounds.

Hofmann showed, some years ago, that rosaniline, after treatment with nitrous acid, is capable of forming a platinum compound endowed with explosive properties, but appears not to have followed the investigation further.

Paraf has recently shown that rosaniline salts are converted by nitrous acid into a dye, which he considered to be rosolic acid. We have also investigated the action of nitrous acid on rosaniline, and arrive at the following results:—

When an acid solution of a salt of rosaniline is mixed with nitrous acid, it forms an azo-compound, which corresponds very closely in character to diazobenzol. Like diazobenzol this compound forms explosive salts; like diazobenzol it decomposes with evolution of nitrogen gas when it is boiled with acids. In adding the nitrous acid to the solution of rosaniline to form the compound, it is easy to observe the exact point at which the solution ceases to contain unaltered rosaniline. It thus becomes easy to determine the amount of nitrous acid consumed in the conversion of a given weight of rosaniline into the azo-compound. We have done this by the employment of a method which gives excellent results when applied to aniline and toluidine, and the details of which will shortly be published by one of us. We obtain as the result of our experiments, that one molecule of rosaniline consumes three equivalents of nitrous acid; and the equation representing the reaction will be

$$\underset{\text{Rosaniline.}}{C_{20}H_{19}N_3}+3HNO_2 = \underset{\text{Azo-compound.}}{C_{20}H_{10}N_6} + 6H_2O.$$

On boiling this azo-compound with hydrochloric acid, there is evolution of nitrogen gas. The volume of nitrogen was measured. The result was that one molecule of rosaniline, after conversion into the azo-compound, yields six equivalents of nitrogen.

$$\underset{\text{Azo-compound.}}{C_{20}H_{10}N_6} + 3H_2O = C_{20}H_{16}O_3 + N_6.$$

These changes in composition are accompanied by striking physical effects. The deep-red solution of the salt of rosaniline becomes brown on the addition of excess of hydrochloric acid, then yellow as the nitrous acid is added; then there is much froth, and as the solution is boiled it gradually becomes red yellow, and a large quantity of a deep-coloured solid with cantharides-like lustre separates out.

Seeing that this solid is produced by treating rosaniline with three equivalents of nitrous acid, and that six equivalents of nitrogen are evolved, it must be a non-nitrogenous substance. A careful comparison of its properties and reactions with those of the rosolic acid described by Kolbe and Schmitt, and now made largely as an article of commerce, leads to the conclusion that it is identical with rosolic acid.

The following characters are common to it and to the rosolic acid of Kolbe and Schmitt.

1. A yellowish-red solid with cantharides-like lustre, only sparingly soluble in water, soluble in ether and alcohol.

2. Easily soluble in ammonia, and in alkalies generally, forming red solutions of very great colouring-power. Addition of acids to these solutions decolorizes them, precipitating the colouring-matter in the form of a yellowish precipitate, which varies much in tint.

3. When boiled with aniline and a little benzoic acid, it forms a blue dye, there being no evolution of ammonia. The blue dye is very soluble

in alcohol, not soluble in water; it is the salt of a red-coloured base. It dissolves in strong sulphuric acid, giving a red solution.

4. Submitted to destructive distillation, it gives abundance of carbolic acid, leaving a carbonaceous residue.

5. The deep-red solutions in alkalies are easily reduced on boiling them with zinc powder; so treated they lose their colour; but restoration of the colour takes place on adding ferricyanide of potassium.

There is only one particular in which any difference could be detected between the product obtained from rosaniline and that obtained from carbolic acid by Kolbe and Schmitt's process.

The rosolic acid of Kolbe and Schmitt forms salts the solutions of which are darkened by ferricyanide of potassium. The product obtained from rosaniline does not darken, or darkens only very slightly, on the addition of ferricyanide. An explanation of this difference is afforded by the following experimental facts. The product from rosaniline after reduction with zinc becomes capable of being darkened by ferricyanide; and if leucaniline, instead of rosaniline, be taken, there is obtained a cantharides-like product, which gives red solutions with alkalies, but is darkened by ferricyanide. Furthermore, if a solution of Kolbe and Schmitt's rosolic acid be darkened with ferricyanide of potassium, and then precipitated by the addition of an acid, there results a colour-acid, which dissolves in alkalies, giving solutions of the exact tint of the rosaniline-product, and, like it, incapable of being deepened by ferricyanide. The interpretation of all this is, that the rosolic acid obtained from carbolic acid by the action of sulphuric and oxalic acid (Kolbe and Schmitt) contains more or less leuco-rosolic acid, produced probably by the reducing action of some sulphurous acid. The rosolic acid got from leucaniline also contains more or less leuco-rosolic acid. The rosolic acid obtained from rosaniline is free, or almost free, from leuco-rosolic acid.

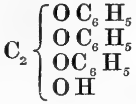
Be this, however, as it may, there can be no doubt that rosaniline and carbolic acid give essentially the same product when the former is treated with nitrous acid in the manner we have described, and the latter with sulphuric and oxalic acids as in Kolbe and Schmitt's process.

Adopting the "Ethylene type," we have the following expressions for the reactions of rosaniline:—

$$\overset{\text{Rosaniline.}}{C_2\left\{\begin{array}{l} N\,C_6\,H_5\,H \\ N\,C_6\,H_5\,H \\ N\,C_6\,H_5\,H \\ H \end{array}\right.} + \begin{array}{l} H\,N\,O_2 \\ H\,N\,O_2 \\ H\,N\,O_2 \end{array} = \overset{\text{Azo-rosaniline.}}{C_2\left\{\begin{array}{l} N_2\,C_6\,H_3 \\ N_2\,C_6\,H_3 \\ N_2\,C_6\,H_3 \\ H \end{array}\right.} + \begin{array}{l} 2H_2\,O \\ 2H_2\,O \\ 2H_2\,O \end{array}$$

$$C_2\left\{\begin{array}{l} N_2\,C_6\,H_3 \\ N_2\,C_6\,H_3 \\ N_2\,C_6\,H_3 \\ H \end{array}\right. + \begin{array}{l} H_2\,O \\ H_2\,O \\ H_2\,O \end{array} = \overset{\text{Rosolic Acid.}}{C_2\left\{\begin{array}{l} O\,C_6\,H_5 \\ O\,C_6\,H_5 \\ O\,C_6\,H_5 \\ H \end{array}\right.} + 3N_2.$$

On comparing the formula of rosolic acid given thus by Griess's process applied to rosaniline, we find that it differs from the formula deduced from Kolbe and Schmitt's analysis, viz.

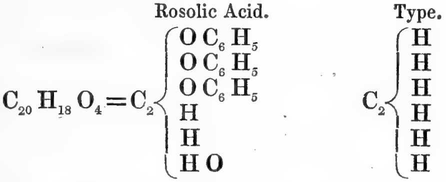


$$C_2\left\{\begin{array}{l} O\,C_6\,H_5\\ O\,C_6\,H_5\\ OC_6\,H_5\\ O\,H \end{array}\right.$$

by one atom of oxygen.

We are inclined to think that this difficulty must be got over by correcting the formula deduced from Kolbe and Schmitt's research. If the numbers required by the formulæ $C_{20}\,H_{16}\,O_4$ and $C_{20}\,H_{18}\,O_4$ be calculated, it will be found that both of them fall sufficiently near the analytical results actually obtained to allow of the deduction of either from the analyses.

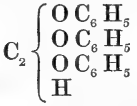
Taking the latter, viz. $C_{20}\,H_{18}\,O_4$, all will become intelligible:—



Rosolic Acid. Type.

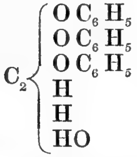
$$C_{20}\,H_{18}\,O_4 = C_2\left\{\begin{array}{l} O\,C_6\,H_5\\ O\,C_6\,H_5\\ O\,C_6\,H_5\\ H\\ H\\ H\,O \end{array}\right. \qquad C_2\left\{\begin{array}{l} H\\ H\\ H\\ H\\ H\\ H \end{array}\right.$$

Thus rosolic acid appears as an ethyl-hydride, and its generation in the Griess process applied to rosaniline is obvious. To the formula



$$C_2\left\{\begin{array}{l} O\,C_6\,H_5\\ O\,C_6\,H_5\\ O\,C_6\,H_5\\ H \end{array}\right.$$

which is derived from rosaniline by the straightforward action of nitrous acid and water, we have to add $H_2\,O$, and we get



$$C_2\left\{\begin{array}{l} O\,C_6\,H_5\\ O\,C_6\,H_5\\ O\,C_6\,H_5\\ H\\ H\\ HO \end{array}\right.$$

which is, as was said, a possible expression of the analyses of the rosolic acid obtained from carbolic acid.

II. "On the Chemical and Mineralogical Composition of the Dhurmsalla Meteoric Stone." By the Rev. SAMUEL HAUGHTON, M.D., F.R.S., Fellow of Trinity College, Dublin. Received June 6, 1866.

On the 14th July 1860, at 2.15 P.M., a remarkable meteoric stone fell at Dhurmsalla, in the Punjab; a small specimen of which was forwarded to the Geological Museum of Trinity College, which I have analyzed with the results contained in the following paper.

The direction of the motion of the meteorite was ascertained to be from N.N.W. to S.S.E.

The cold of the fragments that fell was so intense as to benumb the hands of the coolies who picked them up but who were obliged, in consequence of their coldness, instantly to drop them.

The specific gravity of the Trinity College specimen was found as follows:—

Weight in air	3335·4 grs.
Weight in water	2354·1 „
Sp. gr.	= 3·399.

The stone is grey, close-grained, and splintery in fracture, and presents fewer specks of metallic iron and magnetic pyrites than usual, and was coated with the ordinary black pellicle on its outer side.

From 100 grs. acted on with iodine, which dissolved the alloy of iron and nickel, there were obtained, of peroxide of iron 9·85 grs., and of protoxide of nickel 1·96 gr.

The portion insoluble in iodine was next acted on by dilute muriatic acid and evaporated to dryness at 212°, then moistened with muriatic acid and filtered, by which process it was divided into a soluble and insoluble portion; the portion left on the filter was boiled with carbonate of soda, so as to dissolve the free silica, which was found to be 18·95 grs. This was added to the portion originally soluble in muriatic acid, so as to give the following results:—

	grs.	
Silica	18·95	
Alumina	0·14	
Peroxide of iron	14·11	Present originally as protoxide and protosulphuret of iron.
Carbonate of lime	none	
Pyrophosphate of magnesia	51·31	
Potash and soda chlorides	0·30	
Platino-chloride of potassium	0·20	
Oxide of manganese $(Mn_3 O_4)$	0·66	

On treating another 100 grs. of the meteorite for sulphur, by boiling in muriatic acid, and conducting the sulphuretted hydrogen into an ammoniacal solution of sulphate of copper, so as to form a black precipitate of sulphuret of copper, there were found by the usual methods 14·8 grs. of sulphate of barytes.

There were left, after treatment with iodine, muriatic acid, and carbonate of soda, 38·3 grs. of the 100 grs. originally acted upon.

From the foregoing facts, we readily obtain—from treatment with iodine and for sulphur—

	grs.		grs.
Peroxide of iron	9·85	....	6·88 iron
Protoxide of nickel	1·96	....	1·54 nickel
Sulphate of barytes	14·80	....	5·61 protosulphuret of iron.

Hence we find, as the primary analysis of the meteorite—

I. *Primary Analysis.*

1. Metallic iron	6·88
2. Metallic nickel........	1·54
3. Magnetic pyrites	5·61
4. Earthy mineral (soluble)........	47·67
5. Earthy mineral (insoluble)	38·30
	100·00

The results of the analysis of the soluble portion (considering that 5·61 of $Fe S$ is equivalent to 5·10 of $Fe_2 O_3$) give the following :—

II. *Earthy Mineral (soluble).*

	grs.		per cent.		Oxygen.
1. Silica	18·95	....	39·75	....	20·637
2. Alumina	0·14	....	0·29	....	0·135
3. Protoxide of iron....	8·10	....	16·99	....	3·768
4. Protoxide of manganese	0·66	....	1·38	....	0·308
5. Lime..............	none	....	——	....	——
6. Magnesia	18·34	....	38·47	....	15·374
7. Potash	0·04	....	0·10	....	0·016
8. Soda	0·13	....	0·28	....	0·071
9. Loss	1·31	....	2·74	....	——
	47·67		100·00		40·309

Adding together the oxygen of the protoxides, we find—

$$\begin{array}{ll} RO \ldots\ldots\ldots\ldots & 19{\cdot}537 \\ Si\,O_3 \ldots\ldots\ldots\ldots & \left.\begin{array}{r} 20{\cdot}637 \\ 0{\cdot}135 \end{array}\right\} 20{\cdot}772 \\ Al_2\,O_3 \ldots\ldots\ldots\ldots & \end{array}$$

From the preceding result, it is evident that the soluble mineral in this meteorite is chrysolith, or the silicate of magnesia and iron represented by the formula

$$3\,RO, \quad Si\,O_3;$$

in which magnesia preponderates greatly over the iron.

The 38·3 grs. of mineral, insoluble in muriatic acid and in carbonate of soda, were now divided into two equal portions, of which one was fluxed with carbonates of soda and potash, and the other with lime and chloride of ammonium—with the following results :—

	grs.
Silica	10·85
Alumina	0·23
Peroxide of iron	2·51
Oxide of manganese ($Mn_3\,O$)	0·30
Oxide of chrome ($Cr_2\,O_3$)	1·42
Carbonate of lime	none
Pyrophosphate of magnesia	11·50
Potash and soda chlorides	0·30
Platino-chloride of potassium	0·50

Assuming the chrome to be present as chrome-iron,

$$Fe\,O,\, Cr_2\,O_3,$$

we find

	grs.
Original weight	19·15
Chrome-iron	2·08
Earthy insoluble	17·07

If we now omit the chrome-iron and make the necessary reductions in the foregoing results, we obtain—

III. *Earthy Mineral* (*insoluble.*)

	grs.	per cent.	Oxygen.
Silica	10·85	63·56	33·000
Alumina	0·23	1·34	0·525
Protoxide of iron	1·60	9·37	2·078
Protoxide of manganese	0·30	1·75	0·392
Lime	none	——	——
Magnesia	4·13	24·19	9·666
Soda	0·08	0·47	0·119
Potash	0·09	0-52	0·087
[Gain]	[0·21]	[1·20]	——
	17·07	100·00	45·867

The oxygen of the protoxides of the preceding analysis amounts to 12·342 per cent., but it would be fallacious to form any opinion as to the composition of the whole, so long as we are not acquainted with the constituent minerals that compose it.

Collecting together into one view the preceding results, we find—

IV. *Mineralogical Composition of the Dhurmsalla Meteorite.*

1. Nickel-iron	8·42	Iron	6·88
		Nickel	1·54
2. Protosulphuret of iron	5·61		
3. Chrome-iron*	4·16		
4. Chrysolith (*peridot* or *olivine*)	47·67		
5. Minerals insoluble in muriatic acid	34·14		
	100·00		

I shall here add, for the purpose of comparison, the results of my analysis of the meteoric stone that fell at Dundrum, co. Tipperary, at 7 P.M. of the 12th August, 1865.

Dundrum Meteorite.

Sp. gr. .... 3·066 to 3·570.

I. *Mineralogical Composition.*

1. Nickel-iron	20·60	Iron	19·57
		Nickel	1·03
2. Sulphur-iron	4·05		
3. Chrome-iron	1·50		
4. Chrysolith	33·08		
5. Earthy minerals insoluble in muriatic acid	40·77		
	100·00		

II. *Chemical Composition of the Chrysolith.*

Silica	38·74	38·86
Alumina	0·45	——
Protoxide of iron	16·55	19·74
Protoxide of manganese	0·15	——
Lime	0·84	0·72
Magnesia	40·93	36·85
Potash	0·51	0·47
Soda	0·24	0·22
Loss	1·59	3·14
	100·00	100·00

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* The quantity of chrome found in this meteorite is unusually large, being represented by 2·84 per cent. of Cr_2O_3, and by 4·16 per cent. of FeO, Cr_2O_3; yet it is not without precedent, for in the meteoric stone that fell at Nobleborough, Maine, U. S. A. on the 7th of August, 1823, Webster found 4 per cent. of Cr_2O_3.

III. *Chemical Composition of the Earthy insoluble Minerals.*

Silica	61·33
Alumina	1·72
Protoxide of iron	6·06
Protoxide of manganese	0·78
Lime	3·99
Magnesia	22·02
Soda	1·38
Potash	0·83
Loss	1·89
	100·00

III. "On the Preparation of Ethylamine." By J. Alfred Wanklyn, and Ernest T. Chapman. Communicated by Dr. Frankland.

Received June 8, 1866.

Having recently had occasion to prepare a considerable quantity of ethylamine, we have made the observation that this base may be obtained with much greater facility than is usually believed.

We digested together equal volumes of iodide of ethyl, strong alcohol, and aqueous ammonia. The digestion was carried on at a very moderate temperature, certainly not exceeding 80°, but the tubes were constantly agitated. In this manner the reaction is completed in about half an hour. The mixed iodides thus obtained were evaporated to expel excess of ammonia, introduced into a retort; enough potash was added to neutralize $\frac{1}{10}$ of the iodine present, and the mixture was distilled into dilute hydrochloric acid. The receiver was then changed, the same quantity of potash again added, and the products collected as before. Then six times as much potash was added, and the products collected. The remaining two-tenths of the potash were added separately.

Portions of each of these fractions were converted into platinum salts, and the amount of platinum was determined by ignition. Each of the first four fractions corresponding to $\frac{9}{10}$ of the total bases obtained, gave more platinum than corresponds even to pure ethylamine, and therefore contained ethylamine and ammonia. Only the last fraction, corresponding to $\frac{1}{10}$ of the entire bases, gave a lower percentage of platinum than corresponds to ethylamine, and therefore contained the di- and tri-bases.

Subjoined are the platinum determinations in this last fraction:—

I. 0·2133 grm. gave 0·0830 Platinum
II. 0·3590 „ „ 0·1382 „

or,
I. Platinum per cent. 38·91
II. „ „ „ 38·50

$Pt\,Cl_2\,N\,C_2\,H_5\,H_3\,Cl$ contains 39·37 per cent. of Pt.
$Pt\,Cl_2\,N\,(C_2\,H_5)_2\,H_2\,Cl$ contains 35·42 „ „

From which it will be seen that even the last tenth of the bases did not contain much di- and tri-bases.

The former $\frac{9}{10}$ gave, as aforesaid, rather more platinum than ethylamine salt yields, and therefore contained ammonia.

In order to make out whether there was any di- and tri-base along with the ethylamine in these former fractions, we took No. 3 (corresponding to $\frac{6}{10}$) and made a platinum-fractionation as follows:—

No. 3 gave 40·62 and 40·13 per cent. of platinum. No. 3, after some pure chloride of ammonium had crystallized out (the crystals were analyzed and proved to be quite pure chloride of ammonium), was converted into a platinum-salt, and that platinum-salt fractionated so as to leave the portion richest in the high bases. This salt, which would be rich in the di- and tri-bases if any had been present in No. 3, gave this result:—

·7666 grm. gave ·3090 grm. of platinum = Pt per cent. 40·03.

From which it is manifest that di- and tri-bases were absent.

From the foregoing we concluded that all the higher bases were concentrated in the last fraction, but that ammonia passed over during the whole distillation. The objection might therefore be made that mixtures of ammonia and di- and tri-ethylamines would give the same numbers. In order to deprive this objection of its force we made the following experiments. Iodide of ethyl, alcohol, and ammonia were digested as before, and the mixed iodides so obtained distilled from excess of potash, the vapour thus obtained absorbed by dilute sulphuric acid, care being taken that the acid was not in excess. The fluid so obtained was rendered very slightly acid with the same acid, and then evaporated on the water-bath. The semisolid residue was treated with strong alcohol, and allowed to stand one night. It was then filtered, the filtrate measured, and the amount of sulphuric acid determined in a measured portion. Enough KHO was then added to the alcoholic liquid to liberate $\frac{8}{10}$ of the bases present. The potash was added in solution in water. The liquid was then distilled, the distillate being received in dilute hydrochloric acid. A portion of this liquid was then evaporated to dryness and placed over sulphuric acid. Its percentage of chlorine was then determined:

·3826 of salt gave ·6696 Ag Cl;
∴ 43·29 per cent. Cl. Theory 43·55.

From these numbers it is obvious that the substance must have been pure ethylamine. It could have contained no ammonia, or at most only minute traces, and therefore the above objection is completely disposed of.

A portion of this pure hydrochlorate of ethylamine was then treated with aqueous ammonia in insufficient quantity to combine with the whole of the hydrochloric acid present. We distilled this liquid into dilute hydrochloric acid, evaporated it to dryness, and determined the amount of chlorine present in the residue. The result was:—

·4528 of salt gave 1·3155 Ag Cl ; ∴	59·94	per cent. Cl.
Chloride of ammonium would give	66·33	,,
Chloride of ethylamine..........	43·55	,,

From these numbers it appears that ammonia is capable of partially expelling ethylamine from its compounds, and thus we have an explanation of the apparent anomaly of the appearance of ammonia with the ethylamine in the above-described experiments. It appears therefore that pure ethylamine may be readily obtained in the following manner. Equal volumes of iodide of ethyl, strong alcohol and ammonia are to be digested together for about half an hour, at a temperature somewhat below that of boiling water, with continual shaking; the product of this operation boiled to expel excess of ammonia, and then distilled from potash—the distillate being received into dilute sulphuric acid, the ammonia separated by means of alcohol, and the alcoholic solution of the mixed sulphates treated with sufficient potash to liberate about $\frac{9}{10}$ of the bases present. On distilling this mixture, pure ethylamine, accompanied with alcohol and water, are the sole products found in the distillate.

IV. "On the Expansion by Heat of Metals and Alloys."

By A. Matthiessen, F.R.S. Received June 14, 1866.

(Abstract.)

In a paper "On the Expansion by Heat of Water and Mercury"*, a method of determining the expansion of bodies is described, by which good results can be obtained with comparatively small quantities of the substances to be experimented with. This method, that of weighing the body in water at different temperatures, has been employed for the present research. The results obtained are given in the following Tables:—

Table I.—Formulæ for the Correction of the Cubical Expansion by Heat of the Metals.

Cadmium......	$V_t=V_0(1+10^{-4}\times 0{\cdot}8078t+10^{-6}\times 0{\cdot}140t^2)$†.
Zinc..........	$V_t=V_0(1+10^{-4}\times 0{\cdot}8222t+10^{-6}\times 0{\cdot}0706t^2)$.
Lead..........	$V_t=V_0(1+10^{-4}\times 0{\cdot}8177t+10^{-6}\times 0{\cdot}0222t^2)$.
Tin	$V_t=V_0(1+10^{-4}\times 0{\cdot}6100t+10^{-6}\times 0{\cdot}0789t^2)$.
Silver	$V_t=V_0(1+10^{-4}\times 0{\cdot}5426t+10^{-6}\times 0{\cdot}0405t^2)$.
Copper........	$V_t=V_0(1+10^{-4}\times 0{\cdot}4463t+10^{-6}\times 0{\cdot}0555t^2)$.
Gold..........	$V_t=V_0(1+10^{-4}\times 0{\cdot}4075t+10^{-6}\times 0{\cdot}0336t^2)$.
Bismuth	$V_t=V_0(1+10^{-4}\times 0{\cdot}3502t+10^{-6}\times 0{\cdot}0446t^2)$.
Palladium......	$V_t=V_0(1+10^{-4}\times 0{\cdot}3032t+10^{-6}\times 0{\cdot}0280t^2)$.
Antimony......	$V_t=V_0(1+10^{-4}\times 0{\cdot}2770t+10^{-6}\times 0{\cdot}0397t^2)$.
Platinum	$V_t=V_0(1+10^{-4}\times 0{\cdot}2554t+10^{-6}\times 0{\cdot}0104t^2)$.

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* Phil. Trans. 1866, part 1.

† I have employed this method of writing the formulæ to prevent mistakes in the

TABLE II.—Formulæ for the Correction of the Linear Expansion by Heat of the Metals.

Cadmium ...... $L_t=L_0\,(1+10^{-4}\times 0\cdot2693t+10^{-6}\times 0\cdot0466t^2)$.
Zinc .... $L_t=L_0\,(1+10^{-4}\times 0\cdot2741t+10^{-6}\times 0\cdot0234t^2)$.
Lead .... $L_t=L_0\,(1+10^{-4}\times 0\cdot2726t+10^{-6}\times 0\cdot0074t^2)$.
Tin .... $L_t=L_0\,(1+10^{-4}\times 0\cdot2033t+10^{-6}\times 0\cdot0263t^2)$.
Silver .... $L_t=L_0\,(1+10^{-4}\times 0\cdot1809t+10^{-6}\times 0\cdot0135t^2)$.
Copper .... $L_t=L_0\,(1+10^{-4}\times 0\cdot1481t+10^{-6}\times 0\cdot0185t^2)$.
Gold .... $L_t=L_0\,(1+10^{-4}\times 0\cdot1358t+10^{-6}\times 0\cdot0112t^2)$.
Bismuth ...... $L_t=L_0\,(1+10^{-4}\times 0\cdot1167t+10^{-6}\times 0\cdot0149t^2)$.
Palladium...... $L_t=L_0\,(1+10^{-4}\times 0\cdot1011t+10^{-6}\times 0\cdot0093t^2)$.
Antimony...... $L_t=L_0\,(1+10^{-4}\times 0\cdot0923t+10^{-6}\times 0\cdot0132t^2)$.
Platinum ...... $L_t=L_0\,(1+10^{-4}\times 0\cdot0850t+10^{-6}\times 0\cdot0035t^2)$.

TABLE III.—Formulæ for the Correction of the Cubical Expansion by Heat of the Alloys.

$Sn_4\,Pb$: $V_t=V_0\,(1+10^{-4}\times 0\cdot6200t+10^{-6}\times 0\cdot0988t^2)$.
$Pb_4\,Sn$: $V_t=V_0\,(1+10^{-4}\times 0\cdot8087t+10^{-6}\times 0\cdot0332t^2)$.
$Cd\,Pb$: $V_t=V_0\,(1+10^{-4}\times 0\cdot9005t+10^{-6}\times 0\cdot0133t^2)$.
$Sn_4\,Zn$: $V_t=V_0\,(1+10^{-4}\times 0\cdot6377t+10^{-6}\times 0\cdot0807t^2)$.
$Sn_6\,Zn$: $V_t=V_0\,(1+10^{-4}\times 0\cdot6236t+10^{-6}\times 0\cdot0822t^2)$.
$Bi_{44}\,Sn$: $V_t=V_0\,(1+10^{-4}\times 0\cdot3793t+10^{-6}\times 0\cdot0271t^2)$.
$Bi\,Sn_2$: $V_t=V_0\,(1+10^{-4}\times 0\cdot4997t+10^{-6}\times 0\cdot0101t^2)$.
$Bi_{24}\,Pb$: $V_t=V_0\,(1+10^{-4}\times 0\cdot3868t+10^{-6}\times 0\cdot0218t^2)$.
$Bi\,Pb_2$: $V_t=V_0\,(1+10^{-4}\times 0\cdot8462t+10^{-6}\times 0\cdot0159t^2)$.
Cu+Zn (71 p. c. Cu) : $V=V_0\,(1+10^{-4}\times 0\cdot5161t+10^{-6}\times 0\cdot0558t^2)$.
$Au\,Sn_2$ $V_t=V_0\,(1+10^{-4}\times 0\cdot3944t+10^{-6}\times 0\cdot0289t^2)$.
$Au_2\,Sn_7$ $V_t=V_0\,(1+10^{-4}\times 0\cdot4165t+10^{-6}\times 0\cdot0263t^2)$.
$Ag_4\,Au$ $V_t=V_0\,(1+10^{-4}\times 0\cdot5166t)$.
$Ag\,Au$ $V_t=V_0\,(1+10^{-4}\times 0\cdot4916)$.
$Ag\,Au_4$ $V_t=V_0\,(1+10^{-4}\times 0\cdot3115+10^{-6}\times 0\cdot1185t^2)$.
Ag+Pt (66·6 p. c. Ag) $V_t=V_0\,(1+10^{-4}\times 0\cdot4246t+10^{-6}\times 0\cdot0322t^2)$.
Au+Cu (66·6 p. c. Au) $V_t=V_0\,(1+10^{-4}\times 0\cdot4015t+10^{-6}\times 0\cdot0642t^2)$.
Ag+Au (36·1 p. c. Ag) $V_t=V_0\,(1+10^{-4}\times 0\cdot4884t+10^{-6}\times 0\cdot0552t^2)$.
Ag+Au (71·6 p. c. Ag) $V_t=V_0\,(1+10^{-4}\times 0\cdot4413t+10^{-6}\times 0\cdot0130t^2)$.

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number of the zeros. I have also preferred keeping the exponents constant, adding, instead of altering them, a zero after the decimal point when required.

TABLE IV.—Formulæ for the Correction of the Linear Expansion by Heat of the Alloys.

$Sn_4Pb \quad L_t=L_0(1+10^{-4}\times 0{\cdot}2066t+10^{-6}\times 0{\cdot}0329t^2).$

$Pb_4Sn \quad L_t=L_0(1+10^{-4}\times 0{\cdot}2696t+10^{-6}\times 0{\cdot}0111t^2).$

$CdPb \quad L_t=L_0(1+10^{-4}\times 0{\cdot}3002t+10^{-6}\times 0{\cdot}0044t^2).$

$Sn_4Zn \quad L_t=L_0(1+10^{-4}\times 0{\cdot}2126t+10^{-6}\times 0{\cdot}02691t^2).$

$Sn_6Zn \quad L_t=L_0(1+10^{-4}\times 0{\cdot}2079t+10^{-6}\times 0{\cdot}0274t^2).$

$Bi_{44}Sn \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1264t+10^{-6}\times 0{\cdot}0090t^2).$

$BiSn_2 \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1666t+10^{-6}\times 0{\cdot}0034t^2).$

$Bi_{24}Pb \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1293+10^{-6}\times 0{\cdot}0073t^2).$

$BiPb_2 \quad L_t=L_0(1+10^{-4}\times 0{\cdot}2821+10^{-6}\times 0{\cdot}0053t^2)$

$Cu+Zn$ (71 p. c. Cu) $L_t=L_0(1+10^{-4}\times 0{\cdot}1720t+10^{-6}\times 0{\cdot}0086t^2).$

$AuSn_2 \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1315t+10^{-6}\times 0{\cdot}0096t^2).$

$Au_2Sn_7 \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1388t+10^{-6}\times 0{\cdot}0088t^2).$

$AgAu \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1722t).$

$AgAu \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1638t).$

$AgAu_4 \quad L_t=L_0(1+10^{-4}\times 0{\cdot}1038t+10^{-6}\times 0{\cdot}0395t^2).$

$Ag+Pt$ (66·6 p. c. Ag) $L_t=L_0(1+10^{-4}\times 0{\cdot}1415t+10^{-6}\times 0{\cdot}0107t^2).$

$Au+Cu$ (66·6 p. c. Au) $L_t=L_0(1+10^{-4}\times 0{\cdot}1338t+10^{-6}\times 0{\cdot}0214t^2).$

$Ag+Cu$ (36·1 p. c. Ag) $L_t=L_0(1+10^{-4}\times 0{\cdot}1628t+10^{-6}\times 0{\cdot}0182t^2).$

$Ag+Cu$ (71·6 p. c. Ag) $L_t=L_0(1+10^{-4}\times 0{\cdot}1471t+10^{-6}\times 0{\cdot}0433t^2).$

From the above the following conclusion is drawn—namely, that just as it may be said that the *specific gravity of an alloy is approximately equal to the mean specific gravities of the component metals,* so also from the foregoing we may deduce that the *volume which an alloy will occupy at any temperature between* 0° *and* 100° *is approximately equal to the mean of the volumes of the component metals at the same temperature,* or, in other words, *the cubical or linear coefficients of expansion by heat of an alloy between* 0° *and* 100° *are approximately equal to the mean of the cubical or linear coefficients of expansion by heat of the component metals.*

V. "On the Colouring and Extraction Matters of the Urine.—Part II." By EDWARD SCHUNCK, F.R.S. Received June 20, 1866.

See abstract, *anteà*, p. 1.

VI. "On the Absorption and Dialytic Separation of Gases by Colloid Septa." By THOMAS GRAHAM, F.R.S., Master of the Mint. Received June 20, 1866.

(Abstract.)

It appears that a thin film of caoutchouc, such as is furnished by varnished silk or the transparent little balloons of india-rubber, has no porosity, and is really impervious to air as gas. But the same film is capable of liquefying the individual gases of which air is composed, while oxygen and nitrogen, in the liquid form, are capable of penetrating the substance of the membrane (as ether or naphtha do), and may again evaporate into a vacuum and appear as gases. This penetrating power of air becomes more interesting from the fact that the gases are unequally absorbed and condensed by rubber, oxygen $2\frac{1}{2}$ times more abundantly than nitrogen, and that they penetrate the rubber in the same proportion. Hence the rubber-film may be used as a dialytic sieve for atmospheric air, and allows very constantly 41·6 per cent. of oxygen to pass through, instead of the 21 per cent. usually present in air. The septum keeps back, in fact, one-half of the nitrogen, and allows the other half to pass through with all the oxygen. This dialysed air rekindles wood burning without flame, and is, in fact, exactly intermediate between air and pure oxygen gas in relation to combustion.

One side of the rubber-film must be freely exposed to the atmosphere, and the other side be under the influence of a vacuum at the same time. The vacuum may be established within a bag of varnished silk, or in a little balloon, the sides being prevented from collapsing, by interposing a thickness of felted carpeting between the sides of the varnished cloth, and by filling the balloon with sifted sawdust. For commanding a vacuum in such experiments, the air-exhauster of Dr. Hermann Sprengel* is admirably adapted. It possesses the advantage that the gas drawn from the vacuum can also be delivered by the instrument into a gas-receiver placed over water or mercury. The "fall-tube" has merely to be bent at the lower end.

The surprising penetration of platinum and iron tubes by hydrogen gas, discovered by MM. H. Sainte-Claire Deville and Troost, appears to be connected with a power resident in the same and certain other metals, to liquefy and absorb hydrogen, possibly in its character as a metallic vapour. Platinum, in the form of wire or plate, at a low red heat may take up and hold 3·8 volumes of hydrogen, measured cold; but it is by palladium that the property in question appears to be possessed in the highest degree. Palladium foil from the hammered metal, condensed so much as 643 times its volume of hydrogen, at a temperature under 100° C. The same metal had not the slightest absorbent power for either oxygen or nitrogen. The capacity of fused palladium (as also of fused platinum) is considerably

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* Chemical Society's Journal, ser. 2, vol. iii. p. 9 (1865).

reduced; but foil of fused palladium, for which I am indebted to Mr. G. Matthey, still absorbed 68 volumes of gas. A certain degree of porosity may be admitted to exist in these metals, and to the greatest extent in their hammered condition. It is believed that such metallic pores, and indeed all fine pores, are more accessible to liquids than to gases, and in particular to liquid hydrogen. Hence a peculiar dialytic action may reside in certain metallic septa, like a plate of platinum, enabling them to separate hydrogen from other gases.

In the form of sponge, platinum absorbed 1·48 times its volume of hydrogen and palladium 90 volumes. The former of these metals, in the peculiar condition of platinum-black, is already known to take up several hundred volumes of the same gas. The assumed liquefaction of hydrogen in such circumstances appears to be the primary condition of its oxidation at a low temperature. A repellent property possessed by gaseous molecules appears to resist chemical combination, as well as to establish a limit to their power to enter the minuter pores of solid bodies.

Carbonic oxide is taken up more largely than hydrogen by soft iron. Such an *occlusion* of carbonic oxide by iron, at a low red heat, appears to be the first and a necessary step in the process of acieration. The gas appears to abandon half its carbon to the iron, when the temperature is afterwards raised to a considerably higher degree.

Silver has a similar relation to oxygen, of which metal the sponge, fritted but not fused, was found to hold in one case so much as 7·49 volumes of oxygen. A plate or wire of the fused metal retains the same property, but much reduced in intensity, as with plates of fused platinum and palladium in their relation to hydrogen.

VII. "Notes on the Rearing of *Tænia echinococcus* in the Dog, from Hydatids, with some Observations on the Anatomy of the Adult Worm." By Edward Nettleship, Mem. Royal Agric. Coll. Communicated by T. Spencer Cobbold, M.D. Received May 24, 1866.

On March 28th, 1866, I obtained from Clare Market the liver and lungs of a sheep containing numerous Echinococcus hydatids; in some the outer cyst was partly calcified, but all the hydatids contained clear fluid, and great numbers of scolices attached to the endocyst. Within two hours of the death of the sheep to which the organs belonged, I gave two or three of the smaller hydatids to a young dog, about six months old; first puncturing the hydatid and administering the collapsed cyst, and then making him drink the fluid of the cyst, in which some Echinococci were floating.

The next day I gave him a second feeding of the remaining hydatids; this second batch he threw up within half an hour of the feeding, but he afterwards swallowed the broken membranes again, and did not afterwards vomit. I was careful to administer the hydatids from both calcified and non-calcified cysts.

The animal remained perfectly healthy, and became very much fatter; he was fed for the most part on cooked kitchen refuse, but I cannot be positive that he never obtained any raw food.

On May 15th (the forty-seventh day after the first feeding) I killed the animal, and examined the intestines. In the first ten inches of bowel below the pylorus there were no Tæniæ; at that distance a single *Tænia echinococcus* appeared, moving actively; for the next two or three inches there were none, but at about fourteen inches below the pylorus several more appeared, and immediately after this they became so numerous as to present almost the appearance of distended lacteals; this continued for about a foot in extent, and then they gradually became less numerous, and ceased at about three feet from the pyloric orifice.

There were also four specimens of *T. marginata,* varying from two to three feet in length, and two of *T. cucumerina.*

The part of the intestine containing *T. echinococcus* was immediately put into dilute carbolic acid, and the worms not examined until two days subsequently; after that interval they were still tolerably adherent to the mucous membrane, though a good many had fallen off. On detaching them with needles, or examining those which had fallen off, nearly all were found to be quite destitute of hooks; but one (from which the outline sketch, Plate VIII. fig. 1, was taken) had a tolerably complete double row; in this specimen, however, the hooks and proboscis were inverted, while in most specimens the latter part was protruded.

In fig. 2 is a single hook of *T. echinococcus,* probably from the posterior circlet, magnified more highly.

Fig. 3 shows at *a,* a hook of the posterior circlet, and at *b,* one from the anterior circlet of an *Echinococcus-scolex* from an ox. There seems very little difference between corresponding hooks of the *scolex* and of the adult tapeworm, unless the latter be rather stouter and have larger processes.

On $\frac{1}{16}$th of a square inch, where the worms were thickest, I counted twenty-five of them; by calculation there were about twenty-two square inches of intestine covered in this way, allowing for the more thinly scattered parts at each end of the infected part; this gives a total of 8800 specimens of *T. echinococcus* in this dog's bowel.

I have examined carefully numerous specimens of these worms with the microscope, with especial reference to the arrangement of the sexual organs; the great majority are sexually mature, and contain a greater or less number of eggs. It is not easy to make out accurately the arrangement and connexions of the ovary, yelk-forming glands, and uterus, as described by Leuckart[*], but fig. 4 gives a tolerably correct representation, in the 3rd (immature) segment.

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[*] Vide Cobbold, "Entozoa," p. 256, 1864.

At *a* is the vagina, continued as an indistinct tube to *b*, which I suppose to be the seminal vesicle; immediately beyond this is a dark mass (*o*) containing spherical, highly refractive bodies; this is the ovary. On either side and in front of this are the yelk-forming glands (*e*), two somewhat indefinite lobular organs apparently communicating with the vagina in front of the seminal vesicle. At *c* and *d* are the globular testicles; *f* is the rudimentary vas deferens. Running forwards from the vagina is another narrower tube *g*, which passes quite to the anterior end of the segment, where it becomes at first slightly dilated (fig. 5, *u'*), and afterwards enlarged into the head of the uterus; a large spherical sac full of ova is readily seen with the naked eye in many sexually mature segments (fig. 4, *ut.*). The greater part of the body of this tube also becomes enlarged into the uterus (*u*, fig. 5), but I think that the hinder part continues tubular, and probably constitutes the " wide cavity " leading from *vagina to uterus*, described by Leuckart. In fig. 5 there is some appearance of this tube continued backwards from *u* (*a*, fig. 5), but I cannot follow it to any communication with the vagina (*va.*). The same is true in this specimen of the ovary (*ov.*) and seminal pouch (*s. p.*). The vitelligene glands (*e*, fig. 4) have disappeared in fig. 5, and the ovary has become less distinct, but the seminal pouch has developed somewhat, while in fig. 6, taken from a sexually mature segment, the latter organ is still plainer; the canal of the vagina has elongated, and shows besides the seminal pouch a smaller dilatation (*a*, fig. 6) nearer the orifice. In none of the mature segments have I been able to make out any communication between the vagina and uterus, either in front of or behind the seminal pouch.

Although Dr. Cobbold has succeeded in rearing a variety of tapeworms from their respective larvæ, the *Tœnia echinococcus* has not hitherto been reared in this country.

The importance of this creature in its pathological relations, and the desideratum of more information as to its anatomy, have induced me to place the foregoing facts on record. In conducting the investigation I have taken every precaution to prevent the escape and distribution of the ova and their contained proscolices.

EXPLANATION OF PLATE.—Fig. 5.

va. Vagina.	*a.* Tube (?) leading from uterus to vagina.
ov. Ovarium.	*t.* Testicles.
s. p. Seminal pouch.	*v. d.* Vas deferens.
u, u'. Uterus partly developed.	*c. p.* Cirrhus pouch.

VIII. " Observations on the Ovum of Osseous Fishes." By W. H. Ransom, M.D. Communicated by Dr. Sharpey. Received June 21, 1866.

(Abstract.)

In this paper the author has communicated the details of observations of which the principal results were stated in a short paper published in the

Fig. 1.

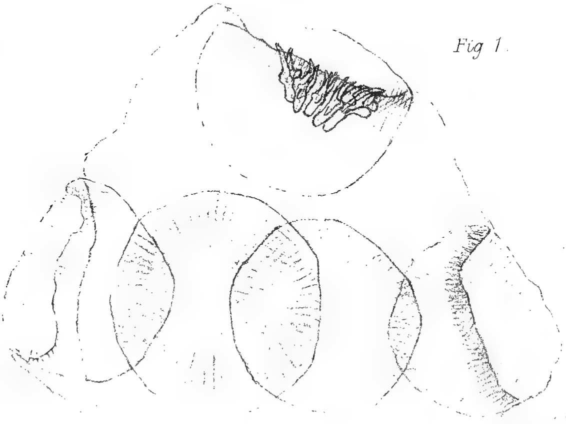


Fig. 2.

Fig. 3.

Tænia Echinococcus.

Fig. 4.

Fig. 5.

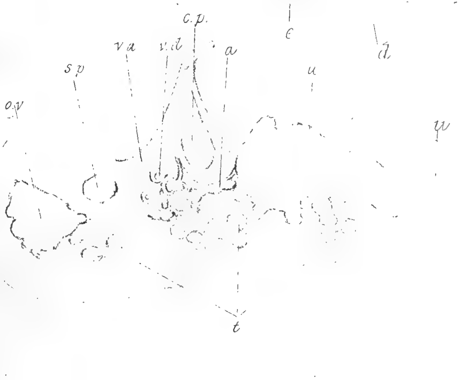
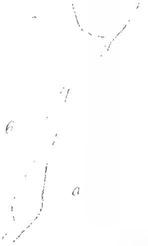


Fig. 6.



J. Basire. lith.

Proceedings of the Royal Society in 1854, and of further researches on the structure and properties of the egg in several species of osseous fishes. The methods employed in determining the functions of the micropyle, and in conducting the various inquiries entered upon are described. The development of the ovarian ovum is traced in two species of *Gasterosteus*, and the yelk-sac is shown to increase by interstitial growth, and not by apposition of layers on either surface. A minute description of the germinal vesicle and its contents is given, and the germinal spots are shown to be drops of a thick fluid substance, so apt to change their normally round form, and to vacuolate in their interior, that no perfectly indifferent medium was found in which to examine them. The primitive yelk first formed around the germinal vesicle is shown to differ in some of its chemical and physical properties from that of the ripe ovum; it is solid, and does not consist of two distinguishable portions. On its surface a yelk-sac was found in very early ova, but in the smallest eggs examined it could not be separated.

The reactions of a variety of albumen allied to myosin, which the author has found in variable proportions in the yelk of all the fishes, amphibia, and birds which he has examined, are described, the yelk of the salmon being selected for experiment. This substance, to which the name albumen C. is given provisionally, is remarkable, in addition to its being easily precipitable by water in excess, for forming under certain conditions a solution in dilute nitric acid not coagulable by boiling.

Some account is rendered of the reactions of an acid compound of phosphoric acid with an organic substance also met with in the yelk of various animals.

The phenomena which follow impregnation prior to the commencement of cleavage are described, and are shown to be chiefly due to the influence upon the yelk of water which has passed through the yelk-sac.

Some variations which occur in this respect in different species of osseous fishes are described, and the ova of *Gasterosteus* are shown to be remarkable in having a viscid mucoid covering derived from the oviduct, which prevents the imbibition of water through the yelk-sac, so that it only then enters and forms a breathing-chamber after impregnation, when it passes through the aperture in the apex of the micropyle; whereas in the eggs of salmon and in those of most other fishes, unimpregnated ova rapidly absorb water by the whole surface of the yelk-sac, the yelk contracting at the same time to form the breathing-chamber.

The concentration of the formative yelk, originally forming a thin layer over the whole yelk-ball, at the germinal pole is also proved to be due to the action of water, of which it requires a free supply sufficient to distend the yelk-sac, and to be independent of fecundation.

The contractions of the yelk are shown to be also independent of the action of the spermatozoids, and to be reactions following the entrance of water into the breathing-chamber; and this not only as regards the rhythmic waves, which pass over the surface of the food-yelk, but also the fissile contractility of the formative yelk, by virtue of which it cleaves into irregular and unsymmetrical masses, and which the author conceives to be regulated only by the influence of the seminal particles.

The cortical layer of the food-yelk or inner sac, which is shown to resist in a remarkable manner osmosis, is found to be the rhythmically contractile part, although requiring for its manifestation the presence of acid food-yelk upon its inner surface.

Evidence is given to show that the contractile property of the yelk of both kinds requires, as an essential condition of its manifestation, the presence of oxygen in the surrounding medium, and that the food-yelk, while the rhythmic waves are passing over it, consumes less than does the formative yelk, while regularly cleaving after fecundation; also that some product of oxidation is formed during these movements, which itself tends to check them, but which the author failed to determine the nature of.

Proofs are also given that a certain moderate rise of temperature increases the activity of these contractions. Experiments are related which show the extreme limits the yelk will bear without destroying them, and the temperature at which commencing chemical change prevents further contraction.

The reactions of the substance of the yelk under the stimulus of galvanism are recorded, and evidence afforded that the food-yelk and the cortical layer alone are excited to contraction by it, attempts made to induce fissile or other contractions of the formative yelk resulting in electrolysis of that highly unstable substance.

Experiments made to ascertain the effects produced by poisonous substances on the contractions of the yelk are recorded, and the general fact ascertained of the extreme indifference to such agents of yelk protoplasm.

Carbonic acid, however, is shown to destroy the contractility rapidly, and chloroform to arrest it for a time.

The process of cleavage is described, and experiments are given which show that oxygen in the surrounding medium is an essential condition of its occurrence. The influence of heat in quickening it, and the comparative indifference which it shows to the action of a galvanic current and to most poisons, are proved by a series of experiments, in which also the remarkable and destructive activity of carbonic acid is evidenced.

The author has considered the egg as a cell, its contents as a protoplasm, of which the firmer cortical layer is the equivalent of the primordial utricle, and the fluid food-yelk of the liquid contents, while the formative yelk is represented by the granular accumulation around the nucleus. Two stages or grades of development of protoplasm are conceived to be represented by the two forms of yelk, and a parallelism is attempted to be drawn between them and the stages of development through which many amœboid organisms pass, and which the author believes to have a wide, if not a universal existence in the organic world;

the lower grade represented by the homogeneous food-yelk with a cortical layer, and possessed of rhythmic contractility, passing into the higher represented by the formative yelk of a granular structure, and possessed of a fissile contractile property only.

IX. "Variations in Human Myology observed during the Winter Session of 1865-66 at King's College, London." By JOHN WOOD, F.R.C.S., Demonstrator of Anatomy. Communicated by Dr. SHARPEY. Received May 3, 1866.

In the present paper are given the results of observations, made with the greatest possible accuracy and care, of the muscular anatomy of thirty-four subjects, chiefly of the male sex, with an especial view to the study of the *combinations* of these abnormalities, and the directions in which they chiefly tend. To enable the reader more readily to comprehend these results, the author has tabulated them in the sheet appended to the paper. In the Table the names of the muscles placed at the head of each column refer to those in which more than one variety has been observed in the session. They will be found to correspond very closely with those given in the former papers by the author. In columns 4, 21, and 27 are placed those of which only one example has been met with. Some of these, however, are of much importance.

To explain the nature of the abnormality more precisely than could be done in the Table, a word or two will be necessary on such of the specimens as may be considered novel or typical.

Four columns are occupied by variations of the *head* and *neck*, the examples of which amount in the aggregate to twenty-two; some of the muscles in these may, however, strictly be considered as muscles also of the upper extremity, especially those in col. 3, which I have denominated *cleido-occipital*.

Col. 1. *Platysma myoides.*—The first of the two varieties noted (in subject 20) was connected with the inner side of the lower end of the normal muscle, the fibres passing in a broad band downwards and inwards, over the origin of the *sterno-cleido-mastoid*, the clavicle, and upper fibres of the *pectoralis major* to be inserted into the fascia covering the sternum as far down as the third costal cartilage.

The second (subject 29) was connected internally with the sternal fascia between the second and third costal cartilages, and crossing obliquely outwards and downwards over the lower fibres of the *pectoralis major* and axillary cavity, became attached to the tendon of the *latissimus dorsi*, exactly as we find its homologue, the *panniculus carnosus*, to do in the lower animals. This variety of the *Platysma* does not appear to have been previously recorded.

2. *Digastric.*—The two varieties of this muscle were found, as usual, in the anterior belly, which was double. In the first (No. 1) the redundant belly was attached by the median raphé to the one on the other side, and

was implanted upon the tendon in the usual manner. The second decussated with its fellow on the opposite side, and each became attached to a part of the digastric fossa on the opposite half of the mandible, as given in the author's first paper, and described by Henle and other anatomists.

3. *Cleido-occipital.*—The author has ventured to bestow this name upon a muscle which proved, when looked after, to be so common that not less than eleven specimens were found out of the thirty-four subjects. It will be best understood by reference to fig. *7 a*, in the subject of which it was found in conjunction with a *sternoclavicular* muscle. It is placed along the hinder border of the *sterno-cleido-mastoideus,* usually separated, however, by a distinct areolar interval from both the sternal and clavicular fibres of this muscle. It is attached below to the junction of the inner and middle thirds of the upper border of the clavicle, and above to the superior curved line of the occipital bone, close to the origin of the *trapezius* muscle. It is described by Meckel (Handbuch der mensch. Anatomie, 1816, p. 474) as an *accessory* to the *sterno-cleido-mastoid* sometimes met with. It may be considered as a lateral extension and separation of part of the clavicular fibres of the *sterno-cleido-mastoid,* which, in the normal arrangement, are crossed and entirely covered at the upper part by the sternal portion, and do not extend at their insertion beyond the mastoid portion of the temporal bone. The author has found that in the Guinea-pig and some other Rodents it constitutes a separate muscle, entirely distinct from the *sterno-mastoid,* carrying with it the whole of the clavicular fibres of the *sterno-cleido-mastoideus.* In the Dog and Cat, and probably in the other Carnivora, it forms part of the long muscle, the *cephalo-humeral.* In these animals the *cleido-mastoid* is a distinct muscle, joining with the *cephalo-humeral* at the rudimentary clavicle. In the Hedgehog, on the other hand, the *cleido-mastoid* is blended with the *sterno-mastoid,* while the *cleido-occipital* is placed as a distinct muscle behind it. In the Apes and Monkeys it is always present, but continuous with the hinder border of the true *sterno-cleido-mastoid,* with a more or less distinct intermuscular space*. The above peculiarities of its comparative anatomy, and the fact of its separate attachment to the occipital bone, instead of the mastoid portion of the temporal bone, have induced the author to propose the name here given to it.

4. Of the single specimens in this column the *levator claviculæ* was in most respects the counterpart to that given in the author's last paper; but it was found only on the left side, arising from the three upper cervical transverse processes in front of the *levator anguli scapulæ,* and inserted into the outer half of the clavicle, behind the anterior fibres of the *trapezius.* On the opposite side it was not found, but a very distinct *cleido-occipital* was present. This again was not found on the left side, but appeared to supply the place of the *levator claviculæ.* The *costo-fascialis cervicalis,* of subject 28, was in every respect like those described in the

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* Meckel describes, in the Marmot, the Squirrel, and some other Rodents, and in some Marsupials, two *cleido-mastoids,* of which the hinder corresponds entirely to the muscle here called *cleido-occipital* (Anatomie Compar. 1829–30, vol. vi. p. 163).

author's two last papers. The *sternalis brutorum* was a small development on the right side only, reaching from the third to the sixth costal cartilage. The rest of the muscles in this column scarcely need a more extended description than is there found.

No less than seventeen out of the twenty-seven columns denoting the different kinds of variety are occupied by those of the *arm,* of which there are seventy-one examples.

5. Epigastric slips of the *pectoralis major* presented various degrees of development of the so-called *chondro-epitrochlear* muscle of the Apes and Monkeys, but reaching only as far as the insertion of the rest of that muscle into the bicipital ridge, and terminating distinctly from it. None of them were so complete in their development as those described in the author's first paper. One specimen is seen in fig. 1 *a,* in significant combination with the ape-like variation placed in the next column.

6. The developments of the *pectoralis minor* given in this column are such as may easily be overlooked, but when closely sought for, as in the last session, have yielded no less than five specimens out of thirty-four subjects. The variety consists in the prolongation of the tendon of insertion as a flat tendinous slip, sometimes connected with a large portion of the muscular fibres, over the upper surface of the coracoid process, which is grooved distinctly for its reception. This tendon then joins that of the *supraspinatus* muscle, and blending with it and the capsular ligament, is implanted into the upper facet of the greater tuberosity of the humerus (see fig. 1 *b*). In the subject of the sketch (a female) another tendinous slip was directed upwards and outwards, and lost upon the coraco-acromial ligament. The insertion of the *pectoralis minor* into the shoulder-joint capsule is mentioned by Meckel (*op. cit.* p. 467), giving Gantzer as his authority*. This prolongation of the tendon to the humerus reaches to a greater extent in the Monkeys in proportion to the diminution in size of the coracoid process. In the Carnivora it is entirely inserted into the greater tuberosity, and blends more or less intimately with the *pectoralis major.* In subject 22 the upward direction of the insertion of the *pectoralis minor* becomes more marked as an insertion of the upper muscular fibres into the costo-coracoid membrane and the clavicle itself. The origin of the muscle was in this case higher than usual, reaching to the first intercostal aponeurosis. This upward development of the *pectoralis minor* is an approximation to the condition of its insertion in the Rodents, and, as the author believes, is a formation identical with the *sternoclavicular* muscle, and found in subject 27, column 21 (fig. 7).

7. In this column are given two opposite tendencies of development of the *latissimus dorsi.* Of the first are those not uncommon slips of communication between this muscle and the insertion of the *pectoralis major,* passing in front of the axillary vessels and nerves (see fig. 1 *c*), described by Meckel, Langer, and Gruber (*Achselbogen*). The author re-

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* De Souza describes a case in which the whole of the *pectoralis minor* was inserted into the capsule of the shoulder-joint (Gazette Médicale, 1855, No. 12).

Fig. 1.—Subject No. 5.

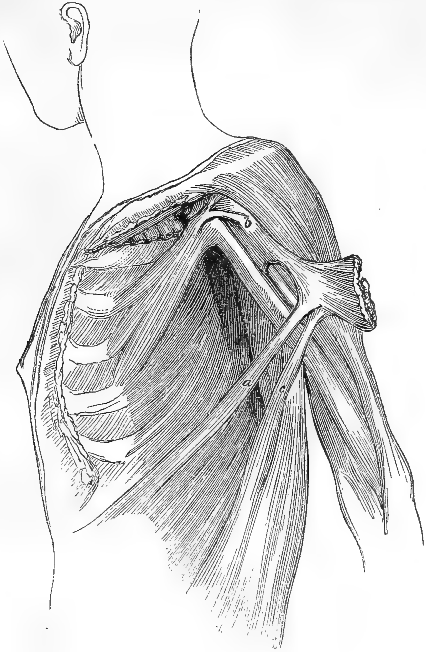


Fig. 2.—Subject No. 10.

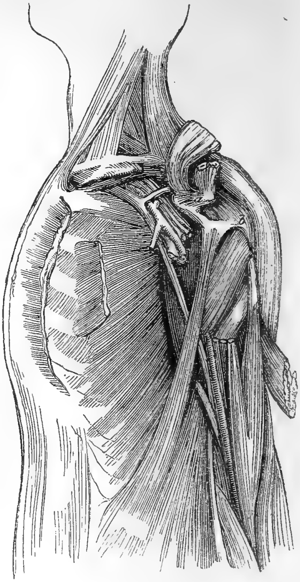


Fig. 5.—Subject No. 9.

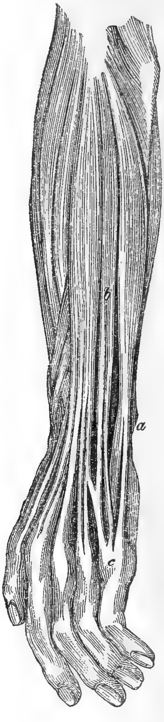
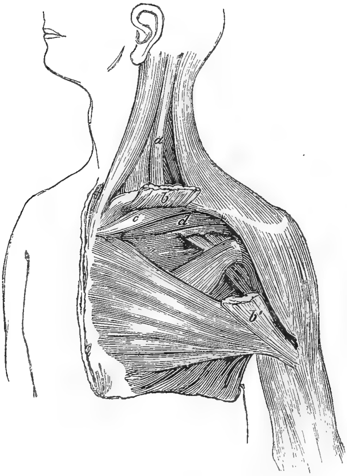


Fig. 7.—Subject No. 27.



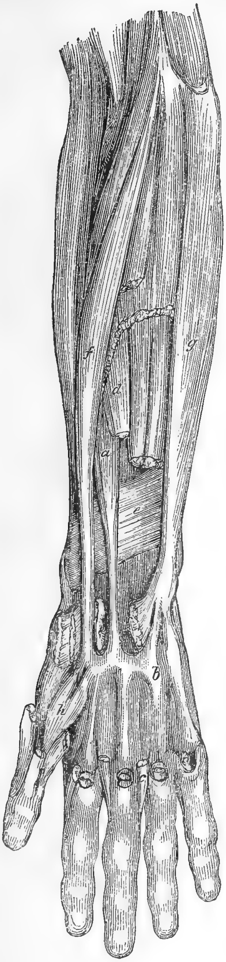


Fig. 3.—Subject No. 32.

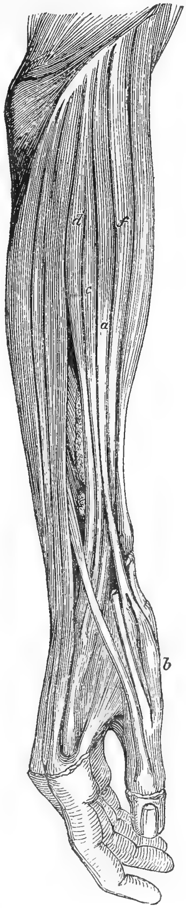


Fig. 4.—Subject No. 21.

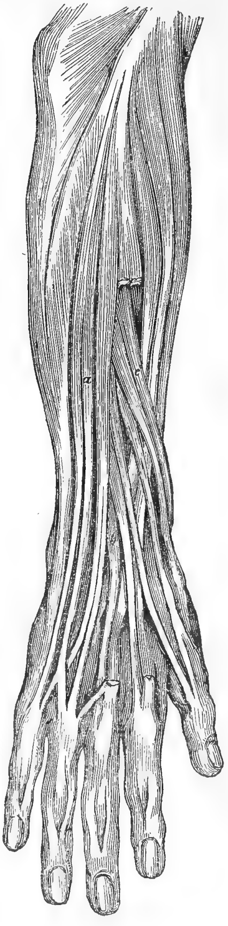


Fig. 6.—Subject No. 32.

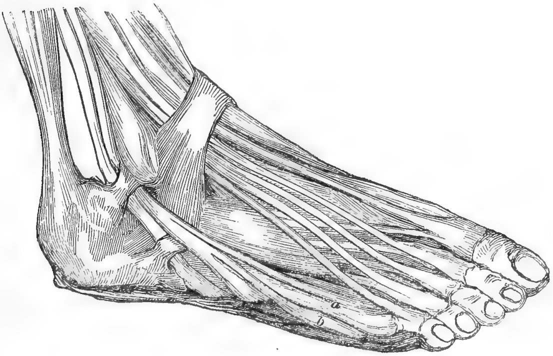


Fig. 8.—Subject No. 7.

gards these as imperfect developments of the so-called *dorsi-epitrochlear* muscle of the lower animals. None have been found this session extensively developed, and these only in three subjects, of which the last (No. 32) was one remarkable for the number of its muscular abnormalities.

The other varieties found were two specimens of the remarkable offset sent from the *latissimus* upwards towards the coracoid process, which the author described in his paper read two years ago to the Society, under the name of the *chondro-coracoid* muscle. It arises with the upper costal fibres of the *latissimus* from the ninth and tenth rib-cartilages, ascending so as to cross the axillary cavity obliquely outwards. In the specimen figured in the author's first series it was inserted with the *pectoralis minor* into the tip of the coracoid process. In subject 10 (fig. 2) it is inserted partly into the lower surface of that process (*a*), and partly into the capsular ligament of the shoulder with the tendon of the *supra-spinatus* (*b*). It will be observed to pass between the trunks of the axillary plexus, separating the posterior from the lateral cords. The latter and the vessels are cut to show the muscle. In subject 28 this muscle was connected with the origin of the *coraco-brachialis,* and passed with it to the tip of the coracoid. A similar slip of muscle, passing from the 5th, 6th, and 7th ribs to the muscles connected with the coracoid process, was observed by Theile (*Jourdan's Translation,* p. 204). In all the specimens the unvarying origin of this curious slip, and its relation to the one which joins with the insertion of the *pectoralis major*, and to the *chondro-epitrochlear* muscle of the lower animals, show it to be the result of an upward displacement of the same typical structure, ranging from the insertion of the *pectoralis major* to that of the *pectoralis minor.*

8. The increased number of the heads of the *biceps* in the two subjects in this column were of the more usual kind described by Meckel and others, arising from the fibres of the *brachialis anticus* and from the greater tuberosity of the humerus.

9. In subjects 14 and 17 the *brachialis anticus* presented a continuity of a large portion of its outer fibres of origin into those of the *supinator longus,* which is not uncommon, although not, as far as the author is aware, previously noted in the human subject. It is a very common arrangement in the Apes and Monkeys, assisting them very materially in climbing and twisting the body while hanging by the anterior extremities. In subject 30 there was a *brachio-fascialis* or quasi-third head of the *biceps,* similar to that described in former papers. On the opposite arm was found a curious fusiform muscle, springing high up from the *brachialis anticus,* and connected below with the *pronator radii teres* towards its insertion.

10. The *flexor sublimis digitorum* in subjects 5 and 13 gave off on th outer side of its coronoid origin a separate slender tendon, which becam the sole origin of a *first lumbricalis* muscle. In both, another *first lumbricalis* was given off from the usual place on the indicial tendon of th *perforans.* In No. 13 also a curious division of the abnormal *lumbricali*

occurred, into two parts, of which the innermost was implanted upon the tendon of the *perforatus,* near its division within the sheath of the index finger; while the outermost joined the normal *lumbricalis* near its usual place of insertion. Higher up in the arm another muscular slip connected the redundant tendon from the *sublimis* with the indicial tendon of the *profundus.* In subject 9 was a muscular connexion of the *flexor sublimis* with the origin of the *flexor longus pollicis,* like that described in the last paper.

11. The variations of the *flexor profundus digitorum* in three subjects consisted merely in a not uncommon distinct and superficial muscular origin from the coronoid process along with the fibres of the *sublimis,* such as is frequently met with in the lower animals. In subject 8 was found a tendon connecting the indicial part of the *flexor profundus* with the tendon of the *flexor longus pollicis.* In subject 20 this arrangement was reversed, the tendon going from the *flexor pollicis* above to the *flexor profundus* below. This variety is found in the next column.

12. In addition to the variety just mentioned the *flexor pollicis longus* gives, in subject 2, a tendon to the *first lumbricalis* on both sides.

13. The *lumbricales* have been found irregular in three other subjects besides those just mentioned, in which the *first* was seen to arise from the *flexor sublimis* and *flexor longus pollicis.* In subject 5 the *third lumbricalis* on the right arm was bifurcated, one half going to the ulnar side of the middle finger and the other to the radial side of the ring finger. Both sides of the long finger were thus provided with this muscle, an arrangement which was repeated in both the hands of the 22nd subject. On the right arm of the 32nd subject the whole of the *third lumbricalis* (see fig. 3 *c*) was inserted into the inner side of the long finger, while the *fourth* was entirely absent. This was also the case with the left arm of subject 5.

14. The name at the head of this column—*flexor carpi radialis brevis*—I have applied to a muscle which is given in fig. 3 *a,* drawn from subject 32. This subject was, from the number and character of its varieties, the most remarkable of the whole. In this case the muscle arises from the middle third of the front surface of the radius, between the *flexor longus pollicis* above (*d*) and the *pronator quadratus* (*c*) below. Passing under the annular ligament as a distinct rounded tendon, close to the carpal bones, inside of and parallel with the sheath of the tendon of the *flexor carpi radialis* (*f*), it is inserted partly into the os magnum, but chiefly into the base of the middle metacarpal bone, where it gave attachment to some fibres of the *flexor brevis pollicis* muscle. It is evidently a flexor of the third metacarpal bone, corresponding to the *flexor carpi radialis* of the second metacarpal. During the last session a precisely similar muscle was described by Dr. Norton, of St. Mary's Hospital, and exhibited by him at a meeting of the Zoological Society. The subject of figure 3 is further remarkable for a distinct slip of tendon from the *flexor carpi ulnaris* (*g*) to the base of the fourth metacarpal bone (seen at *b*). In this arm we

have, therefore, a flexor for each of the metacarpal bones, reckoning the *opponens* as one. But further than this, the outer fibres of the *extensor ossis metacarpi pollicis* in the same arm (see fig. 6 *c*) are separated from the rest by a cellular interval, and arise partly from the fascia covering the radial extensors and supinator longus. They are connected with a distinct tendon, which is implanted into the front part of the outer border of the base of the first metacarpal bone. Traction upon this tendon showed that its action was rather of the nature of flexion with abduction than of extension. This peculiarity of origin is noticed by Henle (Muskellehre). The arm from which figs. 3 and 6 were taken is now in the custody of the Curator of the Hunterian Museum. In fig. 3 is seen also the abnormal insertion of the *third lumbricalis* (*c*), the absence of the *fourth,* and at *h* the *first palmar interosseus* of the thumb, described by Henle. In fig. 6 the additional special extensors of the third and fourth fingers are given.

In subjects 13 and 31 were found muscles which are entirely homologous with the *flexor carpi radialis brevis* just described. They had exactly similar origins, were of the same shape and with like tendons, but were inserted into the inner side of the base of the *second* instead of the *third* metacarpal bones, one of them passing, with the tendon of the *flexor carpi radialis,* through the groove in the trapezium and annular ligament, but the other going outside of that sheath.

In subject 6 was found a large fusiform muscle, having its origin from the radius outside of that of the *flexor longus pollicis,* and reaching as high up as the oblique line below the *flexor sublimis.* Ending in a distinct, strong, and rounded tendon, it was implanted into that deeper portion of the annular ligament which separates the groove for the *flexor carpi radialis tendon,* and with this into the trapezoid, magnum, and base of the middle metacarpal bones. A muscle precisely similar to this was described by the author in his last paper (subject 1). Its resemblance in appearance, position, connexions, and influence upon the carpus, have induced him to class it with the foregoing *flexor carpi radialis brevis* as a variation of the same type of muscle, the flexor of the middle metacarpal bone.

15. The variations of the *radial extensors* found in this column are of two kinds, which appear to be closely allied. The first kind mentioned are the interchanging muscles frequently seen and recorded by anatomists. These, arising with one of the twin muscles, pass as distinct tendons over to the other, and become inserted with it. Of these, three were found. In the absence of a name the author has ventured to distinguish this form of muscle by that of the *extensor carpi radialis intermedius.* Of the other kind is found only one example. It is the *extensor carpi radialis accessorius* first recorded, named and described by the author in his former papers*. It has been chosen as the subject of fig. 4, taken from the right arm of subject No. 23, because of its coexistence with the *intermediate* form of the radial extensors, which was not the case in any of the specimens

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* This muscle seems to be present in about the proportion of 1 in 35 subjects.

before found. It arises from the condyloid ridge of the humerus, below the *extensor carpi radialis longior* (*f*), lies between that muscle and the *extensor intermedius* (*c*), which separates it from the *extensor carpi radialis brevior* (*d*). It has a distinct tendon of considerable size, which, crossing that of the *longior*, goes through the sheath of the extensors of the metacarpus and first phalanx of the thumb, and divides into two slips, one to be implanted into the base of the *first* metacarpal, and the other to give part origin to a double *abductor pollicis* (*b*). The tendon of the *intermedius* (*c*) can be seen to be implanted upon the *second* metacarpal with that of the *longior*. In the left arm of the same subject only the *intermediate* form was found. The close relation of these two forms is well seen in the figure. The *accessorius* has an origin somewhat similar to the *intermedius*, while its insertion and connexion with the *short abductor* is precisely similar to that often found in the tendon of *extensor ossis metacarpi pollicis*, with which the radial extensors have usually so close a connexion at its origin, as before alluded to in describing the redundancy of that muscle in subject 32 (fig. 6 *c*). If we suppose the germ of an *extensor intermedius* to become blended with that of the upper and outer portion of a double *extensor ossis metacarpi pollicis*, the result would be the *extensor accessorius* here described*.

16. The *extensor carpi ulnaris* in two subjects sent, in both arms, tendinous slips to the little finger, which were blended with the common extensor aponeurosis (fig. 5 *a*). This variety is mentioned by Meckel. Henle considers it as homologous with that of the *peroneus quinti* in the foot. In both subjects the arrangement of the slip was strikingly similar to that of the last-named abnormality, but in neither of them was the *peroneus quinti* found in the foot. In the same arms the *extensor minimi digiti* (*b*) was found provided with two tendons, both inserted, with a slip from the common extensor, into the dorsal aponeurosis of the fifth digit (*c*).

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* On looking over Meckel's description of the *extensor ossis metacarpi pollicis*, in reference to this subject, the author finds that he mentions the rare occurrence of a *double-bellied* long *abductor of the thumb*, arising from the outer condyle of the humerus, and inserted into the base of the *first phalanx* of the thumb (*op. cit.* Muskellehre, p. 517). This has evidently been the *extensor carpi radialis accessorius* of the author, passing entirely into the outer head of a double *abductor pollicis brevis*, without any insertion into the metacarpal bone, as found in one of the specimens figured in the author's first series.

In many of the lower animals, and occasionally in the human subject (Henle), the radial extensors are represented by one large muscle, which gives off two tendons to the second and third metacarpal bones respectively. This original connexion seems to be represented by the *intermediate* form of muscle just described. In the Anteater is found a muscle arising from the humerus above the long supinator, and inserted either into the ensiform bone or into the muscular substance of the palm (Meckel, Anatomie Compar., 1829–30, pp. 327–8, vol. vi.). In the *Echidna* and *Ornithorhyncus* a small muscle is found under the common extensor of the 2nd and 3rd metacarpal bones, which is considered by Meckel (*De Ornithorhynco*) to be a *supinator longus*. These may probably be the homologues of this muscle.

17. In three other subjects, besides the two last mentioned, the *extensor minimi digiti* was found to have a double tendon. In one the muscle also was doubled.

In subjects 5 and 32 the outermost of the tendons of this double muscle was inserted into the extensor aponeurosis of the *ring finger*, thus forming a special extensor of this digit (fig. 6 *a*), joining, before reaching it, with a slip from the common extensor, which directly afterwards again left it, carrying some of its fibres to join the little finger. This arrangement was described by the author in his first paper, and has been also noticed by Vesalius, Meckel, and Hallett in Man, and by Church in the Apes.

18. In this column are found two specimens of that differentiation of the *extensor indicis* muscle which results in a *proper extensor of the middle finger*. In fig. 6 (taken from the extensor aspect of the same arm as fig. 3, subject 32) is seen a complete double set of extensor tendons for each of the five digits, in addition to the interesting varieties found on the flexor side (including a whole set of flexors for the five metacarpal bones), and in other parts of the body. It was not found in the left arm. In subject 13, the *extensor medii digiti* was found in both arms, and, what is significant, it was associated in both with the *flexor carpi radialis brevis* before described, showing a special tendency to development of the muscles of the middle digit. A similar tendency is shown in subject 15, by a duplication of the tendon of the *indicator*, both tendons in this case being inserted into the first digit.

19. Of the *extensor brevis digitorum manus*, described in the author's last papers, fewer examples than usual have been found, none of them very complete. In subject 32 a single slip to the middle finger is found associated with the *proper extensor* and a *flexor of the metacarpal bone* of that digit; a combination which was present also in the remarkable subject (1) described in the last paper, and, with the exception of the proper extensor, in one other subject last session. It may be taken as a further proof of the specializing tendency in the middle finger in this subject.

20. Of the *interossei*, five specimens of differentiation are noted, chiefly belonging to the first, or *abductor indicis*. Two specimens of a *palmar interosseus* of the *thumb* are found in Nos. 5 and 32, both presenting numerous other variations.

21. Among the miscellaneous muscles of the arm the most noteworthy specimen is that of a *sternoclavicular* muscle, similar to that described by Haller, and more recently by Mr. Berkeley Hill. This muscle (given in fig. 7 *c*, from subject 27) arises by a thin tendon from the front of the manubrium sterni, just below the origin of the sterno-mastoid. Spreading as a muscular layer upwards and outwards under the clavicular fibres of the *pectoralis major* (*b b′*, cut and turned up in the figure), it is inserted into the lower border of the clavicle, just in front of the subclavius muscle, from which it is separated by the costo-coracoid membrane (*a*), extending nearly as far outwards as the origin of the *deltoid*.

This muscle has been found in Birds, Bats, and Moles. The author has also found it remarkably well-developed in the Guineapig and some others of the Rodentia. He believes it to be closely allied to the upward extension of the *pectoralis minor,* before alluded to, and to result from a differentiation of such an upward extension. In the subject of the sketch it was associated with a *cleido-occipital* (*a*), and with an increase in the number of tendons to the little finger.

In subject 26 was a curious muscular slip extending behind the axillary vessels and nerves, from the insertion of the *subscapular* muscle to the fascia covering the long head of the triceps, and derived from the tendon of the *latissimus dorsi.* Apparently this is an imperfect form of development of a short *coraco-brachialis* muscle, such as that described in a former paper.

In subject 13 was a high fascial origin of the *abductor minimi digiti,* from the lower third of the forearm, like that described and figured in the author's first series. This has been observed by Günther (Chirurgische Muskellehre, Taf. xx. Fig. V. 18).

A double *abductor pollicis brevis* was found in three cases, besides that in which it was connected with a slip from the *extensor carpi radialis accessorius* (given in fig. 4). In subject 23, and also in 32, was a double *extensor ossis metacarpi pollicis,* the tendon of one being inserted entirely into the annular ligament and origin of the *abductor pollicis.*

The rest of this column scarcely needs further description.

The six remaining columns are occupied by abnormalities of the muscles of the leg, of which there are thirty-nine examples.

22. *Peroneus tertius.*—Two out of five anomalies in this column result from the total absence of this characteristically human muscle, giving a very ape-like appearance to the foot. In No. 7 it was absent on the right side only (see fig. 8), in No. 16 on both sides. In subjects 11 and 32 a distinct tendinous slip from it was implanted into the base of the fourth metatarsal. In another, the tendon was doubled, though both were inserted into the fifth metatarsal, spreading towards the fourth.

23. *Peroneus quinti.*—In three out of five specimens found, this tendinous slip from the *peroneus brevis* to the extensor aponeurosis of the little toe was perfect, as described and figured in the last paper. In the remaining two, the tendinous slip from the *brevis,* instead of reaching the toe, became implanted upon the upper border of its metatarsal bone, near the front end (fig. 8 *a*). In both cases this slip supplied the place of the *peroneus tertius,* which was totally wanting, except in the left foot of subject 7, in which both the slip and the muscle was present, though small. In the subject of the figure the *peroneus brevis* tendon gives also a slip of origin to a bundle of muscular fibres which join the *abductor minimi digiti* as a separate muscle (*b*) on the outer side. This is a somewhat similar arrangement to that of a specimen given in the author's first series, in which the *peroneus quinti* was provided with a separate muscular belly on the outer border of the foot.

24. The *extensor longus primi internodii hallucis,* described in the author's first paper, was found in no less than ten out of the thirty-four subjects, in all, except one, in both legs. In seven the muscles and tendons were fully developed and distinct from the fibres of the *extensor proprius hallucis,* generally arising above, but sometimes below this muscle. In the remaining three (Nos. 11, 19, and 21) the abnormal muscle was represented by a tendon given from the *extensor proprius* near the ankle. In subject 21 this tendon was also, on the right side, contributed to by one from the *extensor longus digitorum.* In all the tendon was attached to the base of the first phalanx of the great toe, close to the joint, either distinct from or in union with that from the *extensor brevis digitorum pedis.*

25. *Flexor longus accessorius.*—The high origin of this muscle, by an additional head from the lower third of the fibula, or from the fascia covering the *flexor longus hallucis,* has been observed in three cases. In all it was provided with a distinct tendon, which joined separately in the union of the long flexor tendons in the middle of the sole. In subject 26 the *flexor accessorius* was made up of *four* distinct heads. 1. The long head as above described; 2, another tapering fleshy belly from the upper part of the os calcis and insertion of the *plantaris* tendon, in front of the tendo Achillis, and ending in a distinct tendon; 3, the usual "massa carnea" from the hollow surface of the os calcis; and 4, the outer tendinous slip from the long plantar ligament. These all uniting, joined with a large slip from the tendon of the *flexor longus pollicis,* to form a distinct deep or third set of flexor tendons passing to the four outer toes. Each of these joined that of the *perforans* in the inside of the sheath, about the first joint.

26. *Abductor ossis metatarsi quinti.*—This muscle, as described and figured by the author in former papers, was found only in seven subjects this session, in all in both feet. This is much less than the proportion found last session. In two out of the five *females* dissected, they were found well developed, of a fusiform shape, arising from the outer tubercle of the os calcis, and inserted by a distinct rounded tendon into the base of the fifth metacarpal bone. In five *males* only, out of the twenty-nine dissected, were they found as muscles distinct from the fibres of the *abductor minimi digiti.*

In the previous session the proportion of female subjects to males was very much greater than in the last. The specimens of this muscle found were also much more numerous, so much so as to be estimated by the author at nearly half the number of subjects. Whether this remarkable difference depends upon the sex, or is accidental, must be decided by future observations.

27. In the miscellaneous column we have nine single specimens, as compared with eleven or twelve in the arms, and six or seven in the head and neck.

In a female (31) was an areolar separation of the front fibres of the *gluteus minimus* forming a *scansorius* muscle, like that described in a former paper. In subject 30 the *perforatus* tendon of the little toe was derived from the *flexor accessorius,* as seen also last session. In subject 7, the same tendon is derived from the fifth tendon of the *perforans,* as found in the Apes and Monkeys. The two last appear to be, respectively, the imperfect or transitional, and the complete stages of this significant change. Two varieties, which I have not found recorded by any anatomical writer, were noticed in subjects 12 and 26. In the former the *abductor hallucis,* and in the latter the *flexor brevis hallucis* sent a considerable muscular slip to the base of the first phalanx of the second toe. In the former the slip passed deeper than the *transversus pedis* muscle, and in the latter superficial to it.

In subject 25 the *third lumbricalis* took origin from the tendon of the *perforatus* instead of the *perforans,* presenting an analogue to the origin of the first *lumbricalis* in the hand from a tendon of the *flexor sublimis perforatus* in subjects 5 and 13.

In subject 2 the fourth *plantar interosseus* arose from a slip of the tendon of the *peroneus longus,* as in the instance described and figured in the author's last paper. In subject 16 was a curious double origin of the *adductor longus femoris,* the abnormal head arising with the fibres of the *pectineus.*

In reference to the *combinations* of the above muscular variations an inspection of the Table will show the following points:—

First, that only in *two* subjects out of the thirty-four examined were *no* muscular abnormalities found; *i. e.* no deviations from the ordinary type sufficiently striking to be recorded. It is, indeed, highly probable that variabilities of every kind are limited only by the possibilities of the permutations and combinations of the whole of the structures of the human body. It will be observed also that the great majority of the abnormalities were *symmetrical,* or found on both sides.

Secondly, that of the total number of muscular variations, 132 (not reckoning both sides when alike), 71, or more than one half, are found in the arms. If we reckon with these those muscles which, though found in the neck, act chiefly upon the clavicle, a bone of the upper extremity, viz. the *cleido-occipital* and the *levator claviculæ,* we shall have 12 more to add to the 71, increasing the proportion of the arm muscles, and diminishing those proper to the head and neck to 10. The number of abnormalities in the legs amount to 39, or rather more than half the number of those in the arms; while in the abdomen and lower part of the trunk not one is recorded, though, of course, some may have escaped observation.

Thirdly. The greatest number of abnormalities combined in the same individual is 14 (in subject 32), a very muscular male, in whom the proportions are 10 in the muscles of the arms (including the *cleido-occipital*); 3 in those of the legs, and 1 only in the head and neck. The similarity between this subject and No. 1 of the last year's paper is remarkable. In the latter the number of departures from the ordinary type was 16, of which all except 7 were found in the arms (including the *levator claviculæ*), 5 in the legs, and 2 in the head and neck. With the exception of the *levator claviculæ*, *costo-fascialis*, and *supra-costalis*, found in the last-mentioned subject, but not in No. 32, the correspondence (especially in the arms) between the different *lines of abnormal departure* also is sufficiently close to become significant, as the reader will have gathered in going through the preceding pages. In subject 4 of the Table, which presented the only specimen of the *levator claviculæ* found this session, only one other abnormality was found, viz. the *abductor of the fifth metatarsal bone*. This was also found associated with it in last year's subject, which presents otherwise a marked contrast in the number of its abnormalities.

In Nos. 5 and 7, the one a male and the other a female, there are nine variations respectively, of which, in No. 5, there is but one which is not in the arm; and in No. 7, four in the legs and three in the arms. In No. 2 are eight abnormalities, of which six are in the arms and the rest in the legs. In No. 26 are found seven, of which three are in muscles belonging to the upper extremity, and four in the legs. In No. 13 are six, all in the arms. In thirteen subjects none were found in the legs; and in four they were found in the legs only, to the extent, in one case, of four examples; and in all four subjects highly characteristic. None were found in the head, neck, or trunk only*.

The extent of *correspondence in combination* of varieties in the subjects arranged in the Table (taken in the horizontal lines) cannot be said to be striking. The variations seem to crop out here and there without much reference to each other. This may, however, be partly owing to the comparatively small number reviewed, and we should scarcely be safe in drawing deductions from it before a much greater number of subjects are treated in a similar way.

The correspondence seems to be the greatest in the arm and hand, which here also assume a prominence over the rest of the frame. This, however, may be due to the greater number of instances found in the upper extremity.

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* In estimating the proportion of the abnormalities contained in the Table, the numbers and names at the head of the columns, together with those down the miscellaneous columns, must be compared with the total number of the muscles in the corresponding parts of the human body. Thus, taking the number of the voluntary muscles of the head, neck, trunk, and perineum, excluding those of the back, internal ear, larynx, and the intercostals (as subject to minor irregularities which have not been noted), we have about 72. Comparing these with the kinds of variety in the Table, viz. 10, we have a proportion of about 1 in 7. In the upper extremity we have 60 muscles; of lines of variation we have 26, or nearly half. In the lower extremity we have 61 muscles; of varieties 14, or not quite one-fourth.

The varieties classed in the Table include the greater number of those that have been previously observed by the author and others. *Ten* only, which have been mentioned in former papers as being subject to other irregularities than duplication and deficiency, are absent from this list.

This clearly shows that notable departures from the ordinary type of the muscular structures run in definite *grooves* or *directions*, which must be taken to indicate some unknown factor, of much importance to a comprehensive knowledge of general and scientific anatomy.

[*To face p.* 243.]

TABLE OF VARIETIES

	Sex.	Platysma.	Digastric.	Cleido-occipital.	St sp	or nis.	Flexor profundus.	Flexor longus pollicis.	Lumbricales.	
		1.	2.	3.		.	11.	12.	13.	
1.	M.	... ...	2 ant. bellies	...	...	...	... ...	... ...	... ...	...
2.	M.	... ...	... ...	...	...	...	... ...	B. to 1st lumbric., *c*.	B. 3rd bifurcated.	...
3.	F.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
4.	M.	... ...	... ...	R.	L.	...	... ...	... ...	... ...	...
5.	F.	... ...	... ...	...	R. br	1st bric.	... ...	... ...	R. 3rd bif. L. no 4th, *i*.	...
6.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	R. a
7.	M.	... ...	2 ant. bell. *m*.	...	L.	...	... ...	... ...	... ...	...
8.	M.	... ...	... ...	...	...	...	B. to flexor pollicis.	... ...	... ...	...
9.	M.	... ...	... ...	B.	R. p	exor cis.	... ...	... ...	... ...	...
10.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
11.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
12.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
13.	M.	... ...	... ...	...	...	1st bric.	B. fr. Coron'd. pr., *a′*	... ...	... ...	R. 2
14.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
15.	M.	... ...	... ...	...	...	...	B. fr. Coronoid. pr.	... ...	... ...	...
16.	M.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
17.	M.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
18.	F.	... ...	... ...	...	st	...	... ...	... ...	... ...	...
19.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
20.	M.	B. sternal slip, *g′*.	... ...	...	...	...	... ...	B. to flexor prof., *h′*	... ...	...
21.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
22.	M.	... ...	... ...	B.	...	...	B. fr. Coronoid. pr.	... ...	B. 3rd bifurcated.	...
23.	M.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
24.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
25.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
26.	M.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
27.	M.	... ...	... ...	B.	...	...	... ...	... ...	... ...	...
28.	M.	... ...	... ...	B.	B	...	... ...	... ...	... ...	...
29.	M.	L. to latiss. dorsi.	... ...	...	...	...	... ...	... ...	... ...	...
30.	M.	... ...	... ...	...	...	...	... ...	... ...	... ...	...
31.	F.	... ...	... ...	...	...	...	... ...	... ...	... ...	R
32.	M.	... ...	... ...	B.	s	...	... ...	... ...	R. 4th, abs. 3rd to m. *w′*	R
		2	2	11			4	2	4	

The figures which are placed at the end of each line in the Table refer to the number of varieties recorded in each entire subject. Those at the bottom of each column refer to the number of each variety of muscular abnormality.

EXPLANATION

Of the *Abbreviations* and *References* in the Table.

R. Indicates the right side of the body on which the abnormality was found.

L. Indicates the left side of the body on which the abnormality was found.

B. Indicates both sides of the body on which the abnormality was found.

a. Detached muscular slip from 6th cartilage, with separate insertion into humerus. Also in No. 5.

b. Muscular slip from *latissimus dorsi* to insertion of *pectoralis major*. Also in Nos. 5 and 32.

c. Tendon from *flexor longus pollicis*, giving origin to first lumbricalis.

d. Single muscle with two tendons, both inserted into little finger. Also in Nos. 9, 25, and 27.

e. *Palmaris longus*, with belly of muscle below instead of above.

f. Fourth *plantar interosseus* arising from tendon of *peroneus longus*.

g. *Extensor carpi radialis intermedius* arising (muscular) with *longior*, and inserted tendinous with *brevior*, or *vice versâ*. Also in No. 5, and the right side of No. 21.

h. *Pectoralis minor*, giving tendinous slip to greater tuberosity of humerus; joining with the tendon of *supraspinatus* and capsular ligament.

i. (Right side) third *lumbricalis* bifurcated to opposed sides of middle and ring-finger. (Left side) no *lumbricalis* to little finger. Also seen in No. 22.

j. *Extensor minimi digiti* gave a tendon to the ring-finger on both sides. Also in No. 32.

k. A first *palmar interosseous* (of Henle), under *flexor brevis* to pollex.

l. *Flexor carpi radialis brevis*, vel *profundus*, as a fusiform muscle, arising from the oblique line of radius, and inserted into the annular ligament.

m. Double anterior belly of *digastric*, decussating across the median line.

n. Muscular slip from *complexus* to *rectus capitis posticus major*.

o. Detached muscular slip from epitrochlea to olecranon over ulnar nerve.

p. Tendinous slip from *peroneus brevis* to upper border of fifth metatarsal.

q. *Flexor longus accessorius* muscular head arose from deep fascia covering *flexor longus digiti*.

r. *Perforatus* tendon of fifth toe arose from tendon of *perforans*.

s. Double *stylo-pharyngeus*, one behind the other.

t. Detached slip of *pectoralis major*, arising from abdominal tendon at epigastrium, and inserted separately into tendon at humerus. Also in Nos. 9 and 32.

u. Tendinous slip prolonged from *extensor carpi ulnaris* to *extensor* tendon of little finger. Also seen in No. 27.

v. Detached slip from *latissimus dorsi* at ninth rib, to coracoid process at insertion of *pectoralis minor* (*chondro-coracoid*).

w. *Extensor longus primi internodii hallucis*, arising from fibula and interosseous ligament above *extensor proprius hallucis*. Also in Nos. 18, 24, 25, 26, and 30.

x. Tendinous slip of insertion of *peroneus tertius* to base of fourth metatarsal, as well as its usual insertion into the fifth. Also in No. 32.

y. *Extensor longus primi internodii hallucis* tendon given off from that of *extensor proprius hallucis*. Also in Nos. 19 and 21.

z. *Abductor hallucis* sent a large slip to base of first phalanx of second toe.

a'. Muscular slip from coronoid process to *flexor profundus digitorum*. Also in the two next notices in the same column.

b'. A distinct and separate *extensor of the middle digit*: in the next notice in the same column the *indicator* had two tendons, both inserted into the forefinger.

[*To face p.* 243.]

TABLE OF VARIETIES IN HUMAN MYOLOGY.

	Sex.	Platysma.	Digastric.	Cleido-occipital.	Sundries: single specimens.	Slips of pectoralis major (detached).	Pectoralis minor.	Slips of latissimus dorsi.	Biceps.	Brachialis anticus.	Flexor sublimis.	Flexor profundus.	Flexor longus pollicis.	Lumbricales.	Flexor carpi radialis brevis.	Extensor carpi radialis.	Extensor carpi ulnaris.	Extensor minimi digiti.	Extensor indicis and medii digiti.	Extensor brevis digit. manûs.	Interossei.	Sundries: single specimens.	Peroneus tertius.	Peroneus quinti.	Exten. long. pr. intern, hallucis.	Flexor longus accessorius.	Abd. os. met. quinti.	Sundries: single specimens.	No. in each subject.
		1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.	27.	
1.	M.	...	2 ant. bellies	...	...	B. condro-epitroch. *a.*	...	B. to pectoralis major, *b.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	3
2.	M.	...	...	...	...	...	...	...	...	...	...	...	B. to 1st lumbric., *c.*	B. 3rd bifurcated.	...	...	...	B. 2 tendons, *d.*	...	L. 2 slips...	B. 1st p. bipenif.	palm. long. inverted, *e.*	...	...	...	...	B.......	4th plant. inteross., *f.*	8
3.	F.	...	...	B.	...	...	...	...	...	...	...	...	...	...	...	B. intermedius, *g.*	...	...	...	...	...	...	...	...	...	...	...	...	2
4.	M.	...	...	R.	L. levat. clavic.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B.	...	3
5.	F.	...	...	...	R. sternalis brutorum.	L. chondro-epitroch.	B. to humerus, *h.*	L. to pectoralis major.	...	...	B. to 1st lumbric.	...	...	R. 3rd bif. L. no 4th, *i.*	...	B. intermedius.	...	B. to ring finger, *j.*	...	...	B. palmar to pollex, *k.*	...	...	...	...	...	...	...	9
6.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	R. rad. to ann. ligt., *l.*	...	...	...	...	...	...	...	...	...	...	...	...	...	1
7.	M.	...	2 ant. bell. *m.*	...	L. compl., *n*	B. epigastric, *t.*	...	...	B. 3 hds	...	...	...	...	...	...	...	...	...	...	...	...	R. epitr. to olecr., *o.*	R. absent	B. to 5th metac., *p.*	...	B. from fasc.	...	perforat. of 5th, *r.*	9
8.	M.	...	...	...	...	...	B. to humerus.	...	...	...	...	B. to flexor pollicis.	...	...	...	...	...	...	...	...	...	...	...	...	...	B. from fib.	...	...	3
9.	M.	...	...	B.	R. stylo-pharyng., *s.*	B. epigastric, *t.*	...	...	...	...	B. to flexor pollicis.	...	...	...	...	...	B. to little finger, *u.*	B. 2 tendons	...	...	...	...	...	...	...	...	...	...	6
10.	M.	...	...	...	...	...	...	L. to pectoralis minor, *v.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. above ext. prop., *w.*	...	...	...	2
11.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. to 4th metac., *x.*	B. complete	B. from ext. prop., *y.*	...	...	...	3
12.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B.	add. hallucis, *z.*	2
13.	M.	...	...	...	...	...	B. to humerus.	...	...	...	B. to 1st lumbric.	B. fr. Coron'd. pr., *a'*	...	...	R. rad. to 2nd metac.	...	...	...	B. ex. med. dig. *b'.*	...	...	L. abd. min. dig., *c'.*	...	...	...	...	...	...	6
14.	M.	...	...	...	...	...	...	...	...	B. to supinator long., *d'*	...	...	...	...	...	...	...	...	...	...	...	L. flex. br. pol., *e'.*	...	...	...	...	...	...	2
15.	M.	...	...	...	...	...	...	...	...	...	...	B. fr. Coronoid. pr.	...	...	...	...	...	...	B. 2 tendons	...	...	...	...	...	...	...	...	...	2
16.	M.	...	...	B.	...	...	B. to humerus.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. absent.	B. to 5th metac.	...	...	...	Doub. add. longus.	5
17.	M.	...	...	B.	...	...	...	...	...	B. to supinator long.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
18.	F.	...	...	...	stylo-glossus, *f'.*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. double abd. pol.	...	...	B. above ext. prop.	...	...	...	3
19.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. double abd. pol.	...	...	B. from ext. prop.	...	...	...	2
20.	M.	B. sternal slip, *g'.*	...	...	...	...	...	...	...	...	...	...	B. to flexor prof., *h'*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
21.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	L. accessorius, *i'.* R. intermedius.	...	...	...	...	Differentiated.	...	...	...	B. from ext. prop.	...	...	...	4
22.	M.	...	...	B.	...	...	B. to clavicle, *k'.*	...	...	...	...	B. fr. Coronoid. pr.	...	B. 3rd bifurcated.	...	...	...	...	...	...	...	...	...	...	...	...	B.	...	5
23.	M.	...	...	B.	...	...	...	...	...	...	...	...	...	...	...	...	...	B. 2 muscles	...	...	...	ext. oss. met. doub., *l'.*	...	...	B. complete	...	...	...	4
24.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. double tendon	...	B. complete	...	B.	Flex. long. halluc., *m'.*	4
25.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	B. 2 tendons	...	...	...	...	...	...	B. complete	...	...	3rd lumbricalis, *n'.*	3
26.	M.	...	...	B.	...	...	...	...	B. 4 hds	...	...	...	...	...	...	...	...	...	...	...	...	L. subscap., *p'*	...	B. complete	B. complete	R. from fib	...	Flex. brev. halluc., *o'.*	7
27.	M.	...	...	B.	...	...	...	...	...	...	...	...	...	...	...	...	B. to little finger.	B. 2 tendons	...	...	B. 1st dors. double.	L. st. clav., *q'.*	...	...	...	...	...	...	5
28.	M.	...	...	B.	B. costo-fascialis, *r.*	...	...	B. to coraco-brachialis, *s'.*	...	...	...	...	...	...	...	...	...	...	...	...	...	B. double abd. pol.	...	...	...	...	...	...	4
29.	M.	L. to latiss. dorsi.	...	...	...	...	...	...	...	B. to pron. teres, *t'*	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
30.	M.	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	L. complete	...	...	R. perf. of 5th toe, *u'*	2
31.	F.	...	...	...	...	...	...	...	...	...	...	...	...	...	R. rad. to 2d metac.	...	...	...	...	...	...	...	...	...	...	...	B.	B. scansorius, *z'*	3
32.	M.	...	...	B.	splenius colli, *v'.*	L. epigastric.	...	B. to pectoralis major.	...	...	...	...	...	R. 4th, abs. 3rd to m. *w'*	R. rad. to 3d metac.	...	...	B. to ring. finger.	R. ext. med. dig.	R. to mid. finger.	R. 1st dors. double, *x'*	R. flex. carp. uln., *y'.*	B. to 4th met.	B. complete	...	...	B.	...	14
		2	2	11	7	5	5	5	2	3	3	4	2	4	4	4	2	7	3	2	5	11	5	5	10	3	7	9	132.

The figures which are placed at the end of each line in the Table refer to the number of varieties recorded in each entire subject. Those at the bottom of each column refer to the number of each variety of muscular abnormality.

EXPLANATION

Of the *Abbreviations* and *References* in the Table.

R. Indicates the right side of the body on which the abnormality was found.

L. Indicates the left side of the body on which the abnormality was found.

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b. Muscular slip from *latissimus dorsi* to insertion of *pectoralis major.* Also in Nos. 5 and 32.

c. Tendon from *flexor longus pollicis,* giving origin to first lumbricalis.

d. Single muscle with two tendons, both inserted into little finger. Also in Nos. 9, 25, and 27.

e. *Palmaris longus,* with belly of muscle below instead of above.

f. Fourth *plantar interosseus* arising from tendon of *peroneus longus.*

g. *Extensor carpi radialis intermedius* arising (muscular) with *longior,* and inserted tendinous with *brevior,* or *vice versâ.* Also in No. 5, and the right side of No. 21.

h. *Pectoralis minor,* giving tendinous slip to greater tuberosity of humerus; joining with the tendon of *supraspinatus* and capsular ligament.

i. (Right side) third *lumbricalis* bifurcated to opposed sides of middle and ring-finger. (Left side) no *lumbricalis* to little finger. Also seen in No. 22.

j. *Extensor minimi digiti* gave a tendon to the ring-finger on both sides. Also in No. 32.

k. A first *palmar interosseous* (of Henle), under *flexor brevis* to pollex.

l. *Flexor carpi radialis brevis,* vel *profundus,* as a fusiform muscle, arising from the oblique line of radius, and inserted into the annular ligament.

m. Double anterior belly of *digastric,* decussating across the median line.

n. Muscular slip from *complexus* to *rectus capitis posticus major.*

o. Detached muscular slip from epitrochlea to olecranon over ulnar nerve.

p. Tendinous slip from *peroneus brevis* to upper border of fifth metatarsal.

q. *Flexor longus accessorius* muscular head arose from deep fascia covering *flexor longus digiti.*

r. *Perforatus* tendon of fifth toe arose from tendon of *perforans.*

s. Double *stylo-pharyngeus,* one behind the other.

t. Detached slip of *pectoralis major,* arising from abdominal tendon at epigastrium, and inserted separately into tendon at humerus. Also in Nos. 9 and 32.

u. Tendinous slip prolonged from *extensor carpi ulnaris* to *extensor* tendon of little finger. Also seen in No. 27.

v. Detached slip from *latissimus dorsi* at ninth rib, to coracoid process at insertion of *pectoralis minor* (*chondro-coracoid*).

w. *Extensor longus primi internodii hallucis,* arising from fibula and interosseous ligament above *extensor proprius hallucis.* Also in Nos. 18, 24, 25, 26, and 30.

x. Tendinous slip of insertion of *peroneus tertius* to base of fourth metatarsal, as well as its usual insertion into the fifth. Also in No. 32.

y. *Extensor longus primi internodii hallucis* tendon given off from that of *extensor proprius hallucis.* Also in Nos. 19 and 21.

z. *Abductor hallucis* sent a large slip to base of first phalanx of second toe.

a′. Muscular slip from coronoid process to *flexor profundus digitorum.* Also in the two next notices in the same column.

b′. A distinct and separate *extensor of the middle digit*: in the next notice in the same column the *indicator* had two tendons, both inserted into the forefinger.

c'. High origin of *abductor minimi digiti* from fascia of forearm.
d'. Muscular slip connecting the *brachialis anticus* with the *supinator longus.* Also in the next notice in the same column.
e'. Superficial slip connecting the two portions of *flexor brevis pollicis.*
f'. Origin of *stylo-glossus* from the angle of the lower jaw.
g'. Subcutaneous slip from sternal fascia to cervical fascia.
h'. Musculo-tendinous slip from *flexor pollicis longus* to indicial portion of *profundus.*
i'. *Flexor carpi radialis accessorius,* a separate muscle connected with the origin of *longior,* to be inserted into the base of the metacarpal of *pollex,* giving a slip to *abductor pollicis.*
k'. Slip of *pectoralis minor* inserted into costo-coracoid membrane and clavicle.
l'. Double muscle, the lower inserted into annular ligament and origin of *abductor pollicis.*
m'. *Flexor longus hallucis* exchanged slips with *flexor longus digitorum.*
n'. *Third lumbricalis* arose from *flexor perforatus* instead of *perforans.*
o'. *Flexor brevis hallucis* sent a large slip to base of first phalanx of second toe.
p'. Muscular slip from insertion of *subscapularis* to the neck of the humerus and tendon of *latissimus,* an imperfect *coraco-brachialis brevis.*
q'. A distinct muscle from front of manubrium to lower border of clavicle, under fibres of *pectoralis major* (*Sterno-clavicular*). *Subclavius* also present.
r'. Distinct muscle arising from first rib with *sterno-thyroid,* and inserted into cervical fascia under *sterno-cleido-mastoid* (*costo-fascialis cervicalis*).
s'. Muscular slip from *latissimus dorsi* to join *coraco-brachialis* just below coracoid process, across the vessels of the axilla.
t'. On the left side a muscular slip from *brachialis anticus* to the fascia of the forearm; on the right side a fusiform muscle forming a high origin of the *pronator rad. teres.*
u'. *Perforatus* tendon of little toe from *flexor accessorius.*
v'. Muscular slip from *splenius colli* to *serratus magnus.*
w'. Third *lumbricalis* to inner side of *middle* finger. Fourth absent altogether.
x'. First *dorsal interosseus* double. Also in 27.
y'. Tendinous slip from *flexor carpi ulnaris* to base of *fourth,* as well as the fifth metacarpal
z'. Separation of anterior fibres of *gluteus minimus,* forming distinct muscle.

X. "On the Muscular Arrangements of the Bladder and Prostate, and the manner in which the Ureters and Urethra are closed." By JAMES BELL PETTIGREW, M.D. Edin., Assistant in the Museum of the Royal College of Surgeons of England. Communicated by Dr. SHARPEY. Received June 21, 1866.

(Abstract.)

The present communication, which is based on an extensive series of dissections* and illustrated by photographs, is intended to show that the muscular fibres of the bladder, contrary to the received opinion, are spiral fibres, and with few exceptions form figure-of-8 loops. The loops are variously shaped, according as they are superficial or deep, the more superficial loops being attenuated or drawn out so as to resemble longitudinal or

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* Of these upwards of sixty are preserved in the Museum of the Royal College of Surgeons of England.

vertical fibres, the deeper ones being flattened from above downwards, and resembling circular fibres.

These loops are directed towards the apex and base, and are arranged in four sets; an anterior and a posterior set which are largely developed, and a right and left lateral set which are accessory and less fully developed. The bladder is consequently bilaterally symmetrical.

The superficial loops are confined principally to the anterior, posterior or lateral aspects, but the deeper ones radiate and expand towards the apex and base, so that they come to embrace the entire circumference of the bladder in these directions. The expansion of the fibres is greatest towards the apex, and the aggregation of the terminal loops of the anterior and posterior fibres at the cervix (assisted by the lateral fibres) form a well-marked sphincter vesicæ in this situation. The fibres pursue definite but varying courses, those which are longitudinal or vertical at one point becoming slightly oblique at a second, oblique at a third, and very oblique or transverse at a fourth. The fibres consequently change their direction and position on the vesical parietes gradually and according to a fixed principle. The principle involved is readily explained. The most external and most internal fibres are always the most vertical and most feebly developed; those which succeed or follow becoming more and more oblique and stronger and stronger. The fibres from this circumstance are divisible into two orders, viz., an external and an internal, and these, as has been stated, are grouped in two principal and two subsidiary sets. The two principal sets occur on the anterior and posterior aspects of the viscus, and are so arranged that the terminal or transverse portions of the anterior set intersect the more vertical portions of the posterior set nearly at right angles and the reverse. Similar remarks apply to the subsidiary sets. Between what may be called the vertical or longitudinal and the circular or transverse fibres, other fibres having different degrees of obliquity occur. These consist for the most part of the deeper anterior and posterior spiral fibres, and of the subsidiary spiral fibres from the sides. There is consequently no part of the vesical parietes in which longitudinal, slightly oblique, oblique, and very oblique external and internal fibres may not be found. The additional strength secured by this arrangement cannot well be estimated.

The external and internal fibres are similarly disposed on the anterior, posterior, and lateral aspects, and if the dissection be conducted from without inwards, the fibres first removed are the mesial, vertical, or longitudinal fibres; then the slightly oblique fibres inclined on either side, and crossing at acute angles as in an attenuated figure of 8; then the oblique fibres, crossing at wider vertical angles as in the more perfect figure of 8. Lastly, the very oblique fibres, crossing at such obtuse angles as to have been, up to the present, regarded as circular fibres. The fibres which are still deeper and which constitute the proper internal fibres, have a precisely similar arrangement, but are rudimentary, and consequently not so readily traced. The external and internal fibres, as will be seen from this description, become more and more oblique, both from without and from within, or in proportion as the centre of the vesical parietes is reached, the deepest or most oblique external and internal sets forming, by the blending of their terminal or transverse portions, what is commonly known as the central layer.

The most external or superficial fibres are connected directly and indirectly with the slightly oblique external fibres, the slightly oblique with the oblique, and the oblique with the very oblique. The very oblique external fibres, on the other hand, are connected with the oblique internal, these in their turn being connected with the slightly oblique internal, and the slightly oblique internal with the longitudinal or vertical internal. In some instances the longitudinal external are connected directly with the longitudinal internal, and so of the slightly oblique, oblique, and very oblique external and internal fibres.

The apex and base of the bladder are similarly constructed, and resemble in their general configuration the other portions of the vesical walls; *i. e.*, they are composed of longitudinal or vertical, slightly oblique, oblique, and very oblique or circular fibres which cross in given directions on the external and internal surfaces.

The four sets of longitudinal or vertical fibres have a crucial arrangement at the apex and base, and the slightly oblique fibres are drawn together at the urachus and cervix by the constrictions which in the embryo separate the bladder from the allantois and urethra. The slightly oblique fibres consequently converge towards the apex and base respectively; and this arrangement at the cervix greatly assists in closing the urethra, as the fibres naturally come together to form an impervious funnel-shaped projection which is directed downwards and forwards. The closure of the urethra is completed by the contraction of the very oblique or circular fibres forming the sphincter, and by the prominence of the uvula vesicæ (luette vesicale) and median ridge in the female, and the caput gallinaginis or verumontanum in the male.

The longitudinal or vertical, slightly oblique, oblique, and very oblique external and internal fibres at the base are continued forward within the prostate to the membranous portion of the urethra, and the external and internal surfaces of the corpus spongiosum.

The coats of the urethra are therefore to be regarded as the proper continuation of the walls of the bladder in an anterior direction.

The longitudinal or vertical, slightly oblique, oblique, and very oblique spiral fibres which form the tunics of the bladder and urethra are curiously enough repeated in the prostate of the male, and the analogous structure n the female, so that this gland would seem to be composed chiefly of fibrous offsets from the fibres in question.

The relations existing between the prostate, urethra, and cervix of the bladder are best seen when vertical, horizontal, and antero-posterior or transverse sections of the bladder and prostate are made.

In such sections the external longitudinal or vertical anterior, posterior, and lateral fibres are seen to pass forward on the external surface of the urethra; a certain proportion passing outwards to be inserted into the anterior, posterior, and lateral surfaces of the capsule of the prostate, others passing inwards or through the gland in a vertical or longitudinal and likewise in a horizontal or transverse direction. The crucial arrangement of the four sets of external fibres at the apex and base is thus clearly traceable in the prostate. Such of the external fibres as are not inserted into the capsule of the prostate are attached to the posterior surface of the pubis, the internal border of the aponeurosis of the levator ani, and the fascia covering Guthrie's muscle. The external fibres investing the dorsal, ventral, and lateral aspects of the urethra and prostate are separated by a considerable interval, thus showing that, although the relations existing between the urethra and prostate are of the most intimate description, they may nevertheless be regarded as independent. What has been said of the external longitudinal fibres applies equally to the slightly oblique, oblique, and very oblique external and internal ones, these bifurcating and distributing themselves with considerable regularity to the walls of the urethra, and the substance of the prostate respectively. The urethra and prostate are thus composed of fibres crossing in every direction as in the bladder itself.

The very oblique external and internal fibres are interesting because of the very obtuse angles at which they intersect, and because they are principally concerned in forming the sphincter of the bladder, and the so-called circular layer of the prostate. The very oblique fibres, like the other fibres described, are arranged in an anterior and a posterior set, which are largely developed, and a right and left lateral set, which are developed less feebly. The anterior fibres, which are directed posteriorly, form the posterior half of the sphincter vesicæ, and the posterior fibres, which have an opposite direction, the anterior. The sphincter is thus bilaterally symmetrical, and is somewhat oval in shape, the long axis being directed transversely, or from side to side. The two sets of lateral fibres, which also enter into the formation of the sphincter, intersect the angles formed by the crossing of the anterior and posterior fibres, and render its aperture more circular than it would otherwise be. This circumstance, taken in connexion with the fact that the fibres pursue a very oblique direction, has given rise to the belief that the fibres of the sphincter and neck of the bladder generally are circular fibres, which, as the author shows, is not the case. The fibres of the sphincter are best seen by inverting the bladder and dissecting from within, or by making transverse sections of the prostatic portion of the urethra in the direction of the fundus. They are most strongly pronounced at the cervix, but are continued forward on the urethra and backwards into the bladder. In the female they extend even to the meatus urinarius. The very oblique or circular fibres of the urethra are separated from the corresponding fibres of the prostate by the longitudinal, slightly oblique, and oblique fibres forming the outer half of the urethral wall and the inner portion of the prostate. The interval is particularly evident at the cervix, where the sphincter is most distinctly pronounced;

and here the two sets of very oblique or circular fibres have different axes. Further forward, or towards the apex of the prostate, the space gradually diminishes, the circular fibres of the gland curving in an upward direction into the verumontanum or caput gallinaginis, and blending with the circular fibres of the urethra. While, therefore, the very oblique or circular fibres of the urethra are entirely distinct at one point, they are indissolubly united at another. This is important, as it shows how the sphincter may act independently of the prostate, and the reverse.

The longitudinal or vertical internal fibres posteriorly, connect the median or central portion of the trigone (trigone vesical, trigonum vesicæ, Lieutaud) with the verumontanum in the male, and the uvula and median ridge in the female. The slightly oblique internal fibres bound the trigone laterally, and are continued into the verumontanum, where they cross slightly. The oblique fibres which assist in forming the base of the trigone, are likewise continued in a downward direction on the verumontanum, where they cross, and are mixed up with the continuations of the very oblique fibres which form the sphincter at the neck, and with the circular fibres of the prostate. The arrangement of the fibres in the trigone resembles that found at the cervix and fundus generally, and the author is of opinion that Sir Charles Bell was in error when he described the "muscles of the ureters" as separate structures.

The ureters enter the vesical parietes at a very obtuse angle, and the angle increases according to the degree of distension of the bladder. These tubes receive accessions of fibres from the longitudinal, slightly oblique, oblique, and very oblique external and internal fibres of the bladder in their vicinity, and are continued upon each other within the bladder in the form of a strong transverse band. The transverse band which connects the ureters together within the bladder, or between the uretral orifices, is equal in volume to the ureters themselves within the vesical parietes. The band in question is best seen when the base of the bladder is detached and held against the light, and seems to be formed by the obliteration of the uretral tubes between the uretral orifices.

The uretral channels seek the internal surface of the bladder even more obliquely than the ureters, and the inner walls of the ureters become so thin, particularly towards the uretral orifices, that they act mechanically as moveable partitions or valves, as in the smaller veins*. The canals of the ureters are consequently closed, partly by the contractions of the muscular walls, and partly by the mechanical pressure exercised by the urine about to be expelled.

From the foregoing description it will be evident that the various sets of external and internal fibres forming the bladder, urethra, and prostate are antagonistic, not only as regards themselves, but also as regards the territory or region they occupy; the loops formed by the anterior fibres crossing

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* "On the Relations, Structure, and Functions of the Valves of the Vascular System in Vertebrata," by the author, Trans. Roy. Soc. Edin. p. 763.

each other at more or less acute angles according to their depths, the anterior fibres, as a whole, crossing the posterior or homologous fibres as a whole. While, therefore, the fibres, in virtue of their twisted looped arrangement, antagonize each other individually, the aggregation of the fibres in any one region check, antagonize, and coordinate a similar aggregation of fibres at an opposite point; the anterior fibres, *e.g.*, acting on the posterior, and the right lateral upon the left lateral. This arrangement, which is productive of great strength, ensures that the external and internal fibres shall act in unison or together, and fully explains the views of the older anatomists, who described the bladder as consisting of fibres crossing in every direction, and forming an intricate network. It likewise accords with the more modern opinion, that the fibres of the bladder may be divided into strata or layers.

The fibres, when their points of attachment are taken into consideration, can only contract spirally from above downwards, and from without inwards; they in fact converge, or close spirally in the direction of the cervix, which may be said to diverge or open in an opposite direction as the contraction proceeds. As a result of this twisting movement, the urine, like the blood, is projected spirally*.

Finally, the fibres of the bladder, urethra, and prostate pursue at least seven well-marked directions; the fibres crossing with remarkable precision at wider and wider angles, as the central portion of either is reached, as in the left ventricle of the vertebrate heart†. In fact, the fibres of the bladder and heart have a strictly analogous arrangement, and the author is inclined to believe that functionally also they possess points of resemblance. Very similar remarks may be made regarding the structure and functions of the stomach and uterus.

XI. "Results of the Magnetic Observations at the Kew Observatory.—No. III. Lunar Diurnal Variation of the three Magnetic Elements." By Lieut.-General EDWARD SABINE, P.R.S. Received June 21, 1866.

(Abstract.)

The subject of this paper is the lunar-diurnal variation of the magnetic declination and of the horizontal and vertical components of the magnetic force, derived from a seven years' series of photographic records obtained at the Kew Observatory between January 1, 1858 and December 31, 1864.

The discussion which it contains has for its objects—1st, to exemplify the consistent and systematic character of the lunar-diurnal influence thus derived; and 2ndly, to serve both as a guide and as an encouragement to the several establishments at home and abroad which have adopted, or are

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* *Op. cit.* p. 794.

† "On the arrangement of the Muscular Fibres in the Ventricles of the Vertebrate Heart," by the author, Phil. Trans. part iii. 1864, p. 451.

adopting the Kew methods of magnetic investigation. The completeness of the photographic process is shown by the fact, that of 175,344 hourly positions which should have been recorded in the interval under notice, there were only 1497 failures from all causes whatsoever; and even of these few a considerable portion is shown to be due to the employment of the instruments in other experimental investigations. The paper contains a full statement of the processes of tabulation from the photograms, and of the different stages of reduction through which the tabular results were passed, for the purpose of deriving from them the facts connected with the lunar influence on the terrestrial magnetic elements. A lunar-diurnal variation is shown to exist in each of these elements,—of very small amount, but having peculiar and well-marked systematic characteristics. It is further shown that these characteristics present a similarity and accordance, which it is impossible to regard as accidental, with the results obtained at several other and widely-separated localities in the middle latitudes of both hemispheres, as for example at Hobarton, Toronto, Philadelphia, Pekin, and the Cape of Good Hope. A magnetic variation shown to be thus obviously dependent upon the moon's position relatively to the terrestrial meridian, and agreeing in its principal features in such various localities, is urged by the author as being ascribable with great probability to the direct magnetic action of the moon, made sensible at the surface of the earth through the production of phenomena which, in the present state of our knowledge as regards the magnetism both of the earth and of the moon, it is as yet difficult wholly to explain, but which are likely to lead to a considerable advance of our knowledge in both these respects.

The further prosecution of the investigation, both at Kew and elsewhere, is recommended as highly deserving the attention of those who occupy themselves in the pursuits of inductive philosophy.

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

I. "On the Congelation of Animals." By JOHN DAVY, M.D., F.R.S., &c. Received July 19, 1866.

In a very interesting and elaborate paper by M. Puget, entitled "Sur la Congélation des Animaux," published in the 'Journal de l'Anatomie et de la Physiologie,' the Number for January and February of this year, he refers to a statement of mine, made many years ago*, that the leech may be frozen without loss of life. The experiments which he has instituted, and which appear to have been conducted with great care, have led him to an opposite conclusion, viz. that congelation is not only fatal to the leech, but to animals generally, without a single exception. He considers the cause of death, the *vera causa*, to use his own words, to be an altered condition of the blood. In consequence of this statement, I thought it right

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* Researches, Physiol. and Anat. ii. p. 121.

to repeat the experiments on the leech, and to extend them to some other animals. They were begun at Oxford in May, in the laboratory of Professor Rolleston, with the kind assistance of Mr. Edward Chapman and Mr. Robertson; and since then, in the following month, they have been continued at home in Westmoreland.

At Oxford the trials were made on leeches and frogs; at home, on these animals, and on the toad and some insects. The freezing mixture was made of pounded ice and common salt; the temperature by it was commonly reduced to below 10° Fahr., or at times so low as 2° or 3°. The results obtained were briefly the following:—

1. A leech was exposed to the mixture in a small glass tube just large enough to hold it, using the tube for stirring the mixture. Taken out when perfectly rigid and hard, and gradually thawed, it showed when punctured a faint indication of irritability; there was a just perceptible contraction of the part punctured, the oral extremity, and nowhere else. It did not revive.

2. Another leech was similarly exposed, but for a shorter time. When divided by an incision, it was found not frozen throughout. When punctured, it showed marks of irritability in a slight degree stronger than the preceding: it soon died.

3. Two leeches were similarly treated at home, and for a somewhat longer time; the temperature reduced to 3°. These, when gradually thawed, one exposed to the air, the other left in the mixture, showed no marks of revival; but they retained a certain elasticity, so that when bent they shortly recovered their former attitude, after a manner somewhat resembling a vital movement; but inasmuch as they did not respond by the slightest contraction to puncture, it may be inferred that the movement was not vital. They resisted putrefaction for many days.

4. A frog in a thin glass vessel was kept in the mixture about a quarter of an hour. It was very rigid when taken out; thawed, no part on puncture afforded any indications of life; watched two or three hours it proved to be dead.

5. The heart of a frog, removed immediately after decapitation, whilst still pulsating, was subjected to the freezing mixture in a small glass tube. After having been frozen, on thawing it remained motionless, even when punctured. It had been kept in the mixture only a few minutes.

6. The inferior extremities of a frog kept extended by a bandage and thus introduced into a glass tube, were submerged in the mixture, the body of the frog being held in the warm hand; taken out after some minutes they were quite hard and motionless, whilst the body and upper extremities did not appear to be affected. It moved about, dragging the lower extremities as if they were dead. In about four hours it recovered the use of its femoral muscles; on the following day the use of the muscles of the legs; the day after it was able to bend and extend these limbs; but there was no proof that its feet had recovered sensibility. On the fourth day it was found dead.

7. The lower extremities of a large toad were immersed in direct contact with the mixture, the temperature falling to 3°. Gradually thawed, the parts showed no marks of life. This toad, which before the trial was in a dull state, afterward became almost torpid, and so continued until the following morning, when it was apparently dead: opened, the auricles were found feebly acting, ceasing after a few seconds*.

8. A similar experiment was made on the lower extremities of an active frog, and with a similar result, except that the vivacity of the animal was for a short time but little impaired: after four hours it was apparently dead; opened, its auricles contracted when punctured. It may be right to mention that, before exposing the toad and frog to the freezing mixture in direct contact, it was ascertained that the frog bore the immersion of its lower extremities in a saturated solution of common salt without any apparent loss of sensibility or motive power†.

9. The lower extremities of an active frog of a large size were wrapped in tin-foil, and together with one of its upper extremities not so wrapped, were kept in a freezing mixture about a quarter of an hour. The frozen parts in thawing showed no marks of life. The frog died in about three hours.

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* This toad was a female which had shed her ova; the oviduct was still large; the stomach was distended with caterpillars, slugs, &c., seeming to show that there was no diseased state. It is noteworthy that the apertures of the cutaneous glands appeared to be closed; for when the animal was irritated there was no ejection of the acrid fluid, a circumstance I had before noticed in a female during the breeding-season, suggestive of a condition of surface favourable to the male in the generative act. When the tubercles were incised, they were found to contain the acrid fluid in plenty, and judging from its bitter taste, and the irritating effects of an extremely small portion applied to the tongue, not deficient in activity. The same state of the cuticular glands was found in another female toad killed by congelation, which had shed few of its ova,—this on the 23rd of June. It was of a lighter colour than usual. It was found likewise in two examined in July, in which some ova remained.

† The effect of immersion of the lower extremities of a frog in a saturated solution of common salt varies, I find, according to the length of time; if for a very few minutes, it is inconsiderable; if for many, it is well marked; and if much prolonged it is fatal. In one instance, after a quarter of an hour's immersion, the limbs seemed paralyzed, the animal in a state approaching to torpor: after having been well washed in fresh water it slowly recovered its activity, and the limbs their motive power and sensibility; the motive power first, their sensibility later; indeed not until the following morning, judging from the effects of puncture. After a longer immersion, with a fatal result, the limbs had become rigid and somewhat hard, especially the feet, as if their juices had been extracted by osmotic action. Opened after three hours, even the auricles were motionless, and this when punctured. The muscles of the limbs no longer showed a striated structure, whilst those of the upper extremities displayed this structure distinctly.

The toad with a thicker skin was found to bear the immersion of its extremities for a longer time; but the difference seemed to be only in degree; much longer continued, the same effects were produced, viz. rigidity, with loss of motion and sensibility, which (the immersion not being too long) were slowly recovered after fresh water ablution.

The blood-corpuscles, acted on by the same solution, underwent a change, contracting slightly, and acquiring a granular appearance, commencing in their nuclei.

10. A cockroach, a flesh-fly, and a minute insect, an ichneumon* (*Cœlineus niger* ?), confined together in a small glass tube, were kept some minutes in the mixture. Thawed, they were found all three dead.

These results, so far as the particular instances are concerned, are sufficiently confirmatory of M. Puget's, and on my mind they leave little doubt that his general proposition (his inference from his very numerous experiments) is correct, that congelation is fatal to animal life. It is hardly worth while to attempt to account for the different conclusion I had come to, that referred to by him relative to the leech, it being partly founded on the fact that leeches which had been enveloped in ice for many days were not thereby killed, and partly on witnessing some marks of vitality in leeches which were believed to have been artificially frozen, and which very soon after died.

Whilst admitting that congelation, thorough congelation of an animal is incompatible with life, the cause of death from congelation seems open to question, and more especially that assigned by M. Puget as the *vera causa,* a change in the blood, and chiefly in its corpuscles. That these corpuscles are changed by freezing in form and condition seems to be certain. Before seeing M. Puget's paper I had ascertained the fact, and not only that the corpuscles were changed, but also that the entire blood was to some extent altered, leading me at the time to ask whether some of the injurious effects of frost-bite may not be mainly owing to the freezing of the blood, and the changes in consequence in the corpuscles and in a less degree in the fibrin†; and since, in examining the blood of the animals exposed to the freezing mixture, I have had this confirmed; but the change in these instances was comparatively slight; even in those of the congealed limbs of the frogs and toad the majority of the corpuscles appeared little altered; some few seemed ruptured, some corrugated, and more contracted.

Judging from the effect of congelation on the heart of the frog in experiment No. 5, and from the effects of congelation partially produced, as in the extremities of the frog and toad, I would rather attribute the death to the freezing of the organs, not excluding the blood, than to the freezing of the blood alone; and I would ask, is not this view most in accordance with the pathology of the subject, with all that we know of frost-bite and its consequences in man, and with the results of Mr. Hunter's experiments on the local effects of congelation in animals—those on the ear of the rabbit and wottle of the cock‡? and do not some even of M. Puget's results

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* For the name of this insect I am indebted to Dr. Gray, F.R.S. It was selected on account of its minuteness: it weighed hardly $\frac{1}{100}$ of a grain; it seemed probable, on account of the minuteness of its vessels, that its fluids might escape congelation after the manner of fluids in capillary tubes, which may be reduced many degrees in temperature without being frozen.

† Physiological Researches, 1863, p. 371. See also Trans. Royal Society of Edinburgh, 1865, vol. xxiv. p. 26.

‡ Phil Trans. 1778, p. 34.

give it support, such as the opacity of the crystalline lens, he admitting that, were it possible for an animal to revive after complete congelation, it would be blind from cataract? Now, if the crystalline lens, if the blood-corpuscles suffer and undergo an appreciable change from congelation, it would be very remarkable indeed did not the brain and nerves, and the organs generally suffer from the same cause, and experience changes incompatible with life. In the instance of man, we know that a certain reduction of his temperature merely, not reaching to congelation, suffices to extinguish life*, and that in the instances of other animals, especially the hybernating and insects, a moderate reduction occasions torpor, ending in death if too prolonged. That the organs generally suffer from congelation M. Puget himself admits, as expressed in the subjoined paragraph†. I have found, too, that the muscles, after having been frozen, exhibit a marked change; thus, in one instance, that of a frog, in which, after decapitation, an upper and lower extremity were frozen, the muscles of these limbs, when thawed, compared with those which had not been frozen, showed a well-marked difference under the microscope. Thus, whilst in the latter the striated structure was very distinct, in the former it was no longer visible; and after a few hours, viz. on the following morning, whilst the unfrozen muscles had undergone no perceptible alteration, those which had been frozen had become of increased tenderness, yielding to a slight rending force, and breaking short, as if the coherence of the particles forming the fasciculi was greatly diminished.

II. "Letter to the President from Lieut.-Colonel WALKER, R.E., F.R.S., Superintendent of the Trigonometrical Survey of India."

Dehra Doon *viâ* Bombay, 31st May, 1866.

MY DEAR GENERAL,—Captain Basevi has just returned to my head quarters, on the close of the operations of his first field-season with the pendulums.

You will be glad to hear that his progress has on the whole been very satisfactory. At the outset he met with numerous difficulties; the vacuum apparatus was very troublesome, the air-pump constantly getting out of order, and the receiver as constantly leaking. It is very easy for philosophers to suggest improvements and refinements in the *modus operandi* of such operations, but it is not so easy to carry them out practically. Capt. Basevi has undergone a great amount of labour and anxiety, but he has successfully surmounted all his difficulties.

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* Instances have occurred in the Lake District of persons who have perished on the hills from prolonged exposure to strong wind and rain, *storm-stricken*, in the language of the country.

† "... La congélation complète a même si profondément altéré les tissus de l'organisme que quand l'animal est tout à fait dégelé, son corps est flasque et mou, ses cristallins sont blancs et opaques, et souvent sa coloration est tout à fait altérée" (p. 24).

He has taken experiments at the following stations, which you will find in the chart at the end of Everest's description of the measurement of the Indian Arc, Dehra Doon, Nojli, Kaliana, Dateri, and Usira. At Dehra Doon he took six sets of observations with each pendulum; but finding he could advance more rapidly than the observatories could be got ready for him, at the subsequent stations he took ten sets of observations with each pendulum. Each set was carried over eight to nine hours, from 9 A.M. to 5½ P.M. The pendulum was set in motion in the morning, the coincidences were observed, and the thermometers and arc of vibration were read hourly, until the set terminated in the afternoon. The first and last coincidences will be used only for determining the number of vibrations; but the whole of the intermediate temperatures and arcs will be employed for determining the corrections for temperature and arc. He was very fortunate in his transits, only losing four nights in sixty; complete sets of observations were twice taken at Kaliana (the northern extremity of the Indian arc) in December, when the temperature was lowest, and again this month when it was highest; the range was not so great as we had anticipated, being 58° in the first instance, and 92° in the second, so that the results will scarcely be applicable to the observations at Kew, where, to the best of my recollection, the temperature was between 40° and 50°; but they will amply suffice for all the observations that are likely to be taken in India. He endeavoured to observe at a constant pressure, but the leaking of the cylinder prevented this; however, the whole range of pressure does not exceed 3 inches, and the average range is much less, the maximum being 5 inches and the minimum 2. He is about to commence a series of observations at Masoori, at an altitude of about 6800 feet above the sea. He hopes to complete these during June, before the rainy season sets in, when transits will be impossible.

During the rains he will be employed in completing the calculations connected with his experiments. By September I hope to be able to send you the final results.

He has had so much to do in surmounting the difficulties arising from his new apparatus, that he could not manage to take any magnetic observations. In future, however, he hopes to be able to take these observations regularly at each of his pendulum stations.

I trust that you will be gratified with this account of his first year's operations. No pains have been spared to secure results of the highest possible value, and to reward the confidence you reposed in us, when you suggested to the India Office that we should undertake these delicate and difficult operations.

Believe me, yours sincerely,

General Sabine, P.R.S. J. WALKER.

November 15, 1866.

Lieut.-General SABINE, President, in the Chair.

In accordance with the Statutes, notice of the ensuing Anniversary Meeting for the Election of Council and Officers was given from the Chair.

Dr. Gladstone, Mr. Huggins, Mr. Lassell, Sir John Lubbock, and Colonel Smythe, having been nominated by the President, were elected by ballot Auditors of the Treasurer's accounts on the part of the Society.

Dr. John Charles Bucknill, Dr. William Augustus Guy, and Mr. John William Kaye, were admitted into the Society.

The following communications were read :—

I. "On the Congelation of Animals." By JOHN DAVY, M.D., F.R.S., &c. Received July 19, 1866. (See page 250.)

II. "Letter to the President from Lieut.-Colonel WALKER, R.E., F.R.S., Superintendent of the Trigonometrical Survey of India." (See page 254.)

III. "Spectroscopic Observations of the Sun." By J. NORMAN LOCKYER, F.R.A.S. Communicated by Dr. SHARPEY, Sec. R.S. Received October 11, 1866.

(Abstract.)

The two most recent theories dealing with the physical constitution of the sun are due to M. Faye and to Messrs. De la Rue, Balfour Stewart, and Loewy. The chief point of difference in these two theories is the explanation given by each of the phenomena of sun-spots.

Thus, according to M. Faye*, the interior of the sun is a nebulous gaseous mass of feeble radiating-power, at a temperature of dissociation; the photosphere is, on the other hand, of a high radiating-power, and at a temperature sufficiently low to permit of chemical action. In a sun-spot we see the interior nebulous mass through an opening in the photosphere, caused by an upward current, and the sun-spot is black, by reason of the feeble radiating-power of the nebulous mass.

In the theory held by Messrs. De la Rue, Stewart, and Loewy†, the appearances connected with sun-spots are referred to the effects, cooling and absorptive, of an inrush, or descending current, of the sun's atmosphere, which is known to be colder than the photosphere.

In June 1865 I communicated to the Royal Astronomical Society ‡

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* Comptes Rendus, vol. lx. pp. 89–138, abstracted in 'The Reader,' 4th February, 1865.

† Researches on Solar Physics. Printed for private circulation. Taylor and Francis, 1865.

‡ Monthly Notices Roy. Ast. Soc. vol. xxv. p. 237.

some observations (referred to by the authors last named) which had led me independently to the same conclusion as the one announced by them. The observations indicated that, instead of a spot being caused by an *upward* current, it is caused by a *downward* one, and that the results, or, at all events, the concomitants of the downward current are a dimming and possible vaporization of the cloud-masses carried down. I was led to hold that the current had a downward direction by the fact that one of the cloud-masses observed passed in succession, in the space of about two hours, through the various orders of brightness exhibited by *faculæ*, general surface, and *penumbræ*.

On March 4th of the present year I commenced a spectroscopic observation of sun-spots, with a view of endeavouring to test the two rival theories, and especially of following up the observations before alluded to.

The method I adopted was to apply a direct-vision spectroscope to my 6¼-inch equatoreal (by Messrs. Cooke and Sons) at some distance outside the eyepiece, with its axis coincident with the axis of the telescope prolonged. In front of the slit of the spectroscope was placed a screen on which the image of the sun was received; in this screen there was also a fine slit corresponding to that of the spectroscope.

By this method it is possible to observe at one time the spectra of the umbra of a spot and of the adjoining photosphere or penumbra; unfortunately, however, favourable conditions of spot (*i. e.* as to size, position on the disk, and absence "of cloudy stratum"), atmosphere, and instrument are rarely coincident. The conditions were by no means all I could have desired when my first observations were made; and, owing to the recent absence of spots, I have had no opportunities of repeating my observations. Hence I should have hesitated still longer to lay them before the Royal Society had not M. Faye again recently called attention to the subject.

On turning the telescope and spectrum-apparatus, driven by clock-work, on to the sun at the date mentioned, in such a manner that the centre of the umbra of the small spot then visible fell on the middle of the slit in the screen, which, like the corresponding one in the spectroscope, was longer than the diameter of the umbra, the solar spectrum was observed in the field of view of the spectroscope with its central portion (corresponding to the diameter of the umbra falling on the slit) greatly enfeebled in brilliancy.

All the absorption-bands, however, visible in the spectrum of the photosphere, above and below, were visible in the spectrum of the spot; they, moreover, appeared thicker where they crossed the spot-spectrum.

I was unable to detect the slightest indication of any bright bands, although the spectrum was sufficiently feeble, I think, to have rendered them unmistakeably visible had there been any.

Should these observations be confirmed by observations of a larger spot free from "cloudy stratum," it will follow, not only that the phenomena presented by a sun-spot are not due to radiation from such a source as that indicated by M. Faye, but that we have in this absorption-hypothesis a complete or partial solution of the problem which has withstood so many attacks.

The dispersive power of the spectroscope employed was not sufficient to enable me to determine whether the decreased brilliancy of the spot-spectrum was due in any measure to a greater number of bands of absorption, nor could I prove whether the thickness of the bands in the spot-spectrum, as compared with their thickness in the photosphere-spectrum, was real or apparent only*.

On these points, among others, I shall hope, if permitted, to lay the results of future observations before the Royal Society. Seeing that spectrum-analysis has already been applied to the stars with such success, it is not too much to think that an attentive and *detailed* spectroscopic examination of the sun's surface may bring us much knowledge bearing on the physical constitution of that luminary. For instance, if the theory of absorption be true, we may suppose that in a deep spot rays might be absorbed which would escape absorption in the higher strata of the atmosphere; hence also the darkness of a line may depend somewhat on the depth of the absorbing atmosphere. May not also some of the variable lines visible in the solar spectrum be due to absorption in the region of spots? and may not the spectroscope afford us evidence of the existence of the "red flames" which total eclipses have revealed to us in the sun's atmosphere; although they escape all other methods of observation at other times? and if so, may we not learn something from this of the recent outburst of the star in Corona?

IV. "On a Crystalline Fatty Acid from Human Urine." By E. Schunck, F.R.S. Received September 21, 1866.

(Abstract.)

After referring to the various forms in which fatty matter occurs in human urine, and to our extremely defective knowledge regarding its physical and chemical properties, the author proceeds to describe a process whereby he obtained from healthy urine a small quantity of a substance having the properties characteristic of the fatty acids which are solid at the ordinary temperature. The process consists in passing urine, after having been filtered in order to separate all insoluble matter which may have been deposited, through animal charcoal in an ordinary percolating apparatus. The urine is thereby completely decolorized and deodorized, a small quantity of charcoal producing this effect on a large quantity of urine. The charcoal, after being thoroughly washed with water, is treated with boiling alcohol, to which it communicates a bright yellow colour like

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* Irradiation would cause bands of the same thickness to appear thinnest in the more brilliant spectrum.

that of urine itself. The filtered alcoholic liquid is evaporated, and the residue is treated with water, which leaves undissolved a quantity of brownish-yellow fatty matter. This, after being purified in the manner described by the author, is found to consist principally of a fatty acid, having the properties characteristic of the group to which palmitic and stearic acid belong. The acid is white, crystalline, has a pearly lustre, melts at 54°·3 C., volatilizes unchanged when heated, and is insoluble in water but easily soluble in alcohol and ether. It is soluble in caustic potash and soda-lye, in aqueous ammonia, and in solutions of carbonate of potash and carbonate of soda. The solutions froth on being boiled like ordinary soap and water. The potash compound is obtained from the watery solution in the form of small pearly scales, and from an alcoholic solution in prismatic crystals. The soda-compound separates from a boiling-hot solution on cooling as a thick, white, amorphous soap, a very small quantity of which is sufficient to cause the liquid to gelatinize. The watery solution of either of these compounds gives white curd-like precipitates with salts of barium, calcium, lead, and silver. The quantity of the acid obtained in the author's experiments was too inconsiderable to enable him to determine its composition and atomic weight, and it therefore remains uncertain whether it is identical with any of the known fatty acids or not. The author inclines to the opinion that it is a mixture of stearic and palmitic acid, which according to modern investigations constitute together what was formerly called margaric acid. The author does not venture to assert that it forms a normal constituent of the healthy secretion, though the urine employed in his experiments in no case exhibited anything peculiar. The experiments described do not throw any light on the question how this acid, which belongs to a class of substances almost insoluble in water, comes to be dissolved in a liquid like urine, which is itself usually acid.

V. On Oxalurate of Ammonia as a Constituent of Human Urine." By E. Schunck, F.R.S. Received November 15, 1866.

(Abstract.)

When ordinary healthy urine is passed through animal charcoal in the manner described in the preceding paper, several organic substances are separated and absorbed by the charcoal in addition to the fatty acid there referred to. The liquid obtained by treating the charcoal with boiling alcohol having been evaporated, the residue is treated with water, which leaves the fatty acid undissolved. The filtered liquid yields on evaporation a quantity of crystals, which, after being purified in the manner described by the author, are found to have the properties and composition of oxalurate of ammonia. The watery solution of the substance gives with acids a white crystalline precipitate of oxaluric acid; with nitrate of silver it produces a precipitate which dissolves without change in boiling

water, the solution on cooling depositing white silky needles of oxalurate of silver. The lead compound produced by adding acetate of lead to the watery solution, forms well-defined prismatic crystals. With chloride of calcium the watery solution gives no precipitate, but on adding ammonia and boiling, there is an abundant precipitation of oxalate of lime. By treatment with strong acids the substance is decomposed, yielding oxalic acid and urea. Its composition was found to correspond with the formula $C_6 H_7 N_3 O_8$, which is that of oxalurate of ammonia.

The author's experiments were not sufficiently numerous to decide the question whether this salt is a normal constituent of human urine or not. There is no doubt, however, that its presence, whether exceptional or not, affords an easy and satisfactory explanation of a phenomenon which has until now proved very puzzling, viz., the formation of oxalate of lime in urine long after its emission. It is doubtless owing to the decomposition of oxaluric acid, which takes up water and splits up into urea and oxalic acid; the latter then combines with lime, of which there is always a sufficient quantity present to saturate the acid. There can be little doubt also that oxaluric acid is derived in the animal frame, as in the laboratory, from uric acid, the oxidation of which is its only known source.

VI. "On the Structure of the Optic Lobes of the Cuttle-Fish." By J. Lockhart Clarke, F.R.S. Received September 26, 1866.

(Abstract.)

The brain of the Cuttle-fish consists of several ganglia closely aggregated around the upper part of the œsophagus. The foremost or pharyngeal ganglion, which is much the smallest, is bilobed and somewhat quadrangular. The next is a large bilobed ganglion which forms the roof of the canal for the œsophagus. Beneath the œsophagus is another large and broad mass, which is connected on each side with the supra-œsophageal masses by bands that complete the œsophageal ring.

From each side of the cephalic masses springs a thick optic peduncle which ends in the optic lobe. Each optic lobe is larger than all the other cerebral masses taken together, and has a striking resemblance in shape to the human kidney. It is completely enveloped in a thick layer of optic nerves disposed in flattened bands which issue from all parts of its substance and proceed to the back of the eye in a fan-like expansion, the upper and lower bands crossing each other in their course. The substance of each lobe consists of two distinct portions, which differ from each other entirely in appearance. The outer portion resembles a very thin rind or shell, is extremely delicate, and very easily torn from the central substance which it encloses. It consists of three concentric layers—an external dark layer, an internal dark layer, and a middle pale and broader layer containing thin and concentric bands of fibres.

The first or outer layer consists of a multitude of nuclei and a few small

nucleated cells, with which filaments of the optic nerves are connected. The second or middle layer is composed entirely of fine nerve-fibres which form two sets—one vertical, and the other horizontal. The vertical fibres issue at the under surface of the first layer from the network which its nuclei form with the fibres of the optic nerves. Some are continuous with the horizontal fibres, but the majority continue downward across them to the third or inner layer. At the junction of these two layers is a row of nucleated cells which send thin processes in different directions, and with which some of the nerve-fibres are connected. The third or inner layer is composed entirely of closely-aggregated nuclei, which are joined together in a network by the fibres which issue from the under surface of the middle layer.

The cortical substance, consisting of these three layers, forms only a very small portion of the optic lobe. Out of the nuclear network of the inner layer fine nerve-fibres descend into the body of the lobe which it encloses. At first these fibres are vertical, parallel, and arranged in uniform series, with scattered nuclei between them; but as they descend to the centre of the lobe, they diverge more and more, and cross each other to form a plexus, first with oval and then with broader meshes, in which the nuclei and nucleated cells are collected into groups of corresponding shape and size.

From the plexus at the inner side of the lobe bundles converge from all parts to form the lower half of the peduncle, the upper part of which consists of masses of small nuclei, and gives attachment, by a short pedicle, to a small tubercle. This tubercle consists of closely-aggregated nuclei connected by fibres which converge to its neck and escape into the peduncle of the optic lobe.

After concluding his description of the optic lobes, the author gives a short account of the structure and connexions of the remaining cerebral ganglia of the Cuttle-fish, with the view of determining their homologies.

From the nature of the parts which it supplies, the foremost or pharyngeal ganglion would seem to combine the function of the centres which give origin to the trigeminal, the olfactory, and the gustatory nerves in the vertebrata. The second bilobed ganglion appears to correspond partly to the cerebral lobes and partly to the cerebellum of fishes. The posterior portion of the subœsophageal mass is the analogue of the medulla oblongata; while the anterior portion may be regarded as the spinal cord concentrated below the œsophagus and in the neighbourhood of the feet, which derive all their nerves from that source.

November 22, 1866.

Lieut.-General SABINE, President, in the Chair.

In accordance with the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Council and Officers nominated for election was read as follows :—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Lionel Smith Beale, Esq., M.B.; William Bowman, Esq.; Commander F. J. Owen Evans, R.N.; Edward Frankland, Esq., Ph.D.; John Hall Gladstone, Esq., Ph.D.; William Robert Grove, Esq., M.A., Q.C.; William Huggins, Esq.; Thomas Henry Huxley, Esq., LL.D.; William Lassell, Esq.; Professor Andrew Crombie Ramsay, LL.D.; Colonel William James Smythe, R.A.; William Spottiswoode, Esq., M.A.; Thomas Thomson, M.D.; William Tite, Esq.; Vice-Chancellor Sir W. P. Wood, D.C.L.; The Lord Wrottesley, M.A., D.C.L.

The following communications were read :—

I. "On the Laws of Connexion between the conditions of a Chemical Change and its Amount." No. II. "On the Reaction of Hydric Peroxide and Hydric Iodide." By A. VERNON HARCOURT, M.A., and W. ESSON, M.A. Communicated by Sir BENJAMIN COLLINS BRODIE, Bart. Received July 13, 1866.

(Abstract.)

In a former paper, of which an abstract appeared in the Royal Society's Proceedings, vol. xiv. p. 470, the authors gave an account of their first experiments on this subject.

The second chemical change chosen for investigation was that which occurs in a solution containing hydric iodide (hydriodic acid) and hydric peroxide. In this case the amount of change stands in relation to the following conditions,—(1) the nature of the solution, that is to say, its temperature, and the nature and quantity of the different ingredients which it contains in a unit of volume, (2) the quantity of the solution, or the number of such units of volume, (3) the time during which the change proceeds. The relation of the amount of change to the second and third of these conditions is determinate: it varies directly with each; for the solution is homogeneous, and, if all other conditions are fixed, the rate of change is uniform. But the first condition comprises an almost indefinite number of particular conditions; for not only may various iodides and per-

oxides be used without, as far as we know, altering the nature of the reaction, but other substances may be introduced into the solution, and their influence upon the amount of change determined.

The present paper contains an account of the methods employed for observing this reaction, and of the results obtained by varying two particular conditions, namely the amounts of peroxide and of iodide.

If hydric peroxide and hydric iodide, or barytic peroxide, potassic iodide, and hydric chloride, be brought together in dilute solution at the ordinary temperature, a gradual development of iodine takes place. If a drop of a dilute solution of sodic hyposulphite be added to the mixture capable of reducing to iodide all the iodine which has been formed up to the time of its addition, but not all that will be formed in the course of the reaction, the liquid which had become yellow becomes colourless, remains colourless for a while, and then suddenly becomes yellow again. This yellow colour may be exchanged for a more intense blue colour by putting starch into the solution. It was found that in dilute solutions no direct oxidation of hyposulphite by peroxide took place; and that the quantity of hyposulphite required to reduce the iodine liberated by a measure of peroxide was the same, whether the hyposulphite were added little by little during the course of the reaction, or after the primary reaction had completed itself, and when no peroxide remained in the solution.

The method of observation founded upon these facts was briefly as follows:—

Measured quantities of all the standard solutions, except that of hydric peroxide, were introduced into a glass cylinder about 11 inches high by 3 broad, and water was added till the upper surface of the liquid was level with a line drawn round the cylinder. This adjustment of volume was made through a hole in the bung, which closed the cylinder; two other holes admitted a thermometer and an inverted funnel-tube. A current of carbonic acid passing down the funnel-tube to the bottom of the cylinder, and rising in large bubbles from the mouth of the funnel, served at once to stir the liquid constantly, and to protect its upper surface from the air. When the volume and the temperature of the solution had been adjusted, a small measure of hyposulphite was first added, and then the pipetteful (10 cub. centims.) of peroxide. The cylinder was placed on a sheet of white paper in front of a clock beating seconds. By watching the surface of the fluid and counting the seconds when the time of an observation was near, it was possible to note accurately the moment at which the colour changed. A second small measure of hyposulphite was then introduced, and at the proper interval a second observation was made. Each such addition of hyposulphite, with the observation preceding and following it, is spoken of as an experiment, and these experiments were continued until the reducing power of the last measure of hyposulphite being greater than the oxidizing power of the remaining peroxide the blue colour did not return.

The small measures of hyposulphite consisted of single drops collected under circumstances favourable to their perfect uniformity. The peroxide employed was either an acidified solution of sodic peroxide, or dilute hydric peroxide obtained by the distillation of such a solution. In each set of experiments the value of the measures of hyposulphite and of the pipetteful of peroxide could be compared by determining what fraction of a measure of hyposulphite remained in the solution when the set of experiments had been brought to an end. These values could also be compared by determining each with a standard solution of potassic permanganate. In presence of an excess of potassic iodide, sodic hyposulphite may be estimated by this reagent exactly as it may by standard iodine solution.

During each set of experiments, then, all the conditions of the reaction are constant except two, which are progressively modified. One of these is immaterial, the accumulation of a small quantity of sodic tetrathionate; the other is material, the gradual disappearance of the small quantity of peroxide upon whose presence the reaction depends.

The first point, therefore, requiring to be investigated was the law of connexion between the amount of change and the amount of peroxide.

Since the amount of peroxide originally taken is known, and also the amount which corresponds to a measure of hyposulphite, the amount remaining in the solution at the moment of each observation is also known. Thus the data supplied by each experiment are (1) the amount of peroxide present in the solution at a particular moment of time, (2) the time at which this amount is present, (3) the amount present at a subsequent moment of time, (4) the time at which this amount is present. Representing these two amounts of peroxide by y and y' respectively, and the corresponding moments of time by t and t', the result of each experiment is that an amount of chemical change $y-y'$ has been accomplished in an interval $t'-t$. According to the hypothesis proposed in our former paper, namely that the amount of chemical change varies directly with that of each of the substances partaking in it, these quantities should exhibit throughout a set of experiments the constant relation

$$\frac{y}{y'}=e^{\alpha(t'-t)}.$$

The numerical results obtained in various sets of experiments performed under different circumstances are compared with those calculated from equations of this form, and the two are shown to agree within narrow limits of experimental error. It is inferred that in this case the amount of chemical change taking place at any moment is proportional to the amount of peroxide present at that moment in the solution.

The constant α in the preceding equation represents the effect upon the amount of change of those conditions which do not vary in a set of experiments; and it is possible by varying one of these conditions in different sets of experiments, and determining the value of α in each, to inquire into the law of connexion between the condition thus varied and the amount of change.

Accordingly a series of sets of experiments was made, in which, all else being kept constant, different quantities of potassic iodide were used in different sets. It is shown that the values of α derived from the different sets of experiments are proportional to the quantities of iodide used in each case. In this series hydric sulphate was an ingredient of the solutions; a second series was made, in which an equivalent quantity of hydric chloride was substituted. The result, as regards the effect of varying the amount of iodide, was the same as in the previous series. The rate of chemical change, that is to say the amount in a given time with a constant quantity of peroxide, was found to be directly proportional to the amount of iodide in the solution. Thus the law of connexion is the same in this case as in that already investigated of the variation of peroxide.

The total amount of chemical change is a function of all the conditions of the system in which it occurs. If we call this amount Σ, the volume of the solution v, its temperature h, the time during which the change proceeds t, and the amounts of the various ingredients, peroxide, iodide, &c. in a unit of volume, $p, i, a, b, c \ldots$, then

$$\Sigma = f(a, b, c, \ldots h, i, \ldots p, \ldots t, \ldots v \ldots).$$

The form of this function is determinate in the case of two of these conditions, viz. v, t, and has now been determined experimentally in the case of p and i, so that the equation may be written in the form

$$\Sigma = i\,p\,t\,v\,.\,f(a, b, c \ldots h \ldots).$$

The number of ingredients, represented by a, b, c, &c., which may be introduced into the system and affect the amount only, and not the nature of the chemical change, and which may therefore be regarded as so many conditions of the reaction, is doubtless very large. The authors believe that the investigation of the influence of some of these may prove of interest, it being possible thus to compare various substances which may be substituted one for another in the system by a new standard. They find, for example, that comparing equivalent quantities (in the ordinary chemical sense) of hydric sulphate and hydric chloride, the effect of the latter is nearly double that of the former. But the most important condition of the change whose influence is still undetermined is that of temperature, for this condition intervenes under all circumstances of the reaction, and indeed in all chemical changes whatever. The authors have already made many experiments on both these points. The results of this investigation, which will complete the study of the reaction, may, they hope, form the subject of a subsequent communication.

II. "On the Stability of Domes."—Part II. By E. WYNDHAM TARN, M.A., Memb. Roy. Inst. Brit. Architects. Communicated by G. GODWIN, Esq. Received October 23, 1866.

(Abstract.)

In a former paper on this subject which the author presented to the Royal Society, and which is published in the 'Proceedings' (vol. xv. p. 182), he obtained formulæ for calculating the thrust of a spherical dome of uniform thickness, by supposing it to consist of a number of thin ribs, each of which is formed by two vertical planes intersecting at the axis of the dome, and making a small angle with each other; and then treating each rib as forming, with the corresponding one of the opposite side, a complete arch.

In the present paper the author applies the same method to domes of other forms than the spherical. The following are the kinds for which the formulæ are investigated:—

A. The Gothic dome, of which that of the cathedral at Florence is a splendid example. This kind has for its section a pointed arch formed by two segments of circles, the centre from which each is struck being on the springing line, but not in the axis of the dome. The author denotes by a the angle between the vertical and a line drawn from this centre to the point, near the crown of the dome, where its axis is intersected by a circle drawn around that centre with a radius equal to the mean of the radii of the circles which generate the outer and inner surfaces of the dome, and reduces his results to numbers for three different values of the angle a.

B. A dome whose inner surface is a paraboloid of revolution, the thickness of the shell being uniform throughout.

C. The dome whose surface is formed by the revolution of an ellipse about its major axis. In the investigation, the two surfaces are supposed to be generated by the revolution of two concentric elliptic quadrants, whose major axes differ from one another by the same quantity as the minor axes, so that the thickness is very nearly uniform throughout.

D. A form of dome commonly used in eastern countries, sometimes called the ogival dome, the surface of which is generated by the revolution of a curve which has a point of contrary flexure. In the investigation, the author takes for this curve the curve of sines, the equation of which is $y'=r\sin\frac{x'}{r}$, where x', y' are the vertical and horizontal coordinates of a point in the generating curve, and supposes the outer and inner surfaces to be generated by the revolution of two such curves, differing only by the value of r, the origin being the same.

The following Table exhibits the principal results obtained from the author's investigations. All the domes, except the last, are supposed to be of the uniform thickness throughout of 1 foot. In the last the thickness is 1 foot at the springing, but gets rather less towards the top. The domes are

supposed to be built of material weighing 125 lbs. to the cubic foot. The thrust is calculated for the 180th part of the whole dome, being the portion cut out by two planes which make an angle of 2° at the axis.

Table showing the position of the weakest joint in domes of various forms, and the horizontal thrust at that joint.

	Form of Dome.	Span.	Position of the weakest joint, or joint where thrust is greatest.	Greatest horizontal thrust, or thrust at weakest joint.
		feet.		lbs.
1.	Hemisphere	20	Makes with springing line an angle of 20°.	92·01
2.	Gothic, $a=10^\circ$	20	Makes with springing line an angle of 17°.	88·7
3.	Gothic, $a=22\frac{1}{2}^\circ$	20	Makes with springing line an angle of $13\frac{1}{2}^\circ$.	80·418
4.	Gothic, $a=30^\circ$	20	Makes with springing line an angle of 11°.	77·27
5.	Parabolic; height above springing equals the half span.	20	At springing line	79·6
6.	Elliptical, major axis vertical; ratio of major to minor axis as 6 : 5.	20	One-third of semimajor axis above springing line.	90·867
7.	Ogival; contour, the "curve of sines."	20	One-sixteenth of the span above springing line.	62·5

In the preceding cases, except the last, the domes are assumed to be of uniform thickness. The author finally applies his formulæ to the case of the spherical dome in which the thickness at the crown is one-half that at the springing, the inner surface being generated by the revolution of a circular quadrant whose centre is raised above the centre of that which generates the outer surface by half the difference of the radii. Assuming the outer and inner radii R, r to be respectively 11 feet and 10 feet, he finds that the weakest point on a dome of this form appears to be at a height equal to $\frac{4}{11}$ R above the springing line; and with the other numerical values, assumed the same as before, he finds for the horizontal thrust at the weakest joint 55·78 lbs.

For each kind of dome the author forms what he calls the *equation of stability,* giving, for an assumed value of the height of the pier, the least thickness t which will permit of stability. The following are the results for each kind of dome, the height of the pier being taken at 50 feet:—

Spherical dome (from former paper)	$t=2{\cdot}45$	feet.
Gothic dome ($a=22\frac{1}{2}^\circ$)	$t=2{\cdot}259$	,,
Parabaloidal dome	$t=2{\cdot}244$	,,
Elliptic dome	$t=2{\cdot}44$	,,
Ogival dome	$t=2$	,,
Spherical dome (thinner at crown)	$t=1{\cdot}9$	,,

The author considers what he has found as the weakest part of a dome to be the position in which an iron belt must be placed to produce the greatest effect in counteracting the thrust of the dome. If this be done, the thickness of the pier may be considerably diminished, and need not greatly exceed the strength necessary for supporting the superincumbent weight acting vertically downwards.

III. "A Supplementary Memoir on Caustics."

By A. Cayley, F.R.S. Received November 15, 1866.

(Abstract.)

It is near the conclusion of my "Memoir on Caustics," Phil. Trans. vol. cxlvii. (1857), pp. 273, 312, remarked that for the case of parallel rays refracted at a circle, the ordinary construction for the secondary caustic cannot be made use of (the entire curve would in fact pass off to an infinite distance), and that the simplest course is to measure off the distance GQ from a line through the centre of the refracting circle perpendicular to the direction of the incident rays. The particular secondary caustic, or orthogonal trajectory of the refracted rays, obtained on the above supposition was shown to be a curve of the order 8; and it was further shown by consideration of the case (wherein the distance GQ is measured off from an arbitrary line perpendicular to the incident rays), that the general secondary caustic or orthogonal trajectory of the refracted rays was a curve of the same order 8. The last-mentioned curve in the case of reflexion, or for $\mu=-1$, degenerates into a curve of the order 6; and I propose in the present supplementary memoir to discuss this sextic curve; viz. the sextic curve which is the general secondary caustic or orthogonal trajectory of parallel rays reflected at a circle.

November 30, 1866.

ANNIVERSARY MEETING.

Lieut.-General SABINE, President, in the Chair.

Dr. Gladstone, on the part of the Auditors of the Treasurer's Accounts appointed by the Society, reported that the total receipts during the past year, including a balance of £15 9*s.* carried from the preceding year, amounted to £4295 16*s.* 11*d.*; and that the total expenditure in the same period amounted to £3629 15*s.* 5*d.*, leaving a balance of £638 8*s.* 1*d.* at the Bankers, and of £27 13*s.* 5*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

Royal.

His Majesty Leopold, King of the Belgians, K.G.

On the Home List.

Benjamin Guy Babington, M.D.
William Thomas Brande, D.C.L.
Right Hon. Sir James Lewis Knight Bruce, Knt., D.C.L.
Richard John Hely Hutchinson, Earl of Donoughmore.
Sir Charles Locke Eastlake, D.C.L.
Joseph Edye, Esq.
G. W. Featherstonhaugh, Esq.
Charles Lord Glenelg, D.C.L.
William Gravatt, Esq.
John Scandret Harford, D.C.L.
Charles James Hargreave, LL.D.
William Henry Harvey, M.D.
Robert Hunter, Esq.
Percival Norton Johnson, Esq.
Edward Kater, Esq.
John Lee, LL.D.
Rev. Samuel Roffy Maitland, D.D.
Thomas Spring Rice, Lord Monteagle of Brandon.
Francis Thornhill Baring, Baron Northbrook.
Horatio William Walpole, Earl of Orford (in 1858).
George Rennie, Esq.
Henry Darwin Rogers, LL.D.
Edward James Seymour, M.D.
Samuel Reynolds Solly, Esq.
Joseph Toynbee, Esq.
Lieut.-Col. Sir John Maxwell Tylden, Knt.
Rev. William Whewell, D.D.
Right Hon. Sir James Wigram, Knt., M.A.
Nicholas Wood, Esq.

On the Foreign List.

Georg Friedrich Bernhard Riemann.

Change of Name.

Rev. Henry Christmas to Noel-Fearn.

Defaulter.

Colonel John Le Couteur.

Fellows elected since the last Anniversary.

On the Home List.

John Charles Bucknill, M.D.
Rev. Frederic William Farrar.
William Augustus Guy, M.B.
James Hector, M.D.
John William Kaye, Esq.
Hugo Müller, Ph.D.
Charles Murchison, M.D.
William Henry Perkin, Esq.
The Ven. John Henry Pratt, M.A.
Capt. George Henry Richards, R.N.
Thomas Richardson, Esq., M.A.
William Henry Leighton Russell, Esq.
Rev. William Selwyn, D.D.
Rev. Richard Townsend, M.A.
Henry Watts, B.A.

On the Foreign List.

Franz Cornelius Donders.
Georg Friedrich Bernhard Riemann.
Gustav Rose.

Readmitted.

William Bird Herapath, M.D.

The President then addressed the Society as follows:—

Gentlemen,

During the autumnal recess, and in the absence of the President, the Treasurer, on learning that the Government had it in contemplation to assign the main building of Burlington House, now appropriated to the Royal, Chemical, and Linnean Societies, to the Royal Academy (together with ground in its rear for the erection of additional buildings), deemed it advisable in the interests of the Society, and after consultation with the Senior Secretary, to address a letter to the Earl of Derby. The officers of the Society were referred by his Lordship to the Chief Commissioner of Works, by whom they were informed in reply that due provision was to be made for the Societies, and that the Architects, Messrs. Banks and Barry, with whom the Office were to advise on the subject, were instructed to confer with the Council of the Royal Society. Those gentlemen having accordingly communicated to the Council that they are directed to report to the Government on the accommodation that can be provided for the Royal and the other Societies in new buildings, to occupy part of the space between Burlington House and Piccadilly, the Council have appointed a Committee to consider the whole matter and to confer with the Architects thereupon. The Committee have had an interview with Mr. Banks, and have handed in a statement setting forth the nature and extent of accommodation that will be required for the Society in lieu of their present apartments. There, so far as we are informed, the matter rests for the present, but from the tenor of the communications which have passed, there seems every reason to feel confident that the Society will be suitably and amply provided for.

I had the pleasure of announcing at the last Anniversary that the printing of the Catalogue of Scientific Papers, in the preparation of which we have been so long engaged, was at length commenced. Twenty-eight quarto sheets are now printed and eight more are in type. The work would now have been further advanced, had not the Superintending Commitee in the mean time obtained access to additional Periodical Works, containing Memoirs deserving to be included in the Catalogue, and too numerous to warrant their being postponed for a supplement. The printing has therefore been interrupted for a time in order to allow of the insertion of these additional titles in their proper places. Moreover, proofs of every sheet are supplied to several of the Members of the Library Committee who separately revise them; and this mode of proceeding no doubt somewhat protracts the work, but it has the more than compensating advantage of securing the greatest attainable accuracy.

The attention of your Council has been much occupied in the past year in complying with a request from Her Majesty's Government for the advice and co-operation of the Royal Society in the re-organization of the Meteorological Department of the Board of Trade, and in preparing the preliminary arrangements for the establishment of a system of British Land-Meteorology to be carried out under the authorization of that Board.

Perhaps there is no branch of scientific research in which a greater amount of human labour has been expended than has been the case in Meteorology,—a natural consequence of its intimate connexion in so many ways with the interests and pursuits of man; and it may perhaps be said with equal truth that there is no department of natural knowledge in which the labour bestowed has been of a more desultory character, or the value and importance of the conclusions less commensurate with the time and labour bestowed on their acquisition. The object to be desired, therefore, is scarcely so much to give a stronger impulse to the spirit of inquiry, as to aid in giving to that which exists a more systematic direction. This has been attempted in several of the continental States of Europe and America, by the establishment at the expense of the respective governments, and under the superintendence of men eminently qualified by theoretical and practical knowledge, of systematic climatological researches; and, as regards the individual states themselves, it may be confidently said that the results have been very beneficial.

For some time past it has been desired to establish a closer connexion between these independent and separate systems of observation, by effecting an assimilation of instrumental means, and of the modes and times of observation. It was chiefly in this view that the assemblage took place by special invitation, at the Cambridge Meeting of the British Association in 1845, of the Directors of the principal meteorological (and magnetical) observatories in Europe and America; and that a second meeting took place at Brussels in 1853. The difficulty that impeded success on these two occasions cannot be better stated than in the words of Captain Maury, in his Report to the Government of the United States, by whom he had been appointed to visit the principal European Meteorological Observatories for the express purpose of urging a uniformity of procedure.

"I would recommend that the United States should abandon, for the time at least, that part of the 'Universal System' which relates to the Land, and that we should direct our efforts mainly to the Sea, where there is such a rich harvest to be gathered for navigation and commerce. I am inclined to make this recommendation in consequence of the evident reluctance with which Russia, Austria, Bavaria, Belgium, and other powers seem to regard any change in their systems of meteorological observations on shore. Each country seems to have adopted a system of its own, according to which its labourers have been accustomed to work, and to which its meteorologists are more or less partial. Any proposition having in view for these systems a change so radical as to bring them into uniformity, and reduce them to one for all the world would, I have reason to believe, be regarded with more or less jealousy by many,—not so, however, with regard to the sea; that proposal meets with decided favour and warm support."

The most hopeful way of removing the difficulties which impeded the adoption of a system in which all might willingly unite was obviously the introduction of instruments which should be continuously self-recording, and which, after sufficient trial, should receive general approval. The importance of such a substitution had been recognized at the Observatory of the British Association at Kew at a very early date, even before the Cambridge Meeting of the British Association in 1845; and at that meeting the satisfactory performance was announced of a photometrically self-recording barometer, self-compensated also for temperature, devised by Mr. Francis Ronalds, the Honorary Director of the Kew Observatory,—the same to whose merits in regard to the instantaneous transmission of messages by means of electricity, at the very early date of 1823, attention has been recently called in the public journals. Encouraged by this success in one instrument, the Directors of the Kew Observatory proceeded, as rapidly as the means at their disposal enabled them to do, towards the provision of self-recording instruments for the other meteorological elements (as well as for the magnetical elements), and were considerably advanced in their preparations when in 1854 the Board of Trade informed the President and Council that, in fulfilment of a previous earnest recommendation to that effect from the Royal Society, they were "about to submit to Parliament an estimate for an office for the discussion of observations on meteorology made *at sea* in all parts of the globe;" adding that, "as it may possibly happen that observations *on Land* upon an extensive scale may hereafter be made and discussed in the same office, it is desirable that the Royal Society should keep in view and provide for such a contingency."

The subject of a Government system of Meteorological Observations on Land was again brought under the consideration of the Royal Society in a letter from the Board of Trade in May 1865, and the President and

Council were expressly requested to offer suggestions in reference to it. The suggestions which they offered in reply have been made known to the Society, in their leading outlines at least, in my Address of last year, and in No. 82 of our 'Proceedings.' They have been submitted by the Board of Trade to a Committee appointed by itself, whose general approval they are understood to have received; and the whole scheme is now under the consideration of Government in respect to its cost.

The sea-observations, so hopefully spoken of by Captain Maury, were the subject of a very full communication addressed by the President and Council of the Royal Society to the Board of Trade in February 1855, and the recommendations contained therein were made the basis of the instructions given to the Meteorological Office of the Board of Trade at its first formation. The collection of what have since been comprehended under the general designation of "Ocean Statistics" proceeded for some time with much activity and success under the direction of our lamented Fellow the late Admiral FitzRoy. His exertions and those of his assistants were afterwards in great measure diverted to an object which, if it could be—or, to speak more sanguinely, whenever it shall become—practically attainable with a fair measure of scientific certainty, will assuredly both deserve and receive in the highest degree general favour and support. I mean the system popularly known by the appellation of "storm-warnings." Meanwhile, if the recommendations of the Royal Society and the intentions entertained by the Board of Trade shall now receive the hoped-for sanction of the general government, the collection of ocean statistics and their systematic combination will be resumed with fresh vigour, and with all the aids which experience and matured scientific consideration can afford; and at the same time, if our hopes respecting the proposed system of Land-Meteorology are realized, and if its fruits correspond in a fair degree to the expectations which we venture to form respecting them, we shall gradually obtain such a more complete knowledge of the laws which govern the changes of weather in the British Islands and their vicinity, as may enable the predictions of approaching storms or "storm-warnings," if now suspended, to be resumed hereafter, and at no very distant period, with far greater confidence and more assured advantage.

At our last Anniversary I acquainted you that the Legislature of Victoria had voted the sum of £5000 for the construction of a large reflecting telescope to be erected at Melbourne and employed in a thorough survey of the Nebulæ and multiple stars of the southern hemisphere. They also requested the cooperation of the President and Council of the Royal Society in arranging a contract for the work and superintending its execution. We selected for the task one of our Fellows, Mr. Grubb of Dublin, whose well-known optical and mechanical talents gave sure promise of success, and we obtained for him the advantage and assistance of a Superintending Committee, consisting of our late President the Earl of Rosse, Dr. Robinson, and Mr. Warren

De la Rue. The contract for the work was signed on the 19th of January; the progress has been rapid, and I have great pleasure in informing you that, according to all appearance, the instrument will be ready for trial in the spring of 1867. All the large parts of it are ready for mounting, and the rest considerably advanced. The lattice-tube, the appearance of which is known to many of us from the photographs, is put together. It is made of ribs of steel to combine lightness and strength; they are rolled taper to effect this in the highest degree. The equilibrated systems of levers which support the great speculum, with their boxes, are of the same material and are also completed. Three specula have been cast on a plan differing from that of Lord Rosse only by such modifications as were made necessary by their having central apertures. The first speculum came out sound from the annealing furnace, but had two blemishes on its surface which would have required a month to grind out, and Mr. Grubb broke it up without hesitation, though not many years ago such a disk would have been almost inestimable. The second cast has been successfully ground, and its surface is faultless. The third, a duplicate speculum, was cast on the 24th of October. The grinding has been performed by the polishing machine and steam-engine which belong to the telescope, and will accompany it to Melbourne. The trial-piers on which the instrument is to be set up for the examination of the Superintending Committee, are ready.

At the request of the Board of Visitors of the Melbourne Observatory, and at the recommendation of the aforenamed Superintending Committee, I have appointed as Observer with this telescope Mr. Albert Le Sueur, a Graduate at Cambridge and a Wrangler in 1863. He is at present training himself in sidereal astronomy at the Cambridge Observatory under the guidance of Professor Adams, and will be present at the polishing of the specula. Mr. De la Rue has kindly promised to instruct him in the practice of celestial photography; so that there is reason to hope that this magnificent instrument will be used with full intelligence and zeal, and amply repay the munificent spirit that has guided the Legislature of this energetic and prosperous colony.

The large appropriation made by the Colony of Victoria for the construction of this telescope and for its accompanying spectroscope, and for a suitable provision for its effective employment at Melbourne, have not been the only manifestation in the present year of the enlightened spirit which animates and guides the Legislature of that colony. A sum has been remitted and received in this country for the purchase of a complete equipment of self-recording magnetical instruments on the model of those at the Kew Observatory, to be located in the Government Reserve adjacent to the Melbourne Astronomical Observatory, and to be employed under the superintendence of its director, Mr. Ellery. The instruments have been made and verified under the immediate care of the Director of the Kew Observatory, and have been despatched to their destination. The system of intended observation is modelled on that exemplified at the Kew Observatory.

The intention, adverted to in my last year's Address, of the Government of Mauritius to establish there a magnetic observatory, working with the instruments and adopting the methods exemplified at Kew, has been since matured. The necessary funds have been remitted, and the instruments are made and are now under process of verification at Kew, where Professor Meldrum, the Director of the Mauritius Observatory, is daily expected to arrive, with the view of making himself thoroughly acquainted with the instruments, and with the processes in which they are to be employed. The geographical position of Mauritius is a very important one, both for magnetical and meteorological observations. Hitherto meteorology has been exclusively pursued there, and the researches of Professor Meldrum on the cyclonic storms which prevail in the vicinity of the Island are well known.

Those of our Fellows who remember the assiduity and devotion to scientific pursuits manifested by Mr. Charles Chambers during his employment as one of the assistants of the Kew Observatory, will be glad to learn that he has received from the Government of Bombay the temporary appointment of Superintendent of the Bombay Magnetical and Meteorological Observatory, and in that capacity has been required to submit a scheme for the reorganization of the observatory, with instruments and methods of research suitable to the advance which has been made in these respects in the last twenty-five years. Mr. Chambers's report is understood to be on its way from the Government of Bombay to the India Office, with a view to its being submitted to the consideration of the President and Council of the Royal Society; and should it be approved, it will probably lead to the permanent employment of Mr. Chambers in his present temporary position, and to the thorough utilization of the observations of past years, in addition to the prospect of most efficient work at this observatory, under the best conditions, both personal and material, for the future.

I have to add to the list of magnetic observatories established in the present year one at the Roman Catholic College at Stonyhurst, supplied with the Kew instruments, and pursuing the same methods of observation and reduction. I refer to this with the greater satisfaction, because in addition to the value to science of the actual work performed at the observatory, it may be expected to be, and is expressly designed by the authorities of the College to be a means, amongst others adopted by them, of fostering among the students a taste for scientific pursuits, which may remain with them in after life. For this latter purpose Stonyhurst has added to the more ordinary modes of instruction in natural knowledge that practical instruction which is only to be gained by working under proper tuition in observatories and laboratories directed to special scientific pursuits, among which magnetism, terrestrial and celestial, may now be considered to have taken its place.

It was from the twofold motive of aiding the advancement of science by this additional observatory on the one hand, and on the other of contributing to the dissemination of a taste for scientific pursuits among a large class of our gentry, that the Council of the Royal Society thought it right to allot last year from the parliamentary grant annually placed at their disposal, a sum sufficient to defray half the cost of a set of magnetical instruments for Stonyhurst.

This is the fortieth year since Mr. Schwabe began at Dessau his series of observations on the Solar spots, which he has continued without intermission from 1826 to the present time. Impressed with the extreme desirableness of continuing beyond the limits of a single life a series already so valuable, the Committee of the Kew Observatory concerted with Mr. Schwabe for the commencement last year at Kew of a series which should run parallel with his for a time, and which afterwards, when the identity or proximate identity of the two should have been established, might, it was hoped, be prolonged indefinitely through future years. Mr. Schwabe's observations and those at Kew have accordingly been proceeding contemporaneously, and the comparison between their results during the ten months from January to October 1866 inclusive, gives reason to believe that the object will be satisfactorily attained. The number of new groups of spots observed at the two stations in the ten months is identical, and *very* similar, if not always quite identical, in each single month; although, as might have been expected from the difference between the continental and insular climates, the number of days of observation at Kew is considerably less than at Dessau.

The results of the first year's experiments with the pendulums which were noticed in my last year's Address as having been supplied to the Indian Trigonometrical Survey, have been received from Colonel Walker, R.A., F.R.S., Superintendent of the Survey. They were made by Captain Basevi at several stations where the triangulation is now proceeding. In a letter to myself accompanying them, dated August 30 of the present year, Colonel Walker says, "Already these experiments are beginning to throw light on the subject of Himalayan attraction; for the observations clearly show that the force of gravity is less than it should be theoretically at the stations in the vicinity of the Himalayas, and that the difference between theory and practice diminishes the further the station is removed from the Himalayas. This seems a remarkable confirmation of the Astronomer Royal's opinion, that the strata of the earth below mountains are less dense than the strata below plains and the bed of the sea. Combining these observations with those which were used by Mr. Baily, including, I believe, all your own, the value of the ellipticity will be $\frac{1}{289}$." The general result obtained from the thirteen stations of my equatorial and arctic voyages (1821–1823) was $\frac{1}{288 \cdot 4}$. That obtained by Mr. Baily from

Captain Foster's experiments in his equatorial and antarctic voyage (1828–1830) was $\frac{1}{289\cdot2}$.

In our Transactions of the present year we have an elaborate and valuable memoir by Mr. Abel "On the Manufacture and Composition of Gun-cotton," pointing out the causes of the difference in the analytical results obtained by many of the earlier inquirers into its nature, and confirming its composition as determined by Crum and more fully proved by Hadow, and again stated by the Committee of Chemists who reported on Baron von Lenk's Gun-cotton. In pursuing these analytical inquiries, Mr. Abel has tested the methods, both synthetical and analytical, formerly employed, and has devised some modes of analysis of his own. The multiplied and varied experiments which he has made leave no room to doubt the accuracy of his results, though they differ from those of M. Pelouze.

With regard to the processes of manufacture, Mr. Abel has proved by experiments, both in the laboratory and on a manufacturing scale, that Baron von Lenk's method, although it does not at first sight present any important features of novelty, yet unquestionably ensures the attainment of greater uniformity and purity of the product, though Mr. Abel has himself suggested one or two modifications of importance. The most valuable practical result deduced by Mr. Abel from his experiments is, that the instability which has been observed in certain samples of gun-cotton, producing the gradual decomposition of such samples by prolonged keeping, is due to insufficient purification of the material employed, in consequence of which oxidized products of small quantities of resinous and other foreign substances are formed in the manufacture, and are still retained by the tubular fibre of the cotton. These undergo decomposition, and the change extends to the mass. Many of these impurities are removed by the action of the alkaline bath upon the cotton before treating it with the nitric and sulphuric acids, and others may in great measure be dissolved by a final boiling with a weak alkaline solution.

Mr. Abel's paper is an important contribution towards a more complete knowledge than has hitherto obtained of the precautions which are required in the manufacture of gun-cotton in order to diminish still further both the risk of accidents, and the liability to injury of the material when not stored,—as it ought invariably to be when no paramount reason requires an exception,—either under water or in a state of moisture precluding ignition.

Since my Address last year little has been done in regard to the successful application of gun-cotton to the large ordnance employed in the public service. The desideratum for this purpose may be stated to be a form of cartridge, which with the required velocity of the projectile on quitting the piece, shall have produced an approach to an equality of strain upon every point of the bore, from the instant of ignition to that of the discharge. In the absence of a test of the degree to which this is accomplished, experiments on different forms of the cartridge are necessarily tentative, and the instruction derived from them of comparatively little value. The production of an apparatus by which the actual strain experienced in successive parts of the bore may be recorded, is in fact the first step in a scientific inquiry which should establish the proper relations between the form of the cartridge and the gun. But for the production of an efficient apparatus for this purpose, a chronoscope of greater delicacy and perfection than any we have hitherto possessed in England is indispensable. A chronoscope recently devised by Captain Schultz of the French Artillery appears, as far as can be judged previous to a practical trial, to correspond to these conditions, and to be likely to supply a means of surmounting the difficulty; it is not improbable that it will be tried in the present year.

I proceed to the award of the Medals.

The Copley Medal has been awarded to Professor Julius Plücker, Foreign Member of the Royal Society, for his researches in Analytical Geometry, Magnetism, and Spectral Analysis.

To an audience not exclusively mathematical it is obviously impossible to enter into details of researches which deal with geometrical questions of no ordinary difficulty. Amongst these, however, may be indicated, as especially appreciated by those who are interested in the progress of analytical geometry, his theory of the singularities of plane curves as developed in the "Algebräische Curven," with its six equations connecting them with the order of the curves: the papers on point and line coordinates and on the general use of symbols, may also be noticed as establishing his claim to a position in the department of abstract science which is attained by few even of those who give to it their undivided attention. But Professor Plücker has high merits in two other widely different fields of research, viz. in Magnetism and Spectrology: and to these I may more freely invite your attention.

Shortly after Faraday's discovery of the sensibility of bodies generally to the action of a magnet and of diamagnetism, Professor Plücker, in repeating some of Faraday's experiments, was led to the discovery of magnecrystallic action,—that is, that a crystallized body behaves differently in the magnetic field according to the orientation of certain directions in the crystal. The crystals first examined were optically uniaxal, and it was found that the optic axis was driven into the equatorial position; (that is, of course, assuming that the magnecrystallic action is not masked, in consequence of the external form of the body, by the paramagnetic or diamagnetic character of the substance). New facts, discovered both by Faraday and by Plücker himself, led him to a modification of this law, to the effect that the optic axis was impelled, according to the nature of the crystal, *either* into the equatorial or the axial position. This subject was afterwards followed out by Professor Plücker into the more complicated cases in which the conditions of crystalline symmetry are such as to leave the crystal optically biaxal; and after having recognized the insufficiency of a first empirical generalization of the law applicable to crystals of the rhombohedral or pyramidal system, and accordingly to uniaxal crystals, he was led to assimilate a crystal to an assemblage of small ellipsoids, capable of magnetic induction, having for their principal planes the planes of crystalline symmetry where such exist; and to apply Poisson's theory. The result of this investigation is contained in an elaborate paper read before the Royal Society in 1857, and published in the Philosophical Transactions for the following year. In this paper Professor Plücker has deduced from theory, and verified by careful experiments, the mathematical laws which regulate the magnecrystallic action. These laws have not necessarily involved in them the somewhat artificial hypothesis respecting the magnetic structure of a crystal from which they were deduced; and at the close of his memoir Professor Plücker recognizes the theory of Professor Sir William Thomson, with which he then first became acquainted, as a sound basis on which they might be established. The laws, however, remain identically the same in whichever way they may be derived.

Another subject to which Professor Plücker has paid much attention is the curious action of powerful magnets on the luminous electric discharge in glass tubes containing highly rarefied gas. In this case the luminous discharge is found to be concentrated along certain curved lines or surfaces. He has succeeded in obtaining the mathematical definition of these curved lines or surfaces, by a simple application of the known laws of electromagnetic action, regarding an element of the discharge as the element of an electric current. With regard to the blue negative light, for instance, starting from a point in the negative electrode, he has shown that there are two totally distinct paths, one or other of which, according to circumstances, it may take, going either within the enclosed space along a line of magnetic force, or else along the surface of the glass in what he calls an "epipolic curve," which is the locus of a point in which the inner surface of the vessel is touched by the line of magnetic force passing through that point.

Ångström appears to have been the first to notice that the spectrum of the electric spark striking between metallic electrodes through air on another gas at ordinary pressures is a compound one, consisting of very bright lines varying with the metal, and others, usually less bright, depending only on the gas. Under the circumstances which presented themselves in his experiments, the latter can frequently be but ill observed; and the diffused light of a rarefied gas in a wide tube is but faint, and does not form very definite spectra. But Plücker found that by employing tubes which were capillary in one part, brilliant light and definite spectra were obtained in the narrow part. These spectra were observed by him with great care, and were found to be characteristic of the several gases and to indicate their chemical nature, though the gases might be present in such minute quantity as utterly to elude chemical research. It further appeared that compound gases of any kind were instantaneously, or almost instantaneously, decomposed; at least the spectra they offered were the spectra of their constituents.

In a recent memoir, which has only just been published in the Philosophical Transactions, Professor Plücker has investigated the two totally different spectra frequently afforded by the same elementary substance according as it is submitted to the instantaneous discharge of a Leyden jar charged by an induction-coil, or rendered incandescent by the simple discharge of the coil, or else, in some cases, by ordinary flames. The two spectra show a remarkable difference in *character*, and are not merely different in the number and position of the lines which they show. Some phenomena which he had previously noticed receive their explanation by this twofold spectrum.

This difference of spectra is attributed by Professor Plücker, with the greatest probability, to a difference in the temperature of the flowing gas when the two are respectively produced. The discovery opens up a new field of research, the exploration of which may throw much light on the correct interpretation of celestial phenomena, especially in relation to the physical condition of nebular and cometary matter.

PROFESSOR MILLER,

As we have not the pleasure of the presence of Professor Plücker, I must request you, as our Foreign Secretary, to transmit to him this Medal. He will see in it the strongest evidence of the high estimation in which his labours in various lines of scientific research are held in this country; and I trust you will also express to him the great pleasure which this award gives to his numerous friends here, some of whom have been his fellow-labourers in the same researches.

A Royal Medal has been awarded to Mr. William Huggins for his Researches on the Spectra of some of the Chemical Elements, and on the Spectra of certain of the Heavenly Bodies; and especially for his Researches on the Spectra of the Nebulæ, published in the Philosophical Transactions.

The researches on the stellar spectra referred to in this award were made by Mr. Huggins conjointly with our highly valued Treasurer and senior Vice-President, Dr. William Allen Miller. The position of the latter as one of the officers of the Society and a member of its Council has forbidden the consideration of his claims to share in the honours which the Society can bestow; and in conformity with the spirit of this, our "self-denying ordinance," it would be my duty to dwell altogether on the merits of our Medallist, if, indeed, it were possible in this case to separate between the merits of the two authors of the conjoint research. This is scarcely possible, and I must be excused, therefore, if I sometimes speak of them together.

Fraunhofer, Lamont, and others have at various times attempted to observe the spectra of the planets and fixed stars; yet, though provided with powerful instruments, they obtained no important results.

Mr. Huggins and Dr. Miller devised a method of seeking in the spectra of the fixed stars that evidence of the existence in them of known elementary substances which had been obtained in the case of the sun by Bunsen and Kirchhoff. A preliminary investigation of the spectra of the more important of the terrestrial chemical elements, and their direct comparison with the lines in the spectrum of common air, was undertaken by Mr. Huggins, with the view of providing a standard scale of comparison, which, unlike the solar spectrum, would be always at hand when stellar observations are possible. This was in itself a work of enormous labour; and when completed, the spectra of the fixed stars, including those of some double stars of contrasted colours, were attacked by the two investigators; and by a happy adaptation of comparatively moderate instrumental means, and unwearied diligence in observing and determining by micrometrical measurements the positions of objects that all but elude human vision, their researches have been rewarded by the most complete success.

The spectra of the stars were compared by a method of simultaneous observation with the spectra of many of the terrestrial elements. It is upon this method of *direct comparison* that the trustworthiness of the results obtained chiefly depends, and in this respect these observations stand alone. [In 1815 Fraunhofer recognized several of the solar lines in the spectra of the Moon, Venus, and five of the fixed stars. In 1862 Donati published diagrams of three or four lines in fifteen stars. Recently Secchi, Rutherford, and the Astronomer Royal have given diagrams of the positions, obtained by measurement only, of a few strong lines in several stars.] Eighteen stars have afforded spectra containing lines coinciding with the lines of many of the elementary substances. In thirty-seven more the spectra are full of lines which have not yet been fully compared.

On extending these researches to the Nebulæ, Mr. Huggins made the most unexpected discovery that the spectra of certain of these bodies are *discontinuous,* consisting of bright lines only, whence he drew the conclusion that "in place of an incandescent solid or liquid body transmitting light of all refrangibilities through an atmosphere which intercepts by absorption a certain number of them—such as our sun appears to be—we must probably regard these objects, or at least their photo-surfaces, as enormous masses of luminous gas or vapour. For it is alone from matter in a gaseous state that light consisting of certain definite refrangibilities only, as is the case with the light of these nebulæ, is known to be emitted."

During the last two years Mr. Huggins has examined the spectra of more than sixty nebulæ and clusters. This examination shows that these remarkable bodies may be divided into two great groups: viz. 1st, *true or gaseous* nebulæ, which furnish a discontinuous spectrum, consisting of two or three bright lines only; and 2nd, what we may distinguish as *spurious nebulæ,* or nebulous matter with clusters, which give a spectrum apparently continuous. Of the latter group a large proportion show signs of resolvability into clusters by telescopes of high power.

In the present year also Mr. Huggins has made a remarkable observation upon the small comet, known as comet No. 1, 1866. He ascertained that the minute nucleus gave a gaseous or discontinuous spectrum; whilst the spectrum of the coma, as though formed by suspended particles which reflected solar light, gave a continuous spectrum.

Since the publication of these results by Mr. Huggins, our investigators have examined with great care the spectrum of the star which is either new or has greatly increased in brilliancy in Corona borealis, and have found that that star has a spectrum of absorption, and also a gaseous spectrum of which hydrogen is probably the source. They have also connected, in one instance at least, the change in a variable star with a variation in its spectrum.

Discoveries like these, which acquaint us not only with the constituents of the heavenly bodies but also with their state of aggregation, are of the nature of those which occur only once in the course of centuries,—like the discovery of the satellites of planets, of solar spots, or of double stars, —and have the strongest possible claim on the Royal Society for such honours as the Society has at its disposal.

MR. HUGGINS,

It gives me the greatest pleasure to present you with this Medal: a testimonial of the very high estimation in which your successful labours (conjointly with Dr. Miller) are held by the Society, which has the satisfaction of regarding you as one of its most distinguished Members. You are at an age at which you may reasonably indulge in the prospect of having a long career before you; yet, however long, it will scarcely exhaust the noble field of research which you have opened for yourself, and which is one in which you are quite sure to be accompanied by the sympathy of men of science throughout the world.

A Royal Medal has been awarded to Mr. William Kitchen Parker for his researches in Comparative Osteology, and more especially on the Anatomy of the Skull, as contained in papers published in the Transactions of the Zoological Society and the Philosophical Transactions.

Mr. Parker has for several years past made investigations of great extent and distinguished merit among the *Foraminifera,* the results of which are embodied in Memoirs published by him from 1859 to 1865, in conjunction with Professor Rupert Jones and with Dr. Carpenter in the Annals of Natural History, the publications of the Ray Society, and the Philosophical Transactions.

The award of a Royal Medal to Mr. Parker has been based, however, not so much on his work in this department of zoology as on his labours in a very different and much more difficult branch of anatomy, Vertebrate Osteology.

In 1860 Mr. Parker published a memoir "On the Osteology of *Balæniceps Rex*," and in 1862 another "On the Osteology of the Gallinaceous Birds and Tinamous," in the Transactions of the Zoological Society; while a third still more important memoir, on the "Skull of the Ostrich Tribe," was read before the the Royal Society in March 1865, and is now published in the Philosophical Transactions. In these elaborate and beautifully illustrated memoirs, Mr. Parker has not only displayed an extraordinary acquaintance with the details of Osteology, but has shown powers of anatomical investigation of a high order, and has made important contributions towards the establishment of the true theory of the vertebrate skull.

Mr. William Kitchen Parker,

I present you with this Medal in testimony of the high esteem in which your investigations are held by those of our body who are qualified to appreciate them, and who look at once with approval and with expectation at the increased and still increasing interest of the communications contributed by you to our Transactions.

The Council have awarded the Rumford Medal to M. Armand Hippolyte Louis Fizeau for his Optical Researches, and especially for his Investigations into the Effect of Heat on the Refractive Power of Transparent Bodies.

In 1849 M. Fizeau rose into celebrity as the experimenter who first succeeded in measuring the velocity of light by observations limited to objects on the surface of the earth. He noted the time required for the passage of light from the place of the observer to a mirror, normal to the path of the light, 8633 metres distant, and back again, by means of a wheel having 720 teeth revolving rapidly. When the wheel made 126 revolutions in 10 seconds, the light that had passed through the notch between two teeth towards the mirror was stopped completely on its return, after reflexion, by the second of the two teeth; showing that the light had travelled 17,266 metres while the wheel had revolved through half the angle subtended at its centre by the distance between the summits of two adjacent teeth, or $\frac{1}{18144}$ second.

In 1859 he wrote a remarkable memoir, "Sur une expérience qui paraît démontrer que le mouvement des corps change la vitesse avec laquelle la lumière se propage dans leur intérieur."

His "Méthode propre à rechercher si l'azimuth de polarisation du rayon refracté est influencé par le mouvement du corps réfringent" (1860) involves such complicated arrangements that great hesitation was felt about accepting the results. But whoever has seen his experiments on the expansion and the alteration of the refrangibility of bodies when heated, is not disposed to question any conclusions regarded by Fizeau himself as well founded.

In 1862 he published "Recherches sur les modifications que subit la vitesse de la lumière dans le verre et plusieurs autre corps solides sous l'influence de la chaleur,"—the first of a series of memoirs on the change of dimensions and refractive powers of various kinds of glass, and many crystallized substances.

On the 23rd of May, 1864, he read before the Institut "Recherches sur la dilatation et la double refraction du cristal de roche échauffé;" and on the 21st and 28th of May, 1866, "Mémoires sur la dilatation des corps solides par la chaleur."

In these observations he has availed himself of the possibility of forming Newton's rings with the monochromatic sodium light when one of the interfering rays is 52,205 waves in advance of the other, a fact which, conjointly with M. Foucault, he announced in 1849. Using the length of a wave of sodium light (0·0005888 millimetre) as the standard of measure, the position of a ring being observable to within $\frac{1}{10}$ of the distance between two consecutive rings, the variation of the distance between two surfaces producing the Newton's rings can be measured to within $\frac{1}{33967}$ millimetre.

A plate of the substance to be experimented on (let us suppose it to be fluor), usually from 10 to 15 millimetres thick, bounded by parallel plane surfaces, rests upon the platform of a metal tripod (the metal was steel in the earlier observations, platinum with $\frac{1}{10}$ of iridium in the later). The feet of the tripod are screws of equal lengths terminating above in obtuse points. On these points, at a distance of about $\frac{1}{50}$ millimetre above the upper surface of the fluor, rests a plate of glass. By counting the number of rings or bands that pass over a mark on the upper surface of the fluor during a given change of temperature, the corresponding variation of distance between the lower surface of the glass and the upper surface of the fluor is given, and the expansion of the metal being known by a similar process, the expansion of the fluor is found.

The Newton's rings formed between the two surfaces of the fluor depend upon its thickness and its refractive power. The number of rings that pass over the mark on the fluor during a given change of temperature being observed, and its expansion having been found by the preceding observation, the change of its refractive power due to the change of temperature becomes known.

In this way M. Fizeau obtained several very unexpected results. Of these a few may be noticed.

The indices of refraction of most substances were found either to increase or to remain unaltered with an increase of temperature, but in fluor the index of refraction diminishes with an increase of temperature. Diamond, cuprite, and beryl have a maximum density, like water—diamond at

—42°·3 C., cuprite at —4°·3, and beryl at —4°·2. Beryl, when heated, unlike calcite, contracts in the direction of its axis, and expands in a direction making right angles with the axis. Quartz, rutile, cassiterite, spartalite, corundum, and hematite were found to expand in every direction when heated; but in each case the expansion in the direction of the principal axis was different from the expansion in a direction at right angles to the principal axis.

M. Fizeau is also the author of many other important researches—on Moser's images, on photography, on the automatic engraving of photographic images on copper, on the interference of rays of heat, on electricity and the velocity of its propagation, and on the position of the plane of polarization of light reflected from striated metallic surfaces.

PROFESSOR MILLER,

I have to request you to transmit this Medal to Monsieur Fizeau, in testimony of our interest in his researches and our high respect for his merits, which you have yourself been especially instrumental in placing in a clear light before the Council.

On the motion of Vice-Chancellor Sir W. Page Wood, seconded by Mr. Hogg, it was resolved,—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The Statutes relating to the election of Council and Officers having been read, and Mr. Balfour Stewart and Mr. C. V. Walker having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected; and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.—{ William Sharpey, M.D., LL.D.
George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Lionel Smith Beale, Esq., M.D.; William Bowman, Esq.; Commander F. J. Owen Evans, R.N.; Edward Frankland, Esq., Ph.D.; John Hall Gladstone, Esq., Ph.D.; William Robert Grove, Esq., M.A., Q.C.; William Huggins, Esq.; Thomas Henry Huxley, Esq., LL.D.; William Lassell, Esq.; Professor Andrew Crombie Ramsay, LL.D.; Colonel William James Smythe, R.A.; William Spottiswoode, Esq., MA.; Thomas Thomson, M.D.; William Tite, Esq.; Vice-Chancellor Sir W. P. Wood, D.C.L.; The Lord Wrottesley, M.A., D.C.L.

The thanks of the Society were voted to the Scrutators.

Receipts and Payments of the Royal Society between December 1, 1865, *and November* 30, 1866.

	£	*s.*	*d.*
Balances on hand	15	9	0
Annual Subscriptions, Admission Fees, and Compositions	1697	16	0
Rents	253	18	0
Dividends	1541	10	3
Ditto, Trust Funds	283	0	6
Sale of Transactions, Proceedings, &c.	383	0	2
Repayments	96	3	0
Prof. Sylvester, repaid to Donation Fund	25	0	0
	£4295	16	11

				£	*s.*	*d.*
Balance due to Bankers				135	7	10
Salaries, Wages, and Pension				1031	12	0
The Scientific Catalogue				178	17	6
Books for the Library and Binding				335	16	6
Printing Transactions and Proceedings, Paper, Binding, Engraving, and Lithography				1334	14	8
General Expenses (as per Table subjoined)				483	9	2
Donation Fund	80	0	0	129	17	9
Wintringham Fund	35	5	0			
Copley Medal Fund	4	15	0			
J. Clerk Maxwell, Bakerian Lecture	4	0	0			
Rev. Dr. Stebbing, Fairchild Lecture	2	18	9			
Croonian Lecture (Poor of St. James's Parish)	2	19	0			
				3629	15	5
Balance at Bank				638	8	1
Balance of Catalogue Account				18	19	9
,, Petty Cash Account				8	13	8
				£4295	16	11

WILLIAM ALLEN MILLER,

Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £126 0*s.* 0*d.* per annum.
Estate at Acton, Middlesex (34 A. 3 R. 11 P.), £110 0*s.* 0*d.* per annum.
Fee farm near Lewes, Sussex, rent £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per Cent. Annuities.
£28,969 15*s.* 7*d.* Consolidated Bank Annuities.
£513 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock—Bakerian and Copley Medal Fund.

Scientific Relief Fund.

Investments up to July 1865, New 3 per Cent. Annuities £6052 17 8

£6052 17 8

Dr.

	£	s.	d.
Balance	181	10	8
Subscriptions	13	13	0
Dividends	178	11	2
	£373	14	10

Cr.

	£	s.	d.
By Grants	160	0	0
Balance	213	14	10
	£373	14	10

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1866.

				£	s.	d.
Annual Subscriptions				1111	16	0
Admission Fees				170	0	0
Compositions				416	0	0
Rents				253	18	0
Dividends on Stock (exclusive of Trust Funds)				997	3	10
„ on Stevenson Bequest				544	6	5
Sale of Transactions, Proceedings, &c.				383	0	2
Chemical Society, for Proceedings, 1865–66				50	0	0
Chemical Society, Tea Expenses	£16	7	8	32	15	4
Linnean Society, Tea Expenses	16	7	8			
Geographical Society, Gas at Evening Meetings	8	4	8	13	7	8
Cambridge Local Examination Committee, Gas	4	8	0			
St. George's Rifles—Gas		15	0			
Income available for the Year ending Nov. 30, 1866				3972	7	5
Expenditure in the Year ending Nov. 30, 1866				3364	9	10
Excess of Income over Expenditure in the Year ending Nov. 30, 1866				£607	17	7

				£	s.	d.
Salaries, Wages, and Pension				1031	12	0
The Scientific Catalogue				178	17	6
Books for the Library				197	5	0
Binding ditto				138	11	6
Printing Transactions, Part II. 1865, and Part I. 1866	350	18	9	1334	14	8
Ditto Proceedings, Nos. 78–86	276	14	9			
Ditto Miscellaneous	76	6	0			
Paper for Transactions and Proceedings	228	16	5			
Binding and Stitching ditto	78	2	5			
Engraving and Lithography	323	16	4			
Upholstery, Cleaning, and Repairs				48	8	8
Miscellaneous Expenses				39	9	0
Coal, Lighting, and Gas-fittings				136	6	9
Tea Expenses				49	2	1
Fire Insurance				28	11	6
Donation:—Mablethorpe Schools				2	2	0
Taxes				8	19	2
Law Expenses				126	7	10
Advertising				9	15	6
Postage, Parcels, and Petty Charges				34	6	8
				£3364	9	10

WILLIAM ALLEN MILLER,

Treasurer.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Having compounded.	Paying £2 12*s.* annually.	Paying £4 annually.	Total.
November 30, 1865.	6	47	309	3	274	639
Since elected	......	+3	+6	......	+9	+18
Since readmitted ..	......	......	......	......	+1	+1
Since compounded..	......	......	+1	......	−1	
Since deceased	−1	−1	−14	......	−15	−31
Since defaulter	......	......	......	......	−1	−1
November 30, 1866.	5	49	302	3	267	626

December 6, 1866.

Lieut.-General SABINE, President, in the Chair.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents:—

The Treasurer,
Mr. Bowman,
Mr. Spottiswoode,
Vice-Chancellor Sir W. Page Wood.

The following communications were read:—

I. "Discussion of Tide-Observations at Bristol." By T. G. Bunt, Esq. Communicated by the Astronomer Royal. Received October 24, 1866.

(Abstract.)

This Paper contains the results of a Discussion of about 19,000 observations of Times and Heights of High Water at Bristol, for the purpose of obtaining the Empirical Laws of the Diurnal Inequalities of the Times and Heights, and of the Solar Inequality of the Times, of the tides at that port. Curves on Diagrams which accompany the Paper exhibit the results.

The Observations were taken by a Self-registering Tide-Gauge, the Clock of which has from the first been regulated by transit observation.

The Diurnal Inequalities of the tides at Bristol are not large, that of time averaging only 2 minutes (earlier or later), and that of height 2½ inches (greater or less).

Although it was stated by Sir John W. Lubbock (in 1839) that the diurnal inequality in time is too minute to be observed on our coasts, the slightest examination of the Diagram (No. 4) will be sufficient to show that this remark is inapplicable to Bristol. On this diagram are laid down the times and heights of tide registered there during six months of the year 1865. In consequence of the tranquil state of the weather, the agreement of the observed with the predicted times was unusually close; the average error in time, during the six months, being only 2½ minutes, and for six weeks less than 1·9 minute. The diurnal inequality, of time as well as of height, is throughout the diagram most conspicuous; and the agreement of the calculated with the observed inequality close and satisfactory.

For each of the Diurnal Inequalities, the residues, or errors, of the calculated Times and Heights were arranged for every half month, and for each of the twenty-four hours of Lunar transit, making (24×24=) 576 Groups. The averages of these, laid down in Curves, are shown in Diagrams No. 1 and 2.

Diagram No 3 shows the Solar Inequality of Time, obtained in a similar

manner, together with curves of the separate effects of the Solar Parallax and Solar Declination.

A sheet from the Tide-Gauge Cylinder has also been sent, as a specimen of the regularity of the registered Curves in tranquil weather.

II. "On the Heating of a Disk by rapid Rotation *in vacuo*." By Balfour Stewart, M.A., F.R.S., and P. G. Tait, M.A.

(Continuation of a Paper read before the Royal Society on June 15, 1865, and published in the 'Proceedings.') Received October 30, 1866.

16. The apparatus and certain preliminary experiments having been described in the previous paper, the authors now proceed to relate what further experiments have been made.

In the preliminary experiments it was conclusively shown (art. 8) that the effect on the pile caused by rotation of the disk was due to radiant heat, and also (art. 9) that this effect was not due to the heating of the rock-salt, which in most of the experiments was placed before the mouth of the cone.

It was also rendered probable that the effect was not due to radiation from heated air, by the two following considerations:—

(1) Because in order that nearly dry air of such a tenuity might give such a radiation it would require to be heated enormously.

(2) Because when the lampblack was removed from the aluminium disk, leaving it a rough metallic surface, the indication afforded by the galvanometer was reduced to about one-fourth of the amount with the blackened disk.

The following observations tend to strengthen this proof:—

(3) The heating effect is the same in hydrogen or in coal-gas as in air, although there is no question that the absorptive, and therefore the radiative power of coal-gas is much greater than that of air. This is shown by the following sets of experiments, which were made with the blackened aluminium disk insulated with ebonite, and with rock-salt in the cone.

No. of set.	No. of observations in each set.	Time at full speed, in seconds.	No. of turns of handle at full speed.	Heat indication.		Nature of gas.	Tension of gas, in inches.	Probable purit y of gas (absolutely pure gas = 100).
				Divisions.	Fahr.			
VII.	2	30	22	22·5	...	hydrogen	0·6	95
VIII.	3	30	22	23·3	...	air	0·7	
IX.	2	30	20	...	0°·95	hydrogen	0·5	97
X.	2	30	20	...	0°·87	air	1·1	
XI.	3	30	20	...	0°·85	hydrogen	0·25	98·5
XII.	3	30	20	...	0°·86	coal-gas	0·25	95

(4) It may be objected to (2) that the greater heating effect from a

blackened aluminium disk than from an unblackened one does not prove that this heating effect may not be due to air, since the blackened surface may be imagined to *lay hold* of the air more than the metallic one. But the following sets of experiments prove that the heating effect of the aluminium disk with both sides blackened is the same as when only one side is blackened.

No. of set.	No. of observations in each set.	Time at full speed, in seconds.	No. of turns of handle at full speed.	Heat indication, in ° Fahr.	Tension of air.	
X.	2	30	20	0·9	1·1	Disk blackened on one side.
XIII.	3	30	20	0·8	0·4	Disk blackened on both sides.

It would therefore appear to be proved that in these experiments the heating effect is due to the increased temperature of the disk.

17. Before proceeding further it may be advisable to detail some experiments made with an ebonite disk $\frac{1}{10}$ inch thick. In these experiments care was taken that the ebonite should have the same temperature throughout its thickness, so that there might be no flow of heat from the interior to the surface, or *vice versâ*. The experiments were made with rock-salt in the cone.

No. of set.	No. of observations in each set.	Time at full speed, in seconds.	No. of turns of handle at full speed.	Heat indication, in divisions.	Nature of gas.	Tension of gas, in inches.	Probable purity of gas (absolutely pure gas=100).
XIV.	3	30	20	32	air	1·1	
XV.	1	30	20	30	air	0·26	
XVI.	1	30	20	31	air	1·1	
XVII.	2	30	20	28·5	air	0·26	
XVIII.	2	30	20	28·5	air	0·25	
XIX.	3	30	20	29	hydrogen	0·25	90

From these experiments it may be taken for granted that the heat indication given by an ebonite disk is, like that from an aluminium disk, independent both of the density and chemical constitution of the residual gas. It is also highly probable that the unknown cause of the heating effect is the same for both disks.

18. To return now to the aluminium disk, it may be shown that the heating of this disk is not caused by revolution under the earth's magnetic force; for (1) the following calculation, kindly furnished by Professor Maxwell, shows that the heating effect due to this cause would, for the aluminium disk $\frac{1}{20}$ of an inch in thickness, amount, under the circumstances of rotation, only to $\frac{1}{61270}$ of a degree Fahr., whereas the observed effect is more than half a degree.

An ellipsoid, semiaxes a, b, c, revolves about the axis c with velocity ω, in a uniform magnetic field. To find its electrical state at a given instant. At the given instant let the axes of x, y, z coincide with a, b, c; then using the notation in the paper on the Electromagnetic field *,

$$\mathrm{P}=\mu\omega\gamma x-\frac{d\psi}{dx}=p\rho,\ \mathrm{Q}=\mu\omega\gamma y-\frac{d\psi}{dy}=q\rho, \mathrm{R}=-\mu\omega\,(\beta y+\alpha x)-\frac{d\psi}{dz}=r\rho,$$

P Q R electromotive force, $\alpha\,\beta\,\gamma$ magnetic intensity, ψ electric tension, μ=coefficient of magnetic induction=1 for everything but iron, p, q, r electric currents, ρ=resistance of cubic unit of volume.

The condition of the currents being confined to the ellipsoid, is

$$\frac{px}{a^2}+\frac{qy}{b^2}+\frac{rz}{c^2}=0.$$

Solving, we get

$$p=\frac{\mu\omega}{\rho}\ \frac{a^2\alpha}{a^2+c^2}z, \qquad q=\frac{\mu\omega}{\rho}\ \frac{b^2\beta}{b^2+c^2}z, \qquad r=-\frac{\mu\omega}{\rho}\left(\frac{\alpha x}{a^2+c^2}+\frac{\beta y}{b^2+c^2}\right)c^2,$$

$$\psi=\tfrac{1}{2}\mu\omega\gamma(x^2+y^2)-\mu\omega z\left(\frac{a^2\alpha x}{a^2+c^2}+\frac{b^2\beta y}{b^2+c^2}\right)+\mathrm{C}.$$

This is the complete solution. The heat (measured as energy) produced in unit of volume in unit of time is $\rho(p^2+q^2+r^2)$. The whole heat produced in the ellipsoid in unit of time is

$$\frac{4\pi}{15}\ \frac{\mu^2}{\rho}\omega^2\,abc^3\ \left\{\frac{\alpha^2a^2}{a^2+c^2}+\frac{\beta^2b^2}{b^2+c^2}\right\}.$$

If $a=b$, this is $$\frac{4\pi}{15}\ \frac{\mu^2}{\rho}\ \frac{a^4c^3}{a^2+c^2}\ (\alpha^2+\beta^2).$$

If c is small compared with a, it becomes

$$\frac{4\pi}{15}\ \frac{\mu^2}{\rho}\omega^2a^2c^3(\alpha^2+\beta^2).$$

If the axis is horizontal,

$$\alpha^2+\beta^2=\mathrm{H}^2\sin^2\theta+\mathrm{V}^2,$$

where H=horizontal magnetic force, and V=vertical magnetic force, and θ=angle between the axis of rotation and the magnetic meridian, a=radius and c half the thickness, $\omega=2\pi n$, where n denotes the revolutions per second; $\mu=1$; ρ is the resistance of unit length and unit section.

Now, the resistance of 1 metre long and 1 millimetre diameter

$$=\frac{4\rho}{\pi}10^6=0{\cdot}0375\times10^7$$

for aluminium in metrical units by Matthiessen, or $\frac{\pi}{\rho}=\frac{32}{3}$.

ρ is the same for metrical and for British measure.

At Kew horizontal force=3·81, dip 68°·10; θ=90° in the experiments $\therefore\ \alpha^2+\beta^2=104{\cdot}8$ British measure.

The revolving body is not an ellipsoid but a cylinder, equally thick throughout; to correct for this we shall put $\frac{15}{2}c^3$ for c^3.

We get for the energy converted into heat by electrical action per second 12·91 in grain-foot-second measure.

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* Phil. Trans. 1865, p. 459.

Now $772g$=energy required to raise 1 grain of water 1° Fahr.

The disk $=\frac{70,000}{16}\times\cdot22$ grain of water, ∴ energy corresponding to 1°

$$=772\times\frac{32\cdot2}{16}\times15400,$$

or rise of temperature per second$=\frac{12\cdot91}{1540\times15400}=\frac{12\cdot91}{23716000}$ degree Fahr.

or about $\frac{1}{61270}$ degree in 30 seconds.

This is when the heat is uniformly distributed through the thickness of the disk, which it will be in less than thirty seconds. If there were no conduction, the rise of temperature at the surface would be about twice the value found above.

(2) It would appear from the above formula that the heating effect due to this cause should increase with the thickness of the disk. The following experiments show that, on the contrary, the heating effect, as regards temperature, diminishes as the thickness of the disk increases.

No. of set.	No. of observations in each set.	Time at full speed.	No. of turns of handle at full speed,	Heat indication, in ° Fahr.	Tension of air, in inches.	
XIII.	3	30	20	0·8	0·4	Aluminium disk $\frac{1}{20}$ inch thick.
XX.	3	30	21	1·7	0·4	Aluminium disk $\frac{1}{40}$ inch thick.

(3) The heat indication afforded by an ebonite disk is against the conclusion that this effect is due to rotation under the earth's magnetic force.

It would therefore appear to be proved that in these experiments the increased temperature of the aluminium disk is not due to rotation under the earth's magnetic force.

19. It might perhaps be said that the heating of the disk may be due to heat conducted from the bearings into the disk and then distributed outwards; and this conjecture will require to be examined, since the bearings are, no doubt, heated by friction during the motion. This heating effect on the bearings was measured by means of a very delicate thermometer, which was inserted into a small hole in the bush through which oil is supplied to the spindle, and made to be in metallic contact with the sides of this hole; the mean of three observations made the heating effect at the spindle due to rotation to be 4° Fahr.

In the next place, the aluminium disk, separated from its metallic spindle by the ebonite washer, and in every respect the same as when made to rotate, had its spindle heated artificially by a mercury bath, generally at least 40° Fahr. above the previous temperature. After the lapse of two minutes the effect upon the pile was hardly perceptible—not more than five divisions.

1) It would appear from this experiment that the heating effect ob-

served in rotation cannot be due to heat conducted from the bearings through the ebonite washer, since a temperature difference between the bearings and the disk ten times greater than that produced by rotation causes a heating effect at least six times less than that caused by rotation in a somewhat less time.

(2) The ebonite washer used to prevent the heat of the bearings from reaching the aluminium disk, is a cylindrical disk, its thickness being two-tenths of an inch, and the area of one of its faces 3·15 square inches. It is shielded behind by a brass disk, of similar size, which brass disk being near the bearings, and metallically connected with them, we may suppose to have the same temperature as the bearings. Thus one face of the ebonite washer is in contact with brass, having the temperature of the bearings, while the other is in contact with the aluminium disk. Supposing that this washer, used to protect the aluminium disk from the heat of the bearings, was of iron instead of being of ebonite, we can calculate approximately from Principal Forbes's determinations of the absolute conductivity of this metal, how much heat would be conducted across the washer during the experiment. According to these observations, if a cube of iron whose side is one foot, have one of its faces kept permanently at a temperature 1° C. higher than the opposite face, the quantity of heat conducted across in one minute will be ·011 unit nearly, a unit denoting the amount of heat required to raise a cubic foot of water 1° C. Since in these observations both the temperature difference and the unit are expressed in the same thermometric degrees, we may, if we choose, substitute degrees Fahr. for degrees Centigrade in the above expression for conductivity. Now, if we assume as an approximation that during the whole experiment of rotation the heat conducted across such a washer is the same as if for one minute the temperature difference between both sides of the washer were kept at 2° Fahr., and if we make allowance for the surface and for the thickness of the washer, we obtain the following expression as approximately representing the heat conducted across the washer during the experiment,

$$\text{Heat} = \cdot 011 \times 2 \times \frac{3\cdot 15}{144} \times \frac{12}{\cdot 2} = \cdot 028 \text{ unit nearly,}$$

where the first factor is on account of the double temperature difference, the second on account of the surface, and the third on account of the thickness.

But a unit of heat in the above expression denotes the amount necessary to raise a cubic foot of water (or nearly 1000 ounces) 1° Fahr. Now the weight of the disk is 10·5 ounces, and its specific heat is 0·22. Hence the above amount of heat will raise the disk

$$\cdot 028 \times \frac{1000}{10\cdot 5} \times \frac{1}{\cdot 22} = 12^\circ \text{ Fahr. in temperature.}$$

Hence we see that if the material of the washer had been of the metal bismuth, of which the conductivity is 7 times less than that of iron, and if we suppose the circumstances of the experiment to be equivalent to a temperature difference of 2° Fahr. between the two sides of the washer lasting for one minute, then the quantity of heat conducted across the washer will be a little greater than that observed. But the conductivity of ebonite is no doubt very much less than that of bismuth, and therefore on this account we cannot suppose that the heating effect observed is due to conduction.

(3) In this investigation no account has been taken of the unequal distribution of temperature from the centre to the circumference of the disk, the tendency of which would be to diminish the effect upon the pile (which was directed to the circumference of the disk) of the heat passing through the washer; and indeed, when this element is taken into account, it is not surprising to find, as was actually the case, that in some preliminary experiments, where the disk was metallically connected with the spindle, the effect was not greater than with the ebonite washer.

(4) The short time in which the effect attains its maximum value is against the supposition that it is caused by conduction from the bearings.

(5) The fact that (as we shall afterwards see) the temperature effect in three aluminium disks of different thicknesses is inversely proportional to the thickness, is also against this supposition.

(6) And so is the fact that a heat-effect obeying apparently the same laws, holds for an ebonite disk in which there is but a very feeble conduction.

On the whole, therefore, we cannot suppose this effect to be due to conduction, or at least we must conclude that the effect of conduction constitutes only an exceedingly small fraction of that observed.

20. It was suggested to the authors by Professor Stokes and by Mr. Grove, that the effect might be due to vibrations of the disk, the energy of which, owing to the viscosity of the disk for such vibrations, might ultimately become converted into heat; and it is necessary to examine this question.

(1) The thickest aluminium disk was found to be out of truth not more than ·015 inch on each side. Hence, the thickness of this disk being ·05 inch, when turned with moderate rapidity, its apparent thickness should be

$$\cdot015+\cdot05+\cdot015=\cdot08;$$

and experiment showed that when turned very fast, its apparent thickness was no greater. The greatest possible range of vibrations of the disk at its circumference could not, therefore, be more than ·015 inch on either side of the position of rest.

Again, it was ascertained by means of the note given by this disk, that it vibrates about 250 times per second.

Let us suppose the whole mass to have the same range of excursion (this will of course increase the result), the equation of vibration (not allowing for loss by viscosity) is

$$x=\cdot015\cos nt$$

and also time of vibration

$$=\frac{2\pi}{n}=\frac{1}{250}.$$

Hence

$$n=500\times3\cdot14=1570, \text{ say,}$$

$$\therefore \frac{dx}{dt}=-\overset{\text{in.}}{\cdot015}\times1570\sin nt, \therefore \text{ greatest velocity } =\overset{\text{in.}}{23\cdot55},$$

or say 2 feet per second.

Hence the energy of this motion in foot pounds,

$$=\text{weight of disk in pounds}\times\frac{v^2}{2g}$$

$$=\text{weight of disk in pounds through } \tfrac{1}{16} \text{ of a foot.}$$

But an approximate experiment performed by causing the disk to ring, and noticing how long the sound lasted, would seem to show that probably the energy of vibration of the disk diminishes at first, and therefore constantly (if it is maintained) at the rate of the whole in 3 seconds. Hence in 30 seconds it loses 10 times as much as the whole; that is to say, in 30 seconds the heat produced cannot be greater than that due to the energy produced by the disk falling under gravity through $\frac{10}{16}$ of a foot. Reducing this to its heat-equivalent, the greatest possible heat effect due to vibration during 30 seconds rapid turning will be less than

$$\frac{10}{16}\times\frac{1}{772}\times\frac{1}{\cdot22}=\frac{1}{272}{}^\circ \text{ Fahr.,}$$

which is a very small fraction of the effect observed.

(2) The thin aluminium disk was out of truth about ·02 inch on each side. Its note of vibration was as nearly as possible one octave lower than that of the thick disk, while its coefficient of viscosity was somewhat greater, say in the proportion of 3 to 2, than that of the thick disk. On the supposition that the heat generated is due to vibration, if we call the heat generated during 30 seconds in the thick disk $=1$, then that generated during the same time in the thin disk ought to be

$$1\times\frac{1}{2^2}\times\left(\frac{\cdot02}{\cdot015}\right)^2\times\tfrac{1}{2}\times\tfrac{3}{2}=\tfrac{1}{3},$$

where the first factor is on account of difference of time of vibration, the second on account of difference of range, the third on account of difference of mass, and the fourth on account of difference of viscosity.

But the heating effect (as far as quantity of heat is concerned) produced in the thin disk is as nearly as possible the same as that produced in the thick disk.

This fact is therefore against the hypothesis that the heating effect is due to vibration.

(3) In order to estimate the effect (if any) of want of truth in the disk, the thick aluminium disk was purposely put out of truth about $3\frac{1}{2}$ times

its usual amount; but the heating effect was as nearly as possible the same in both cases, being 32 divisions of the scale in both.

On all these grounds it would appear that the heating effect cannot be due to vibration of the disk.

21. It is hardly necessary to mention that the heating effect cannot be due to radiation and convection from the wheelwork, which is no doubt slightly heated during the experiment, for the mass of this metallic matter is so great, that we cannot imagine it to be heated more than 1° Fahr. Now the radiation from this against the back of the disk may certainly be neglected, while the convection must be very small, since in the experiments the pressure of the air was very small. Besides, the heating effect, as will be seen shortly, was found to be independent of the pressure.

22. It has thus been shown that the disk is heated during the experiment, and that this heating effect—

(1) Is not due to rotation under the earth's magnetic force;
(2) Is not due to conduction of heat from the bearings;
(3) Nor to radiation or convection from the wheelwork;
(4) Nor to vibrations of the disk.

And in view of the large and constant nature of this heating effect it may be asserted that it cannot be sensibly due, either to one of these causes singly, or to their combined effect.

23. It will now be shown that the heating effect is independent both of the density and chemical constitution of the residual air and vapour around the disk.

In art. 16, if we compare together the 7th, 8th, 9th, 10th, 11th, and 12th sets of experiments, we shall see that the heating effect was sensibly the same, whether the residual gas was atmospheric air, or hydrogen or coal-gas.

As hydrogen diffuses very quickly, it might perhaps be supposed that when heated by rotation it might find access to the pile through the rock-salt cover more easily than heated atmospheric air, so that while the whole effect might appear the same in hydrogen as in air, yet only part of that in hydrogen might be due to radiant heat, the remainder being due to heated gas which had obtained access to the pile. This was, however, disproved by an experiment, which showed that by blackening the interior of the cone, the effect upon the pile was just as much diminished in a hydrogen vacuum as in an air vacuum; and hence in both cases the whole effect is due to radiant heat.

But, besides the residual gas, it may with truth be supposed that there is always more or less of aqueous vapour, and also a little of the vapour of oil, and perhaps of the vapour of mercury in the receiver. As regards the hygrometric state of the residual air and its influence on the disk, this would appear to be of the following nature:—

(1) When the vacuum has just been made, there is generally a hygrometric difference between the air and the surface of the disk, on account of

which there is a strictly temporary effect, either in the direction of heat or cold, at the surface of the disk, owing probably to condensation or evaporation of small quantities of aqueous vapour; but this effect disappears the moment the motion is stopped, leaving behind the permanent effect apparently unaltered.

(2) This temporary effect disappears when the disk has been left for some hours in the vacuum.

Next, with regard to vapour of oil, we cannot suppose its effect to be so large or so different in character and constancy from that of aqueous vapour as to account for the effect observed. Add to this that the effect takes place with an uncoated metallic disk probably to the same extent as with a coated one. The same remark may be made with regard to vapour of mercury. The effect would therefore appear to be independent of the chemical nature of the residual gas and vapour around the disk.

In order to prove that this effect is also independent of the pressure of the residual gas, it is only necessary to refer to the whole body of experiments which have been described, to see that between 4 inches and 0·25 inch there is no perceptible variation in the effect observed.

24. The following generalization may now be made :—

(1) If a perfectly true aluminium disk (without vibrations) be made to rotate in a vertical plane at the earth's surface, after the manner herein described, there will be an increase of the temperature of the disk, which is not due to communication of heat from the bearings or machinery, nor to the earth's magnetic force.

(2) This heating effect is independent of the density and chemical constitution of the residual air and vapour which surround the disk.

(3) It is probable that the quantity of heat developed in disks of similar extent of surface and similar circumstances of motion is the same. For, in the first place, the quantity of heat developed in three aluminium disks, ·05, ·0375, ·025 of an inch in thickness respectively, would appear to be the same, the relative thermometric effect for these disks varying inversely as their thickness, and being in the following proportions, 30, 43, 60, as determined by one complete set of experiments.

Again, the quantity of heat developed in the thick aluminium disk, with its surfaces both uncoated, was probably the same as when one surface was coated and one left bare, or as when both surfaces were coated.

25. The authors will not attempt here a further generalization, but they would desire to make one remark. In absence of definite knowledge of the nature of that medium which transmits radiant light and heat, it might be supposed possible that when a radiant body is in rapid motion, the intensity of its radiation is somewhat increased. But if we bear in mind that in these experiments the effect was observed after bringing the disk to rest, and that the temporary effect during rotation sometimes observed can probably be otherwise accounted for, we are forced to conclude that, as far as we may judge from these experiments

(and they are of a very delicate nature), there is no perceptible effect of motion upon radiation.

In conclusion the authors desire to say that they are much indebted to Mr. Beckley, who not only invented the apparatus, but assisted at all the experiments, and without whom they could not have been performed in a manner so satisfactory. They are also indebted to Mr. Atkinson for his kindness in lending them a large gasometer, and to Mr. Browning and Mr. Ladd for exceedingly true aluminium and ebonite disks.

III. "On the Bones of Birds at different Periods of their Growth." By John Davy, M.D., F.R.S., &c. Received October 23, 1866.

In this paper I beg to submit to the Royal Society the results of some further observations on the bones of birds, and more especially on those bones which in the adult contain air.

In offering them, I would wish them to be considered as a continuation of those communicated on a former occasion, and published in the Proceedings of the Society*.

In engaging in the inquiry I have had two objects chiefly in view: one to endeavour to determine whether at an early stage the bones, which at maturity contain air, differ essentially from those of birds which are then, and are permanently filled with marrow; another, to endeavour to ascertain in the instance of the former, the rate at which their early contents are absorbed, or an approximation to the time that air takes the place of the medulla.

The birds of the first kind subjected to observation have been the following: the common fowl, duck, goose, turkey, pheasant, partridge, grouse, rook, common crow, owl, sparrow-hawk, buzzard, blue tit; of the second kind, the woodcock, blackbird, water-ouzel, marten, swift, greenfinch, titlark, sparrow, stonechat, blackcap, yellow ammer, little sandpipe, canary.

I. *Of the Common Fowl.*—Of this bird I have examined the bones at different ages in a large number of instances. A few are selected as most characteristic:—

1. Of a fœtal chick taken from the egg on the fourteenth day of incubation, the bones of the extremities were much advanced, the inferior more than the superior. Their epiphyses were almost gelatinous; but the shafts were partially ossified, and had already some firmness. In both the humeri and femora, medullary matter was found. It was of a red colour, and under the microscope exhibited the usual character of this tissue. Broken up, in each were seen numerous oil-globules, with which were mixed blood-corpuscles and other corpuscles of a smaller size, some circular, some of an irregular form.

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* Vol. xiv. 337, 440, 475.

2. A chicken twenty-eight days old, reckoning from the time of hatching, was found dead on the 12th of January; during the preceding night there had been a severe frost. It weighed 10,275 grs.; one of its humeri in its moist state weighed 2·5 grs.; one of its femora 8 grs. In each of them a soft red medullary matter was detected, which, like the preceding, was composed of oil-globules, blood-corpuscles, and smaller colourless corpuscles or cells. The contents of the ulna and clavicular arch were similar, but in the latter the quantity of medullary matter was very minute.

3. Of a pullet three months old, which had been hatched on the 6th of April, and weighed, when examined, two pounds, the humeri thoroughly ossified, sank in water. From one of them, opened under water, a few air-bubbles only escaped, which came from its superior extremity. The canal of the bone, excepting the small portion from which the air proceeded, was full of red marrow of the usual appearance of medullary tissue, and with similar contents. A communication was found to exist by the air-foramen between the cancellated structure towards the head of the bone and the lung, through the adjoining air-sac.

4. Of another pullet of the same brood, but of more advanced growth, which, though three days younger when examined, weighed two pounds and three-quarters, the humeri sank in water, but in sinking maintained a perpendicular position. One of them laid open was found to contain in its inferior portion, to the extent of ·7 inch, marrow; in its superior, to the extent of 1·8 inch, air.

5. Of a pullet three months and eleven days old, which had been hatched on the 28th of May, the humeri floated in water. In one of them, laid open, a small portion of marrow was found in the cancellated structure of its distal extremity; and in this portion a conspicuous blood-vessel terminated,

6. Of a cock five months old, which weighed five pounds, the testes large, and abounding in sperm-cells and spermatozoa, the humeri contained only air.

II. *Of the Partridge.*—Of one shot on the 1st of September, which weighed 4136 grs., the humeri and femora sank in water; in sinking the distal extremity of the former reached the bottom first. From one of these, opened under water, a very little air escaped. The lining membrane was very vascular, and the lower one-fourth of the canal was full of blood of a dark colour, contained apparently in varicose vessels. Under the microscope, no oil-globules were distinguishable in the blood, only blood-corpuscles; nor in any part of the cavity were there found any remains of marrow.

2. Of another, shot on the same day, which weighed 5226 grs., the humeri swam in water, the femora sank. Air only was found in the former. On the lining membrane there were two or three delicate vessels containing florid blood. The femora were full of a reddish marrow.

3. Of another, shot on the 19th of October, which weighed 5432 grs.,

the state of the humeri and femora was very similar to that of the preceding. The humeri contained only air, the femora only marrow; the lining membrane of the former was finely vascular; the medullary tissue of the latter abounded in oil-globules and corpuscles suggestive of blood-corpuscles altered.

III. *Of the Pheasant.*—Of a female shot on the 9th of October, both the humeri and femora swam in water. Both contained air without any marrow. The former were perfectly white, and no vessels were visible in their lining membrane; the latter were of a reddish hue, and their lining membrane was beautifully vascular, the vessels delicately ramifying, of a bright florid hue, and anastomosing, and this throughout the whole of the circumference.

2. Of another female, shot on the 17th of October, the humeri contained only air, the femora air with a trace of marrow*. The latter were redder than the former, and their lining membrane was very vascular.

IV. *Of the Goose.*—Of one hatched in the spring, which, when examined on the 26th June, weighed eight pounds, the humeri sank in water. They were full of marrow of a light bright-red colour. The quantity collected from one of them was about 82 grs. It exhibited the usual character of medullary cellular structure, and was rich in oil, which in drying exuded copiously from it; but it contained comparatively few blood-corpuscles or blood-vessels, of which these corpuscles were the index.

2. Of another hatched on the 22nd of April, which on the 7th of August weighed nine pounds and a half, a humerus dissected out weighed 561 grs., was 7·5 inches in length, and its shaft between ·4 and ·5 inch in width. It sank in water. Its upper third portion was of a darker hue than its inferior two-thirds. The latter was full of a light-red marrow, abounding in oil. It owed its reddish hue to blood-vessels in the medullary structure. The former, entirely without air, contained a collection of blood-vessels resting on a cancellated structure. These vessels had the appearance of veins, seemed largely varicose, and were full of dark blood, which (examined whilst warm) was still fluid. Of the other wing, the humerus was somewhat different—the difference was in the superior portion. Although the bone sank in water, a little air was found in this portion, and the blood-vessels in it were less distended, and contained, instead of dark blood, aërated blood of a florid hue. The marrow, which occupied about two-thirds of the shaft, terminated abruptly superiorly, and there a pretty large blood-vessel united the two parts—the medullary and the varicose portions.

3. Of another hatched in the spring, examined on the 31st of October, the humeri contained only air. The lining membrane of its cavity was rich in blood-vessels.

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* It was detected by washing out the cavity of the bone with a weak solution of salt. Oil-globules were found suspended in the solution.

4. Of the humerus of a goose hatched in the spring, weighing on the 2nd of November nine pounds, the canal was found full of air, with the exception of its inferior one-third; this contained marrow rich in oil. In one of several portions of it submitted to the microscope, a red vessel was seen containing granules and oil-globules. In one direction it ended abruptly, as if torn; in the other it was lost, as it were, by diffusion into, or blending with the cells of the medullary tissue. The upper portion of the marrow, it may be remarked, where it came in contact with the air, had, as in some other specimens, a rounded well-defined surface. In this instance no large blood-vessels were found in any part of the bone; and on the delicate membrane connecting the trabeculæ of the cancellated structure, only a very few delicate vessels of a florid hue were to be seen.

5. Of a fifth, also hatched in the spring, when examined on the 20th of November, the humeri, like those of the last but one, were found to contain only air. Their lining membrane was very vascular and partially varicose.

V. *Of the Common Duck.*—Of a young drake hatched on the 26th of April, which when weighed on the 13th of August was four pounds, the humeri sank in water. They containad a light reddish marrow, and were entirely destitute of air.

2. Of two ducks of the same brood as the drake, the humeri, examined on the 15th of August, sank in water. Two-thirds of each were filled with marrow; one-third, the upper portion, with dark blood, seemingly in varicose vessels, which were connected with a light-coloured delicate membrane.

3. Of a duck, little more then one year old, the humeri contained only air.

VI. *Of the Red Grouse.*—Of one, which on the 12th of August was not fully fledged (it was shot for examination), the humeri contained air, the femora marrow.

2. Of another more advanced, shot on the 31st of August, weighing a pound and a quarter, the humeri contained air, the femora a little marrow, but more air, the former inferiorly.

3. Of a third, shot on the 28th of August, which weighed 8770 grs., the humeri were hollow, the femora partially so, a little reddish marrow only remaining, and this in their inferior extremity.

4. Of one shot on the 27th of August, which weighed 11,856 grs., and which, judging from its plumage, was probably a spring bird, both the femora and humeri swam in water and were free from any trace of marrow. The lining membrane of the cavities of the femora was beautifully vascular, the vessels of a florid hue from the well-aërated blood which they contained. The lining membrane of the humeri was similarly vascular, but in a less strongly marked manner.

VII. *Of the Rook.*—Of a young one not quite capable of flight, shot on the 11th of May, and which then weighed 6132 grs., the humeri sank in water; they contained no air, but a soft marrow, of a blood-red colour. The bones were very vascular, and were easily cut.

2. Of another, shot on the 22nd of June, when capable of flight, though the quill-feathers of its wings were only partially hollow, its weight 5113 grs., the humeri sank in water in a perpendicular position. Of one which was examined, its inferior two-thirds were found full of a reddish marrow, its superior one-third of air. The proportion of oil seemed greatest in the distal part of the marrow. The bones were less vascula than those of the preceding, and of greater firmness.

3. Of a third, shot on the 23rd of June, then weighing 4982 grs., which, judging from the state of its feathers, its bursa Fabricii, ovary, and oviduct, had been hatched in the spring, the humeri were full of air without a vestige of marrow *.

VIII. *Common Crow.*—Of one shot on the 1st of June, then weighing 5402 grs., its quill-feathers not fully formed, the humeri sank in water, and contained only marrow.

2. Of another shot on the 21st of June, weighing 6533 grs., capable of flight, the humeri contained air. The lining membrane was very vascular.

IX. *Of the Tawny Owl.*—Of a young one, examined on the 21st of June, then weighing 4796 grs., its quill-feathers not fully formed, the humeri contained a very red marrow, and were entirely destitute of air.

2. Of one of uncertain but mature age, judging from its general appearance, and which weighed on the 8th of April 5776 grs., the humeri were full of air. There was also air in the scapular arch, and partially in the furcula, its proximate portion.

X. *Of the Sparrow-hawk.*—Of a young one, which on the 31st of July weighed 3686 grs., its sternum then only partially ossified, still flexible, the humeri were for the most part hollow; the little marrow they contained was confined to their distal portion. The same remark was applicable to the scapular arch. The femora contained even less marrow than the humeri; they were very nearly full of air.

XI. *Of the Buzzard.*—Of a young one taken from its nest on the 10th of June, when supposed to be about a fortnight old, then weighing 5293 grs., the quill-feathers of wings only sprouting, the bones generally were very vascular; the humeri and femora sank in water. Their ossification was much advanced, but the sternum was still cartilaginous. After drying, all the bones floated in water. The humeri and femora, now laid open, were found to contain a red matter lining but not filling the cavities, suggestive of its having been, in its moist state, a fluid or a semifluid. In this, its dried state, it was of a firm consistence. After soaking in water and trituration, it formed an emulsion, which, as seen under the microscope, was found to contain oil-globules and particles of different kinds. Digested and

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* This bird, like most rooks, was infested with parasites, lice. They were plentiful even in the cavity of the wing quill-feathers. According to Dr. Gray, F.R.S., to whom I sent one, and who kindly gave me its name, it is a Decophorus (*D. atratus*).

heated in alcohol, it was partially dissolved, the solution becoming turbid on cooling. Evaporated, and as seen under the microscope, the residue, proportionally small, seemed to consist chiefly of fatty and oily matter (stearine and olein ?), with some needle crystals.

XII. *Of the Blue Tit.*—Of a young one taken from the nest on the 31st of May, then weighing 28·5 grs., quite naked, except a few delicate fibre-like feathers on the head, some yolk still remaining in the abdominal cavity, the humeri were very small and pale, so small that no attempt was made to examine them, except by crushing. Comminuted with a few drops of solution of salt, and subjected to the microscope, there were seen mixed with the fragments of cartilage, blood-corpuscles and oil-like globules.

2. Of a young one, nearly fully fledged, which on the 3rd of August weighed 142 grs., the humeri sank in water, maintaining a perpendicular position. They were completely formed and well ossified. Excepting towards their inferior extremity they were white, there to a small extent they were red. The white portion, at least nine-tenths of the entire length, contained air; the red, not exceeding one-tenth, marrow which, as seen under the microscope, had all the character of medullary tissue and abounded in oil.

3. Of a third, shot in the same place as the preceding, and probably of the same brood, which on the 18th of August weighed 179·5 grs., the humeri floated in water, and were entirely full of air. The feathers of the abdomen were not fully formed, and the bursa Fabricii was much smaller than that of the preceding.

From these results, may not the following conclusions be drawn?

First, that at an early stage, and up to a certain period of growth, marrow exists in the bones specified, of all the birds first named; and that about the time of hatching the medullary tissue abounds less in oil or fatty matter than at a later, the proportion varying in different instances; least probably in birds of prey, such as the buzzard and owl; most in birds, the food of which is mostly vegetable, such as the goose.

Secondly, that the substitution of air for marrow in those bones which are eventually hollow, varies as to time in different species; is earlier in the rook, the crow, the grouse, the tit, than in the common fowl, duck, and goose, especially the latter; the exchange of one for the other having probably some relation to the time of taking wing and the use of the parts; and, in accordance, the humeri, except in the instance of the sparrow-hawk, seemed to have the marrow absorbed somewhat earlier than the femora. It may be conjectured that, like the residual yolk in the young bird, the marrow in the bones in question may serve in part as food, nourishing in the act of its removal.

Relative to the structure of the hollow bones, I have but a few words to offer.

In the humeri there is a peculiarity which may be deserving of mention. Towards the head of the bone, near the pneumatic foramen, the cancellated structure, connected more or less by a delicate membrane, performs the part of a valve, as indicated by its permitting a free access of air in one direction, but preventing in the other, its exit. This is clearly shown by the use of the blowpipe, and in no instance have I found an exception.

In the humerus of the common fowl, in three instances (a section of the bone having been made) the air has been found to enter from the pneumatic foramen by a small bony canal contiguous to the side of the bone, in length about ·6 inch, in diameter about ·06 inch *.

It is said that the trabeculæ, or minute columnæ in the cancellated structure of the hollow bones, are also hollow. In some of those of the humeri of the adult buzzard I have found a canal, but not in others; these were solid. Nor have I found them otherwise than solid in the humerus of the common fowl, goose, and turkey.

As to the bones of those birds of the second kind, in which the marrow is persistent through life, I may briefly remark that in them, as in the former, at an early period the marrow seems to be comparatively poor in oily matter; and that the earlier, the nearer the embryo state, the less is its degree of consistence, the nearer it is to a liquid, and the larger is the proportion of blood-corpuscles and of albuminous matter, and the smaller the proportion of the adipose.

When mature of growth, the bones of these birds appear to be richer in oily matter than those of the former permanently without air. Thus the tibia of the one kind in the dried state invariably sinks in water, whilst that of the first kind only partially sinks; the marrow in drying in the latter, from containing less oil, contracting more, and allowing of the entrance of air. In the radius and ulna the difference is less strongly marked; these bones in their dried state commonly sinking in water, even when belonging to birds of the first kind.

As regards the quality of marrow in the bones of different birds, the trials I have made have been very limited. I am disposed to infer from them that, besides differing, as in many instances it does in colour, it may differ also in composition, in the proportion of adipose matter and its kind, and in the proportion of albuminous matter; in some, as in the bones of the goose, oil most abounding; in others, as in those of the rook and buzzard, albuminous matter and fat of the stearine kind. Even in the bones of birds of the second kind, such as their long bones, there is a difference in this respect; of these, all that I have examined sink in water, with the exception of the femora, which only partially sink, the marrow in them being less rich in adipose matter, and consequently in drying contracting more, and, as before remarked, admitting more air.

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* In one instance the same kind of structure was found in the femur of a pheasant.

As to the marked difference of birds of the two kinds in relation to the condition of their bones, the rationale is not very obvious. Perhaps an approximation to the truth, or to the probable, may be made by comparing the bones of birds of the two kinds, which are possessed of similar powers, the swift, for instance, and the buzzard, rivals in swiftness of flight and enduring power of wing. How different are their humeri! of the former, very short, strong, and compact, provided with firm and large processes for the attachment of muscles; in the latter, long, hollow, and light, and comparatively brittle, yet sufficiently firm to bear without fracture the muscles which act on them. Here, have we not after a manner a kind of substitution of qualities? great strength and extended surface in small space in the one, for lightness with greater length of leverage in the other. Further, the one kind of bone, that which contains marrow, being less brittle than that which contains air, and more yielding, may be less liable to fracture; a quality which, in the bird, before the ossification is complete, may be of essential service; so that, teleologically considered, it may perhaps serve to account for the bones which are eventually hollow having primarily marrow in place of air.

December 13, 1866.

WILLIAM BOWMAN, Esq., Vice-President, in the Chair.

Among the Presents announced were two manuscript volumes, by Solomon Drach, Esq., F.R.A.S., containing various Tables in Pure Mathematics, presented by the author.

The following communications were read:—

I. "On Poisson's Solution of the Accurate Equations applicable to the Transmission of Sound through a Cylindrical Tube; and on the General Solution of Partial Differential Equations."

By R. Moon, M.A., late Fellow of Queen's College, Cambridge. Communicated by Prof. J. J. Sylvester. Received November 14, 1866.

(Abstract.)

The pair of equations

$$\pm\frac{v}{a}=\log_{\varepsilon}\frac{\rho}{\mathrm{D}},$$

$$v=\phi\{(v\pm a)t-q\},$$

which constitute Poisson's solution of the accurate equations applying to the transmission of sound through a cylindrical tube derived by La Grange's method, have long attracted the attention of mathematicians.

For La Grange's equations we may substitute the following, viz.—

$$\left.\begin{aligned}\frac{dv}{dt}+\frac{a^2}{\mathrm{D}}\frac{d\rho}{dx}=0,\\ \frac{dv}{dx}+\frac{\mathrm{D}}{\rho^2}\frac{d\rho}{dt}=0.\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (\mathrm{A})$$

The first of these is obtained from the equation of the Encyc. Met. (Art. "Sound"), by putting v for $\frac{dy}{dt}$ and $\frac{\mathrm{D}}{\rho}$ for $\frac{dy}{dx}$.

The second results from a similar substitution in the analytical condition,

$$\frac{d}{dx}\left(\frac{dy}{dt}\right)=\frac{d}{dt}\left(\frac{dy}{dx}\right).$$

Poisson's solution has always been regarded as imperfect, and may easily be shown to be so.

I was some time ago struck by finding that the above equations, while they yielded with facility the result of Poisson, notwithstanding their simplicity, baffled every effort to extract from them one more consonant to the general exigencies of the problem.

I had at this time arrived at the conclusion that the law of pressure assumed in the received theory of Fluid Motion could not be generally true, and in a Paper* communicated to the Royal Society, had pointed out that, in a certain case of motion, the assumption of the truth of that law led to a contradiction; while in another case of motion the expression for the pressure given by the received theory was palpably erroneous.

It occurred to me, therefore, that the defective law of pressure of the received theory accounted for the defective solution which alone was obtainable from the equations of motion derived from it. If the law of pressure of the received theory was not always true, if it held only when certain conditions were satisfied, those conditions would obviously have the effect of dismissing from the complete solution of the problem obtained on a perfect theory at least one of the two arbitrary functions which it must necessarily involve.

With a view to establishing this point, assume the solution of the equations of motion to be contained in the pair of equations,

$$\left.\begin{aligned}\mathrm{F}(xyt\rho v)=\phi\{f(xyt\rho v)\},\\ \bar{\mathrm{F}}(xyt\rho v)=\psi\{\bar{f}(xyt\rho v)\}.\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (\mathrm{B})$$

If these equations satisfy the equations (A), the latter will equally be satisfied by the pair of equations,

$$\mathrm{F}(xyt\rho v)=v,$$
$$\bar{\mathrm{F}}(xyt\rho v)=\psi\{f(xyt\rho v)\}.$$

But it is shown in my Paper that these latter can only satisfy the equations (A) on the supposition that F is of the form

$$\mathrm{F}=\mathrm{F}(v\pm a\log_{\varepsilon}\rho).$$

Moreover, since we have in equations (B) the function F equal to an

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[*] On the True Theory of Pressure as applied to Elastic Fluids in Motion.

arbitrary function of f, conversely we must have f equal to an arbitrary function of F. Hence, in exactly the same way in which it is proved that F must have the above form, we may equally prove that f, F, f must severally have the same form. It clearly results, therefore, from the above assumed solution (B), that the equations (A) are insoluble, except on the assumption that the velocity may be expressed in terms of the density alone; *i. e.* that we may assume

$$v=f(\rho).$$

But substituting this value of v in equations (A), the latter become

$$f'(\rho)\frac{d\rho}{dt}+\frac{a^2}{D}\frac{d\rho}{dx}=0,$$

$$f'(\rho)\frac{d\rho}{dx}+\frac{D}{\rho^2}\frac{d\rho}{dt}=0,$$

whence we get

$$\overline{f'(\rho)}\Big|^2=\frac{a^2}{\rho^2}, \qquad \overline{\frac{d\rho}{dt}}\Big|^2=\frac{a^2\rho^2}{D^2}\overline{\frac{d\rho}{dx}}\Big|^2;$$

and eventually, D being the value of ρ when $v=0$,

$$\pm\frac{v}{a}=\log.\frac{\rho}{D},$$

$$\rho=\phi\left\{x\mp\frac{a\rho}{D}.t\right\},$$

which is the most general solution of which the equations of motion are susceptible; and which, making allowance for the difference of the ordinates employed in the two cases, y referring to the particle when in motion, and x to its position of rest, is identical with that of Poisson.

The failure of mathematicians to derive from the equations of motion of the received theory a solution containing two arbitrary functions has hitherto, I apprehend, been universally attributed to difficulties of integration. So far is this view from being well founded, however, that in a postscript to my Paper it is shown that, *assuming the pressure to follow any law whatever*, a solution of the equations of motion can be obtained containing two arbitrary functions; a result, however, which requires that the expression for the pressure shall satisfy certain conditions, which conditions are *violated* when the pressure is assumed to vary with the density alone.

Whatever be the law of pressure, it must always be capable of being expressed in terms of x and t. Moreover, the velocity and density are in like manner severally capable of being expressed in terms of x and t; whence it follows that we may always express x in terms of ρ and v, and equally that we can express t in terms of ρ and v; so that, whatever be the law of pressure, we may assume it to be a function of ρ and v.

Hence, assuming

$$\frac{dp}{dx}=\frac{dp}{d\rho}\cdot\frac{d\rho}{dx}+\frac{dp}{dv}\cdot\frac{dv}{dx}=R\frac{d\rho}{dx}+V\frac{dv}{dx},$$

where R and V are functions of ρ and v only, we |have for our equations of motion the following, viz.

$$0=\frac{dv}{dt}+\frac{\mathrm{R}}{\mathrm{D}}\frac{d\rho}{dx}+\frac{\mathrm{V}}{\mathrm{D}}\frac{dv}{dx},$$

$$0=\frac{dv}{dx}+\frac{\mathrm{D}}{\rho^2}\frac{d\rho}{dt},$$

of which the following pair of equations constitute the solution, viz.

$$f_1(\rho,v)=\phi\left\{x-\frac{\mathrm{V}+\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2}}{2\mathrm{D}}.t\right\},$$

$$f_2(\rho,v)=\psi\left\{x-\frac{\mathrm{V}-\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2}}{2\mathrm{D}}.t\right\}.$$

where $f_1(\rho,v)=$const., and $f_2(\rho,v)=$const. are the respective integrals of the ordinary differential equations,

$$dv-\frac{\mathrm{V}-\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2}}{2\rho^2}d\rho=0,$$

$$dv-\frac{\mathrm{V}+\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2}}{2\rho^2}d\rho=0,$$

which involve the variables v and ρ only. But this result is dependent on the fact of the following conditions being satisfied, viz., that we have

$$\mathrm{K}_1\frac{d\mathrm{K}_1}{d\rho}+\mathrm{R}\frac{d\mathrm{K}_1}{dv}=0,$$

$$\mathrm{K}_2\frac{d\mathrm{K}_2}{d\rho}+\mathrm{R}\frac{d\mathrm{K}_2}{dv}=0,$$

where

$$\mathrm{K}_1=\mathrm{V}-\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2},$$

$$\mathrm{K}_2=\mathrm{V}+\sqrt{\mathrm{V}^2+4\mathrm{R}\rho^2},$$

which conditions *cannot* be satisfied if the law of pressure depends upon the density only (in which case V=0 and R contains ρ only), as may easily be shown.

With regard to the theory of Partial Differential Equations, I conceive that the methods indicated in the Paper will serve to elicit *every* solution of a partial differential equation of the second order and first degree, save one, viz., an integral solution consisting of a simple relation between the variables x, y, z, free from arbitrary functions, and which is not derivable from a solution containing arbitrary functions, by assigning particular values to the latter.

If the equation

$$0=\mathrm{R}\frac{d^2z}{dx^2}+\mathrm{S}\frac{d^2z}{dxdy}+\mathrm{T}\frac{d^2z}{dy^2}-\mathrm{V}$$

have a first integral consisting of the pair of equations

$$\mathrm{F}(xyzpq)=\phi\{f(xyzpq)\},$$

$$\bar{\mathrm{F}}(xyzpq)=\psi\{\bar{f}(xyzpq)\},$$

or consisting of the pair of equations

$$F(xyzpq)=0,$$
$$\overline{F}(xyzpq)=\psi\{\overline{f}(xyzpq)\},$$

or of the single equation

$$F(xyzpq)=\phi\{f(xyzpq)\},$$

or of the single equation

$$F(xyzpq)=0,$$

then in every one of these cases we must have

$$0=R\,.\,\overline{F'(q)}|^2-S\,.\,F'(q)\,.\,F'(p)+T\,.\,\overline{F'(p)}|^2,$$
$$0=R\,.\,F'(x)\,.\,F'(q)+T.F'(p).F'(q)+\{R.F'(q)\,.p+T.F'(p)\,.\,q\}F'(z)$$
$$+V\,.\,F'(p)\,.\,F'(q)*.$$

It is also shown with more or less of generality, and it is capable of being shown generally, that if the given equation admit of a complete integral solution containing two arbitrary functions, it will necessarily have two first integrals, each of which will be of the form

$$F(xyzpq)=\phi\{f(xyzpq\}.$$

It might have been added, that if the general equation of the second order and second degree be written

$$0=P_{r_2}\,.\,r^2+P_{s_2}\,.\,s^2+P_{t_2}\cdot t^2+P_{rs}rs+P_{rt}\,.\,rt+P_{st}\,.\,st$$
$$+P_r r+P_s\,.\,s+P_t+P;$$

and it is satisfied by the equation

$$F(xyzpq)=\phi\{f(xyzpq)\},$$

then F and f must severally satisfy *all three* of the following equations, viz.

$$F'(p)-l\,F'(q)=0,$$
$$F'(x)+F'(z)p-mF'(p)=0,$$
$$F'(y)+F'(z)q-nF'(q)=0,$$

where

$$0=P_{t_2}\,.\,l^4-P_{st}\,.\,l^3+(P_{s_2}+P_{rt})l^2-P_{rs}\,.\,l+P_{r_2},$$
$$0=P_{r_2}m^2+P_{rt}mn+P_{t_2}n^2-P_r m-P_t n+P,$$
$$0=(2P_{r_2}-P_{rs}\,.\,l+P_{rt}\,.\,l^2)m$$
$$+(2P_{t_2}l^2-P_{st}\,.\,l+P_{rt})n$$
$$-(P_t l^2-P_s l+P_r).$$

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* The functions f, $\overline{F}$, $\overline{f}$ must satisfy the same conditions, except in the second of the above cases.

II. "Abstract of the Results of the Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India, Australia, made at the Ordnance Survey Office, Southampton." By Captain A. R. Clarke, R.E., F.R.S., &c., under the Direction of Colonel Sir Henry James, R.E., F.R.S., &c., Director of the Ordnance Survey. Received November 15, 1866.

(Abstract.)

In the preface to this paper, Sir Henry James gives an account of the circumstances under which the work was undertaken, as follows. (A Table of results is appended, p. 313.)

The principal triangulation of the United Kingdom was finished in 1851; and the triangulations of France, Belgium, Prussia, and Russia were so far advanced in 1860, that, if connected, we should have a continuous triangulation from the Island of Valentia on the south-west extremity of Ireland, in north latitude 51° 55′ 20″, and longitude 10° 20′ 40″ west of Greenwich, to Orsk on the River Ural in Russia.

It was therefore possible to measure the length of an arc of parallel in latitude 52° of about 75°, and to determine, by the assistance of the electric telegraph, the exact difference of longitude between the extremities of this arc, and thus obtain a crucial test of the accuracy of the figure and dimensions of the earth, as derived from the measurement of arcs of meridian, or the data for modifying the results previously arrived at.

The Russian Government, therefore, at the instance of M. Otto Struve, Imperial Astronomer of Russia, invited (in 1860) the cooperation of the Governments of Prussia, Belgium, France, and England, to effect this most important object, and to their great honour they all consented, and granted the necessary funds for the execution of the work.

The portion of the work which was assigned to me, was the connexion of the triangulation of England with that of France and Belgium, and I published the results of this operation in 1862*. But this work has been done in duplicate; for when application was made to the French Government to permit the necessary observations to be made in France, they not only consented to allow this, but at the same time volunteered to join in the labour and expense of the work itself.

It would obviously have been wrong to mix up observations made with different kinds of instruments and on different principles, and therefore it was agreed that the work should, in fact, be made in duplicate, both the French and English geometricians using exactly the same stations.

The results obtained by the French geometricians is published in the

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* Extension of the Triangulation of the Ordnance Survey into France and Belgium. London, 1863.

Supplement to vol. ix. of the 'Mémorial du Dépôt Général de la Guerre,' 1865, and the agreement with the results obtained by the English is truly surprising.

But however accurately the trigonometrical observations might be performed, it is obvious that, without a knowledge of the exact relative lengths of the standards used as the units of measure in the triangulation of the several countries, it would be impossible accurately to express the length of the arc of parallel in terms of any one of the standards.

It was therefore necessary that a comparison of the standards of length should be made, and as we had a building and apparatus expressly erected for the purpose of comparing standards at this Office, the English Government, on my recommendation, invited the Governments of the several countries named to send their standards here, and we have had the following compared with the greatest accuracy :—

1. Russian standard, double toise, P.
2. Prussian standard toise.
3. Belgium standard toise.
4. Platinum metre of the Royal Society, compared with the standard metre of France by M. Arago.
5. English standard yards, A, B, C, 29, 47, 51, 55, 58.
6. Ordnance Survey 10-foot standard bar.
7. Indian 10-foot standard bars, new and old.
8. Australian 10-foot standard bar.
9. In addition to the above, the 10-foot standard bar of the Cape of Good Hope was compared here in 1844.

We have invited the Governments of Austria, Spain, and the United States of America, also to send their standards. We have been promised that of Austria, and, but for the unfortunate war in which she has been lately engaged, we should have received it before this.

I have entrusted the execution of the work of comparison and the drawing up of the results to Captain Alexander R. Clarke of the Royal Engineers, who designed the apparatus used. The numerous comparisons to be made entailed a great amount of labour upon him and his assistants, Quartermaster Steel and Corporal Compton, of the Royal Engineers.

Before the connexion of the triangulation of the several countries into one great network of triangles, extending across the entire breadth of Europe, and before the discovery of the electric telegraph and its extension from Valentia to the Ural Mountains, it was not possible to execute so vast an undertaking as that which is now in progress. It is, in fact, a work which could not possibly have been executed at any earlier period in the history of the world. The exact determination of the figure and dimensions of the earth has been the great aim of astronomers for upwards of two thousand years, and it is fortunate that we live in a time when men are so enlightened as to combine their labours to effect an ob-

Measures.	Expressed in Terms of the Standard Yard. —— 𝔜.	Expressed in inches. —— Inch $=\frac{1}{36}$ 𝔜.	Expressed in lines of the Toise. —— Line $=\frac{1}{864}$ 𝔗.	Expressed in Millimetres. —— Millimetre $=\frac{1}{1000}$ 𝔐.
The Yard .... °	1·00000000	36·000000	405·34622	914·39180
Copy No. 55 of the Yard at its Standard Temperature of 62·00 F.	0·99999960	35·999986	405·34606	914·39143
Ordnance Standard Foot ,, ,, ,, 62·00	0·33333284	11·999982	135·11521	304·79681
Indian Standard Foot ,, , ,, 62·00	0·33333611	12·000100	135·11653	304·79980
Ordnance 10-foot Bar O_1 ., ,, ,, 62·00	3·33333717	120·000138	1351·15563	3047·97616
Ordnance 10-foot Bar OI_1 ,, ,, ,, 62·00	3·33335432	120·000755	1351·16259	3047·99184
Indian 10-foot Bar I_S ,, ,, ,, 62·00	3·33340138	120·002450	1351·18166	3048·03488
Indian 10-foot Bar I_B ,, ,, ,, 62·00	3·33353284	120·007182	1351·23495	3048·15508
Indian 10-foot Bar I[illegible] ,, ,, ,, 62·00	3·33331457	119·999324	1351·14647	3047·95550
Australian Standard O_4 ,, ,, ,, 62·00	3·33330427	119·998954	1351·14230	3047·94608
Australian Standard OI ,, ,, ,, 62·00	3·33333747	120·000149	1351·15576	3047·97644
Ordnance Toise ,, ,, ,, 61·25	2·13166458	76·739925	864·06219	1949·17660
Ordnance Metre ,, ,, ,, 61·25	1·09374800	39·374928	443·34662	1000·11420
Royal Society's Metre à traits ,, ,, ,, 32·00	1·09360478	39·369772	443·28857	999·98324
Prussian Toise No. 10 ,, ,, ,, 61·25	2·13150911	76·734328	863·99917	1949·03444
Belgian Toise No. 11 ,, ,, ,, 61·25	2·13150851	76·734306	863·99893	1949·03390
Russian Double Toise P ,, ,, ,, 61·25	4·26300798	153·468287	1727·99419	3898·05952
The Toise..	2·13151116	76·734402	864·00000	1949·03632
The Metre	1·09362311	39·370432	443·29600	1000·00000

ject desired by all, and at the first moment when it was possible to execute it.

A full detailed account of the 'Comparisons of the Standards of Length,' with numerous plates, has just been published, and may be obtained from the agents for the sale of the publications of the Ordnance Survey.

December 20, 1866.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On the Formation of 'Cells' in Animal Bodies." By E. Montgomery, M.D. Communicated by J. Simon, Esq. Received November 8, 1866.

(Abstract.)

I.—*Observations.*

So called organic "cells," chiefly those of various cancerous tumours, were seen, on the addition of water, to expand to several times their original size, and at last to vanish altogether into the surrounding medium.

The "nucleus" did not always participate in this change, but at times remained unaltered, whilst the outer constituents of the "cell" were undergoing this process of expansion.

This curious phenomenon of extreme dilatation is intelligible only on the supposition that the spherical bodies in question are in reality globules of a uniformly viscid material, which by imbibition swells out till at last its viscosity is overcome by the increasing liquefaction.

In embryonic tissues and in various tumours, single "nuclei" were seen, each surrounded by a shred of granular matter. On the addition of water there would bulge from one of the margins of the granular mass a segment of a clear globule, which continued growing until it had become a full sphere, which ultimately detached itself, and was carried away by the currents. At other times no such separate globule would be emitted, but the entire granular shred would itself gradually assume the spherical shape, ultimately encompassing the "nucleus," and constituting with the same the most perfect typical "cell."

Not only single "nuclei" were found, each surrounded by a shred, but also clusters of two, four, and more were seen similarly enclosed by a proportionately large granular mass. Under these circumstances it sometimes occurred that, on the addition of water, the whole granular mass of such a cluster became transformed into a large sphere, containing two, four or more "nuclei." The resulting body was to all appearance identical with shapes well known under the name of "mother-cells." In all these cases the granular shred must have partly consisted of a viscid material, which, on imbibition, naturally assumed the spherical shape.

Primary globules were surrounded by a secondary globule, and thus the typical "cell" was completed under the observer's eye.

In some instances the globules resulting from the transformation of the granular mass were at first bright and transparent, the granules having completely disappeared. They, however, gradually reformed, showing at first molecular motion, then crowding more and more, till at last the whole mass seemed to undergo coagulation.

Alternate liquefaction and coagulation of the same material was found to play an important part in the development of "cells."

Masses of certain viscid materials do not, on imbibition, expand uniformly throughout their entire bulk, but globules of a definite size are emitted, as many as the mass will yield.

The crystalline lens of many young animals affords, when treated with water, a beautiful illustration of this fact. Its homogeneous material is transformed, under the influence of imbibition, into a vast number of globules of nearly equal size.

Hyaline embryonic tissues display, under similar conditions, the same phenomenon.

Certain inferences lead one to suspect that this size-limiting property is due to the crystallizing propensity of some ingredient of these viscid substances.

Blood-corpuscles, human blood corpuscles at least, are evidently tiny lumps of a uniformly viscid material.

When broken up into fragments, each fragment assumes the spherical shape.

On slow imbibition, they often emit a clear sphere, or a segment of one.

In various specimens of fœtal blood, each blood-corpuscle was seen to emit as many as two and even three equally sized globules, the original corpuscle being at last no more distinguishable from its descendants. This is sufficient proof of the uniformly viscous nature of the blood-corpuscles.

In many cancers the most recently formed part consists of mere fibres. These after a time become "nucleated." The "nuclei" are at first very elongated, this being due to the lateral pressure of the still fibrous texture. But as the mass gradually softens, the ovals expand more and more into spheres, forming the primary globules, round which, as has already been shown, a secondary globule is often seen to shape itself.

Chemical differentiation transforms first one portion of the fibrous mass into viscid material. This at once strives, by imbibition, to assume the globular shape. The remaining portion may or may not ultimately undergo similar transformation.

Inflamed serous membranes become often densely "nucleated." In the deeper layers, the "nuclei" are very elongated. At the surface they are perfectly globular, and are detached as minute opaque balls. These balls are the granulation- or the pus-corpuscles. On imbibition, one portion of their soft material swells out, encompassing the rest, which, when forming a single uniform globule, goes under the name of granulation-corpuscle; when, on the other hand, broken up into several granules, constitutes the famous pus-"cell." This is an example of a second mode of "cell"-formation. Here the secondary globule is shaped from a portion of the primary mass.

In some instances these "nuclei" or balls will, when still enclosed within the surrounding texture, undergo the above-mentioned change on imbibition, and thus whole rows of granulation or pus-corpuscles are seen to form.

This second mode of "cell"-formation is still more strikingly manifested in epithelial textures. In the mucous membrane of the nose, for instance, the faint oval "nuclei" of the large scales become during disintegration more and more distinct and globular. The surrounding material of the scale gradually liquefies, and the minute balls, thus liberated, expand by imbibition into mucus- or pus-corpuscles. It often succeeds to make them form in all perfection whilst they are still contained within the scale.

In abscesses of the skin the pus-corpuscles are formed in exactly the same manner. They can often be watched, fully shaped, still enclosed within the scale. Here, it would seem, are "cells," not the result of life, but rather of death.

The multiple "nuclei" of pus-corpuscles are not the result of over-fecundity, but are simply due to the disintegration of the non-imbibing portion of those oval or spherical clear-cut bodies, which are themselves so well known under the name of "nuclei."

The disintegration of this non-imbibing portion can be traced through all possible stages, down to the cluster of most irregularly shaped granules (which, notwithstanding, have been looked upon as the result of fissiparous division), and has been made to represent the crowning feature of the cell theory.

The same minute balls found swimming in the serum of a blister were seen, when treated with water, to disclose single bright clear cut "nuclei;" when treated with acetic acid, to reveal the most typical multiple nuclei of pus-cells.

II.—*Experimental Verification.*

In all the above-cited observations the existence of a viscid, imbibing material was proved with almost conclusive evidence,—a viscid material which is capable of forming globules of a definite size, and which in the living organism actually forms such globules; shapes, the nature of which has been hitherto mistaken.

After a long search, the substance known under the name of myeline was found to be the desired material.

When to myeline in its dry amorphous state water is added, slender tubes are seen to shoot forth from all free margins. These are sometimes wonderfully like nerve-tubes in appearance. They are most flexible and plastic. From this curious tendency of shooting forth in a rectilinear direction it was inferred that a crystallizing force must be at work.

To counteract this tendency, and to oblige the substance to crystallize into globules, it was intimately mixed with white of egg. The result was most perfect. Instead of tubes, splendid clear globules, layer after layer, were formed, resembling closely those of the crystalline lens formed under similar conditions.

Here was actually found a viscid substance which, on imbibition, formed globules of a definite size.

The remaining task was comparatively an easy one. By mixing the myeline with blood-serum, globules were obtained showing the most lively molecular motion.

When the serum somewhat preponderated, the whole globules seemed, after a while, to undergo coagulation, and appeared often as beautifully and finely granulated as any real "cell."

When this mixture of myeline and serum was spread very thinly over the glass slide, there often started into existence, on the addition of water, small primary globules, round which an irregular mass of granular material became gradually detached from the glass slide. It at last shaped itself into a secondary globule, enclosing the primary one, and constituting with it, down to the minutest details, the most perfect typical "cell." In many instances the nucleolus did not fail, and the narrow white margin, so often mistaken for a cell-wall, was always present. Beautiful "mother-cells" were formed in the same manner.

The next endeavour was to form "cells" according to the second mode.

If the amorphous myeline be very thinly spread on the glass slide, instead of tubes there will form bodies looking like rings. They are actually double globules, the inner globule being more transparent than the outer. They correspond to the inner and outer substance of the above-mentioned tubes. When these are left to dry, and then again acted upon with water, one portion will swell out into a clear globule, enclosing the rest as "nucleus." These "nuclei" are either large and single, like those of granulation corpuscles, or they are multiple, exactly like those of pus-cells. Whole layers of perfect pus-corpuscles are thus formed. But, of course, more complicated shapes occur as well. Among these, for instance, many such pus-cell-like bodies enclosed within one large sphere.

If, instead of water, serum be added to the thinly-spread myeline, biconcave disks will form, only generally much larger than blood-corpuscles.

"Cells" being thus merely the physical result of chemical changes, they can no longer afford a last retreat to those specific forces called vital. Physiology must aim at being something more than the study of the functions of a variety of ultimate organic units. And pathology will gain new hope in considering that it is not really condemned to be the interpreter of the many abnormities to which the mysterious life of myriads of microscopical individuals seemed to be liable.

II. "Preliminary Notice of Results of Pendulum Experiments made in India." By Lieut.-Col. WALKER, F.R.S.: in a Letter to the President. Received September 21, 1866.

I have the pleasure to inclose a provisional abstract of the results of Capt. Basevi's observations with his pendulums during the past field season. Though provisional, it will probably be found to agree very closely with the final results, which will be deduced as soon as the corrections for buoyancy, temperature, &c. are finally known.

Already these experiments are beginning to throw light on the subject of Himalayan attraction; for the observations clearly show that the force of gravity is less than it should be theoretically at the stations in the vicinity of the Himalayas, and that the difference between theory and practice diminishes the further the station is removed from the Himalayas.

This is a remarkable confirmation of Airy's opinion, that the strata of the earth below mountains are less dense than the strata below plains and the bed of the sea.

Combining these observations with those that were used by Mr. Baily (including, I believe, all your own), the value of the ellipticity will be diminished from $\frac{1}{285}$ to $\frac{1}{289}$ (approx.), and will therefore tend more closely to assimilate with Capt. Clarke's value, $\frac{1}{294}$*.

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* The pendulum result is $\frac{1}{288 \cdot 4}$.—E. S.

Provisional Abstract of Results of Pendulum Observations. Field Season 1865–66.

Reduced to Mean Temperature 69°·69.

Name of Station.		Latitude.	Height above Mean Sea-Level.	Observed vibrations per diem *in vacuo*, reduced to mean sea-level.		Means.	Reduction to Usira, using ellipticity.		Computed Vibrations in terms of Usira.		Value of ellipticity, each compared with	
				No. 4.	No. 1821.		Clarke, $\frac{1}{294}$.	Baily, $\frac{1}{285 \cdot 3}$.	Clarke.	Baily.	Usira.	Kew.
Dehra *		30° 20′ 0″	feet. 2289	86076·426	85975·631	86026·029	+ 11·192	+ 10·970	86031·288	86031·066	$\frac{1}{171}$	$\frac{1}{349}$
Nojli	Stations of Col. Everest, Great Arc.	29 53 28	881	86076·843	85975·724	86026·284	9·681	9·490	86029·777	86029·586	$\frac{1}{189}$	$\frac{1}{335}$
Kaliana ...		29 30 55	826	86075·992	85975·466	86025·729	8·408	8·241	86028·504	86028·337	$\frac{1}{195}$	$\frac{1}{329}$
Dateri......		28 44 5	719	86075·156	85974·782	86024·969	5·795	5·681	86025·891	86025·777	$\frac{1}{236}$	$\frac{1}{316}$
Usira		26 57 7	812	86070·520	85969·672	86020·096	......	......	....	....	......	$\frac{1}{309}$
Kew		51 28 10(?)	......	86165·483	86064·410	86114·947	91·682	89·871	86111·778	86109·967		

* Dehra is situated in a valley between the Himalayas and the Siwaliks.

The Society then adjourned over the Christmas recess, to Thursday, January 10.

January 10, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communication was read:—

"On the Appendicular Skeleton of the Primates." By St. George Mivart, F.Z.S., Lecturer on Comparative Anatomy at St. Mary's Hospital. Communicated by Prof. Huxley. Received November 22, 1866.

(Abstract.)

The author began by mentioning the principal variations found in the order Primates, as to the absolute and relative length of the pectoral limb with and without the manus; and then taking each bone separately, described the modifications undergone by each in all the genera of the order*; as also the relative size of the segments and bones of the limb compared to each other and to the spine. The pelvic limb was then similarly treated of, and, in addition, its segments and bones were compared with the homotypal segments and bones of the pectoral limb.

The author after this reconsidered the question as to the use of the terms "hand" and "foot," and the applicability of the term "Quadrumanous" to Apes and Lemuroids.

He controverted the position lately assumed by Dr. Lucae†, that both anatomically and physiologically the pes of apes is more like the human hand than the human foot. At the same time he recommended the use of unambiguous homological terms, such as "manus" and "pes" (already adopted by some) instead of "hand" and "foot," in all treatises on comparative anatomy.

Tables of the dimensions and proportions of the limbs, their segments, and bones were then given, exhibiting the variations presented in these respects throughout the whole series of genera.

The author then considered the more peculiar forms of the order, beginning with Man.

The principal resemblances and differences in form, size, and proportion between the human appendicular skeleton and that of other primates were given in detail, followed by a list of those points in which man differs, as to the bony structure of his limbs, from all other primates.

The limb-skeletons of the Orang, Marmoset, Indri, Slender Lemur, Tarsier, and Aye-aye were then similarly reviewed, and lists given of the absolute peculiarities found in each.

The conclusion arrived at from these comparisons was, that Man differs less from the higher Apes than do certain primates below him from each

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* Except certain Lemuroids, of which no specimens exist in this country.

† Abhandlungen von der Senckenbergischen Naturforschenden Gesellschaft. Frankfort, 1865, vol. v. p. 275.

other; and that he, *thus judged*, evidently takes his place amongst the members of the suborder *Anthropoidea.*

A list of the principal osteological variations presented by the several groups and genera of the order, before unmentioned, was then given; and the author concluded by stating what he believed to be the degrees of affinity existing between the various forms, as far as could be ascertained from the consideration of the appendicular skeleton exclusively.

January 17, 1867.

WILLIAM SPOTTISWOODE, Esq., Vice-President, in the Chair.

The following communications were read:—

I. "Actinometrical Observations among the Alps, with the Description of a new Actinometer." By the Rev. GEORGE C. HODGKINSON. In a Letter to Professor STOKES, Sec. R.S. Communicated by Professor STOKES. Received December 2, 1866.

SIR,—I have the honour to forward you an account of some actinometrical observations made last summer on the summit of Mont Blanc and at Chamonix, and at the same time to thank the Committee of the Royal Society for the grant which they were so good as to vote me for that object.

I reached Chamonix on the 7th of July, in bad weather, which had been prevailing for some time, but which ushered in a fine week very opportunely for my work. After allowing a few days for the weather to settle and for the snow to consolidate, I left Chamonix in the afternoon of Friday the 13th for the Grands Mulets, having previously arranged for a corresponding series of observations being taken the next morning in the valley. Leaving the Grands Mulets at about $2\frac{1}{2}$ A.M. on the 14th, I reached the summit of Mont Blanc about 8 A.M., and proceeded at once to work.

I had brought with me from England two of Newman's mountain-barometers, a thermobarometer of Casella, six small thermometers graduated on the stem (three for the dry-, and three for the wet-bulb observations), three of the tubes described in Appendix (A), with two of the actinometers in each. I carried besides an aneroid by Cooke, which proved to be of excellent quality. The third set of apparatus was taken in some faint hope that I might be able to arrange for a third set of simultaneous readings at the Grands Mulets. In this I was disappointed. Notwithstanding the greatest care had been taken, one of the barometers was found on the Brevent on the 9th to be deranged, and one of the actinometers to be broken; and on the 12th a second actinometer was broken at Chamonix by an accident. I thought it best to leave the remaining barometer for the valley observations, and to depend upon the thermobarometer, as being more portable and less liable to fracture, for the readings on the summit. I was eventually obliged to rest satisfied with a single observation of this; and the downward range of the small thermometer unfortunately proved too limited for the wet-bulb readings. Thus the meteorological observations at the upper station are of the scantiest. Neither above nor below were the actinometrical readings so continuous as I had wished to make them. I had no one with me on the summit capable of rendering me the smallest assistance; but it is some consolation to think that, even had this been otherwise, the results could not, under the circumstances, have been materially enhanced.

There either did not exist, or I failed to detect, as the sun's altitude increased, anything like a *uniform progression* of actinic power at either station during the limited time in which the observations were continued.

The results do little more than determine the ratio of the average intensity at the two stations for a portion of the forenoon. This indeed was the main object which I had in view. For looking at the experience of Principal Forbes under easier conditions, when the continuance of the observations, as long as the clearness of the sky might last, presented no difficulty, I did not at all anticipate being able to trace a dependence of the actinic power on the hygrometric state of the atmosphere. He thus remarks (Bakerian Lecture, Phil. Trans. part 2 for 1842, p. 253) of the experiments on the Faulhorn and at Brienz, that "it cannot be affirmed they are sufficient to show the kind of dependence which the opacity has on the dampness, and that the values of the coefficient of extinction do not present any correspondence with the hygrometric variations;" and again, p. 268, "It must be confessed that no evident relation to the hygrometric condition of the air appears in the individual observations."

From the experiments of the 14th of July the actinic ratio between the summit of Mont Blanc and Chamonix, from $9^h\ 31^m$ to $10^h\ 11^m$ apparent time, presents, with a single exception, a gradual decrease from 1·244 to 1·206. The interest of a comparison of these results with those which Principal Forbes obtained between the Faulhorn and Brienz is unfortunately diminished by the fact that his actinometer was not furnished with an internal thermometer for ascertaining the temperature of the liquid employed. This was ammonio-sulphate of copper, which has a coefficient of dilatation varying from 1 at 60° F., to 2·562 at 32° F., and 0·626 at 100° F. His recorded numbers for three hours before and three hours after apparent noon derived from his freehand curve, are as follows:—

Hour.	Ratio.
9	1·141
10	1·214
11	1·345
12	1·219
1	1·078
2	1·207
3	1·217

At 10^h on the Faulhorn the ratio seems to have been rapidly increasing; on Mont Blanc it was slowly diminishing. The actual amount of the ratio at 10^h is almost exactly coincident in the two cases; but at 11^h on the

Faulhorn it was 1·345, a value much higher than any which was obtained at any time on Mont Blanc, or seemed likely to have been obtained at that hour had the observations been continued so long. What share the greater depression of the lower station in the experiments of 1832, the more complete isolation of the upper station in those of 1866, or variable atmospheric conditions in both sets may severally have had in contributing to this effect, remains a matter for future investigation. The respective heights of the stations are as follows:—

	English ft.		English ft.
Faulhorn	8799	Difference	6853
Brienz	1946		
Mont Blanc	15784	Difference	12359
Chamonix	3425		

Professor Forbes gives the numbers—

Faulhorn	8747	Difference	6844.
Brienz	1903		

The sky during the observations was not only cloudless, but, as seen from the summit, remarkably clear.

The observations have all been reduced by means of Tables derived from Gmelin's 'Chemistry,' vol. i. p. 231, to what they would have been had the mean temperature of the liquid during each minute been 32° F.

By a prolonged and careful comparison of actinometers (K) and (A), the factor for reducing the indications of (K) to the standard of (A) was found to be 1·29.

Considerable practice is necessary to acquire expertness in the use of the actinometer employed. It is desirable, as nearly as may be, to work it at such a temperature that the rise in the sun may be equal to the fall in the shade. If the mean of the two mean temperatures of the liquid, in taking the shade observations which precede and follow a given sun, differ much from the mean temperature of the liquid during that sun, a sensible error will be introduced. This, however, is to a great extent eliminated by taking the mean of three, and still more completely by taking the mean of five successive actinic results in column (I).

The difficulty of using the instrument was overcome by the kind cooperation of several friends for the Chamonix observations. To the good offices of my cousin, Mr. G. F. Hodgkinson, were added those of a lady, a worthy sister of one of the foremost mathematicians of his year, and her two nieces. Under her auspices an admirable arrangement of the work was made, by which each of the party was responsible for a precise and definite function, the adjustment and direction of the instrument, with the shading and unshading, the watch, the readings, and the records. To this friendly and efficient help I am greatly indebted for whatever success has been achieved. How small this is, no one can be more sensible than myself; yet I venture to hope that when the difficulty of the undertaking is considered, to those at least who are acquainted with the experience of Principal Forbes in 1832, 1841, and 1842, as given in his Bakerian Lecture, the results will not appear either disappointing or discouraging. The season was extremely unfavourable for the further prosecution of the work. Looking to the imperfection of the instrument employed by Principal Forbes in his observations in 1832, it would seem to be highly desirable that his experiments on the column of air between the Faulhorn and Brienz should be repeated, and that other pairs of stations, intermediate in character to that and the Chamonix pair, should be essayed. I have selected, in the hope of future opportunities, the following among others :—

	English ft.	English ft.
Becca di Nona	10384	Difference 8415
Aosta	1969	

and should the Piz Stella prove readily accessible,

Piz Stella	11175	Difference 9856
Chianenna	1319	

While simultaneous observations at several adjacent stations of progressive heights are much to be desired, it should not be forgotten how largely the condition of simultaneousness at even only two stations adds to the difficulty of the work. And the question arises, whether detached readings of the actinometer (with the accompanying meteorological facts) taken at various points, as opportunity offers, may not be encouraged with advantage. An accumulation of these, carefully reduced and tabulated, could hardly fail to be valuable; and they may be obtained with comparative facility. It would indeed only be prosecuting these observations as we do those of atmospheric temperature and pressure. In process of time we might hope to obtain the mean actinic power at stations of various heights and circumstance for different altitudes of the sun.

Since the scale of each actinometer is empirical, in order that observations with different instruments may be comparable, a standard of reference is necessary. If such a standard were kept at Kew, and each instrument employed were marked with a factor of reduction, ascertained by careful comparison, a great encouragement would be afforded to actinometry; nor can any material progress in that department of observation be looked for until some such arrangement is made. The actine-standard of Sir J. Herschel can hardly be said now to have been preserved; to recover it, a careful set of observations under a vertical sun would be necessary; and since an arbitrary standard, which may be assigned without any such trouble will answer every purpose, it seems best at once to resort to this.

I would venture, in conclusion, to couple with my thanks to the Committee for their kind encouragement, an earnest recommendation that measures be taken to provide a standard actinometer accessible for comparison, under such regulations as may seem best to them.

I have the honour to be, Sir,
Your obedient Servant,
GEORGE C. HODGKINSON.

November 27, 1866.

Summit of Mont Blanc, July 14, 1866, Actinometer (K).

A. Apparent time of commencing each obs.	B. Sun O, shade ×.	C. Initial reading.	D. Terminal reading.	E. Change in sun, +.	F. Change in shade, −.	G. Solar effect unreduced.	H. Temperature of liquid.	I. Solar effect reduced to 32° F.	K. Actinometer (K) reduced to (A).	L. Averages.
h m										
8 28½	×	1732	1192		540					
8 30	O	1172	1300	128		716	51°	708	913	
8 31½	×	1238	602		636					
8 33	O	600	714	114		738	50	730	942	
8 34½	×	662	50		612					
9 6½	×	2098	1792		306					
9 8	O	1890	2272	382		718	50	711	917	
9 9½	×	2200	1834		366					
9 11	O	1840	2234	394		772	50	764	986	955
9 12½	×	2200	1810		390					
9 14	O	1882	2270	388		753	50	745	961	
9 15½	×	2120	1780		340					
9 19½	×	1460	1198		262					
9 21	O	1300	1800	500		788	48	781	1007	
9 22¾	×	1852	1538		314					
9 24	O	1582	2032	450		775	48	768	991	995
9 25½	×	2026	1690		326					
9 27	O	1754	2178	424		772	49	764	985	994
9 29½	×	1760	1400		360					
9 31	O	1472	1898	426		789	49	781	1007	993
9 32½	×	1906	1540		366					
9 33	O	1568	1974	406		765	49	757	977	996
9 35½	×	1972	1620		352					
9 36	O	1692	2150	458		787	49	779	1003	998
9 38½	×	2146	1840		306					
9 39	O	1880	2358	478		789	49	781	1007	1002
9 41½	×	2290	1974		316					
9 43	O	2068	2514	446		781	50	773	996	
9 44½	×	2454	2100		354					
9 51½	×	1908	1580		328					
9 53	O	1558	1980	422		760	50	752	970	
9 54½	×	1894	1546		348					
9 56	O	1608	2042	434		788	50	780	1006	992
9 57½	×	2022	1662		360					
9 59	O	1680	2094	414		783	50	775	1000	988
10 0½	×	2062	1684		378					
10 2	O	1788	2164	376		758	51	750	967	993
10 3½	×	2126	1740		386					
10 5	O	1820	2220	400		783	51	774	999	994
10 6½	×	2190	1810		380					
10 8	O	1904	2290	386		776	51	768	991	1001
10 9½	×	2426	2026		400					
10 11	O	2060	2440	380		795	52	786	1014	
10 12½	×	2380	1950		430					
10 19½	...	1270	992		278					
10 21	...	1086	1580	494		792	52	783	1010	
10 22½	...	1600	1282		318					
10 24	...	1386	1828	442		778	52	769	992	
10 25½	...	1764	1410		354					

The recorded numbers denote tenths of the scale which is divided to millimetres, the last figure being assigned by estimation.

Chamonix, July 14, 1866, Actinometer (A).

A. Apparent time of commencing each observation.	B. Sun O.; shade ×.	C. Initial reading.	D. Terminal reading.	E. Change in sun, +.	F. Change in shade, —.	G. Solar effect unreduced.	H. Temperature of liquid.	I. Solar effect reduced to 32° F.	L. Averages.
h m									
8 22½	×	1230	780		450				
8 24	O	1160	1560	400		837	85°	811	
8 25½	×	1514	1090		424				
8 27	O	1140	1554	414		818	85	793	812
8 28½	×	1484	1100		384				
8 30	O	1180	1640	460		857	85	831	799
8 31½	×	1504	1094		410				
8 33	O	1204	1614	410		830	85	804	794
8 34½	×	1330	900		430				
8 36	O	950	1304	354		779	84	755	805
8 37½	×	1214	794		420				
8 39	O	890	1280	390		814	84	789	803
8 40½	×	1230	802		428				
8 42	O	842	1310	468		872	84	846	800
8 43½	×	8112	802		380				
8 45	O	812	1280	468		848	84	822	803
8 46½	×	1202	822		380				
8 48	O	940	1380	440		814	84	789	802
8 49½	×	1280	912		368				
8 51	O	1040	1460	420		795	85	770	791
8 52½	×	1312	930		382				
8 54	O	1032	1462	430		806	85	781	797
8 55½	×	1350	980		370				
8 57	O	1092	1542	450		820	85	795	795
8 58½	×	1452	1080		370				
9 0	O	1182	1660	478		878	85	851	807
9 1½	×	1744	1330		414				
9 3	O	1394	1780	386		801	86	776	
9 4½	×	1620	1204		416				
9 6½	×	1204	800		404				
9 8	O	890	1324	434		841	84	816	
9 9½	×	1230	820		410				
9 11	O	950	1394	444		844	85	818	821
9 12½	×	1264	874		390				
9 14	O	1014	1480	466		856	85	830	807
9 15½	×	1420	1030		390				
9 17	O	1124	1554	430		808	85	783	800
9 18¼	×	1420	1054		366				
9 20	O	1220	1670	450		813	85	788	
9 21½	×	1560	1180		360				
9 23	O	1310	1790	480					
9 27½	×	15 54	1294		260				
9 29	O	1414	1914	500		820	86	794	
9 30½	×	1774	1394		380				
9 32	O	1560	2000	440		820	86	794	798
9 33½	×	1870	1490		380				
9 35	O	1614	2100	486		831	86	805	811
9 36½	×	2024	1710		310				
9 38	O	1664	2210	546		876	86	849	809

TABLE (*continued*).

A. Apparent time of commencing each observation.	B. Sun O, shade ×.	C. Initial reading.	D. Terminal reading.	E. Change in sun, +.	F. Change in shade, −.	G. Solar effect unreduced.	H. Temperature of liquid.	I. Solar effect reduced to 32° F.	L. Averages.
h m									
9 39½	×	2070	1720		350				
9 41	O	1590	2084	494		837	86°	811	822
9 42½	×	1990	1654		336				
9 44	O	1590	2082	492		813	86	787	835
9 45½	×	1940	1634		306				
9 47	O	1500	2064	564		885	86	857	838
9 48½	×	2040	1704		336				
9 50	O	1854	2454	600		896	87	869	
9 51½	×	2036	1780		256				
10 4½	×	1190	830		360				
10 6	O	1094	1584	490		857	89	828	
10 7½	×	1494	1120		374				
10 9	O	1334	1820	486		846	89	818	826
10 10½	×	1690	1344		346				
10 12	O	1374	1880	506		862	90	833	841
10 13½	×	1750	1384		366				
10 15	O	1490	2050	560		888	90	858	853
10 16½	×	1960	1670		290				
10 18	O	1810	1404	594		899	91	869	
10 19½	×	2270	1950		320				
10 27½	×	1540	1234		306				
10 29	O	1424	1934	510		821	93	792	
10 30½	×	1820	1504		316				
10 32	O	1688	2224	536		849	94	818	814
10 33½	×	2130	1820		310				
10 35	O	1740	2300	560		865	95	833	
10 36½	×	2182	1882		300				

The recorded numbers denote tenths of the scale which is divided to millimetres, the last figure being assigned by estimation. The numbers in column L are the means of the five nearest numbers in the preceding column when not less than two observations precede and follow the one against which the average number is placed, otherwise the mean of the three nearest numbers.

Comparison of Results, Summit of Mont Blanc and Chamonix.

Apparent time.	M. Blanc. Actinometer (K) reduced to actinometer (A).	Chamonix. actinometer (A).	Ratio of actinometers, Mont Blanc and Chamonix.	Apparent time.	Actinometer (K) reduced to actinometer (A).	Chamonix actinometer (A).	Ratio of actinometers, Mont Blanc and Chamonix.
h m				h m			
8 27		812		9 34	996		
8 30		799		9 35	......	811	1·228
8 33		794		9 37	998		
8 36		805		9 38	......	809	1·234
8 39		803		9 40	1002		
8 42		800		9 41	......	822	1·219
8 45		803		9 44	......	835	
8 48		802		9 47	......	838	
8 51		791		9 56	992		
8 54		797		9 59	988		
8 57		795		10 2	993		
9 0		807		10 5	994		
9 11	955	821	1·163	10 8	1001		
9 14		807		10 9	......	826	1·212
9 17		800		10 11	1014		
9 24	995			10 12	......	841	1·206
9 27	994			10 15	......	853	
9 31	993			10 21	1010		
9 32		798	1·244	10 24	992		
				10 32	......	814	

Meteorological Observations on summit of Mont Blanc and at Chamonix, July 14, 1866.

Mean time.	Apparent time.	Mont Blanc.			Chamonix.		
		Thermo-barometer.	Thermometer (dry).	Thermometer (wet).	Barometer corrected and reduced to 32° F.	Thermometer (dry).	Thermometer (wet).
h m	h m						
8 30	8 25				26·90	69° F.	57° F.
8 40	8 35	16·86	22° F.	Below 20°			
9 30	9 25	Boiling-point = 184°·5 F.			......	72 F.	59 F.
9 45	9 40		22·5 F.	Below 20			
10 45	10 40				......	75 F.	58 F.
11 0	10 55		24 F.				
2 5	2 0	......			......	82 F.	63 F.

APPENDIX (A).

Description of the Actinometer.

The actinometer employed consists of a thermometer with a spherical bulb one inch in diameter, and a tube, of which an inch and a half next the bulb is, for a reason which will presently be apparent, left unscaled. The succeeding ten inches is made to represent, as nearly as may be, the range from 40° F. to 45° F. At eleven inches and a half from the bulb the tube is widened, so that the following inch and a half may represent the range from 45° F. to 115° F. The tube then finishes in a spheroidal chamber, of which the diameters are about an inch and half an inch. The widened portion of the tube may be dispensed with, as the correction which it serves to ascertain may be otherwise found by means of a Table experimentally constructed for each instrument. In that case the spheroidal chamber, in which the tube will then terminate at eleven and a half inches from the bulb, should be made somewhat larger. The fluid employed is alcohol coloured with a drop of pure aniline-blue. A considerable quantity of air is left in the chamber. As a running column has to be read at a particular instant, great plainness is the first requisite for the scale. On this account graduation on the tube has not been adopted; but at an inch and a half from the bulb is attached an ivory scale, nine-tenths of an inch broad and eleven and a half inches long (or somewhat less if the widened tube be dispensed with), its other extremity coinciding with the commencement of the spheroidal chamber. This scale is graduated throughout in millimetres. The number of millimetres corresponding to each degree Fahr. on the tube of narrow bore, and to every fifth degree from 45° to 115° on the widened tube, should be noted on the back of the scale.

The principle of the instrument is the same as that of Sir J. Herschel's; and it is to be worked according to the directions given by him in 'The Manual of Scientific Enquiry.' It was devised for mountain use, where the weight of the Herschel and the fragility of its internal thermometer are elements of difficulty. It has also the advantage of being less costly. The air-chamber is made to serve the purpose of the screw in the Herschel, viz. that of altering at will, according to circumstances, the range of the thermometer. This is effected by throwing off into the chamber a greater or less quantity of fluid, retaining it there by holding the instrument with the chamber end somewhat lower than the bulb, and working with the remaining column. As alcohol expands unequally between its freezing- and boiling-points, a small correction is necessary, depending on the temperature of the alcohol at the time of working, This temperature is ascertained by noting the point in the widened tube, at which the column stands, when the fluid is thrown off into the chamber. The excess of this temperature above 45° F., the point from which the fluid is thrown off, has to be added to the temperature between 40° and 45° shown by the head of the working column, in order to have the true temperature. From the openness of the scale, and consequent small range of the instrument *for any one adjustment*, it is necessary to select for working a temperature not much removed from that at which the rise in the sun is equal to the fall in the shade. This temperature, which may be called the temperature of equilibrium, will vary practically, according to the solar intensity, from some 5° F. to 20° F. above the temperature of the surrounding influences. By driving the fluid into the chamber until the temperature of equilibrium is represented at a point near the middle of the tube, the readings will go on for a considerable time without altering the quantity of fluid in the chamber, and ten inches of graduation are found to be ample under all circumstances. By thus taking all the readings, so to speak, on the balance, a uniformity of proceeding is secured, which is not without its value. The instrument, constructed according to the dimensions here given, will denote the intensity of the noonday sun at the summer solstice near the sea-level in England by about 100 divisions of the scale.

Owing to the difficulty of shading satisfactorily, and anomalies found to occur in observing among the snow-fields on the high crests of the Alps, the following contrivance has been adopted:—

A plain telescope-tube of bright metal, 18 inches long and $2\frac{1}{2}$ inches in diameter, open at both ends, is pierced in its central section with a circular hole $1\frac{1}{4}$ to $1\frac{1}{3}$ inch in diameter, from which springs a flanged shoulder projecting about $\frac{1}{3}$ inch to receive a perforated split bung, which clasps the thermometer-stem and holds the bulb firmly in the centre of the axis of the tube. Two caps, fitted at the ends with clean plate-glass, are made to slide off and on at the two ends to admit of the glasses being readily wiped. By protecting these with a little wadding, the tube serves as a case for two actinometers. In the central section of the tube, made by a plane perpendicular to its axis, and nearly 90° from the centre of the circular hole, is a screw to attach the tube to an altitude and azimuth motion, by means of which it may be kept constantly directed towards the sun. Below the joint is provided means of attachment to an alpenstock or ice-axe. The shading is effected by means of a loose-fitting cap, bottomed by a chamber with air-holes. The shadow of the large thermometer-bulb on the lower glass, or on a plane held beneath it, is a guide to a perfect adjustment in the working of the instrument.

II. "An Eighth Memoir on Quantics." By Professor A. CAYLEY, F.R.S. Received January 8, 1867.

(Abstract.)

The present memoir relates mainly to the binary quintic, continuing the investigations in relation to this form contained in my Second, Third, and Fifth Memoirs on Quantics; the investigations which it contains in relation to a quantic of any order are given with a view to their application to the quintic. All the invariants of a binary quintic (viz. those of the degrees 4, 8, 12, and 18) are given in the memoirs above referred to, and also the covariants up to the degree 5; it was interesting to proceed one step further, viz. to the covariants of the degree 6; in fact, while for the degree 5 we obtain three covariants and a single syzygy, for the degree 6 we obtain only two covariants, but as many as seven syzygies. One of these is, however, the syzygy of the degree 5 multiplied into the quintic itself, so that, excluding this derived syzygy, there remain (7−1=) six syzygies

of the degree 6. The determination of the two covariants (Tables 83 and 84 *post.*), and of the syzygies of the degree 6, occupies the commencement of the present memoir. The remainder of the memoir is in a great measure a reproduction (with various additions and developments) of researches contained in Prof. Sylvester's Trilogy, and in a recent memoir by M. Hermite *. In particular, I establish in a more general form (defining for that purpose the functions which I call "Auxiliars") the theory which is the basis of Prof. Sylvester's criteria for the reality of the roots of a quintic equation, or, say, the theory of the determination of the character of an equation of any order. By way of illustration, I first apply this to the quartic equation; and I then apply it to the quintic equation, following Prof. Sylvester's track, but so as to dispense altogether with his amphigenous surface, making the investigation to depend solely on the discussion of the bicorn curve, which is a principal section of this surface. I explain the new form which M. Hermite has given to the Tschirnhausen transformation, leading to a transformed equation, the coefficients whereof are all invariants; and, in the case of the quintic, I identify with my Tables his cubicovariants $\phi_1(x, y)$ and $\phi_2(x, y)$. And in the two new Tables, 85 and 86, I give the leading coefficients of the other two cubicovariants $\phi_3(x, y)$ and $\phi_4(x, y)$. In the transformed equation the second term (or that in z^4) vanishes, and the coefficient $\mathfrak{A}$ of z^3 is obtained as a quadric function of four indeterminates. The discussion of this form led to criteria for the character of a quintic equation, expressed like those of Prof. Sylvester in terms of invariants, but of a different and less simple form; two such sets of criteria are obtained, and the identification of these and of a third set resulting from a separate investigation, with the criteria of Prof. Sylvester, is a point made out in the present memoir. The theory is also given of the canonical forms, which is the mechanism by which M. Hermite's investigations were carried on. The memoir contains other investigations and formulæ in relation to the binary quintic; and as part of the foregoing theory of the determination of the character of an equation, I was led to consider the question of the imaginary linear transformations which give rise to a real equation: this is discussed in the concluding articles of the memoir, and in an annex I have given a somewhat singular analytical theorem arising thereout.

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* Sylvester "On the Real and Imaginary Roots of Algebraical Equations; a Trilogy," Phil. Trans. t. cliv. (1864) pp. 579–666; Hermite, "Sur l'Equation du 5e Degrè,' Comptes Rendus, t. lxi. (1866), and in a separate form, Paris, 1866.

January 24, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

I. "On a New Method of Calculating the Statical Stability of a Ship." By C. W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture. Received January 15, 1867.

The time required for the calculations of the stability of ships has practically restricted the ordinary draughtsman to the use of the metacentre. This implies that the locus of the centres of buoyancy cuts the transverse midship plane in a curve which may be treated as a circle; and this is only true, in general, for very small limits of inclination. In some particular cases it has been felt desirable to supplement this by computing the moment of stability at some definite angle of inclination, by means of the "ins and outs," or immersed and emersed wedges. But this has only been applied to one selected inclination, generally of 10° or 14°; and owing partly to this, and partly to the very scant time left available to the skilled draughtsman or calculator, this has never been a part of the ordinary work of the computation of a ship's quantities. For this reason it becomes of great consequence to find some method of getting at the stability, with an amount of extra work, which should not exceed that of the ordinary sheet known as the "sheer-draught calculation"*.

A method has occurred to me by which, as I think, this object may be attained. Upon conferring with some of my students†, who have suggested and removed certain difficulties of detail, we think we see our way, by an easy calculation, to place the whole account of a ship's statical stability in the hands of any person who understands simple equilibrium, either in an algebraical or geometrical form, as he may prefer.

It will take some time, with my present occupations, to prepare detailed examples. But as the method is complete in respect of principle, I have thought it best to bring it at once before the Society.

The fundamental assumption is, that the locus of the centres of buoyancy can be sufficiently represented by a conic. The stability is then measured by the perpendicular, from the centre of actual weight, on the normal due to the inclination. The chief step, therefore, is to find the conic, of which, I may remark, we already know the vertex, and the tangent and curvature at the vertex; for these are given by the ordinary calculation of the centre of buoyancy and the metacentre. Now I observe that the conic is completely determined if we can find the length of another radius of curvature corresponding to a known inclination. This is obtained by finding the moment of inertia about one of its principal axes (longitudinal)

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[*] See 'Ship-building, Theoretical and Practical,' by Watts, Rankine, Barnes, and Napier, p. 46, for the sheer-draught calculation commonly used in this country.

[†] Messrs. Deadman, Edgar, John, and White.

of the plane of floatation at the inclination. This, divided by the unaltered displacement, gives the radius of curvature required.

But the chief practical difficulty lay in finding the means of drawing an inclined water-line across the body plan, so as to give an unaltered displacement. This I have at length succeeded in overcoming, as follows.

The sheer-draft calculation gives us, *inter alia,* the areas of the level sections, belonging to the upright position, as rectangles. Now, if we make one side of each of these equal to the length of the ship, their breadths form a series of ordinates for a curve of mean section; that is to say, the transverse section of a cylindrical body, of which the displacement at any level immersion will be the same as that of the ship. We then make out a scale of displacement for this section at various immersions, for a selected inclination, taking care to measure the immersions on the middle line of the original body plan. By this means the finding of any water-line at the selected inclination is reduced to a problem of plane geometry; and it is obvious that the place of the water-line so found will be a very close approximation to that of the required plane of floatation in the ship.

The calculations are as follows:—

1. Take out the horizontal areas from the sheer-draught calculation, and divide each by the ship's length. Set them off right and left from a vertical line at their present vertical interval, and draw a curve through their ends.

2. Any practised draughtsman will have little difficulty in drawing, at sight, an inclined line of floatation which shall give an unaltered immersed area on this mean section. He can verify it by measuring the immersed and emersed triangles obtained by his first guess, and make the correction due to the difference, if they do not agree.

3. In strictness, the more accurate course would be this,—through each of the vertical stations draw right lines at the selected angle. Thence, by Simpson's rule, form a scale of areas, ending at the highest inclined water-line. Use the vertical interval of the upright displacement, and neglect the cosine of the inclination. Then divide the upright displacement by the ship's length and by the cosine of the inclination, and find to what immersion this displacement corresponds in the scale of inclined areas. But this is needless, unless the calculations have to be made for different draughts of water.

4. Use this immersion to draw the inclined plane of floatation in the body plan.

5. Calculate the area, common moment, and moment of inertia of this plane, about the longitudinal axis formed by its intersection with the original plane of floatation, upright.

6. Transfer this moment of inertia to the longitudinal axis passing through the centre of gravity of the inclined plane of floatation.

7. Divide the moment so found by the displacement. This will give

the radius of curvature of the locus of the centres of buoyancy, corresponding to the selected inclination.

The conic is now implicitly determined. It remains to show what use is to be made of these data.

Let ρ_θ be the radius of curvature, corresponding to the angle θ, made between the normal and axis of a conic; then

$$\rho_\theta=\frac{a(1-e^2)}{(1-e^2\sin^2\theta)^{\frac{3}{2}}},\quad \rho_0=a(1-e^2).$$

From these we obtain

$$e^2=\frac{\rho_\theta^{\frac{2}{3}}-\rho_0^{\frac{2}{3}}}{\rho_\theta^{\frac{2}{3}}\sin^2\theta}, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (a)$$

$$1-e^2=\frac{\rho_0^{\frac{2}{3}}-\rho_\theta^{\frac{2}{3}}\cos^2\theta}{\rho_\theta^{\frac{2}{3}}\sin^2\theta}, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (b)$$

$$a=\frac{\rho_0\rho_\theta^{\frac{2}{3}}\sin^2\theta}{\rho_0^{\frac{2}{3}}-\rho_\theta^{\frac{2}{3}}\cos^2\theta}, \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (c)$$

$$ae^2=\frac{\rho_0(\rho_\theta^{\frac{2}{3}}-\rho_0^{\frac{2}{3}})}{\rho_0^{\frac{2}{3}}-\rho_\theta^{\frac{2}{3}}\cos^2\theta}; \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (d)$$

and these afford the means of calculating all the elements of the conic.

Now, let us take any other inclination ϕ: we may calculate ρ_ϕ from the foregoing value of e^2 by means of the formula

$$\rho_\phi=\frac{\rho_0}{(1-e^2\sin^2\phi)^{\frac{3}{2}}}. \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (e)$$

Now, if λ be the distance of the centre of gravity of the ship below the metacentre of the upright position, and p the perpendicular from the centre of gravity on the normal of the conic in the inclined position, we shall have

$$\frac{p}{\sin\phi}=\lambda+\frac{\rho_0^{\frac{2}{3}}(\rho_\phi^{\frac{2}{3}}-\rho_0^{\frac{2}{3}})}{\rho_0^{\frac{1}{3}}+\rho_\phi^{\frac{1}{3}}\cos\phi}; \quad\ldots\ldots\ldots\ldots\ldots\ldots (f)$$

and $p\times D$ is the moment of stability, D being the displacement.

Strictly, it is only necessary to use the formulæ (*a*), (*e*), (*f*) in actual work. Formula (*f*) shows clearly how an alteration in the position of the weights affects the stability. If λ be altered, the altered value of p is obtained (geometrically) by a very obvious construction.

In Mr. Scott Russell's treatise on 'Naval Architecture,' p. 604, it is shown how the stability may be obtained by geometrical construction when the conic is known.

It is worth while to remark that the condition that the conic should be a hyperbola, a parabola, or an ellipse, is

$$\rho_0 <, =, \text{ or } > \rho_\theta \,.\, \cos^3\theta\,;$$

and whether the ellipse is referred to its major axis, becomes a circle, or is referred to its minor axis, depends upon whether

$$\rho_0 <, =, \text{ or } > \rho\ \ ;$$

θ having any value whatever within the limits of continuity.

It is to be observed that this method only applies on the supposition that there is no abrupt discontinuity. The immersion of the gunwale, for instance, would vitiate it. But in ordinary ships, experience leads to the conclusion that a conic would be a very accurate representation of the locus of centres of buoyancy within all reasonable limits.

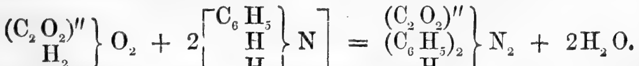
I have not waited to try the method throughout upon a specific example. But every step is separately well known; most of the steps familiarly so, within my own experience. My *estimate* of the extra amount of work is, that it would be rather less than would be involved in making an independent calculation of the ordinary sheer-draught work. I shall have an immediate opportunity of verifying this in my school; but I wished to announce the method publicly before beginning to teach it.

II. “Transformation of the Aromatic Monamines into Acids richer in Carbon.” By A. W. Hofmann, LL.D., F.R.S. Received January 21, 1867.

In a previous communication* to the Royal Society I have described the formation of *methenyldiphenyldiamine*, a substance which I obtained some years ago, by the action of chloroform on aniline, by means of a new method, namely, by treating a mixture of phenylformamide and aniline with trichloride of phosphorus.

The continuation of these researches necessitated the preparation of phenylformamide, and later also of tolylformamide in greater quantities. I have repeatedly obtained these bodies by the action of formic ether on the corresponding monamine, but in consequence of the difficulties with which the preparation of formic acid in large quantities is still beset, I have of late returned to the old method, viz., distillation of the oxalate of the monamine, since I found that by employing the materials in the appropriate proportions, the formation of very large quantities of the formyl compounds may be readily accomplished.

According to Gerhardt, the principal product of the distillation of the secondary aniline-oxalate is *diphenyloxamide*, *phenylformamide* being formed only as by-product. In fact, 1 molecule of oxalic acid and 2 molecules of aniline yield almost exclusively diphenyloxamide by the separation of 2 molecules of water from the secondary aniline-oxalate, thus:—



$$\left.\begin{matrix}(C_2O_2)''\\H_2\end{matrix}\right\}O_2 + 2\left[\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N\right] = \left.\begin{matrix}(C_2O_2)''\\(C_6H_5)_2\\H_2\end{matrix}\right\}N_2 + 2H_2O.$$

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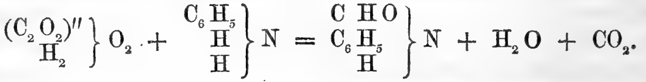
* Proceedings of the Royal Society, vol. xv. p. 55.

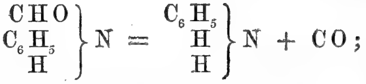
But nothing is easier than to change the conditions of the experiment so as to give rise to the almost exclusive formation of phenylformamide. Act with 1 molecule of oxalic acid on 1 molecule of aniline (or even with 3 molecules of oxalic acid on 2 molecules of aniline), taking care to give at once the highest possible temperature, and phenylformamide will be nearly the only product, 1 molecule of water and 1 molecule of carbonic acid separating from the primary aniline oxalate at first formed,



$$\left.\begin{matrix}(C_2O_2)''\\ H_2\end{matrix}\right\}O_2 + \left.\begin{matrix}C_6H_5\\ H\\ H\end{matrix}\right\}N = \left.\begin{matrix}CHO\\ C_6H_5\\ H\end{matrix}\right\}N + H_2O + CO_2.$$

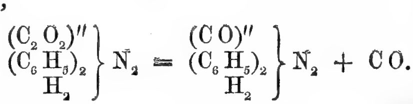
The distillate is a fluid of peculiar odour, which, on the addition of a strong solution of caustic soda, immediately solidifies to the crystalline soda-compound of phenylformamide. This crude product always containing a quantity of aniline, is sufficiently pure for the preparation of methenyl-diphenyldiamine previously described by me. It is only necessary to treat the distillate with trichloride of phosphorus to obtain the methenyl-compound in abundance.

The action of oxalic acid on aniline at a high temperature gives rise, however, to quite a series of other reactions subordinate to the principal changes, but affecting nevertheless a goodly quantity of material. Carbonic oxide is observed to be evolved, together with carbonic acid, during the distillation. It is the result of two secondary processes. In the first place, phenylformamide already formed splits up, according to the analogous decomposition of formamide, into aniline and carbonic oxide,



$$\left.\begin{matrix}CHO\\ C_6H_5\\ H\end{matrix}\right\}N = \left.\begin{matrix}C_6H_5\\ H\\ H\end{matrix}\right\}N + CO;$$

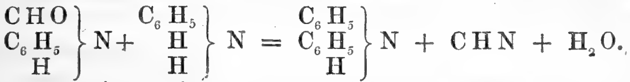
and secondly, diphenyloxamide undergoes a transformation, previously pointed out by me, being changed into diphenylcarbamide with separation of carbonic oxide,



$$\left.\begin{matrix}(C_2O_2)''\\ (C_6H_5)_2\\ H_2\end{matrix}\right\}N_2 = \left.\begin{matrix}(CO)''\\ (C_6H_5)_2\\ H_2\end{matrix}\right\}N_2 + CO.$$

On this occasion the formation of the latter body was once more satisfactorily proved by special experiment. During the distillation a considerable quantity of a crystalline substance saturated with oil had solidified in the neck of the retort. This substance was purified by washing with cold and recrystallizing a few times from hot alcohol; combustion showed that it was pure diphenylcarbamide.

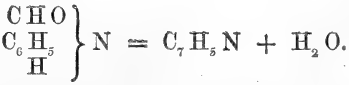
The crude oil obtained by the distillation of 1 molecule of aniline and 1 molecule of oxalic acid contains, further, *hydrocyanic acid,* and it is not difficult to explain in a satisfactory manner the origin of this compound. When the distillate is heated to ebullition with concentrated hydrochloric acid, an oily body passes over with the steam. This oil has a peculiar odour, recalling that of benzonitrile, and exhibits an inclination to crystallize; it is readily proved to be a mixture. If this substance be boiled for some time with a concentrated soda-solution in alcohol, it partly dissolves with evolution of ammonia. When the fluid is allowed to cool, after ammonia has ceased to be evolved and the alcohol distilled off, its surface is found to be covered with oily drops, which after a time become a crystalline solid. This solidification is instantly produced by treating the oil with a small quantity of concentrated hydrochloric acid. When a few drops of strong nitric acid are added to the hydrochloric fluid in which the crystals are floating, and the mixture is then gently heated, a deep blue colour is produced. This reaction is characteristic of *diphenylamine,* with which the crystalline body perfectly accords in every respect. It cannot be doubted that diphenylamine is produced from phenylformamide as a product complementary to hydrocyanic acid; 1 molecule of phenylformamide and 1 molecule of aniline contain the elements of 1 molecule of diphenylamine, 1 molecule of hydrocyanic acid, and 1 molecule of water.



$$\left.\begin{matrix}CHO\\C_6H_5\\H\end{matrix}\right\}N+\left.\begin{matrix}C_6H_5\\H\\H\end{matrix}\right\}N=\left.\begin{matrix}C_6H_5\\C_6H_5\\H\end{matrix}\right\}N+CHN+H_2O.$$

It still remains to give an account of the fluid substance formed together with diphenylamine, which had disappeared with evolution of ammonia when the mixture was heated with soda-solution. Had not both its odour and its behaviour with soda pointed to *benzonitrile,* all doubt of the formation of this body would have been removed, when upon addition of hydrochloric acid to the filtered sodic liquid an abundant quantity of the purest benzoic acid was separated, the nature of which was, moreover, fixed by an analysis of the silver-salt.

The formation of benzonitrile is easily explained. It is produced by a secondary transformation of phenylformamide, from which 1 molecule of water separates,



$$\left.\begin{matrix}CHO\\C_6H_5\\H\end{matrix}\right\}N=C_7H_5N+H_2O.$$

The transition of phenylformamide into benzonitrile is only in part accomplished during the distillation of the mixture of aniline and oxalic acid. The greater portion of the nitrile is evidently formed during the treatment of the crude product of the distillation with hydrochloric acid.

The conversion of aniline into benzoic acid, an acid richer in carbon than this base, claims some interest, inasmuch as the development of the manufacture of coal-tar colours places the aromatic monamines at our disposal in abundant quantity, and at the cheapest price. It was by no means improbable that some of the acids, already known, might in this manner be more easily prepared than heretofore, nor was it doubtful that the formation of many new compounds could be accomplished by this process.

I have therefore, in the first place, endeavoured to establish the generality of the reaction by treating toluidine in a similar manner. The phenomena observed when a mixture of 1 molecule of toluidine with 1 molecule of oxalic acid is distilled, are perfectly analogous to those presented by the corresponding experiment with aniline. It was not necessary for the purposes of the investigation to trace step by step the several phases of the complicated processes. The crude distillate, which contained abundance of tolylformamide, was therefore immediately heated with strong hydrochloric acid and submitted to distillation. The oily substance which distilled over with the water, evolved ammonia on being boiled with soda-solution, and the filtrate from the insoluble residue yielded, on addition of hydrochloric acid, a crystalline acid which combustion, as well as analysis of the silver-salt, proved to be tolylic acid. Here also tolylformamide gave rise to tolonitrile, from which tolylic acid subsequently was produced.

$$\left.\begin{array}{c}(C_2O_2)''\\H_2\end{array}\right\}O_2 + \left.\begin{array}{c}C_7H_7\\H\\H\end{array}\right\}N = \left.\begin{array}{c}CHO\\C_7H_7\\H\end{array}\right\}N + H_2O + CO_2,$$

$$\left.\begin{array}{c}CHO\\C_7H_7\\H\end{array}\right\}N = C_8H_7N + H_2O,$$

$$C_8H_7N + 2H_2O = C_8H_8O_2 + H_3N.$$

Several varieties of tolylic acid are known to exist, and it can scarcely be doubted which of the isomeric modifications is formed in this case. As, however, I intend to follow this reaction somewhat further, I shall not for the present go deeper into the question.

The experience collected in the phenyl- and tolyl-series, as might have been expected, has also been confirmed in the naphthyl-series.

The investigation of the naphthaline group in this direction appeared more particularly interesting. The idea naturally suggested itself of completing this group by taking advantage of the new reaction for the formation of a series of compounds, the existence of which had long been pointed out by theory, but the preparation of which as yet, notwithstanding repeated attempts, had not succeeded.

Naphthaline, the most common product of the action of high temperatures on organic bodies, has, singularly enough, not as yet been traced to a simple reaction. It was not doubtful that the hydrocarbon would in time be met with as the result of the splitting up of an acid, holding to it a relation similar to that which obtains between benzoic acid and benzole.

This acid, which is represented by the formula

$$C_{11}H_8O_2,$$

is in fact procurable by the action of oxalic acid on naphthylamine, the monamine of naphthyl-series. I propose to give a detailed account of this interesting compound and of its derivatives in a subsequent communication.

I cannot conclude this note without expressing my best thanks to Mr. Cornelius O'Sullivan for the assistance he has given me during the performance of the experiments which I have described.

January 31, 1867.

Dr. W. A. MILLER, Treasurer and Vice-President, in the Chair.

The following communication was read:—

"On the Elimination of Nitrogen by the Kidneys and Intestines during Rest and Exercise, on a Diet without Nitrogen." By E. A. Parkes, M.D., F.R.S. Received January 23, 1867.

The experiments recorded in this paper were undertaken to test the results arrived at by Professors Fick and Wislicenus, with respect to the elimination of nitrogen during exercise on a non-nitrogenous diet, as recorded in the Philosophical Magazine for June 1866 (Supplement).

Although these results are supported by the previous experiments of Dr. Speck, who has shown that if the ingress of nitrogen be restricted, bodily exercise causes no, or a very slight increase in the elimination of nitrogen by the urine, it appeared desirable to carefully repeat all the experiments, not only because the question is one of great importance, but because objections might be, and indeed have been, reasonably made to the experiments of Professors Fick and Wislicenus on the ground that no sufficient basis of comparison between periods of rest and exercise was given; that the periods were altogether too short, and that no attention was paid to the possible exit of nitrogen by the intestines.

In making the experiments, I was fortunate in being permitted to use the services of two perfectly healthy soldiers belonging to the Army Hospital Corps, and doing duty at the Royal Victoria Hospital at Netley. When soldiers are steady and trustworthy, as these men were, they are good subjects for experiments of the kind, as they are accustomed to very regular diet and occupation, and moreover, from their habits of obedience, carry out all instructions with great precision. The satisfactory results of my experiments, as shown by the almost perfect agreement in the effect on each man, is owing essentially to the very great care with which these two intelligent men carried out every rule which was laid down.

One of these men, S., is an admirable example of an average man; he is 22½ years old, 5 feet 8 inches in height, weighs close upon 150 lb., is strong, with large bones and firm muscles, with sufficient but not excessive fat; he is very temperate, and is no smoker. He has never been ill in his life. The second man, T., is also a perfectly healthy man, and has only been ill twice, once in China six years ago with tertian ague, and about three years ago with intermittent hemicrania. But he is in size and weight a good contrast to S. He is 36 years of age, very well proportioned and active, but is only 5 feet 4 inches in height, and weighs only

112 lbs. His size is not owing to any imperfection in make or nutrition, but to the fact that he comes of a small race, his father being small, and his mother remarkably so. He has small bones, good firm muscles, but very little fat. In fact, he is a thin man.

In the following experiments, the amounts of the total nitrogen of the urine (by soda-lime), of the urea, of the chloride of sodium, and on certain occasions of the phosphoric and sulphuric acids, were determined. The urea was determined by Liebig's solution, the chlorine being first eliminated, the phosphoric acid by acetate of uranium, the sulphuric acid by baryta and weighing.

The urine was collected from 8 A.M. to 8 A.M., and great care was taken not to lose any, and to pass it at the exact time.

The amount of water, solids, and nitrogen passed from the bowels were also determined on several occasions.

All the ingesta were most carefully weighed and measured, and the amount of water in the crumb and crust of bread and in the meat was determined. The nitrogen in the bread was also determined, but the long time demanded by the other processes prevented a complete analysis of the other food; this was, however, a matter of no importance as regarded the immediate object of the inquiry.

The experiments were commenced on December 6, 1866, and were continued daily till December 23.

First Period of ordinary regulated Diet and Occupation.

The men were first kept under observation for six days, in order to determine the variations in weight and in the excreta, and to see if the metamorphosis of tissue appeared to be healthy. This was found to be the case; in fact, more completely healthy urinary and intestinal excreta could not be conceived.

The weight of the body ranged nearly 1 lb. avoir., or $\frac{1}{2}$ kilog. above and below the mean amount in each man.

The daily average amount of food and drink was only slightly different in each man, and the quantity taken from day to day was very uniform.

The men were not placed on any absolute quantity, but ate according to appetite within narrow limits.

Average daily amount of food in ounces avoirdupois in this period:—

	S.	T.
Cooked meat	7·625	7·625
Bread	16·66	16·26
Vegetables:—$\frac{3}{4}$ potatoes, $\frac{1}{4}$ cabbage	13·87	13
Butter	1	1
Tea, including 3 oz. of milk, and $1\frac{1}{2}$ oz. sugar	20	20
Coffee, including 3 oz. of milk, and $1\frac{1}{2}$ oz. sugar	20	20
Beer	15	15
Water	5·8	2·33

S. took about ·5 oz. salt and T. about ·33, exclusive of salt in food.

Adding the water of the so-called solid food to the water taken as drink, the daily amount of food was, in grms.—

	S.	T.
Water-free solids, in grms.	662·2	610·2
Water, in grms....................	2334·5	2212·3
Total food ingesta	2996·7	2822·5

The mean weight of this period was for S. 67·7 kilogs. and for T. 50·6 kilogs. The ingress of solid food per kilog. of body-weight was 9·78 and 12 grms. respectively. The smaller man eat therefore absolutely rather less, but relatively more.

During four days of this period the mean daily urinary excretion was, in grms. and cubic centimetres—

	Quantity.	Sp. gr.	Urea.	Nitrogen in urea.	Total nitrogen by soda-lime.	Non-ureal nitrogen.	Chloride of sodium.
S.	1226	1028·25	35·001	16·334	17·973	1·639	14·23
T.	1335	1020·5	25·925	12·098	13·409	1·313	11·685

The excretion of nitrogen was fairly constant from day to day, the range of the urea being, in the case of S., from 38·37 to 33·36 grms., or between 2 and 3 grms, above and below the mean amount; and in the case of T. from 27·68 to 24·906, or nearly 1 grm. above and below the mean amount. This shows the daily equality of diet and exercise.

Calculated for body-weight, the amount per kilog. is—

	Urea.	Nitrogen in urea.	Total nitrogen.	Non-ureal nitrogen.
S.	·517	·241	·265	·024
T.	·512	·239	·265	·026

The very close relation, indeed identity, as far as the total nitrogen is concerned, of the excretion per kilog. of body-weight in these two men comes out very clearly, and shows that there must be a real connexion between body-weight and urinary excretion.

It is remarkable that while the heavier man passed 4½ grms. more nitrogen daily from the kidneys than the smaller man, he did not eat any great excess of food. Unfortunately, as the nitrogen in the food was not perfectly determined, it is impossible to know precisely whether the 52 grms. of excess of solid food taken by the larger man would contain 4½ grms. more nitrogen. As, however, the amount of meat was precisely the same, and as the smaller man only took $\frac{1}{2}$ oz. or 14 grms. less bread, and 25 grms. less vegetables, this would seem to be unlikely, and, if so, some of the nitrogen taken by T. must have passed off in other ways.

The mean relation of the ureal to the total nitrogen was very close in each man, being, if the ureal nitrogen is taken as unity, as 1 to 1·1, and as 1 to 1·108 respectively. From day to day, however, the relation varied.

The intestinal excretion was examined only on one day, the last but one of the series.

Composition of Intestinal Excretion of Twenty-four Hours.

	Total weight, in grammes.	Solids.	Water.	Nitrogen in grmmes
S.	171·1	28·58	142·52	1·642
T.	198·47	29·916	168·55	1·98

The smaller man passed rather more solids, water, and more nitrogen than the larger man, and through this channel some of the nitrogen unaccounted for by the urine must have escaped. Whether this would account for the whole of it cannot be stated, as the experiments in respect of the nitrogen in the food and in the intestinal excreta of the whole period were not sufficiently exact to determine this point.

On the day when the intestinal excreta were analyzed, the total discharge of nitrogen by the kidneys and bowels was—

	S.	T.
Urine..........	20·155	13·410
Bowels	1·642	1·980
Total....	21·797	15·390

In S. nearly $\frac{1}{13}$th, and in T. nearly $\frac{1}{8}$th of the total nitrogen passed by the bowels.

On the day when the intestinal excreta were analyzed, the balance of ingesta and egesta, atmospheric oxygen being disregarded, was as follows:—

	S.	T.
Weight of body at commencement of period, in kilogrammes	67·6	50·76
Weight of body at close	68·	50·89
Gain or loss, in grammes..............	+ 400	+ 130
Total ingesta by food and drink, in grammes	3083	2969
Urinary egesta ,, ...	1619	1774·8
Intestinal egesta ,, ...	171·1	198·47
Skin and lung egesta ,, ...	893	866

The tissue-changes in these two men are therefore very closely the same, and the men are quite comparable and well fitted for the experiments. T. has rather a larger excretion (chiefly of water) by the kidneys and bowels, and rather less by the skin, but the difference in not great. He has also a larger excretion of nitrogen by the *bowels* than S.

Second Period.—Non-nitrogenous Diet and Rest.

On the day following the men were placed for two days on a non-nitrogenous diet consisting of arrowroot, sugar, and butter, from which the casein had been separated. The only nitrogenous substance taken was that contained in infusion of tea. I thought it better to allow the use of warm tea, without milk, both for the comfort of the men and because I was afraid of deranging the tissue-changes by too complete an alteration of diet.

The arrowroot was made into cakes with butter and sugar, and was also taken as jelly. Butter and sugar were taken as desired. I put no restriction on quantity, but left it to choice and appetite.

The following was the total diet of two days, December 10th to 11th, and 11th to 12th, in grammes :—

	S.	T.
Water-free arrowroot	480	382·7
Water-free sugar	399·7	294·8
Total dry carbohydrates	879·7	677·5
Butter (without casein)	124·7	84·4
Total water-free food in two days	1004·4	761·9

Proportion of fat to carbohydrates 1 to 7, 1 to 8.

The dry starches and butter being assumed to be of their ordinary composition, the *daily* amount of carbon would be, in grammes,—

	S.	T.
In arrowroot and sugar	195·33	150·4
In butter	49·25	32·83
Total	244·58	183·23
	grms.	grms.
The amount of water taken in the two days	4592	4592

It is of no consequence to calculate the proportion to body-weight, as some starch and sugar passed off by the bowels.

During these two days the men were kept in complete rest. They were allowed to get up for fear keeping in bed should make them feverish, but they sat quite still, or lay down on the bed, and did not leave the room during the time.

The *weight* decreased in the case of S. from 67·7 to 66·5 kilogrammes, and in the case of T. from 50·6 to 49·8 kilogrammes.

The Urinary Excretion was collected as usual on the first day, from 8 A.M. to 8 A.M.; but on the second day it was collected from 8 A.M. to 8 P.M., and again from 8 P.M. to 8 A.M, so that the last twelve hours' urine was secreted forty-eight to sixty hours after the last meal of nitrogenous food.

The full details are given further on, and I will now merely state the mean results.

On the mean of the two days the urea of twenty-four hours fell from 35 grammes to 16·765, or more than one-half in the case of S., and from 26 to 15, or rather less than one-half in the case of T.

The amount of urea in the last twelve hours was only 5 and 4·2 grammes for the two men. The total nitrogen, or a mean of the two days, fell from 17·97 and 13·4 to 8·176 and 7 grammes in the two men, while in the last twelve hours it was only 3·017 and 2·17 grammes, or at the rate of only 6·034 and 4·34 grammes in twenty-four hours.

Calculated according to body-weight, the results are per kilogramme :—

	Urea.	Nitrogen in urea.	Total nitrogen.	Non-ureal. nitrogen.
S.	·252	·118	·136	·018
T.	·301	·141	·159	·018

A more satisfactory comparison may perhaps be made by taking the last day only as representing more complete nitrogenous inanition.

Per kilogramme of body-weight.

	Urea.	Nitrogen in urea.	Total nitrogen.	Non-ureal nitrogen.
S.	·2034	·0949	·1054	·0105
T.	·2540	·1180	·1420	·0130

Therefore during complete nitrogenous inanition the tissues of the smaller and older man furnished a slightly greater amount of nitrogen than those of the larger man, and this is evident both in the ureal and non-ureal nitrogen, so that it could scarcely be accidental.

The sulphuric acid and phosphoric acids were determined on the last day; the latter acid was in almost precisely the same absolute mean amount in each man, viz. ·9533 and ·941 gramme; the larger man passed, however, one-third more sulphuric acid, viz. ·633 as against ·427 gramme.

In the last twelve hours the chloride of sodium fell to 1 and ·42 gramme.

The Intestinal Excretion.

This was examined on the last day. The composition was—

	Total weight, in grammes.	Solids.	Water.	Nitrogen.
S.	42·53	6·6	35·93	·3875
T.	35·44	6·55	28·89	·5360

The amount of solids was almost identical, but S. passed less nitrogen than T., as occurred also in the first period. The excreta were quite bilious, and had a greenish tint.

On the second day of nitrogenous inanition the balance of ingesta and egesta was as follows:—

	S.	T.
Weight of body at commencement of period, in kilogrammes	66·89	50·1
Weight of body at close of period	66·19	49·6
Gain or loss, in grammes	—680	—500
Total ingesta in food and drink, in grammes	2995	2907
Urinary egesta „	2477·5	2306
Intestinal egesta „	45·53	35·44
Skin and lung egesta „	1155	1065·5

The considerable derangement of the usual balance is very evident; it depended in part on the greater amount of water taken as compared with the former period, but not apparently altogether.

The water of the kidneys and the insensible perspiration were increased, while the intestinal water was lessened.

There was no sugar in the urine detectable by common tests.

The effects of the diet in the two men being thus very similar, a satisfactory basis of comparison was obtained for the period of exercise.

Third Period.—Ordinary Food and Occupation.

The men then returned to their former regulated diet and usual occupation for four days. Very nearly the same amount of food was taken as in the first period. At the end of four days the weight of the body in each man had returned almost exactly to its former amount.

The excretion of urea and the total nitrogen (which is given in more detail further on) followed a course very similar in each man.

On the first day after the return to nitrogenous diet the urea was in round numbers 14 and 12 grammes respectively below the mean of the first period, that is to say, it was nearly the same as during the first day of non-nitrogenous feeding; it then, in the case of S., increased day by day till it reached 29·67 grammes on the fourth day. In the case of T. it increased for two days, but fell a little on the fourth day; the total nitrogen, however, increased regularly every day.

The general result was that whereas in four days of the first period on a similar diet and exercise the excretion of nitrogen was 71·892 and 53·636 grammes respectively, during these four days of the third period the excretion of nitrogen in the urine was only 51·952 and 44·38 grammes; so that in the case of S. 19·94 and in the case of T. 9·256 grammes of nitrogen were retained in the body for the nutrition of the nitrogenous tissues which had been brought into a state of nitrogenous inanition for two days by cutting off the supply of nitrogen.

At the end of the four days it was considered that the tissues had recovered their composition.

Fourth Period.—Non-nitrogenous Diet and Exercise.

Diet.—The diet during this period was of the same kind as in the second period. The men were directed to eat what they pleased of arrowroot made into cakes, and jelly, sugar, and the oil of butter.

They took in the two days of December 17–18, and 18–19, the following amounts:—

Non-nitrogenous Food in two days, in grammes.

	S.	T.
Water-free arrowroot	796·6	586·8
Water-free sugar	421·5	360·0
Total dry starches	1218·1	946·8
Butter (without casein)	188·5	127·5
Total water-free food	1306·6	1074·3
Proportion of fat to starches	1 to 6·46	1 to 7·42

The daily proportion of carbon was—

	S.	T.
In starches	270·400	210·189
In butter	74·478	50·395
Total	344·878	260·584

The amount of water drank in the two days was. . 5159·5 4762·6.

Both men eat more during this period, partly because they felt more hungry, partly because the arrowroot-cakes were better made. T. especially took more butter, to which he felt a distaste previously.

The diet satisfied hunger; there was no sinking or craving for other kind of food, but it was monotonous, and neither man wished to continue it.

Exercise.—During these two days the men took the following amount of walking-exercise, on level ground. On the first day the exercise commenced at 9 A.M., and lasted till 7.45 P.M. with intervals. On the second day it commenced at 9 A.M., and lasted till 9 P.M. The men then went to bed.

First day.—23·76 miles=38·23 kilometres.

The work done was calculated according to Professor Haughton's formula, that walking on a level surface is equal to lifting $\frac{1}{20}$th of the weight through the distance walked.

S., weight with clothes, 73·68 kilogrammes. Work done = 140839 kilogramme-metres, or 453·6 tons lifted a foot.

T., weight with clothes, 56·33 kilogrammes. Work done = 107655 kilogrammes-metres, or 346·74 tons lifted a foot.

Second day.—Distance walked 32·78 miles = 52·74 kilometres.

Work done.
S. = 194294 kilogramme-metres.
= 625·8 tons lifted a foot.
T. = 147515 kilogramme-metres.
= 475 tons lifted a foot.

The first day's walking was done pretty well by both men. On the second day both men did the first 20 miles well, but felt very much fatigued during the last 13 miles. During the last 4 miles each man felt pain in the small of his back. Both men could, however, have marched on the following day if necessary.

With regard to the amount of fatigue as compared with other occasions, T. would give no opinion, as he said he had no fair basis of comparison. S., however, was clear that he was much more fatigued than on other food. In 1865 in Ireland he marched 26 miles on one day and 20 on the following, carrying his rifle, accoutrements, and forty rounds of ball-cartridge (an additional weight equal to 18 lb. nearly), and yet he did not feel fatigued at all; while on the present occasion, marching without weight except his clothes, he felt much exhausted.

Both men felt hungry; the food satisfied them; neither had any perceptible action of the skin; the days were fine and rather warm. During these two days S. lost almost precisely 2 kilogrammes in weight, and T. lost $\frac{3}{4}$ of a kilogramme.

The Urinary Excretion.—The urine was collected as usual from 8 A.M. to 8 A.M. on the first day, and from 8 A.M. to 8 P.M., and from 8 P.M. to 8 A.M. on the second day. In order to compare this, I have placed together the chief urinary constituents in the two periods of rest and exercise.

Amount of Urine, in cubic centimetres.

	S.		T.	
	Rest.	Exercise.	Rest.	Exercise.
First 24 hours	2230	2550	2120	1650
First 12 hours of second day (day urine)	1550	1210	1690	1000
Second 12 hours of second day (night urine)	910	1020	600	650

Excretion of Nitrogen, in grammes, in Urine.

	S.				T.			
	Urea.		Total nitrogen by soda-lime.		Urea.		Total nitrogen by soda-lime.	
	Rest.	Exercise.	Rest.	Exercise.	Rest.	Exercise.	Rest.	Exercise.
December 17–18, first 24 hours	20·00	19·125	9·33	10·048	17·3	16·005	8·765	7·994
December 18, first 12 (day) hours of second day	8·525	7·865	4·005	4·533	8·45	8·000	4·912	4·522
December 18–19, last 12 (night) hours of second day	5·005	7·140	3·017	3·360	4·2	5·200	2·170	3·553
Total in two days...	33·530	34·130	16·352	17·942	30·030	29·205	15·847	16·069

The excretion of the urea was very parallel in the two men, and followed this course. In each man in the first twenty-four hours nearly 1 gramme less was excreted by each man in the period of exercise as compared with that of rest; the larger man excreted nearly 3 grammes more urea than the smaller one.

In the next twelve hours each man excreted very nearly ½ a gramme less in the period of exercise as compared with rest. The absolute quantity was almost precisely the same in each man; in other words, the bulk of the larger man now had no effect in the urea.

In the last twelve hours (chiefly rest during night) the urea increased in each man in the period of exercise, as compared with that of rest, the absolute increase in S. being 2 grammes, and in T. 1 gramme.

Taking the whole period,—

	S.	T.
Excretion of urea in two days in the period of exercise as compared with rest......	+0·60	−·825

The results, when the total nitrogen is considered, are as follows:—

Slightly more nitrogen was excreted by S. in the period of exercise throughout; the excess being,—

	grm.
In the first 24 hours	0·718
In the next 12 hours.........................	0·528
In the last 12 hours	·343
Total excess of nitrogen during exercise period .	1·589

In the case of T. the total nitrogen during exercise was like the urea below the period of rest in the first 36 hours, but in the last 12 hours the excess of nitrogen in the period of exercise was so considerable as to cause the nitrogen of the two days of exercise to exceed that of rest by 0·223 gramme.

I draw the conclusion, therefore, that in both these men there was in the first 36 hours a decrease in the amount of urea; but in the last 12 or rest-hours of the 48 hours of the period of exercise, an increase.

That in the case of S. the total nitrogen was increased throughout the whole period of exercise, the total increase being 1·589 gramme, or 24·5 grains of nitrogen, while in the case of T. the total nitrogen, like the urea, was lower in the first 36 hours of the period of exercise, but increased greatly in the last 12 hours.

It may, indeed, be said that the difference between the amounts in the two periods is after all so inconsiderable as to be explained by the necessary errors of observation. But the constancy of the results in the two men, and in the case of T., the amount on the first day after the work-period, as given further on, seem to me to show the excess to be real.

On the same diet the heavier man excreted rather more urea and total nitrogen throughout than the smaller man, except in the first 12 hours of the second active day, when the urea was a trifle less.

The excretion of nitrogen in the urea, as compared with the total nitrogen, was (the ureal nitrogen being taken as unity) as follows:—

	S.	T.
Period of rest	1 to 1·042	1 to 1·13
Period of exercise	1 to 1·126	1 to 1·178

In both cases there appears to have been a greater relative excretion of the nitrogenous substances other than ureal. Is it not probable that the creatinine was increased?

The Phosphoric Acid.

	S.		T.	
	Rest.	Exercise.	Rest.	Exercise.
First period of 24 hours	......	1·873	......	1·144
First 12 hours of second period ..	·4930	·395	·5102	·5305
Second 12 hours of second period	·4603	·749	·4308	·3978
Total in last 24 hours	·9533	1·144	·9410	·9283

On a non-nitrogenous diet the amount of phosphoric acid is not increased in a period of exercise as compared with a like period of rest. The immaterial increase in S. is counterbalanced by as slight a decrease in T.

The Sulphuric Acid.

	S.		T.	
	Rest.	Exercise.	Rest.	Exercise.
First 12 hours of second day (day). .	·372	·3791	·232	·1544
Second 12 hours of second day (night)	·261	·3084	·195	·3011
Total on second day..............	·633	·6875	·427	·4555

The sulphuric acid was slightly increased in each man, but the increase was not great, and is perhaps within the limits of error.

Chloride of Sodium.

As no chloride of sodium was taken with the non-nitrogenous diet, the amounts excreted represent on the second day the mere waste of the tissues.

	S.		T.	
	Rest.	Exercise.	Rest.	Exercise.
First 24 hours	6·914	3·280	4·9	1·866
First 12 hours of second day (day time)	2·81	·673	1·88	1·094
Last 12 hours of second day (night time)	1·01	·119	·42	·150
Total of last day	3·82	0·892	2·30	1·244

As the results agree in both men, it appears that on a diet free from common salt much more chloride of sodium passes with the urine during rest than exercise; it is to be inferred that in the latter case chloride of sodium passes off by the skin.

No sugar was detected in the urine by the ordinary tests of liquor potassæ and Fehling's copper solution.

Intestinal Excretion.

This was examined on the last day, and was as follows:—

	Total weight, in grammes.	Solids.	Water.	Nitrogen.
S. ..	100·5	5·63	94·87	·5318
T. ..	120·7	11·012	119·688	·5739

If these numbers are compared with those of the corresponding period of rest, it appears that the total intestinal excretion was larger, but this arose in one man from an excess of water; in the other the solids were increased. In both men the nitrogen was in excess in the period of exercise, but the difference was not great, and may probably be disregarded.

Balance of Ingesta and Egesta.

On the second day of the non-nitrogenous diet and exercise, the balance of ingesta and egesta was as follows:—

	S.	T.
Weight of body at commencement of period, in kilogrammes	66·66	50·1
Weight of body at close of period	65·73	49·87
Gain or loss, in grammes	−930	−230
Total ingesta in food and drink, in grammes	3639	3124
Urinary egesta	2247	1667
Intestinal egesta	100·5	120·7
Skin and lung egesta	2221·5	1556·3

If these numbers are compared with those given in the corresponding period of rest, it will be seen that in both men the skin and lung egesta were very greatly increased (nearly 100 and 50 per cent. respectively); the intestinal egesta were also much larger; the urinary smaller, especially in the case of T., who passed nearly 800 cub. centims. less of urine, though he took more fluid as drink.

Neither of the men were conscious of any perspiration.

Fifth Period.—Ordinary Diet and Exercise.

The men were now again placed on their weighed diet, and took their ordinary exercise for four days, except that on the day following the walk of 33 miles they were tired and rested a good deal.

This period has now to be compared with the third period, which followed that of rest. As the amount of diet is very important, I give the mean amount in each of the four days of the third and fifth period, in English ounces.

Daily amount, in ounces (437·5 grains).	S.		T.	
	Third, or after rest-period.	Fifth, or after work-period.	Third, or after rest-period.	Fifth, or after work-period.
Cooked meat	8·5	9·81	6·625	7·87
Bread	17	16·18	16·25	16·75
Vegetables—$\frac{3}{4}$ potatoes, $\frac{1}{4}$ cabbage	13·68	14·62	13·5	14·37
Butter	1	1	1	1
Tea, with $1\frac{1}{2}$ oz. of milk, $1\frac{1}{2}$ oz. of sugar	20	20	20	20
Coffee, with same amount of sugar and milk	20	20	20	20
Ale	21	20	18	20
Salt uncertain				

In the fifth period each man took rather more than an ounce more meat; S. took $\frac{8}{10}$ oz. less bread, and T. $\frac{1}{2}$ an ounce more; each man took $\frac{3}{4}$ of an ounce more vegetables, and 1 and 2 ounces more water. It is to be regretted that the diet was not precisely the same; but the differences are not very great, and it was thought desirable to allow the men to satisfy their appetites. They were more hungry after the work-period than after the rest-period.

The *weight* increased in this period. In two days S. gained $1\frac{1}{2}$ kilogramme and T. $1\frac{1}{4}$ kilogramme, each man nearly getting his proper weight.

The Urinary Excretion.

The quantity of Urine.

	S.		T.	
	After rest-period.	After work-period.	After rest-period.	After work-period.
Mean of 4 days	1139	1028	1500	1495

There was scarcely any difference in T., and only 10 per cent. difference in S.

The Nitrogen.

	S.				T.			
	After rest-period.		After work-period.		After rest-period.		After work-period.	
	Urea.	Total nitrogen by soda-lime.	Urea.	Total nitrogen by soda-lime.	Urea.	Total nitrogen by soda-lime.	Urea.	Total nitrogen by soda-lime.
First day	20·67	9·703	20·8	10·237	14·40	7·441	23·00	11·58
Second day	25·68	12·304	26·364	13·065	23·00	11·480	24·36	13·00
Third day	26·29	13·704	28·32	14·590	25·20	12·209	24·57	
Fourth day	29·67	14·260	30·10	15·555	22·99	13·231	21·36	10·395
Mean	25·555	12·988	26·396	13·361	21·397	11·095	23·322	11·658
								Mean of 3 days.

Unfortunately, on the third day, in the case of T., the determination of the nitrogen by soda-lime was not satisfactory, and as some time elapsed before it could be again done, the amount has been omitted. But supposing there was the same relative excess over the ureal nitrogen as in the other days, the total nitrogen would have been 13·97 grms. Adopting this number, the following are the results :—

	S.	T.
Excess of urea in four days in after work-period	3·364	7·700
Excess of total nitrogen in four days in after work-period	1·492	4·560

The question now arises, was this excess of nitrogen excreted during the after work-period the result of the elimination of the products of destroyed muscle during the work-period, or was it the consequence of an excess of nitrogenous food in the four days following the exercise?

S. took 1·31 oz. avoirdupois more meat cooked and $\frac{3}{4}$ oz. vegetables in the fifth than in the third period. The percentage of water in the meat was 57·49, and if the nitrogen be taken at 2·955 per cent., there would be in 1·31 oz. of cooked meat 1·1 grm. of nitrogen. In the vegetable there would be about 0·04 grm. of nitrogen. But from this amount must be deducted ·325 grm. of nitrogen not taken in the bread, making the total daily excess of nitrogen taken in the fifth period ·815 grm.; the daily excess of nitrogen in the urine was, however, only ·375 grm. ; therefore, in the case of S., it cannot be affirmed that any excess of nitrogen was derived from disintegration of muscle during the exercise. In the case of T., the daily excess of nitrogen was larger, amounting daily to

1·14 grm., but as the man took 1·245 oz. more meat, ½ oz. more bread, and almost an ounce more vegetable (in all 1·2 grm. of nitrogen), it is evident that here also the excess of nitrogen in the urine might have been derived from the food. However, it is really probable that some of the very large excess of urea on the first day of this period, in the case of T., was really owing to augmented elimination from the work. No such excess is observable in the case of S., who had, however, a larger elimination than T. in the previous twelve hours.

The Chloride of Sodium.

The chloride of sodium rapidly returned to its previous amount.

	S.	T.
First day	1·444	1·614
Second day	6·169	4·905
Third day	10·25	8·513
Fourth day	8·117	6·446

It will be remembered that T. always took less salt than S. The third period is not comparable with the fifth, as the men took by mistake a great deal of salt on the second day.

The Phosphoric Acid.

	S.	T.
First day	1·565	2·158
Second day	2·413	2·273
Third day	2·548	2·533
Fourth day	2·408	2·065

As the phosphoric acid was not determined in the third period, no comparison is possible, but the above Table shows that no excess passed off in the after-period.

The sulphuric acid was not determined in this period.

The Intestinal Excreta, in grammes.

	S.				T.			
	Total weight.	Solids.	Water.	Nitrogen.	Total weight.	Solids.	Water.	Nitrogen.
Dec. 19–20; first day ...	298	......	......	......	127·5			
Dec. 20–21; second day.	191·7	......	......	......	213			
Dec. 21–22; third day...	134·9	21·86	113·02	1·264	71	11·8	59·2	·7188
Dec. 22–23; fourth day.	171·1	......	......	......	191·7			

The large intestinal excretion on the first day, in the case of S., was owing to a little looseness of the bowels; there were two stools on that day, being the only instance of irregularity in either man. Otherwise, as compared with the first period, there is no evidence in either case of any increased excretion; on the third day, indeed, the nitrogen was below that of the first period in each case.

The balance of ingesta and egesta was as follows on the third day of this period:—

	S.	T.
Weight of body at commencement...	67·1	50·07
Weight of body at close	67·08	50·08
Gain or loss, in grammes	−20	+10
Food and drink ingesta	2891·7	2877·5
Urinary egesta	1808·7	1922·5
Intestinal egesta	134·9	71
Skin and lungs egesta.................	968·1	894

These numbers are fairly accordant with those of the first period, except that the intestinal excretion in T. was slightly less, and the urinary rather more.

The conclusions which can be drawn from the above experiments are not altogether accordant with those of Professors Fick and Wislicenus.

The decrease in the urea during the first thirty-six hours of the exercise-period, as compared with the rest-period, on a diet without nitrogen, which occurred in these two men, is, I think, conformable with the results obtained by the two experimenters mentioned; but this is not the case with the increase in the urea which I found in the last twelve hours. Yet that this increase is real is shown, I believe, by the accordant results in the two men, and by the increase of the total nitrogen of the exercise-period as determined by soda-lime.

The relative greater increase in my experiments of the non-ureal nitrogen (which makes me believe that an excess of nitrogenous compound other than urea, and possibly creatinine especially, was produced by the exercise) is not perceptible in their experiments, yet I cannot but believe that the fact was so, as it comes out with great clearness in the two men. The following Table shows this.

Relation of ureal to non-ureal nitrogen, the former being taken as unity.

	S.	T.
Before rest-period	1 to 1·1	1 to 1·108
Rest-period	1 to 1·042	1 to 1·13
After rest-period	1 to 1·009	1 to 1·116
Work-period	1 to 1·126	1 to 1·178
After work-period	1 to 1·08	1 to 1·06 (?) (three days).

The reason which makes me believe the results are real, is the fact that the individual relation of the ureal and non-ureal nitrogen is preserved; that is to say, in T. the non-ureal nitrogen is, under normal circumstances, a little in excess as compared with S.; the same relative excess is also found in the work-period.

The reason of these differences between Professors Fick and Wislicenus and myself is probably to be found in the short period of time during which their observations were carried on, and also because the urea was not determined by them in the night of the 30th to 31st of August.

But their conclusion is certainly borne out, that on a non-nitrogenous diet exercise produces no notable increase in the nitrogen of the urine—although, when the whole period is considered, it does produce a slight increase.

It may now also be said that, under similar conditions, exercise produces no increase in the excretion of nitrogen by the bowels.

The diminution in the amount of urea during the actual period of work, as compared with the rest-period, which, if I am not mistaken, is obvious in both our experiments, is a very curious circumstance. It shows, not that on a non-nitrogenous diet the nerves and muscles are totally unaffected by exercise, but that changes go on which either retain nitrogen in the body or eliminate it by another channel.

Is it possible that, when the excess of nitrogen is restricted, the exhausted muscle will take nitrogen from the products given off from another portion of decomposing muscle, and thus the nitrogen may be used over and over again? or, after all, is nitrogen really given off in some form by the skin during exercise, as formerly supposed?

Although it is thus certain that very severe exercise can be performed on non-nitrogenous diet for a short time, it does not follow that nitrogen is unnecessary. The largest experience shows, not only that nitrogen must be supplied if work is to be done, but that the amount must augment with the work. For a short period the well-fed body possesses sufficient nitrogen to permit muscular exertion to go on for some time without a fresh supply; but the destruction of nitrogenous tissues in these two men is shown by the way in which, when nitrogen was again supplied, a large amount was retained in the body to compensate for the previous deprivation.

I believe also that in these two men the great exhaustion of the second day showed that their muscles and nerves were becoming structurally impaired, and that, if the experiments had been continued, there would have been on the third day a large diminution in the amount of work.

I have found that the period when a restricted supply of nitrogen begins to tell on the work differs in different men. In one experiment I reduced the nitrogen in the food to one-half its normal quantity in two men: in one, no effect was produced on exercise in seven days; in the other, a lessening of active bodily work was produced in five days. Doubtless the previous nutrition of the muscle would influence the time.

Finally, it may be questioned whether the relation of elimination of nitrogen to exercise can be properly determined in this manner, *i. e.* by cutting off the supply of nitrogen. The true method would probably be to supply nitrogen in certain definite amount, so that the acting muscle might appropriate at once what it required.

February 7, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communication was read:—

"Account of Experiments on Torsion and Flexure for the Determination of Rigidities." By J. D. Everett, D.C.L., Assistant to the Professor of Mathematics in the University of Glasgow. Communicated by Sir William Thomson. Received January 25, 1867.

(Abstract.)

These experiments are a continuation of those described in a paper read February 22, 1866, with some modifications in the apparatus employed which render the comparison between torsion and flexure more direct. The amount of torsion or flexure produced by subjecting a cylindrical rod to a uniform couple throughout its whole length, is measured by means of two mirrors clamped to the rod near its ends, in which, by the aid of two telescopes, the reflexions of a scale overhead are seen and the displacements read off. One end of the rod is fixed, and a couple (of torsion and flexure alternately) is applied to the other end.

Three rods, of glass, brass, and steel, were experimented on, and the results obtained were as follows—M, n, and k denoting the resistances (in kilogrammes per square millimetre) to linear extension, shearing, and cubical compression respectively, and σ denoting the ratio of lateral contraction to longitudinal extension:—

	Glass.	Brass.	Steel.
Value of M......	5851	10948	21793
" n	2390	3729	8341
" k	3533	57007	18756
" σ	·229	·469	·310

February 14, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Relation of Insolation to Atmospheric Humidity." By J. Park Harrison, M.A. Communicated by the President. Received January 30, 1867.

The occurrence of the maxima of insolation on days of great relative humidity which was noticed by Herr v. Schlagintweit in India[*], receives

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[*] Proceedings of the Royal Society, 1865, vol. xiv. p. 111.

confirmation from the fact that the post-solstitial periods of maximum solar radiation in autumn, and also the diurnal maxima, coincide with monthly and daily periods of maximum humidity.

It is proposed to show the extent to which this is the case in England by means of Tables of monthly results of radiation and vapour.

The first Table exhibits the mean monthly values of solar radiation and vapour-tension for the five months from May to September, at Greenwich, in 1860-64*.

Table I.

Monthly Means of Radiation and Vapour at Greenwich (1860–64).

Month.	Solar radiation.	Tension of vapour.	Weight of vapour.
May	97°·8 F.	0·314 in.	3·5 grs.
June.........	103·2	0·364	4·1
July	+108·2	+0·393	+4·4
August......	+107·5	+0·397	+4·4
September..	97·5	0·361	4·0

The plus signs (+) indicate first and second maxima.

The maxima, both of radiation and vapour, occur in July and August. The excess of insolation in July is 5°, and the excess in August 4°·3 above the mean in June. The excess of vapour-tension in the two months is ·030.

To obtain results more exactly comparable, the monthly mean tension of vapour for each of the five months was next deduced at the hours of noon, 2 P.M., and 4 P.M., for the years 1842–47, during which two-hourly observations were made at the Royal Observatory.

In Table II., which contains the monthly means, maximum results again appear principally in July and August.

Table II.

Monthly Means of Tension of Vapour at Greenwich at 0^h, 2^h, and 4^h.

Year.	May.	June†.	July.	August.	September.
1842.	0·358	0·473	0·436	0·545	0·449
1843.	0·390(*a*)	0·414	0·501(*b*)	0·509	0·474(*c*)
1844.	0·365	0·429	0·466	0·430	0·450
1845.	0·331	0·493	0·463	0·443	0·402
1846.	0·379	0·483	0·484	0·496	0·471
1847.	0·376	0·391	0·484	0·497	0·403

(*a*) Mean amount of cloud 8·0. (*b*) Cloud 8·5. (*c*) Cloud 4·5.

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* The vacuum-thermometer was used at Greenwich first in 1860. 1864 is the date of the last published observations. The means of radiation were derived from the daily maxima of the vacuum-thermometer; the means of vapour from the usual number of diurnal observations. It is believed that the observations for the five years are homogeneous.

† See note *, p. 358.

And the monthly maxima of solar radiation (in the next Table) accord with the higher mean tensions of vapour in almost every instance.

Table III.

Monthly Mean Maxima of Solar Radiation.

Year.	May.	June[*].	July.	August.	September.
1842.	82°·1	98°·9	92°·4	102°·2	81°·7
1843.	80·9(*a*)	85·1	90·9(*b*)	93·0	92·6(*c*)
1844.	87·5	95·0	94·1	87·2	87·0
1845.	75·0	93·1	90·3	(91·7)[†]	81·3
1846.	87·1	103·1	97·4	93·3	90·3
1847.	86·3	85·7	93·0	89·0	77·6

(*a*) Mean amount of cloud 8·0. (*b*) Cloud 8·5. (*c*) Cloud 4·5.

In those cases where the values of radiation and vapour do not agree, the exceptions will, it is believed, be sufficiently accounted for; thus the occurrence of the second maximum of solar radiation in September 1843 in place of July, though tension was higher in the latter month, is explained by the obscuration of the sun, and the unusual quantity of rain in July. The tension of vapour in September was ·06 higher than in June[‡].

Heat and Vapour in Canada.—The dependence of the maximum temperature in the day on the quantity of moisture in the air, in winter, at Toronto, though not directly connected with the present inquiry, is closely allied to it, and may be referred to with advantage in the absence of observations of solar radiation, to show the effects of slight variations of vapour in that country.

General (at that time Colonel) Sabine, in a paper in the Transactions of the Royal Society "On the Periodic and Non-Periodic Variations of Temperature at Toronto"[§], pointed out the fact that the period of minimum heat in the year, both hourly and daily, at that station occurs between the 7th and 17th days of February, on the days when vapour is also at its minimum. The means of temperature and tension on the ten days

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[*] In June the maxima of radiation are, as a rule, found to be accompanied by less vapour than in August and September. Thus it will be seen in Tables II. and III. that the difference between the mean tensions in June and August 1846 is ·013; the excess is in favour of August; but the maximum solar radiation is in June. In 1843 tension was at its maximum in July—but not radiation, in consequence of the abnormal quantity of cloud and rain in that month.

[†] Six observations only.

[‡] A still more remarkable instance of monthly maximum solar radiation in September occurred in 1865, when the excess over the mean of the five preceding Septembers was 21°·7, and the increase of contemporaneous vapour-tension ·086. This appears to be accounted for by the rainfall in August; the weather in September 1865 was, in fact, very like that which follows the rainy season in India. The tension of vapour was ·084 higher than in June.

[§] Anno 1853, p. 148.

alluded to are respectively 21°·7 and ·098, as deduced from the hourly observations which were taken in 1842–48 at the Ordnance Office.

The mean readings of the standard thermometer at 2 P.M. (the warmest hour in the winter at Toronto) and the contemporaneous mean tensions of vapour, in January and February, are exhibited in Table IV.

TABLE IV.

Monthly Mean Results at Toronto at 2^h P.M. (1842–48)*.

Month.	Maximum temperature	Wet-bulb thermometer.	Tension of vapour.
	°	°	
January ...	28·6	26·7	0·129
February...	28·3	25·9	0·117

At 2^h on the 7th to the 17th days of February the temperature was 26°·4, and the mean tension of vapour ·111.

Postmeridian maxima of solar radiation.—Though, it is well known at observatories, the hour of mean maximum solar heat occurs in this country after midday, there is no numerical proof of the fact available, excepting the results of six days' observations by Professor Daniell. From experiments made by him in June (1822) at every hour between 9^h 30^m A.M. and 7^h 30^m P.M., the mean highest readings of a black-bulb thermometer were obtained between 1^h 30^m and 2^h. The following are the means for five days:—

	h	m		
At	10	30	....	46°
	12	30	....	63
	1	30	....	65
	2	30	....	63†

It may be assumed, then, that on days when the sun is shining both in the morning and afternoon, solar radiation is of highest apparent force after 0^h‡.

Daily maxima of vapour.—The means of vapour-tension have been deduced at 10^h A.M., noon, and 2^h from the bi-horary observations at Greenwich in 1842–47; the following Table, which contains the monthly results for each of these hours, shows that the means at 2^h are higher than those at 10^h A.M., or at noon, in every month from March to September:—

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* From Tables LV., LVIII., and LX., Toronto Observations, vol. ii. The maxima of heat and vapour occur at Toronto in July and August, vapour-tension at 2^h in July being ·069 higher, and in August ·108 higher than in June.

† Daniell's 'Meteorology,' vol. ii. p. 113.

‡ Ibid. pp. 114–118. On another favourable day in June, Professor Daniell obtained the following results with a solar thermometer covered with black wool:—at 10^h A.M. 111°; at noon 129°; at 2^h P.M. 143°; at 2^h 30^m P.M. 138°. Since this paper was in type, I find these results are confirmed by observations made in March 1858 by Mr. H. S. Eaton.

TABLE V.

Monthly Means of Vapour-Tension at Greenwich at 22^h, 0^h, and 2^h (1842–47).

Hour.	March.	April.	May.	June.	July.	August*.	Sept.
h							
22	·236	0·284	0·357	0·437	0·462	0·473	0·427
0	·246	0·294	0·365	0·446	0·469	0·483	0·441
2	+·249	+0·297	+0·368	+0·449	+0·475	+0·486	+0·444

The mean increase of vapour from 10^h to noon is ·010; and the mean increase from noon to 2^h is ·0034. In six of the months the mean increase is in each case ·003[†].

The results accord with the assumed date of maximum radiation.

Actinometer-observations.—The observations of the actinometer made by Mr. Nash at the Royal Observatory in 1864 supply further evidence of the fact that the maxima of radiation occur in the autumn, and after 0^h.

I have extracted the mean results of the several groups of observations which were taken at or near the same elevation of the sun, in the months of August and September in autumn, and in the months of March and April in spring, and find that the value of the mean increase of the scale-readings in the autumn is about 100 per cent. greater than in the spring[‡].

The mean increase of the scale-readings in March and April is 19·4.

The mean increase of the scale-readings in August and September is 39·6.

The difference is 20·2, at the same mean altitude of the sun.

The means of the contemporaneous observations of the vacuum-thermometer on the grass were as follows:—

In March and April	70°·3 F.
In August and September ...	84°·7 F.

The difference is 14°·4.

And the mean tension of vapour for the four months is, in March and April ·23, and in August and September ·34.

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[*] August is the only month in which the mean tension of vapour is higher at 4^h P.M. than at 2^h P.M.

[†] With the exception of July (when there is the minimum difference of ·007 between the results at 10^h A.M. and noon) and of September (when there is the maximum difference ·014), the mean increase in tension at noon in the several months is also remarkably regular.

[‡] See 'Greenwich Meteorological Results,' 1864, p. xxxviii.

TABLE VI.

Mean Results of Actinometer-observations at Greenwich, March and April 1864.

Month and day.	Mean solar time.	Mean results in scale-divisions.	Altitude of sun.	State of sky.
	h m		°	
March 23.	2 29	18·7	30	Light cirri.
15.	22 23	14·2	32	Cloudless.
18.	2 9	21·1	32	Clear.
18.	0 36	20·4	36	Clear.
24.	1 32	17·0	36	Clear.
15.	23 51	19·1	37	Clear. Light cloud.
April 15.	2 26	27·1	37	Clear.
20.	2 28	25·5	38	Light cirri prevalent.
March 24.	0 49	14·3	39	Clear.
Means.	......	19·4	35	Clear.

TABLE VII.

Mean Results of Actinometer-observations at Greenwich, August and September 1864.

Month and day.	Mean solar time	Mean results in scale-divisions.	Altitude of sun.	State of sky.
	h m		°	
August 30.	3 26	41·6	30	Cloudless (thunder in evening).
September 14.	22 8	43·3	34	Sun free from cloud.
14.	22 12	34·7	35	Light cirri over sun.
August 26.	3 0	38·3	35	Sun free from cloud.
26.	2 55	32·4	36	Sun free from cloud.
29.	2 39	40·1	36	Clear (amount of cloud in afternoon 9, cirrocumuli, cirrus).
5.	3 3	41·7	39	Cloudless.
Means	......	39·6	35	Cloud genera lresent.

Of the very few observations in May and July which were available for comparison, the scale-readings in July were found to reach far higher values than in May *.

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* The observations at altitudes between 24° and 29° in March and October showed similar results. Below 24° and above 60° there were no observations that were comparable.

Table VIII.

Results of Actinometer-observations in May and July 1864.

Month and day.	Mean solar time.	Mean results in scale-divisions.	Altitude of sun.	State of sky.
	h m		°	
May 16.	2 17	28·3	47	Cloudless*.
16.	0 28	21·4	56	Cloudless.
16.	0 14	23·7	57	Cloudless.
July 11.	2 19	46·7	50	Clear about sun.
5.	1 7	36·6	58	Clear.
13.	23 18	39·3	58	Cloudless.

The mean tension of vapour in May was ·30, in July ·38.

The last Table contains, in parts of scale, the results of groups of observations near midday and 2 P.M., on three days, in March, May, and July. The maximum in each case occurs at the later hour.

Table IX.

Actinometer-observations at 0^h and 2^h.

Month and day.	Mean solar time.	Mean result in scale-divisions.	Altitude of sun.	State of sky.
	h m		°	
March 24.	0 49	14·3	39	Clear throughout.
24.	1 32	17·0	36	Clear.
May 16.	0 28	21·4	56	Cloudless.
16.	2 17	28·3	47	Cloudless.
July 14.	23 37	48·6	59	Cloudless.
14.	2 13	57·6	50	Cloudless.

The great accordance between the foregoing results renders it unnecessary to adduce further evidence of the occurrence of maximum insolation some considerable time after the summer solstice and after 5^h.

Increased solar radiation supposed to be due to the action of aqueous vapour.—Herr von Schlagintweit, applying Professor Tyndall's discovery of the absorptive properties of aqueous vapour to the phenomenon of insolation, attributed the high readings of his solar thermometer, in certain parts of India, to the fact that air, when highly charged with moisture, impedes free radiation; that is to say, the air restores to the instrument some portion of the heat which has been radiated off from it.

A like cause has been recently assigned for the variations in temperature which take place on clear nights in Madras under different tensions of va-

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* Compare observations on the same day at $0^h\ 28^m$ and $0^h\ 14^m$.

pour, those nights being considered clear in which the percentage of cloud did not exceed ·10. It was found, on a careful tabulation of hourly observations, that the fall in temperature was decidedly greater when the quantity of vapour was relatively small*.

Cloud, haze, and opalescence† *of the atmosphere more probably the principal cause of the phenomenon.*—It appeared of much importance to ascertain whether the presence of cloud, and the imperfect state of transparency in the sky which usually accompanies it, may not have materially assisted in producing the results alluded to in the last paragraph, and, *à fortiori*, account for the increased insolation noticed in cloudy weather in various parts of India—*e. g.* "on days in the rainy season when the clouds are temporarily broken," and, as in Sikkim, "when a break in the clouds of an hour or two had to be watched for to obtain observations of solar radiation" (Schlagintweit, 'Meteorology of India,' pp. 49 & 51).

To test this point, the results of the observations for the four years ending 1844, over which the inquiry at Madras extended, were divided into groups of contemporaneous observations of temperature, vapour-tension, and percentage of clear sky, when the mean numerical results showed that a progressive fall of about 1° F. for every ·10 of vapour-tension was accompanied by a proportionate increase in the percentage of clear sky,—a result which is the more significant when it is considered that the infusion of visible cloud was limited to ·10.

To prevent mistake, the fall in temperature on the nights of maximum clearness was compared with the fall on nights of minimum clearness within the limits above stated. There were twenty-five nights which were estimated to be perfectly clear, and twenty-two nights when the per. centage of clear sky ranged from ·90 to ·93, the average being ·915 (1·000 representing an entirely clear sky). The results were as follows:—

The mean fall of temperature at an estimated clearness of sky denoted by 1·000 was 8°·3 F.

The mean fall of temperature at an estimated clearness denoted by ·915 was 6° F.

The difference is 2°·3. The contemporaneous mean tensions of vapour were ·68 and ·83 respectively.

It is probable then, as cloud accompanies humidity, that tension of vapour gives some indication of the *state of transparency of the sky*, whilst affording a measure of the quantity of invisible vapour in the air.

In any case it is sufficiently clear, both from the results at Madras and from the slight increase in the tension at 2h P.M., that the amount of aqueous vapour alone is not sufficient to account for increased or diminished solar radiation.

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* Phil. Mag. October 1866; where see Tables and method of deduction.

† This term was first used by Professor Roscoe. It here represents the state of the atmosphere at the moment vapour is in process of condensation previously to its formation into cloud.

The explanation of the phenomenon of the maxima of insolation occurring on days of great relative humidity in India, however, which has been already alluded to, applies with increased force to the absorptive properties of visible moisture; and the known action of even the lightest form of cloud in radiating heat to the earth, would point to this as the principal cause of the phenomenon, though it cannot be doubted that some of the effect is due to the action of invisible vapour, whether as warmed directly by the solar rays, or by heat derived from a secondary source.

The dependence of terrestrial heat on vapour and cloud.—It was to increased or diminished radiation under a clear or clouded sky that the inflections of the curves of mean temperature, which I communicated to the Royal Society in May 1865, were ascribed*. The effects produced were on that occasion considered to be principally due to radiation at night†; but an examination of the daily means proved that a similar action occurred also by day‡,—the phenomenon in fact depending, as in the case of solar radiation, on the quantity of moisture in the air.

The precise values of heat resulting from the action and reaction of solar radiation and vapour cannot be ascertained until mean numerical values have been deduced from a sufficient number of hourly observations to afford trustworthy data for determination. The present paper merely helps to establish a relation between solar radiation and humidity, and suggests an additional cause for the effects which appear to follow from it.

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* It has since been found that the value of the difference of temperature for fifty years at Greenwich, due to terrestrial radiation, was very much understated by me, principally from the fact that the mean temperatures for the greater part of the entire period had been corrected by quantities in the Philosophical Transactions which were afterwards found to be erroneous, and have been disused at Greenwich for several years. All the conclusions which were based specially on the results for fifty years must therefore be considered as withdrawn.

The results derived from the more perfect means for periods of seven and eight years are confirmed by the Oxford thermographs, and by almost identical results which I find were obtained by Dr. Mädler from fifteen years' observations at Berlin nearly thirty years ago.

† See also 'Lectures on Scientific Subjects,' by Sir John Herschel, 1866, p. 149 (10

‡ British Association Reports, 1859, p. 198.

APPENDIX.

No. 1.

Monthly Mean Maximum Solar Radiation at Greenwich (1860–64).

Year.	May.	June.	July.	August.	September.
1860.	103°·7	101°·0	105°·6	103°·5	97°
1861.	96·7	108·6	114·5	116·5	106·9
1862.	99·6	103·1	109·4	110·7	99·8
1863.	95·7	104·1	107·2	106·5	92·9
1864.	93·1	99·3	103·7	100·1	90·7
Means*	97·8	103·2	108·2	107·5	97·5

No. 2.

Monthly Mean Tension of Vapour at Greenwich (1860–64).

Year.	May.	June.	July.	August.	September.
1860.	0·312	0·357	0·393	0·396	0·364
1861.	0·284	0·404	0·413	0·436	0·369
1862.	0·365	0·352	0·394	0·410	0·396
1863.	0·302	0·364	0·384	0·412	0·320
1864.	0·306	0·344	0·382	0·333	0·357
Means*	0·314	0·364	0·393	0·397	0·361

No. 3.

Monthly Mean Weight of Cubic Foot of Air at Greenwich (1860–64).

Year.	May.	June.	July.	August.	September.
	grs.	grs.	grs.	grs.	grs.
1860.	3·5	4·0	4·4	4·4	4·1
1861.	3·2	4·6	4·6	4·9	4·1
1862.	4·0	4·0	4·5	4·6	4·4
1863.	3·4	4·1	4·3	4·5	3·6
1864.	3·5	3·9	4·2	3·7	4·0
Means*	3·5	4·1	4·4	4·4	4·0

* Extracted from Greenwich Observations.

No. 4.

Vapour-Tension at 10^h A.M. at Greenwich (1842–47).

Year.	March.	April.	May.	June.	July.	August.	Sept.	Oct.
1842.	0·263	0·255	0·354	0·453	0·425	0·527	0·442	0·285
1843.	0·260	0·302	0·383	0·400	0·467	0·498	0·461	0·304
1844.	0·238	0·320	0·352	0·412	0·452	0·423	0·435	0·330
1845.	0·187	0·282	0·323	0·467	0·459	0·432	0·382	0·349
1846.	0·261	0·298	0·361	0·516	0·491	0·494	0·465	0·354
1847.	0·210	0·248	0·372	0·377	0·481	0·465	0·376	0·375
Sums	1·419	1·705	2·145	2·625	2·775	2·839	2·561	1·997
Means *	0·236	0·284	0·357	0·437	0·462	0·473	0·427	0·333

No. 5.

Vapour-Tension at *noon* at Greenwich (1842–47).

Year.	March.	April.	May.	June.	July.	August.	Sept.	Oct.
1842.	0·280	0·270	0·358	0·470	0·437	0·546	0·447	0·315
1843.	0·271	0·309	0·390	0·407	0·493	0·499	0·478	0·324
1844.	0·250	0·337	0·358	0·423	0·459	0·429	0·456	0·343
1845.	0·190	0·295	0·331	0·488	0·462	0·445	0·397	0·354
1846.	0·266	0·305	0·377	0·498	0·489	0·491	0·470	0·356
1847.	0·219	0·247	0·375	0·388	0·476	0·491	0·396	0·389
Sums	1·476	1·763	2·189	2·674	2·816	2·901	2·644	2·081
Means *	0·246	0·294	0·365	0·446	0·469	0·483	0·441	0·347

No. 6.

Vapour-Tension at 2^h P.M. at Greenwich (1842–47).

Year.	March.	April.	May.	June.	July.	August.	Sept.	Oct.
1842.	0·284	0·269	0·355	0·472	0·435	0·546	0·447	0·317
1843.	0·273	0·318	0·393	0·419	0·501	0·508	0·476	0·324
1844.	0·253	0·346	0·370	0·432	0·470	0·431	0·453	0·346
1845.	0·197	0·292	0·333	0·496	0·469	0·445	0·408	0·346
1846.	0·271	0·311	0·379	0·485	0·488	0·491	0·473	0·357
1847.	0·218	0·245	0·381	0·392	0·486	0·496	0·407	0·381
Sums	1·496	1·781	2·211	2·696	2·849	2·917	2·664	2·071
Means *	0·249	0·297	0·368	0·449	0·475	0·486	0·444	0·345

* The means are inserted in Table V.

Fall of temperature at Madras, by nights:—

1. Entirely clear sky (1·000). 25 nights.
° 4·5
7·1
8·5
7·7
7·6
11·2
9·5
9·5
9·1
6·9
9·6
9·6
12·2
8·1
9·4
6·6
8·2
4·9
6·5
8·7
8·2
7·5
10·6
9·4
6·5
25) 207·6
8·3

2. Partially clouded sky (·915). 22 nights.	Percentage of clear sky.
° 4·8	·900
6·2	
5·9	
8·4	
4·6	·910
5·0	
7·5	
7·6	
5·3	
5·4	
5·8	
8·5	
3·6	·920
7·2	
7·6	
5·0	
6·7	
5·7	·930
4·2	
6·4	
4·9	
6·3	
22) 132·6	
6·0	·915 Mean.

At Fort Franklin, Sir John Richardson found that the maximum of solar radiation was obtained in the *spring* and at noon. Mr. Forbes accounted for this by the fact that the sun's rays were reflected from the *snow*, and thus the bulb of the thermometers received reflected as well as direct rays of sunshine.

II. "On the Conversion of Dynamical into Electrical Force without the aid of Permanent Magnetism." By C. W. Siemens, F.R.S. Received February 4, 1867.

Since the great discovery of magnetic electricity by Faraday in 1830, electricians have had recourse to mechanical force for the production of their most powerful effects; but the power of the magneto-electrical machine seems to depend in an equal measure upon the *force expended* on the one hand, and upon *permanent magnetism* on the other.

An experiment, however, has been lately suggested to me by my brother, Dr. Werner Siemens of Berlin, which proves that permanent magnetism is not requisite in order to convert *mechanical* into *electrical* force; and the result obtained by this experiment is remarkable, not only because it demonstrates this hitherto unrecognized fact, but also because it provides a simple means of producing very powerful electrical effects.

The apparatus employed in this experiment is an electro-magnetic machine consisting of one or more horseshoes of soft iron surrounded with insulated wire in the usual manner, of a rotating keeper of soft iron surrounded also with an insulated wire, and of a commutator connecting the respective coils in the manner of a magneto-electrical machine. If a galvanic battery were connected with this arrangement, rotation of the keeper in a given direction would ensue. If the battery were excluded from the circuit and rotation imparted to the keeper in the opposite direction to that resulting from the galvanic current, there would be no electrical effect produced, supposing the electro-magnets were absolutely free of magnetism; but by inserting a battery of a single cell in the circuit, a certain magnetic condition would be set up, causing similar electro-magnetic poles to be forcibly approached to each other, and dissimilar poles to be forcibly severed, alternately, the rotation being contrary in direction to that which would be produced by the exciting current.

Each forcible approach of similar poles must augment the magnetic tension and increase consequently the power of the circulating current; the resistance of the keeper to the rotation must also increase at every step until it reaches a maximum, imposed by the available force and the conductivity of the wires employed.

The cooperation of the battery is only necessary for a moment of time after the rotation has commenced, in order to introduce the magnetic action, which will thereupon continue to accumulate without its aid.

With the rotation the current ceases; and if, upon restarting the machine, the battery is connected with the circuit for a moment of time with its poles reversed, then the direction of the continuous current produced by the machine will also be the reverse of what it was before.

Instead of employing a battery to commence the accumulative action of the machine, it suffices to touch the soft iron bars employed with a permanent magnet, or to dip the former into a position parallel to the magnetic axis of the earth, in order to produce the same phenomenon as before. Practically it is not even necessary to give any external impulse upon restarting the machine, the residuary magnetism of the electro-magnetic arrangements employed being found sufficient for that purpose.

The mechanical arrangement best suited for the production of these currents is that originally proposed by Dr. Werner Siemens in 1857* consisting of a cylindrical keeper hollowed at two sides for the reception of insulated wire wound longitudinally, which is made to rotate between the poles of a series of permanent magnets, which latter are at present replaced by

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* See Du Moncel 'Sur l'Electricité,' 1862, page 248.

electro-magnets. On imparting rotation to the armature of such an arrangement, the mechanical resistance is found to increase rapidly, to such an extent that either the driving-strap commences to slip or the insulated wires constituting the coils are heated to the extent of igniting their insulating silk covering.

It is thus possible to produce mechanically the most powerful electrical or calorific effects without the aid of steel magnets, which latter are open to the practical objection of losing their permanent magnetism in use.

III. "On the Augmentation of the Power of a Magnet by the reaction thereon of Currents induced by the Magnet itself." By Charles Wheatstone, F.R.S. Received February 14, 1867.

The magneto-electric machines which have been hitherto described are actuated either by a permanent magnet or by an electro-magnet deriving its power from a rheomotor placed in the circuit of its coil. In the present note I intend to show that an electro-magnet, if it possess at the commencement the slightest polarity, may become a powerful magnet by the gradually augmenting currents which itself originates.

The following is a description of the form and dimensions of the electro-magnet I have employed. The construction, it will be seen, is the same as that of the electro-magnetic part of Mr. Wilde's machine.

The core of the electro-magnet is formed of a plate of soft iron 15 inches in length and $\frac{1}{2}$ an inch in breadth, bent at the middle of its length into a horseshoe form. Round it is coiled in the direction of its breadth, 640 feet of insulated copper wire $\frac{1}{12}$ of an inch in diameter. The armature, which is according to Siemens's ingenious construction, consists of a rotating cylinder of soft iron $8\frac{1}{2}$ inches in length, grooved at two opposite sides so as to allow the wire to be coiled upon it longitudinally; the length of the wire thus coiled is 80 feet, and its diameter is the same as that of the electro-magnet coil.

When this electro-magnet is excited by any rheomotor the current from which is in a constant direction, during the rotation of the armature currents are generated in its coil during each semirevolution, which are alternately in opposite directions; these alternate currents may be transmitted unchanged to another part of the circuit, or by means of a rheotrope be converted to the same direction.

If now, while the circuit of the armature remains completed, the rheomotor be removed from the electro-magnet, on causing the armature to revolve, however rapidly, it will be found by the interposition of a galvanometer, or any other test, that but very slight effects take place. Though these effects become stronger in proportion to the residual magnetism left in the electro-magnet from the previous action of a current, they never attain any considerable amount.

But if the wires of the two circuits be so joined as to form a single circuit, in which the currents generated by the armature, after being changed to the same direction, act so as to increase the existing polarity of the electro-magnet, very different results will be obtained. The force required to move the machine will be far greater, showing a great increase of magnetic power in the horseshoe; and the existence of an energetic current in the wire is shown by its action on a galvanometer, by its heating 4 inches of platinum wire ·0067 in diameter, by its making a powerful electro-magnet, by its decomposing water, and by other tests.

The explanation of these effects is as follows:—The electro-magnet always retains a slight residual magnetism, and is therefore in the condition of a weak permanent magnet; the motion of the armature occasions feeble currents in alternate directions in the coils thereof, which, after being reduced to the same direction, pass into the coil of the electro-magnet in such manner as to increase the magnetism of the iron core; the magnet having thus received an accession of strength, produces in its turn more energetic currents in the coil of the armature; and these alternate actions continue until a maximum is attained, depending on the rapidity of the motion and the capacity of the electro-magnet.

If the two coils be connected in such manner that the rectified current from the coil of the armature passes into the coil of the electro-magnet in the direction which would impart a contrary magnetism to the iron core, no current is produced, and consequently there is no augmentation of magnetism.

It is easy to prove that the residual magnetism of the electro-magnet is the determining cause of these powerful effects. For this purpose it is sufficient to pass a current from a voltaic battery, a magneto-electric machine, or any other rheomotor, into the coil of the electro-magnet in either direction, and it will invariably be found that the direction of the current, however powerful it may eventually become, is in accordance with the polarity of the magnetism impressed on the iron core.

If, instead of the currents in the coil of the rotating armature being reduced to the same uniform direction, they retain their alternations, no effects, or at most very small differential ones, are produced, as no accumulation of magnetism then takes place.

I will now call attention to the fact that stronger effects are produced at the first moment of completing the combined circuit than afterwards. The machine having been put in motion, at the first moment of completing the circuit 4 inches of platina wire were made red-hot, but immediately afterwards the glow disappeared, and only about one inch of the wire could be permanently kept at a red heat. This diminution of effect was accompanied by a great increase of the resistance of the machine. The cause of the momentary strong effect was, that the machine from its acquired momentum continued its motion for a few seconds, though it required a stronger force than could be applied to maintain that motion. Each time the circuit is broken and recompleted the same effect recurs.

On bringing the primary coil of an inductorium (Ruhmkorff's coil) into the circuit formed by connecting the coils of the electro-magnet and rotating armature, no spark occurs in the secondary coil. On account of the great resistance of the circuit, which now also includes the primary coil of the inductorium, the current is not in sufficient quantity to produce any noticeable inductive effect.

A very remarkable increase of all the effects, accompanied by a diminution in the resistance of the machine, is observed when a cross wire is placed so as to divert a great portion of the current from the electro-magnet. The four inches of platinum wire, instead of flashing into redness and then disappearing, remains permanently ignited. The inductorium, which before gave no spark, now gave one a quarter of an inch in length; water was more abundantly decomposed; and all the other effects were similarly increased.

I account for this augmentation of the effects in the following way:—

Though so much of the current is diverted from the electro-magnet by the cross wire, the magnetic effect still continues to accumulate, though not to so high a degree; but the current generated by the armature, passing through the short circuit formed by the armature-branch and cross wire, experiences a far less resistance than if it had passed through the armature and electro-magnet branches; and though the electromotive force is less, the resistance having been rendered less in a much greater proportion, the resultant effect is greater.

I must observe that a certain amount of resistance in the cross wire is necessary to produce the maximum effect. If the resistance be too small, the electro-magnet does not acquire sufficient magnetism; and if it be too great, though the magnetism becomes stronger, the increase of resistance more than counterbalances its effect.

But the effects already described are far inferior to those obtained by causing them to take place in the cross wire itself. With the same application of force, 7 inches of platinum wire were made red-hot, and sparks were elicited in the inductorium $2\frac{1}{2}$ inches in length.

The force of two men was employed in these, as well as in the other experiments. When the interrupter of the primary coil was fixed, the machine was much easier to move than when it acted. For when the interrupter acted, at each moment of interruption the cross wire being, as it were, removed, the whole of the current passed through the electro-magnet, and consequently a greater amount of magnetic energy was excited, while in the intervals during which the cross wire was complete the current passed mainly through the primary coil.

The effects are much less influenced by a resistance in the electro-magnet branch than in either of the other branches.

To reduce the length of the spark in the inductorium (the primary coil of which was placed in the cross wire) to $\frac{3}{4}$ of an inch, it required the resistance of $5\frac{1}{4}$ inches of the fine platinum wire in the cross wire, 5 inches in the armature-branch, and 4 feet in the electro-magnet branch.

When there was no extra resistance in either of the branches, the length of the cross wire being only about a few feet, the intensity of the current in the electro-magnet branch, compared with that in the cross wire, was as 1 : 60; and when the resistance of the primary coil of the inductorium was interposed in the cross wire, the relative intensities were as 1 : 42.

In conclusion I will mention that there is an evident analogy between the augmentation of the power of a weak magnet by means of an inductive action produced by itself, and that accumulation of power shown in the static electric machines of Holtz and others which have recently excited considerable attention, in which a very small quantity of electricity directly excited is, by a series of inductive actions, augmented so as to equal, and even exceed, the effects of the most powerful machines of the ordinary construction.

February 21, 1867.

Dr. W. A. MILLER, Treasurer and Vice-President, in the Chair.

The following communication was read:—

"A brief Account of the 'Thesaurus Siluricus,' with a few facts and inferences." By J. J. Bigsby, M.D. Communicated by Sir R. I. Murchison, Bart. Received January 28, 1867.

I have been led to attempt the preparation of a general view of Silurian life, as far as now known, by my own frequent want of such a record or muster-roll of the constituent members of this great initiatory division of palæozoic zoology,—a task which has been made pleasant by some personal knowledge of two countries rich in the earlier formations.

I have been further encouraged by the great accumulations of the last few years, through the establishment in North America and elsewhere of numerous colleges, each of them having become the centre of more or less field-work. Far more aid still has been derived from many public surveys on a tolerably liberal scale. Nor can we forget the highly meritorious and successful labours which have been, and still are, carried on by private individuals in almost every part of Europe and North America.

As this undertaking required an exactitude and a critical skill in determining species and genera according to late improvements in classification, much beyond an ordinary acquaintance with Silurian life, after my materials were put together, I obtained the very valuable aid of Mr. J. W. Salter, late Palæontologist at the London Museum of Practical Geology.

I was then, through the kindness of Sir Roderick I. Murchison, Bart., allowed to submit my manuscript to Robert Etheridge, Esq., F.R.S.E., the present Palæontologist to the Institution over which Sir Roderick presides.

To the careful superintendence of these two eminent naturalists I am indebted for corrections and suggestions of the greatest importance, and particularly as relates to Britain and to Europe generally.

My matter has been principally found in the voluminous and truly priceless writings of Murchison, Sedgwick, Barrande, Sowerby, De Verneuil, James Hall, M^{c}Coy, Salter, Billings, Angelin, Eichwald, Shumard, and Davidson—together with those of other authors, some of whom are scarcely of inferior merit*.

I have been favoured with many unpublished contributions from my friends Mr. Billings (the learned Palæontologist of the Canadian Survey) and Principal Dawson, F.R.S., of M^{c}Gill College, Montreal,—also, through the kindness of Mr. Salter, from the Himalayas (Colonel Strachey, R.E.), from West Tasmania (Dr. Milligan), from South Wales (Henry Hicks, Esq.), and from the late Mr. Wyatt-Edgell.

I propose to give to this effort the name of "Thesaurus Siluricus." Besides its use for general reference in the closet and in the quarry, the 'Thesaurus' provides a high station from which the student may obtain a broad survey of the Silurian populations of the whole earth. It will assist in tracing the extent, shape, and varying depths of areas, in discovering regional affinities, differences, and those great zoological severances which we call breaks. By its aid we may compare horizons remote from each other, and, moreover, note the frequent changes of many kinds which take place while the epoch is wôrking out its long history. It will place under our examination numberless communities of life, their constituents, habits, rise, and decline.

The 'Thesaurus' points to the universality (as defined) at times proximate everywhere, brings into prominence the riches, magnitude, and wide diffusion of the Primordial stage; illustrates the power of locality over life, and opens out the wonderful march of geographic dispersion through obstacles innumerable.

For a long period naturalists have been arranging the life of the globe into species, genera, orders, &c., with a view to the establishment of types as standards of comparison. It is from such data, well considered and generally acknowledged, that this 'Thesaurus' has been compiled.

As long as an individual mollusk remains unregistered it is deprived of its full usefulness; but even then it may reveal an important fact—as the trilobite speaks of the Palæozoic period, and a nummulite of the Tertiary.

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* Agassiz, Beyrich, Bronn, Brongniart, Conrad, Dalman, D'Orbigny, Vicomte d'Archiac, Dawson, Emmerich, Emmons, Fischer, E. Forbes, Goldfuss, Green, Harkness, Hisinger, Haime, Honeyman, Rupert Jones, Ketley, Kutorga, Lawrow, Linnæus, Lovén, Lonsdale, M^{c}Chesney, Meek, Meneghini, Milne-Edwards, Morris, Owen, Pander, Phillips, Portlock, Rœmer, Rouault, Sars, Sharpe, Safford, Swallow, Triger, Vanuxem, Von Buch, Volborth, Wahlenberg, Winchell, &c. &c.

Until some such record as the present is available, the labours of many living investigators (whose names rise to the lips spontaneously) will rest comparatively fruitless. It has hitherto not been possible to consider widely scattered existences in an aggregate form. Facts (many) have been stored up separately; but generalized truths have been rarely attained. This has not yet been done in a satisfactory manner, not even by Bronn or Goldfuss for any one epoch, and scarcely for the cretaceous period by the American geologist Mr. Gabb, although he has done well.

This 'Thesaurus' contains 7553 species, and therefore gives abundant scope for profitable study; but probably it does not give the tithe of the whole Silurian life yet lying buried in the wilds of the Arctic Circle, of Hudson's Bay, Labrador, the two Americas, Scandinavia, Australia, India, &c. &c. The more accessible countries frequently, to this day, yield new forms, although the search for them is capriciously and idly conducted, and is dependent often on the accident of a new public work or the presence of a competent observer. Many undescribed species are lying in local museums, still more in the great collection at Prague in the possession of a high Ecclesiastic in that city. Owing to the enlightened perseverance of M. Barrande, a few small parishes close to Prague have yielded nearly one-third of the whole earth's Silurian remains within present knowledge; and the greater part of these are not met with elsewhere. How wonderfully rich must be the universal Silurian fauna! What a splendid promise to the future explorer!

The 'Thesaurus' is in the form of a Table. After mentioning the genus (taken alphabetically), its author, and the date of its establishment, the species are successively named, and treated of under four or more heads, along one and the same ruled line. First comes the part of the stage in which it occurs, then, in a given order, its author and locality, or localities, in the column indicative of its proper stage.

More information is thus conveyed, it is believed, than by any other form of Table. The summary which is appended to each order shows some of the organic relations of the Silurian system in Europe and in America to each other; it shows, too, how very little we know as yet of this epoch in Asia and Africa; and, among other things, it tells us the numerical strength of the genera.

Permit me now to lay before the Society a few facts drawn from the mere surface of the 'Thesaurus,' and only in the way of summary or brief remark, in order to suit the purpose of this evening. Much more than this the careful registration of more than 50,000 facts has prevented me from doing.

The Table A (page 375) gives the numerical amount of the Silurian flora and fauna as known in the years 1856 and 1866 respectively.

Table A.—Comparative number of species known in 1856 and 1866.

	Plantæ.	Amorphozoa.	Foraminifera.	Annelida.	Hetero-Pteropoda.	Polyzoa.	Cœlenterata.	Echinodermata.	Trilobitæ.	Entomostraca.	Brachiopoda.	Dimyaria.	Monomyaria.	Gasteropoda.	Cephalopoda.	Pisces.	Class uncertain.	Total.
Prize Essay	18	19	..	10	63	76	108	93	425	8	579	113	14	151	299	10	9*	1995
Thesaurus ...	76	125	25	132	241	389	496	479	1400	247	1408	446	136	721	1192	34	6	7553

This Table, taken from Bronn's Prize Essay published in 1856, and from the 'Thesaurus Siluricus,' shows that within the last ten years the number of known species has more than trebled.

Universality.

In the spirit of the following definition, it would appear that the Silurian system is universal—that is, it overspreads the whole earth more or less completely,—and that its component parts were laid down in a proximate time,—statements approved by M. Barrande, Bull. n. s. xii. 361. Definition:—"*A formation may be considered to be universal when it occupies large and small areas in very many parts of the earth, often remote from and even antipodal to each other, when it is always of like stratigraphical relations, is composed of like materials, and contains numerous genera in common, together with some representative and some identical species.*"

In support of our application of this definition to the Silurian system, the 'Thesaurus' exhibits the widest possible distribution of its fauna—a fauna, it must be remembered, which is pure from admixture with that of any other epoch which might possibly have been progressing at the same time.

The 'Thesaurns' contains many examples of the same species being in twenty to twenty-five different countries, large and far apart—the same creature or creatures marking the route from land to land.

Table B, drawn up under the inspection of Mr. Salter, presents 195 species common to regions very remote from each other, some of them being antipodal—a fact which tells the more forcibly from the tenacity with which a large part of Silurian life clings to locality as well as to horizon. 179 species are common to Europe and America. Sixty Silurian genera have been brought from South Australia by Mr. Selwyn, the chief Geological Surveyor of that colony; and Professor McCoy has met with in that country a Siphonotreta, a Phacops, and eighteen species of Graptolites absolutely identical with those of North America and of Europe. The Professor loudly expresses his surprise and delight. According to M. Barrande, *Orthoceras bullatum* (Sowerby) is at Melbourne (Australia) and in Ireland, Bohemia, Germany, and Russia. *Conocoryphe depressa* is both in Wales and Texas, one of the American States. Western Tasmania, the

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* Morris, Catal. p. 362.

Himalayas, Russia, North and South America, and many other regions offer ample fossil evidence of the general presence of the constituents of this period.

Table B.

Kingdom or Order.	No. of Species.	America and Europe.	America, Europe, and Australia.	America and Australia.	Europe and Australia.	Total.
Plantæ	74					
Amorphozoa	120	5	......	......	......	5
Foraminifera	25					
Annelida	132	4	......	......	......	4
Hetero-Pteropoda	239	16	......	......	......	16
Bryozoa	383	6	3	6	5	20
Zoophyta	432	18	......	......	......	18
Crinodea	456	7	......	......	......	7
Cystidea		1	......	......	......	1
Asteriada		1	......	......	......	1
Trilobitæ	1414	21	2 ?	......	......	23
Entomostraca	242	1	......	......	......	1
Brachiopoda	1372	64	......	......	......	64
Monomyaria	123	2	......	......	......	2
Dimyaria	439	9	......	......	......	9
Gasteropoda	715	9	......	......	......	9
Cephalopoda	955	15	......	......	......	15
Pisces	34	......	......	......	......	...
	7155	179	5 ?	6	5	195

The Silurian beds, it must be borne in mind, are usually visible in mere shreds and remainders, met with in any one place only as a stage or a part of a stage, the other portion being covered for perhaps thousands of square miles by more recent deposits, or removed by denudation; or it may be that certain stages have never existed, as we see in Arctic America with respect to the Lower Stage; while in the South, as in Sardinia, France, and Spain, it is the Upper Stage that is wanting, or very nearly so.

But the visible geographical spread of these strata is often very great. So extensive are the Silurian areas of North America (2000 miles across) that it only needs a short and easy step to induce a belief in a former universal prevalence and domination of this system.

Sufficient territory resting on Silurian rocks has been spared from oscillatory action to enable us to trace it in one or other of its parts over a large part of the earth. We follow it circuitously from England to Australia, or to America—the interspaces being filled up either by sea, by newer rocks, or by kindred palæozoic strata, which themselves irresistibly bespeak its frequent continuous existence near at hand.

This is only a fragment of the argument in favour of the doctrine of Universality of epochs, as just defined.

Locality.

The 'Thesaurus' brings conspicuously into view the great influence of locality on the nature and amount of life, in the same way as we observe at the present time. As each region yields up its fauna to the collector, much of that fauna is found to be new, the bond of connexion with other Silurian districts being in great measure generic.

The physical conditions of sea and land being necessarily local, produced as they are from time to time by agencies limited in space, the dwellers among these conditions must in a certain measure be local too, and typical—subject at any moment to removal. The first occupants of any spot who shall point out?

The maximum of life, meaning by that expression the largest combination of abundance, variety, and rank, is local. It may take place at the beginning of a stage, or of an epoch, in the middle, or at the end, being governed principally by the nature of the sediment. The rich Primordial beds of Western Newfoundland and of Quebec, the crowded Pleta beds of Russia and of Esthonia, the Trenton Limestone of America, the Mid-Silurian rocks of Bohemia* (*E. e.* 1, 2), some of those of Wales, the Lower Helderberg group of New York, are conspicuous examples of this. Parts of the Llandovery stage of Wales and of New York (U. S. A.) present a great dearth of life, and for a well-known reason. How barren are the vast accumulations of Lower Silurian in Bolivia, as at present believed! The Potsdam Sandstone of the valleys of the St. Lawrence and the Mississippi shows no signs of life for hundreds (and perhaps thousands) of square miles, save in small oases peopled chiefly by Lingulæ in incalculable millions of individuals.

Nearly equal areas of Central North-east America (N. latitudes 50°–32°) and Europe may have received about the same attention; but the latter, so far, has proved the richer by above a thousand species, as we see in the subjoined Table C.

Table C.—Known species of America and Europe compared.

Orders.	Species.		Orders.	Species.	
	America.	Europe.		America.	Europe.
Plantæ (kingd.)	56	20	Carried forward	964	931
Amorphozoa	58	64	Asteriadæ	29	29
Foraminifera	......	25	Crust. { Trilobites	396	1008
Annelida	36	98	Crust. { Entomostraca	75	170
Hetero-Pteropoda	96	144	Brachiopoda	678	721
Polyzoa (Bryozoa)	203	177	Monomyaria	78	56
Cœlenterata (Zoophyt.)	262	245	Dimyaria	181	241
Crinoids	193	93	Gasteropoda	421	274
Cystidea	56	63	Cephalopoda	321	861
Sedis incertæ	4	2	Pisces	2?	34
	964	931		3145	4325

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* The extraordinary abundance of Trilobites, Cephalopoda, &c. here is accounted for by the beds being calcareous and overlain by trappose masses, in place of the sand and gravel more commonly seen.

The Cephalopoda, Crustacea, Brachiopoda, and Annelida of Europe appear to largely exceed in number of species those of North America, while in nine Orders (see Table C) the two hemispheres hold nearly equal quantities. America greatly surpasses Europe in the number of its Crinoids, and to a smaller extent in Plantæ and Gasteropoda. I am not prepared with any inference from these facts. We know that the mineral constitution, and the past external influences in these several parts of the earth are different—not that the first is as influential as has been supposed.

Many species are marked as undefined in the 'Thesaurus,' because they are often only known by simple fragments.

About a thousand species have never been seen but in one locality. At least 200 Cyrtocerata are huddled together in the two contiguous parishes of Lockhov and Kozorz, near Prague, and, with other mollusks there, are unknown elsewhere. Other instances of this might be cited.

The two Silurian districts of Sardinia, with not a few fossils in common with Spain, although tolerably well examined by La Marmora and Meneghini, have not hitherto produced a Trilobite; nor has Spain given up a *Pentamerus,* as far as can be learnt. Out of our sixty species of *Asaphus* only one is known in Bohemia. Silurian fish are only mentioned as existing in Britain, Bohemia, and Russia; but doubtless they are in other Silurian areas.

The Trilobite genus *Dikelocephalus* of D. D. Owen contains thirty species. Only three are found in two places. Twelve species are near Quebec, and there only. Nine others are Minnesotan, on the Upper Mississippi; while the States of Texas and Vermont, on Lake Champlain, have each one, and Wales three—all distinct species. Western Newfoundland, although primordial, is thought to be without this remarkable genus.

Each of the twenty-seven known species of the Heteropod Maclurea is confined to one spot; twenty are American; and of these, eleven are confined to Newfoundland West.

Of the forty-five species of the genus *Trochoceras* (Cephalopoda), forty-three are restricted to the vicinity of Prague; and of these twenty-seven inhabited the very small space of 4–6 square miles, in company with many other mollusks. The Brachiopoda of Bohemia are mostly in the Fauna F, and in the two small districts of Konieprus and Mnienian.

Out of 270 species of *Orthis* only two are believed to be in Nova Scotia, and of the 109 species of the Gasteropod Murchisonia, again, two, but not one of the elsewhere most abundant genus *Pleurotomaria.*

On the other hand, Nova Scotia holds one-half of all our Cleidophora; and Tasmania is singularly rich in *Palæarca,* while the Point Levi shales are crowded with the Graptolite family, of extreme beauty, and rarely found in other countries. We further observe that, as it is with the horizontal disposition of Silurian life, so it is with the vertical: only twelve per cent. leave their native horizon, as we shall see.

These few facts have been selected from many, to show the strong tendency to localization inherent in the Silurian fauna.

Primordial Stage.

The ‘Thesaurus’ amply manifests the great extent of this stage, and the high significance of its teachings; but we shall here only speak of a few leading facts relating to Canada, extracted from the ‘Thesaurus’ itself.

While waiting for the results of field-work now in progress, Mr. Billings has treated this subject with his usual great ability in the first volume of the work entitled ‘The Palæozoic Fossils of Canada.’

The Primordial stage of Barrande (Taconic of Emmons) is truly Silurian, and forms the base of that epoch.

In the valley of the St. Lawrence it may average 8600 feet in thickness.

Resting horizontally in America on the inclined Laurentian rocks, the lower break is complete in every respect; while the upper break is very nearly so, although purely organic.

It divides naturally into *Lower* and *Upper* Primordial,—Potsdam sandstone constituting the former, and calciferous sandstone, with the enigmatical Quebec group, the latter, with a few layers of chazy limestone superadded.

The whole flora and fauna of the Primordial stage, American and European, amount to 919 species, while those of the St. Lawrence Valley alone are 560. The western, therefore, seems to be the richer of the two hemispheres; and this comes out still more distinctly in stating the fact that the Primordial genera at present known in America are 134, and those of all Europe 83.

Table D (below) has been constructed from the ‘Thesaurus.’ It exhibits numerically the zoological contents and the zoological relations of the several parts of the Primordial stage; and we see that the differences are great.

Table D.—The American Flora and Fauna of the Primordial Stage (principally Canadian).

		Plantæ.	Amorphozoa.	Annelida.	Hetero-Pterop.	Bryozoa.	Zoophyta.	Crinoidea.	Cystidea.	Asteriada.	Dimyaria.	Monomyaria.	Gasteropoda.	Brachiopoda.	Cephalopoda.	Trilobitæ.	Entomostraca.	Pisces.	Total.
Upper	Quebec Group	...	4	21	19	44	2	...	...	...	5	...	57	42	34	96	3	...	327
	Calciferous Sandst....	6	5	3	5	...	1	...	...	...	1	...	39	6	19	6	2	?	93
Lower	Potsdam Sandstone ...	16?	2	4	2	1	...	...	1	...	...	...	3	31	...	74	6	...	140
	Total	22	11	28	26	45	3	...	1	...	6	...	99	79	53	176	11	...	560

Great interest attaches to every part of this stage, but especially to the

Quebec group and its ill-understood connexion with the immediately contiguous strata.

An intimate acquaintance with this group near Quebec leads me to believe that there, at least, it is a displaced, crumpled, and fractured mass of schist, with thin beds of limestone and calcareous conglomerate interleaved, the last crowded with molluscan and crustacean life.

It is above the Potsdam Sandstone, and on or near the horizon of Calciferous sandstone and the lower layers of Chazy Limestone (Logan and Billings, Report, 1863). Into these (with a distinct tendency still higher) in other parts of North America, the Quebec group probably becomes fused, and assumes their horizontal position, mineral character, and many of their organic contents.

The fauna of the Quebec group, consisting of 327 species at Point Levi (Quebec) and in Western Newfoundland, is peculiar, and, of course, is only found there, with the exception of thirteen species found elsewhere in Calciferous sandstone, and eight in Chazy Limestone. They are one-sixteenth of the whole, and are as follows:—

Calciferous Sandstone:—*Lingula Mantelli, L. acuminata, L. Irene, Cameralla calcifera, Helicotoma gorgonia, H. uniangulata, H. perstriata, Pleurotomaria calcifera, P. postumia, Holopœa dilucula ?, Ecculiomphalus Canadensis, Murchisonia Anna, Piloceras Canadense* (Billings).

Chazy Limestone:—*Ecculiomphalus Atlanticus, Maclurea Atlantica, Stromatopora compacta* (running into B+BL), *Climacograptus antennarius, Ptilodictya fenestrata ?, Leperditia amygdalina, Camerella varians, Cheirurus prolificus* (Billings).

This group contains, besides the thirteen species just enumerated, 174* allied to those of the Calciferous Sandstone of Central North America, or more or less westward of Montreal. It is this which connects it closely with the sandstone. However, 140 remain typical.

The fossils of Chazy Limestone met with in the Quebec group only belong to a few of the basement beds of the former, because these almost immediately, upwards, change into a compact mass of crushed Crinoids, Cephalopoda, Gasteropoda, &c. (143 species)—all quite new, and alien from the life below.

The Calciferous Sandstone, always truly primordial, has in the Canadas and the United States of America 375 species, overspreading vast areas. They may be separated into three sets:—

1. Thirteen enter the Quebec group.
2. One hundred and seventy-four are the allies of that group.
3. One hundred and eighty-eight are foreign to it, and for the most part typical.

Like its two sister groups, the Calciferous Sandstone is shown, in the middle line of Table D, to display a remarkable tendency to abound in complex and powerful existences, and to paucity in the simple species, in-

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* These numbers are for the present only approximate, and may be altered.

TABLE E.—A synoptical view of Silurian Life, with special reference to vertical Range or Recurrence.

	Species typical of one Horizon.							Species Recurrent.																							
	Primordial Silurian.		Lower Silu-rian.	Middle Silu-rian.	Upper Silu-rian.	Total (typic.)	Grand Total	Primordial.								Lower Silurian.				Middle Silurian.				Upper Silurian.				Total Re-currents.	Per cent. on Typical Species.
	Lower.	Upper.						Lower. Horizons.				Upper. Horizons.				Horizons.				Horizons.				Horizons.					
								2	3	4	5	2	3	4	5	2	3	4	5	2	3	4	5	2	3	4	5		
Plantæ	14	...	37	17	5	73	82	4	...	...	...	...	...	...	...	4	...	...	...	...	...	...	...	1	...	...	...	9	11
Amorphozoa	4	6	56	7	25	98	110	...	...	...	...	...	1	...	...	6	4	...	...	...	...	...	...	1	...	...	...	12	11
Annelida	10	7	34	8	26	85	99	...	1	...	...	...	...	...	...	4	1	4	...	...	1	...	...	2	1	...	...	14	15
Hetero-Pteropoda	8	22	98	9	38	175	212	3	2	...	...	...	...	...	...	21	1	4	...	3	...	...	...	3	...	...	...	37	16
Polyzoa	4	51	149	26	64	294	333	...	...	...	...	1	...	...	...	17	9	4	1	6	...	...	...	1	...	...	...	39	11
Cœlenterata	...	2	97	35	179	313	384	...	...	...	...	...	...	...	1	30	13	8	...	12	2	...	...	5	...	...	...	71	20
Crinoidea	...	...	99	10	132	241	267	...	...	...	...	...	...	...	...	13	1	...	...	3	2	...	...	7	...	...	...	26	10
Cystidea	...	1	64	2	31	98	101	1	...	...	...	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	3	3
Asteriada	...	1	24	4	21	50	54	...	...	...	...	...	...	...	...	2	...	1	...	...	...	...	...	1	...	...	...	4	9
Trilobitæ	178	162	538	43	264	1185	1331	3	...	...	...	4	...	...	...	69	15	5	2	14	2	...	...	27	3	2	...	146	11
Entomostraca	11	9	55	4	115	194	205	...	...	...	...	1	...	...	...	4	1	...	...	2	1	...	...	2	...	...	...	11	5
Brachiopoda: Orthis	8	17	113	12	56	206	243	...	...	...	...	...	...	...	...	21	8	1	1	2	1	...	...	2	1	...	...	37	8
Rhynchonella	...	1	20	22	73	116	141	...	...	...	...	...	...	...	...	7	2	2	...	7	2	...	...	4	1	...	...	25	27
Strophomena	...	2	29	9	15	55	80	...	...	...	...	...	...	...	...	7	8	2	...	...	3	...	...	4	...	1	...	25	31
all other Species	40	30	216	80	299	665	759	...	1	...	...	...	...	...	...	24	13	4	1	19	6	2	...	19	6	...	...	94	12
Monomyaria	...	...	27	5	70	102	121	...	...	...	...	...	...	...	...	5	3	...	...	3	1	1	...	6	...	...	...	19	16
Dimyaria	1	5	211	25	127	369	426	...	...	...	...	...	...	...	...	29	6	1	...	8	1	...	...	10	2	...	...	57	13
Gaster.: Murchisonia	...	15	37	6	26	84	105	...	...	...	...	...	...	...	...	15	...	...	...	1	1	...	...	4	...	...	...	21	20
Pleurotomaria	1	18	56	11	20	106	125	...	...	...	...	...	2	...	...	15	...	...	...	...	...	...	...	1	1	...	...	19	17
all other Species	5	30	171	23	131	360	389	...	...	...	...	1	...	...	...	12	1	1	...	7	3	...	...	5	...	...	...	30	7
Cephal.: Gomphoceras	...	...	4	58	16	78	82	...	...	...	...	...	...	...	...	2	...	...	...	...	...	...	...	2	...	...	...	4	5
Cyrtoceras	...	7	36	225	35	303	312	...	...	...	...	...	...	...	...	7	...	...	...	2	...	...	...	9	...	...	...	9	3
Orthoceras	...	25	128	96	95	344	409	...	...	...	...	...	1	...	...	28	10	5	...	5	3	...	...	14	...	...	...	66	16
all other Species	...	13	111	105	69	298	321	...	...	...	...	...	...	...	...	11	1	2	...	1	...	...	...	8	...	...	...	23	7
	284	424	3010	624	1931	5891	6691	11	4	...	...	7	4	...	1	384	98	44	5	95	29	3	...	129	15	3	...	801	12

dividuals, nevertheless, being prodigiously numerous. Trilobites are here very few.

Potsdam Sandstone is rich in Trilobites, Brachiopoda, and Fucoids, but in every other form is very poor; and yet it possesses a Cystid.

In the Primordial group, therefore, we find numerous representatives of nearly every marine Invertebrate; and we have a startling example of the sudden development in very early times of the highest types of molluscan life, Nautili, Lituites, Trilobites, Protichnites, &c., dwelling, even then, in well adjusted communities.

Most of these facts are taken from the 'Thesaurus;' but this interesting portion of the 'Thesaurus' itself is the gift of Barrande, Emmons, Hall, Logan, and Billings *, and is the fruit of their unwearied study in the city, and of their toil in the field.

Recurrence.

A few words on the subject of recurrence, or the vertical range of Silurian life.

What can be more unexpected, or more wonderful, than the upward passage by a filial succession, through stages and epochs, of a mollusk during centuries inconceivably numerous! What an almost interminable series of posterities must have followed the first ancestor! The doctrine of limited duration in species must have its exceptions.

The 'Thesaurus' enumerates 803 recurrents, or 12 per cent. of the whole known life of the epoch—a very notable proportion,—still leaving 6200 species faithful to one horizon.

The synoptical Table E, compiled from the 'Thesaurus,' exhibits many details, and may be trusted approximately, although about 400 species have been passed by for want of sufficient information. It numbers separately the species typical of *one* horizon, and the species frequenting *more than one* horizon (being recurrent). It also introduces some of the greater genera, such as *Orthis, Murchisonia,* &c.

The species are arranged under the heads "Primordial," Lower," "Middle," and "Upper Silurian;" and in the case of the recurrents the number of horizons occupied by them is shown by the figures 2, 3, 4, 5. Thus we find that 69 Lower Silurian Trilobites occupy two horizons, 15 three, and 2 five horizons.

The percentage is stated in the last column, next to that containing the total recurrence of each order.

The Primordial stage only gives	2·7	per cent. of recurrency.
The Lower Silurian	16	„
The Middle	20	„
The Upper	8	„

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* It is well to note that, under Sir William E. Logan's able superintendence, we owe the splendid Primordial harvest gathered in Newfoundland and Anticosti, to the diligence and skill of Mr. Richardson, an explorer in the employ of the Canadian Geological Commission.

The orders vary greatly in respect to recurrency. There is none among fossil fish. In Cystidea it is only 3 per cent., in Gomphoceras 5 per cent., and is greatest in Strophomena, 31 per cent.

Although a considerable number of inferences have been prepared, I shall only venture now to introduce a few.

1. Recurrence is universal, both as to time and place.
2. Recurrences seem to be most numerous in the lower stages of the epoch; but further research may teach otherwise.
3. Species do not often change their horizon, not even when placed in countries far apart.
4. The same species may be typical of a single horizon in one country and recurrent in another.
5. Recurrency shows that a mollusk is not necessarily confined to any one community, but may find a home and flourish in several successively.
6. The number of recurrents measures the amount of change in conditions.
7. Communities, genera, and species disappear sporadically, except in the rare case of a catastrophe.
8. Recurrency is a measure of viability.

Extra-epochal Recurrence.

Few things demonstrate more plainly the sterner discipline now prevailing than the reduction by Mr. Salter to 133 of the 439 palæozoic species which I had tabulated as extra-epochal, although they had the sanction of the best palæontologists of the last fifteen or twenty years.

My Table, as originally made out, deals with the five palæozoic epochs, but in this place only with the forty-two Silurian species which leave for the higher periods. To these, recently, several interesting additions have been made.

I. These recurrents are mostly distinct from the intra-epochal, owing to their first appearance being in the Upper Silurian stage.

II. With the exception of *Chonetes sarcinulata,* they all stop within the Devonian Period.

III. The greater part of these recurrents are of low rank; 20 are Brachiopoda; 11 Zoophytes, 1 an Amorphozoa; 7 are Gasteropoda; 3 Cephalopoda; and 1 Trilobite. *Manon deforme* and *Orthis rugosa,* Lower Silurian fossils, reappear in Devonian, but not in Upper Silurian, where they are "presumably"—to use an expression of Mr. Etheridge.

IV. These species are very migratory—few being found in two epochs in the same country, but in different countries.

V. Opportunities of escape into a new epoch have been common; but the ways and means are frequently concealed by denudations, &c.

VI. Acclimatization must have been necessary.

VII. The length of individual life in proportion to specific extra-epochal life is almost as a unit to infinity.

TABLE F.—Geographical Summary of Silurian Life.

Orders.	America.	Europe.	India.	Africa.	S. Australia.	Tasmania.	Common.	
Plantæ	56	20	...	...	...	...	...	* Tibet.
Amorphozoa	62	63	4*	...	1	1	6	To America and Europe.
Rhizopoda	...	25?	...	...	...	...	...	Not definitely accepted.
Cœlenterata	262	245	1	...	2	1	27	To America and Europe.
Echinodermata. Crinoidea	193	93	...	...	2	...	6	” ”
Cystidea	56	53	...	...	20	...	3	” ”
Asteriada	29	29	...	...	...	...	1	” ”
Annelida	36	98	...	...	1	...	7	” ”
Crustacea. Trilobitæ	396	998	10	...	11	...	30	Various.
Phyllopoda Ostracoda ...	77	170	...	...	2	...	5	
Polyzoa	203	177	2	...	20	...	23	Various.
Brachiopoda	678	721	22	...	19	...	65	Various.
Monomyaria	78	56	...	...	2	...	5	To America and Europe.
Dimyaria	181	241	3	...	8	19	12	” ”
Pteropoda & Heteropoda	103	145	1	...	3	1	15	” ”
Gasteropoda	421	274	9	...	9	13	10	” ”
Cephalopoda	321	861	5	...	...	8	16	” ”
Pisces	...	34	...	...	...	...	...	
Incertæ Sedis	4	2	...	...	...	...	...	
	3156	4305	57	...	100	43	231	

3156+4305=7461 species.

Geographical Distribution of Silurian Life.

The 'Thesaurus' tends to show that North America, east of the Rocky Mountains, may probably be divided into two areas,—the one to the north of 57° (or of Lake Superior) being chiefly Upper Silurian, resting on crystalline rocks, the one to the south of that line, down to the Gulf of Mexico, on the contrary, being fully developed in some part of this great space.

It exhibits the regrettable fact that Asia, Africa, and Australia, taken together, have hitherto yielded only 200 species of Silurian remains; but this arises from the absence of exploration.

I have not yet had opportunity to bring together, harmonize, and compare the Silurian life of the several countries of Europe. The accomplishment of such a task might produce some definite truths, and many more probabilities. Either this vast region would prove to be one great Silurian area, with barriers here and there, and with certain channels of communication, and to be the result of many operations throughout a long interval of time; or it might turn out that the Silurian deposits and their fossils occupy three separate areas:—(1) the Britanno-Scandinavian, which has all the three stages, and the Primordial; (2) the Bohemian, at present of peculiar interest; and (3) the middle and southern area, found in France, Spain, and Sardinia, almost wholly Lower and Mid-Silurian.

Under this head of geographical distribution we have to deal with some curious phenomena—such as concern birthplace or first appearance, generic and specific, the duration of life, tolerance of conditions, mineral habitats. Migration possesses great interest, with its marks, causes, and modes, with its power, direction, and rate of progress, &c.

The transport or removal of dead organic matter from place to place, the "*remaniement*" of French geologists, is an important agency under several aspects, especially in the formation of extensive sheets of rock.

It now has become proper to bring to a close these few observations, or rather this enumeration of heads of Natural-History subjects, by expressing a confident hope that this compilation will find many and well qualified interpreters, and will be useful to geologists in general.

February 28, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On a Transit-Instrument and a Zenith Sector, to be used on the Great Trigonometrical Survey of India for the determination, respectively, of Longitude and Latitude." By Lieut.-Colonel A. Strange, F.R.S. Received February 16, 1867.

In 1862 the Secretary of State for India in Council sanctioned the provision of an extensive equipment of geodesical and astronomical instruments of the first order for the use of the Great Trigonometrical Survey of India; and he did me the honour to entrust to me the task of designing and superintending their construction. After several modifications, the following list was adopted:—

One Great Theodolite, with a 3-feet Horizontal Circle. By Messrs. Troughton and Simms.

Two Zenith Sectors. By Messrs. Troughton and Simms.

Two 5-feet Transit-Instruments. By Messrs. T. Cooke and Sons, York.

Two Smaller Transit-Instruments (*German form*). By Messrs. T. Cooke and Sons, York.

Two 12-inch Vertical Circles (*German form*). By Messrs. Repsold, Hamburg.

Two Galvanic Chronographs for registering Transit-Observations. By MM. Secretan and Hardy, Paris.

Three Astronomical Clocks. By Mr. Charles Frodsham.

The whole of these are nearly ready, and I take the opportunity of now submitting two of them (a 5-feet transit-instrument by Messrs. Cooke, and a zenith sector by Messrs. Troughton and Simms) to the inspection of the Royal Society. Both instruments present peculiarities.

The 5-feet Transit-Instrument.—This has a very powerful telescope of 5 inches clear aperture (a large diameter in proportion to its focal length). The axis is of aluminium bronze, cast in one piece, hollow, and turned both inside and out. The two halves of the telescope are easily separable from the axis for portability.

It is provided with four levels for rendering the axis horizontal; these are mounted on a plan suggested and devised by Mr. Cooke. He remarks that the ordinary striding level usually applied to such instruments watches the pivots only, whereas the observer wishes to be informed whether or not the telescope itself describes a true plane. This it will not do if the flexure of the axis differs, as it may do, in different altitudes. Mr. Cooke therefore attaches the levels to the telescope. His mode of doing this, and of providing for their due adjustment, will, in the absence of drawings, be best understood by inspecting the instrument.

The means of adjusting the axis vertically and azimuthally are also peculiar. The bearings on which the pivots turn are carried by strong three-armed pieces, similar in form to the tribrach of an ordinary theodolite. On one side the tribrach is raised or lowered by means of the three vertical screws which form its feet, and the axis is thus made horizontal; on the other side the tribrach is pushed laterally by two horizontal screws, and the telescope is thus brought into the meridian. Three principal objects are sought in these arrangements—to exclude shake, to obviate strain, and to cause the expansions to take place from the centre outwards. I have been well satisfied with the trials I have made of them. I find these adjustments to be exceedingly delicate in their action, and very stable.

The Zenith Sector.—This is quite unlike any instrument of the same denomination. My endeavour in designing it was to combine maximum power with minimum weight.

A solid steel vertical axis revolves within a hollow wide-based conical cast-iron pillar. Across the vertical axis is placed a frame, in which are formed bearings for the reception of a transverse horizontal axis. This axis carries outside the frame a telescope of 4 feet focal length and 4 inches clear aperture, and a portion of a circle comprising two opposite sectors, each containing about 45°. The telescope being vertical for the observation of stars near the zenith, the sectors are horizontal—that is, transverse to the telescope. The frame which supports the horizontal axis carries also four micrometer-microscopes for reading the sectors. These microscopes are arranged conically, so that all four are illuminated by a single light, in the manner adopted by the Astronomer Royal for the Great Greenwich Transit-Circle. The telescope and sectors revolve together, the microscopes being fixed. When packed for carriage, the telescope and sectors can be made to lie in the same direction, and so take up much less room than if they retained their transversal positions. Two levels are fixed to the horizontal-axis frame. The instrument is duly counterpoised.

The general arrangement of the instrument may be best conceived by supposing an equatoreal of the German form to be adjusted as it would be at the pole, when its polar axis would represent the vertical axis of the zenith sector, and its declination axis the horizontal axis.

The instrument has been too short a time in my hands to admit of my forming an opinion as to its probable success. I anticipate some advantage from the arrangement of the sectors, which, being in identical circumstances, should be liable to no inequalities of either flexure or temperature. Both of the principal axes of the instruments are provided with independent adjustments, the action of which appears to be very satisfactory.

In concluding this brief and imperfect notice, I beg to state that I hope to draw up hereafter a full and detailed description of all the instruments given in the foregoing list. At present my time is absorbed by the trials and experiments to which it is necessary that I should subject them before their despatch to India; and I trust the Royal Society will accept this explanation as an excuse for the meagreness of the present account.

II. "On the Orders and Genera of Ternary Quadratic Forms." By HENRY J. STEPHEN SMITH, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford. Received February 21, 1867.

(Abstract.)

The object of this Paper is to supply demonstrations of the undemonstrated results, relating to Ternary Quadratic Forms, which are contained in an important memoir of Eisenstein's ("Neue Theoreme der höheren Arithmetik," Crelle's Journal, vol. xxxv. p. 117),—and, at the same time, to extend those results to the cases not considered by him in that Memoir. The following are the principal points in which the theory of Eisenstein has been thus further developed:—

1. In Eisenstein's Memoir forms of an even discriminant only are considered. Such forms, and their contravariants, are always properly primitive; they have particular generic characters with respect to uneven primes dividing the discriminant, but have no supplementary characters (*i. e.* characters with respect to 4 or 8). The case of forms of an even discriminant is more complicated. Besides the properly primitive order, there may exist, in this case, an improperly primitive order, in which the forms themselves are improperly primitive, and their contravariants properly primitive,—or, again, an improperly primitive order, in which the forms themselves are properly primitive, and their contravariants improperly primitive. Further, forms of an even discriminant may have characters with respect to 4 or 8; and a complete enumeration of these supplementary characters requires a careful distinction of cases. To facilitate this enu-

meration, a Table is given in the Paper for finding the supplementary characters of any proposed form.

2. A Table is also given for forming the complete generic character of any proposed form. This Table is intended to serve the same purposes, in the theory of ternary quadratic forms, for which the Table of Lejeune Dirichlet is available in the binary theory (Crelle, vol. xix. p. 338). The Table, like that of Lejeune Dirichlet, distinguishes between the possible and impossible generic characters; and the Paper contains a complete demonstration of the criterion by which they are distinguished.

3. Besides the particular characters relating to uneven primes dividing the discriminant, it is convenient, in those cases in which there is no supplementary character, to consider a certain particular generic character which does not appear to have been regarded as such by Eisenstein. This character is termed in the Paper the simultaneous character of the form and its contravariant: its existence is demonstrated; and its introduction as an element of the complete generic character is justified by its use in the distinction of possible and impossible genera.

4. It has been proposed to define a genus of forms as consisting of all those forms which can be transformed into one another by substitutions of which the coefficients are rational and the determinant a unit. It is desirable (in the case of quadratic forms) to add to this definition the limitation that the denominators of the fractional coefficients are to be uneven, and prime to the discriminant. And it is shown, in this Paper, that two ternary quadratic forms are or are not transformable into one another by such substitutions, according as their complete generic characters do or do not coincide.

5. The preceding observations apply equally to the cases of definite and indefinite forms. These two cases are included in the same analysis by means of a convention as to the signs of the two numbers defined by Eisenstein, and termed in this Paper the arithmetical invariants of the ternary form. The first invariant of a form is the greatest common divisor of the first minors of the matrix of the form; the second invariant is the quotient obtained by dividing the discriminant by the square of the first invariant. According to the convention adopted in the Paper, the second invariant has the same sign as the discriminant; and the first invariant has or has not the same sign as the second, according as the form is definite or indefinite.

6. The latter part of the Paper is occupied exclusively with the theory of definite and positive forms. In the case of these forms, the weight (or, as Eisenstein has termed it, the density) of a class is the reciprocal of the number of automorphics (of determinant $+1$) of any form of the class; the weight of a representation of a number by a form is the weight of the form, *i. e.* the weight of the class containing the form; the weight of a genus or order is the sum of the weights of the classes comprised in the genus or order. In his Memoir, Eisenstein has given (but without demon-

stration) the formulæ which assign the weight of a given genus or order of forms of an uneven discriminant. These formulæ are demonstrated in the present Paper, and, with them, the corresponding formulæ relating to the cases in which the discriminant is even. The demonstration is obtained by a method similar to that employed by Gauss and Dirichlet for the determination of the number of binary classes of a given determinant. The sum of the weights of the representations, by a system of forms representing the classes of any proposed genus, of all the numbers contained in certain arithmetical progressions, and not surpassing a given number, is in a finite ratio to the sesquiplicate power of the given number when that number is supposed to increase without limit. Of this limiting ratio, two distinct determinations are obtained; of which the first contains, as a factor, the weight of the proposed genus; and an expression for that weight is obtained by a comparison of the two determinations. Of these determinations, the first is obtained immediately by an elementary application of the integral calculus; the second depends on an arithmetical theorem, which is deduced in the Paper from the analysis employed by Gauss in arts. 279–284 of the 'Disquisitiones Arithmeticæ,' and which may be expressed as follows:—

"The sum of the weights of the representations of a given number (contained in one of certain Arithmetical Progressions) by a system of forms representing the classes of a ternary genus, is equal to the weight of a genus of binary forms, of which the determinant is the product, taken negatively, of the given number by the second invariant of the ternary forms."

By this proposition the determination of the limiting ratio is made to depend on an approximate determination of the weight (or, which is here the same thing, the number) of the binary classes of certain series of negative determinants. Two methods are given in the Paper for effecting this approximate determination. The first method presupposes Lagrange's definition of a reduced form, and depends ultimately on the evaluation of the definite integral

$$\iiint (xz-y^2)\, dx\, dy\, dz = \frac{\pi}{g},$$

of which the limits are given by the inequalities

$$x \geqq 0, \quad y \geqq 0, \quad z \geqq 0, \quad x \leqq z, \quad 2y \leqq x.$$

The second method employs the expression obtained by Lejeune Dirichlet in the form of an infinite series for the number of binary classes of a given determinant, and is thus independent of the definition of a reduced form. The same result is obtained by both methods; but the second is more easily extended to the case of quadratic forms containing more than three indeterminates.

March 7, 1867.

WILLIAM BOWMAN, Esq., Vice-President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Patrick Adie, Esq.
Alexander Armstrong, M.D.
William Baird, M.D.
John Ball, Esq.
Henry Charlton Bastian, M.D.
Samuel Brown, Esq.
Francis T. Buckland, Esq.
Lieut.-Colonel John Cameron, R.E.
Frederick Le Gros Clark, Esq.
Professor Robert Bellamy Clifton.
Joseph Barnard Davis, M.D.
W. Boyd Dawkins, Esq.
Henry Dircks, Esq.
Baldwin Francis Duppa, Esq.
William Esson, Esq.
Alexander Fleming, M.D.
Peter Le Neve Foster, Esq.
Sir Charles Fox.
Edward Headlam Greenhow, M.D.
Albert C. L. G. Günther, M.D.
Julius Haast, Esq., Ph.D.
Captain Robert Wolseley Haig, R.A.
Daniel Hanbury, Esq.
Augustus George Vernon Harcourt, Esq.
Edmund Thomas Higgins, Esq.
Jabez Hogg, Esq.,
John Whitaker Hulke, Esq.
Edward Hull, Esq.
James Jago, M.D.
Professor Thomas Hayter Lewis.
Edward Joseph Lowe, Esq.
David Macloughlin, M.D.
Professor Nevil Story Maskelyne.
John Matthew, Esq.
George Matthey, Esq.
James Robert Napier, Esq.
Thomas Nunneley, Esq.
Admiral Erasmus Ommaney.
James Bell Pettigrew, M.D.
Charles Bland Radcliffe, M.D.
John Russell Reynolds, M.D.
Benjamin Ward Richardson, M.D.
J. S. Burdon Sanderson, M.D.
Edward Henry Sieveking, M.D.
Henry T. Stainton, Esq.
James Startin, Esq.
John Stewart, Esq.
Major James Francis Tennant, R.E.
Colonel Henry Edward Landor Thuillier, R.A.
Charles Tomlinson, Esq.
Rev. Henry Baker Tristram.
Cromwell Fleetwood Varley, Esq.
William Sandys Wright Vaux, Esq.
Edward Walker, Esq.
Professor J. Alfred Wanklyn.
Edward John Waring, M.D.
Henry Wilde, Esq.
Samuel Wilks, M.D.
Professor William Parkinson Wilson.
James Young, Esq.
Colonel Henry Yule, R.E.

"On the Influence exerted by the Movements of Respiration on the Circulation of the Blood." Being the CROONIAN LECTURE for 1867, delivered by Dr. J. BURDON SANDERSON.

(Abstract).

The purpose of the lecture was to show that the explanation usually given by physiologists of the mode in which the respiratory movements of the thorax influence the force and frequency of the contractions of the heart can no longer be entertained.

The doctrine usually taught in this and other countries is stated as follows in one of the most recent text-books:—"During the act of expiration the frequency of the pulse is considerably augmented, whilst the line of mean pressure rapidly rises, indicating increased tension in the arterial walls. . . . . . . During the act of inspiration, on the contrary, the pulsation becomes slower, the curves much bolder, and the line of mean pressure gradually falls; for then the blood readily enters the thorax, and, as a consequence, the great veins, capillaries, and arterial walls become comparatively flaccid" (Carpenter's 'Physiology,' 1864, p. 345). Statements to the same effect are to be found in Budge's 'Lehrbuch der Physiologie,' 1862, p. 350; in Kirke's 'Handbook of Physiology,' 1863, p. 129; in Ludwig's 'Lehrbuch,' 1857, vol. ii. pp. 161, 162.

From numerous experiments, in which the respiratory movements and the variation of pressure in the arteries in the dog were recorded simultaneously by mechanical means, the author had arrived at an opposite conclusion, viz. that in natural breathing each expansion of the chest is followed by increase of arterial tension and shortening of the diastolic interval; in other words, that the immediate effect of inspiration is to increase both the force and frequency of the contractions of the heart.

The *experimental method* was as follows:—For the purpose of recording the movement of air in and out of the chest, the animal is caused to breathe through a T-shaped tube, one arm of which is connected with the trachea, while the other remains open. By the stem it communicates with a disk-shaped bag of thin caoutchouc. The resistance afforded to the ingress and egress of air by the tube, although very inconsiderable, is yet sufficient to produce alternate movements of expansion and collapse of the bag. The variations of arterial pressure are measured by a mercurial manometer, differing from that of Poiseuille, in that the attached arm, which is the longer of the two, is of much smaller diameter than the other, the area of the latter being twelve times as great as that of the former. For the purpose of recording the movements of the dynamometer and of the caoutchouc bag, two light wooden levers of the third kind, each 25 inches in length, are used. These work on steel axes, the bearings of which are so contrived that the axis of the arterial lever is directly above that of the respiratory lever, and that both oscillate in the same vertical plane: by vertical rods they are connected, the upper or arterial lever with a cork float which rests on the surface of the mercury in the wide arm of the dynamometer, the lower with the upper surface of the caoutchouc bag. At their extremities they carry fine sable brushes, by which their movements are inscribed on a roll of paper, to which a horizontal movement is communicated by clockwork. By a mechanical arrangement, which need not be here described, synchronical points can from time to time be marked in the two tracings inscribed simultaneously on the paper by the momentary withdrawal of both brushes. The experiments were of the following nature, dogs being employed throughout.

1. *Experiments as to normal respiration.*—In these experiments (eleven in number) the dynamometer was connected with the femoral artery, while the breathing-tube was connected with the respiratory cavity, either by the trachea, or by means of a mask fixed over the snout. The principal results were as follows:—

Experiment 1.—Respirations per minute, 9; pulsations, 108. Mean arterial pressure 6·2 inches. The tracings show that each respiratory act is divisible into two parts; two-fifths being occupied by thoracic movements, the remainder by the pause. Of the former, two-thirds correspond to inspiration, one-third to expiration. During the pause the arterial pressure gradually sinks, the commencement of inspiration being immediately followed by an increase of pressure, which becomes still more marked during expiration, but again subsides at its completion. The interval between each two succeeding contractions of the heart is seen to be three times as great in those pulsations which immediately *follow* expiration as in those which precede it.

The other experiments of the series were of a similar nature. In some the relative length of the respiratory intervals and the regularity of the pulsations rendered it more easy to judge of the precise relation between the two tracings than in others, but in all the results were in complete accordance with those above stated. Even when the frequency of breathing was such that three pulsations corresponded to one respiration (experiment 4), it was observed that the diastolic interval which immediately followed expiration was twice as long as either of the other two. In one case the respiratory tracing showed that the mode of breathing was peculiar: inspiration was separated from expiration by a pause of considerable duration, during which the arterial pressure declined and the pulse was retarded.

2. *Experiments for the purpose of determining whether the resistance afforded by the* T-*tube to the passage of air in and out of the chest exercise any modifying influence on the results.*—It was obvious that this end could be best attained by observing the effect of increasing the resistance; for by so doing, any modifying influence exercised by it would become more obvious. With this view a series of observations were made on the same animal (under the influence of morphia), in which the resistance was gradually increased by inserting plugs of various sizes into the aperture of the T-tube. The tracings showed that even when the aperture was so diminished

as to produce marked dyspnœa, and great exaggeration of the movements of respiration, it was observed as distinctly as before that the increase of force and frequency of the pulse were increased by the prolonged inspiratory efforts of the animal.

3. *Experiments showing that when the respiratory cavity is completely closed (as by plugging the trachea), the relation between the respiratory movements of the chest and the arterial pressure is reversed.*—The process of death by apnœa may be divided into two stages; the first extending from the moment of occlusion to the cessation of the struggles of the animal and the supervention of apparent insensibility, the second terminating with the extinction of the circulation. In order to observe the characters of the respiratory movements and those of the heart during these two stages, it was necessary to substitute a mercurial manometer for the caoutchouc bag. It was then seen that at first the respiratory movements increase in amplitude without altering in character; but towards the end of the first minute, when the animal begins to struggle, they become irregular, and each struggle is accompanied by strong *expulsive* efforts, during which the mercury in the dynamometer oscillates violently and rises to an enormous height. At the commencement of the second stage, when the animal becomes tranquil, the respiratory movements assume a different character, become almost exclusively inspiratory (gasping), and much more regular. They occur, however, at longer and longer intervals, until they finally cease. As regards the relation between the oscillations of the two manometers, the tracings show distinctly that throughout the whole process they are strictly coincident, both as to the *time* of their occurrence and their extent. Hence it may be concluded that the extraordinary elevation of arterial pressure which has been long known to occur during the second minute in death by apnœa, is not due, as was supposed by Dr. Alison and Dr. John Reid, to obstruction of the capillary vessels, either pulmonary or systemic, but to the violence of the respiratory efforts. The cavity of the chest being closed, the force exercised by the respiratory muscles expresses itself in variations of tension of the enclosed air, which are communicated through the intra-thoracic arteries to those outside of the chest, producing those violent oscillations of the dynamometer which have been referred to.

In support of this inference, it was shown that in an animal under the influence of woorara (when all respiratory movement ceases, while those of the heart are unaffected), the process of apnœa is not only of greater duration, but is not attended with any of those peculiar disturbances of the circulation which have been hitherto attributed to capillary obstruction. The gradual extinction of the force of the contraction of the heart is indicated by a slow and uninterrupted subsidence of the arterial pressure.

4. *Experiments for the purpose of ascertaining in how far the influence exercised by the respiratory movements on the heart in ordinary breathing are chemical.*—For this purpose observations were made on animals which

had been allowed to respire a limited quantity of air (50–100 cubic inches) for a sufficiently long time to ensure the complete cessation of all appreciable reaction of its oxygen on the circulating blood. In this form of apnœa insensibility is not produced until from ten to fifteen minutes after the experiment. As in ordinary suffocation, it is associated with a marked change in the mode of breathing. All expiratory efforts cease, and the animal respires by gasps, each of which is separated from its successor by a pause of variable duration. Under these circumstances, when unquestionably all chemical reaction is out of the question, the effect observed is of the same nature as in ordinary breathing, the only difference being that, in consequence of the length of the intervals and the absence of expiratory effort, it is much more obvious. The moment after inspiration commences, the mercurial column is jerked up by a succession of forcible contractions of the heart.

5. *Experiments showing that in artificial respiration, when the mechanism is reversed, the chemical conditions remaining the same, the mechanical effect is correspondingly modified; and that if the blood is venous, a chemical effect is produced by each injection of air into the lungs, which, although of the same nature, requires a much longer time for its production.*—If, in an animal under the influence of woorara, artificial respiration be discontinued until the arterial pressure sinks several inches, and then air is injected, even in small quantity, no immediate effect is observed excepting a momentary increase of arterial pressure coincident in time with the expansion of the lungs; but after the lapse of six or seven seconds, the heart begins to beat with extreme frequency, rapidly raising the mercurial column until a pressure is attained equal or superior to that originally existing. The length of the time which intervenes between this event and its antecedent is in itself sufficient to show that the relation between the two cannot be mechanical. This is proved by the observation that, if hydrogen be substituted for air in the experiments, no effect is produced.

6. *Experiments showing the relation between the thoracic movements and the arterial pressure after section of the pneumogastric nerves.*—Section of the pneumogastric nerves in the neck, besides its well-known effect in retarding the breathing and accelerating the contractions of the heart, alters the mode of the respiratory movements. The chest is unnaturally dilated even during the pause. Inspiration is performed slowly and with effort, and terminates in a sudden expiratory collapse. The heart not only contracts more frequently, but more forcibly, the arterial pressure rising several inches of mercury. Under these conditions it is observed (1) that the arterial pressure tends to increase during the slow inspiration, and to decline during the pause, and (2) that a more rapid increase of tension occurs simultaneously with expiration. But (3) no variation is observed of the frequency of the pulsations; and (4) all the effects are much less marked than in the normal animal. These peculiarities are to be attributed to the extreme rapidity of the heart's action, to the permanent

distension of the thoracic veins, and to the violence of the expiratory movements.

Theoretical exposition of the mechanical influence of the respiratory movements on the circulation.—(1) It has been demonstrated by Donders that the elastic contents of the chest have at all times a tendency to shrink to a smaller bulk than that of the cavity in which they are contained, so that the viscera within the thorax are constantly distended in a degree which varies according to its ever-varying capacity. As, however, they are not equally elastic, they yield to this distension unequally. When the chest enlarges, the lungs yield most, the veins and heart, in a state of relaxation, next; the contracting heart and the arteries scarcely expand at all. (2) If the veins contained air and communicated with the atmosphere, they would fill as rapidly as the lungs; actually their expansion is much slower. Hence the first effect of inspiration is to increase the proportion of thoracic space occupied by the lungs, by which they become relatively more distended than the other organs. So soon, however, as the veins and auricles have time to fill, equilibrium is more or less restored. (3) Hence it follows (*a*) that the dilatation of the chest in inspiration aids the expansion of the heart during diastole and of the thoracic veins; and (*b*) that these events cannot occur simultaneously with their cause, but must follow at an interval varying according to the condition of the circulation. (4) Other things being equal, the force and frequency of the contractions of the heart are increased by whatever causes accelerate its diastolic impletion. The more rapidly the cavities fill the shorter must be its period of relaxation, the more vigorous its systole, and consequently the greater the arterial pressure. (5) The effect of thoracic expansion on the intra-thoracic veins varies both as regards its degree and the time of its occurrence. Both kinds of variation depend on the velocity of the circulation and the pressure existing in the veins outside of the chest. When the systemic veins are distended, the circulation rapid, and the arterial resistance in consequence diminished, the heart almost empties itself at each contraction, and the expansion of the chest fills the thoracic veins and the relaxed heart with great rapidity. In the opposite case, when the systemic veins are comparatively empty, the cavities of the heart fill slowly, and discharge themselves imperfectly on account of the excessive arterial resistance. (6) Hence *the effect of inspiration* in facilitating the diastolic impletion of the auricles, and consequently *in increasing the frequency and force of the heart's action, varies directly as the velocity of the circulation, inversely as the arterial pressure.*

Conclusions.—1. In natural breathing the influence exercised by the thoracic movements on the heart is entirely mechanical. So long as the respiration is tranquil, variations of air-pressure in the bronchial tubes and vesicles of the lungs do not materially affect the arterial pressure; but violent expiratory movements are accompanied by simultaneous increase of vascular tension.

2. When the respiratory orifices are closed, the variations of blood-pressure in the arteries are synchronical with those of air-pressure in the respiratory cavity, and take place in the same direction.

3. The increased action of the heart which results from chemical changes produced in the circulating fluid by exposure to air, is of the same nature as the mechanical effect of inspiration, both being indicated by increased arterial tension and acceleration of the pulse. The former may be distinguished from the latter (*a*) by the length of time required for the production of the effect, and (*b*) by its dependence on a previous venous condition of the blood.

4. Hence the influence of the thoracic movements on those of the heart may be either directly mechanical, as in suffocation, indirectly mechanical, as in ordinary breathing, or chemical.

March 14, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "Note on Mr. Merrifield's New Method of calculating the Statical Stability of a Ship." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S. Received February 22, 1867.

On the 24th of January, 1867, a paper was read to the Royal Society by Mr. C. W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture, showing how, by determining the radii of curvature of the locus of the centre of buoyancy or "metacentric involute" of a ship in an upright position and at one given angle of inclination, a formula may be obtained for calculating to a close approximation her moment of stability at any given angle of inclination, on the assumption that the metacentric involute can be sufficiently represented by a conic.

It has occurred to me that the latter part of the calculation in Mr. Merrifield's method might be simplified by assuming for the approximate form of the metacentric involute, not a conic, but the *involute of the involute of a circle*; the locus of its centres of curvature, or "metacentric evolute," being assumed to be the involute of a circle.

The involute of the involute of a circle is distinguished by the following property. Let r be the radius of the circle, ρ_0 that radius of curvature of the involute of the involute which touches the involute at its cusp, and ρ another radius of curvature of the same curve making the angle θ with the radius ρ_0; then

$$\rho=\rho_0+\frac{r\theta^2}{2}. \quad \dots\dots\dots \quad (1)$$

Having found, then, the radii of curvature of the metacentric involute in an upright position, and at a given angle of inclination θ_1, let ρ_0 and ρ_1 be those radii respectively; then make

$$r=\frac{2(\rho_1-\rho_0)}{\theta_1^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This will be the radius of the required circle; and its positive or negative sign will show whether it is to be laid off downwards or upwards from the metacentre. For any given angle of inclination the radius of curvature of the metacentric involute will be given by equation (1), which may also be put in the following form:

$$\rho=\rho_0+(\rho_1-\rho_0)\frac{\theta^2}{\theta_1^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let δ be the depth of the ship's centre of gravity below her metacentre, and p the perpendicular let fall from that centre of gravity upon the radius of curvature of the metacentric involute at any given angle of inclination θ; then

$$p=(\delta-r)\sin\theta+r\theta; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and the moment of stability is

$$p\times \text{displacement}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It is obvious that the condition of *isochronous rolling* is that $\delta-r=0$; that is to say, that the centre of the circle which is the evolute of the metacentric evolute shall coincide with the ship's centre of gravity; a proposition already demonstrated by me in a paper read to the Institution of Naval Architects in 1864, and published in their Transactions, vol. v. p. 35.

[*Postscript*.—Received March 11, 1867.]

Since the above was written, I have been informed by Mr. Merrifield, to whom I had communicated my proposed modification of his method, that it has been tried at the Royal School of Naval Architecture and found to answer well.

II. "On the Theory of the Maintenance of Electric Currents by Mechanical Work without the use of Permanent Magnets." By J. Clerk Maxwell, F.R.S. Received February 28, 1867.

The machines lately brought before the Royal Society by Mr. Siemens and Professor Wheatstone consist essentially of a fixed and a moveable electromagnet, the coils of which are put in connexion by means of a commutator.

The electromagnets in the actual machines have cores of soft iron, which greatly increase the magnetic effects due to the coils; but, in order to simplify the expression of the theory as much as possible, I shall begin by supposing the coils to have no cores, and, to fix our ideas, we may suppose them in the form of rings, the smaller revolving within the larger on a common diameter.

The equations of the currents in two neighbouring circuits are given in my paper "On the Electromagnetic Field"*, and are there numbered (4) and (5),

$$\xi = \mathrm{R}x + \frac{d}{dt}(\mathrm{L}x + \mathrm{M}y),$$

$$\eta = \mathrm{S}y + \frac{d}{dt}(\mathrm{M}x + \mathrm{N}y),$$

where x and y are the currents, ξ and η the electromotive forces, and R and S the resistances in the two circuits respectively. L and N are the coefficients of self-induction of the two circuits, that is, their potentials on themselves when the current is unity, and M is their coefficient of mutual induction, which depends on their relative position. In the electromagnetic system of measurement, L, M, and N are of the nature of lines; and R and S are velocities. L may be metaphorically called the "electric inertia" of the first circuit, N that of the second, and L+2M+N that of the combined circuit.

Let us first take the case of the two circuits thrown into one, and the two coils relatively at rest, so that M is constant. Then

$$(\mathrm{R}+\mathrm{S})x + \frac{d}{dt}(\mathrm{L}+2\mathrm{M}+\mathrm{N})x = 0, \quad \ldots \ldots \quad (1)$$

whence

$$x = x_0 e^{-\frac{\mathrm{R}+\mathrm{S}}{\mathrm{L}+2\mathrm{M}+\mathrm{N}}t}, \quad \ldots \ldots \ldots \ldots \quad (2)$$

where x_0 is the initial value of the current. This expression shows that the current, if left to itself in a closed circuit, will gradually decay.

If we put

$$\frac{\mathrm{L}+2\mathrm{M}+\mathrm{N}}{\mathrm{R}+\mathrm{S}} = \tau, \quad \ldots \ldots \ldots \quad (3)$$

then

$$x = x_0 e^{-\frac{t}{\tau}}. \quad \ldots \ldots \ldots \ldots \quad (4)$$

The value of the time τ depends on the nature of the coils. In coils of similar outward form, τ varies as the square of the linear dimension, and inversely as the resistance of unit of length of a wire whose section is the sum of the sections of the wires passing through unit of section of the coil.

In the large experimental coil used in determining the B.A. unit of resistance in 1864, τ was about ·01 second. In the coils of electromagnets τ is much greater, and when an iron core is inserted there is a still greater increase.

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* Phil. Trans. 1865, p. 469.

Let us next ascertain the effect of a sudden change of position in the secondary coil, which alters the value of M from M_1 to M_2 in a time t_2-t_1, during which the current changes from x_1 to x_2. Integrating equation (1) with respect to t, we get

$$(R+S)\int_{t_1}^{t_2} x\,dt+(L+2M_2+N)x_2-(L+2M_1+N)x_1=0. \quad . \quad (5)$$

If we suppose the time so short that we may neglect the first term in comparison with the others, we find, as the effect of a *sudden* change of position,

$$(L+2M_2+N)x_2=(L+2M_1+N)x_1. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This equation may be interpreted in the language of the dynamical theory, by saying that the electromagnetic momentum of the circuit remains the same after a sudden change of position. To ascertain the effect of the commutator, let us suppose that, at a given instant, currents x and y exist in the two coils, that the two coils are then made into one circuit, and that x' is the current in the circuit the instant after completion; then the same equation (1) gives

$$(L+2M+N)x'=(L+M)x+(N+M)y. \quad . \quad . \quad . \quad . \quad (7)$$

This equation shows that the electromagnetic momentum of the completed circuit is equal to the sum of the electromagnetic momenta of the separate coils just before completion.

The commutator may belong to one of four different varieties, according to the order in which the contacts are made and broken. If A, B be the ends of the first coil, and C, D those of the second, and if we enclose in brackets the parts in electric connexion, we may express the four varieties as in the following Table :—

(1)	(2)	(3)	(4)
(AC) (BD)	(AC) (BD)	(AC) (BD)	(AC) (BD)
(ABCD)	(ABC) (D)	(A) (BCD)	(A) (B) (C) (D)
(AD) (BC)	(ABCD)	(ABCD)	(AD) (BC)
	(AD) (BC)	(AD) (BC)	

In the first kind the circuit of both coils remains uninterrupted; and when the operation is complete, two equal currents in opposite directions are combined into one. In this case, therefore, $y=-x$, and

$$(L+2M+N)x'=(L-N)x. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

When there are iron cores in the coils, or metallic circuits in which independent currents can be excited, the electrical equations are much more complicated, and contain as many independent variables as there can be independent electromagnetic quantities. I shall therefore, for the sake of preserving simplicity, avoid the consideration of the iron cores, except in so far as they simply increase the values of L, M, and N.

I shall also suppose that the secondary coil is at first in a position in which $M=0$, and that it turns into a position in which $M=-M$, which will increase the current in the ratio of $L+N$ to $L-2M+N$.

The commutator is then reversed. This will diminish the current in a ratio depending on the kind of commutator.

The secondary coil is then moved so that M changes from M to 0, which will increase the current in the ratio of $L+2M+N$ to $L+N$.

During the whole motion the current has also been decaying at a rate which varies according to the value of $L+2M+N$; but since M varies from $+M$ to $-M$, we may, in a rough theory, suppose that in the expression for the decay of the current $M=0$.

If the secondary coil makes a semirevolution in time T, then the ratio of the current x_1, after a semirevolution to the current x_0 before the semirevolution, will be

$$\frac{x_1}{x_0}=e^{\frac{T}{\tau}}\,r,$$

where

$$\tau=\frac{L+N}{R+S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and r is a ratio depending on the kind of commutator.

For the first kind,

$$r=\frac{L-N}{L-2M+N}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

By increasing the speed, T may be indefinitely diminished, so that the question of the maintenance of the current depends ultimately on whether r is greater or less than unity. When r is greater than 1 or less than -1, the current may be maintained by giving a sufficient speed to the machine; it will be always in one direction in the first case, and it will be a reciprocating current in the second.

When r lies between $+1$ and -1, no current can be maintained.

Let there be p windings of wire in the first coil and q windings in the second, then we may write

$$L=lp^2, \quad M=mpq, \quad N=nq^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

where l, m, n are quantities depending on the shape and relative position of the coils. Since $L-2M+N$ must always be a positive quantity, being the coefficient of self-induction of the whole circuit, $ln-m^2$, and therefore $LN-M$ must be positive. When the commutator is of the first kind, the ratio r is greater than unity, provided pm is greater than qn; and when

$$\frac{p}{q}=\frac{n}{m}\left(1+\sqrt{1-\frac{m^2}{ln}}\right),$$

$$r=\left(1-\frac{m^2}{ln}\right)^{-\frac{1}{2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

which is the maximum value of r.

When the ratio of p to q lies between that of n to m and that of m to l, r lies between $+1$ and -1, and the current must decay; but when pl is less than qm, a reciprocating current may be kept up, and will increase most rapidly when

$$\frac{p}{q}=\frac{n}{m}\left(1-\sqrt{1-\frac{m^2}{ln}}\right),$$

and

$$r=-\left(1-\frac{m^2}{ln}\right)^{-\frac{1}{2}}. \quad \ldots \ldots \ldots \quad (13)$$

When the commutator is of the second kind, the first step is to close both circuits, so as to render the currents in them independent. The second circuit is then broken, and the current in it is thus stopped. This produces an effect on the first circuit by induction determined by the equation

$$\mathrm{L}x+\mathrm{M}y=\mathrm{L}x'+\mathrm{M}y'. \quad \ldots \ldots \ldots \quad (14)$$

In this case $\mathrm{M}=-\mathrm{M}_0$, $y=x$, and $y'=0$, so that

$$(\mathrm{L}-\mathrm{M})x=\mathrm{L}x'; \quad \ldots \ldots \ldots \quad (15)$$

where x is the original, and x' the new value of the current.

The next step is to throw the circuits into one, M being now positive. If x'' be the current after this operation,

$$(\mathrm{L}+\mathrm{M})x'=(\mathrm{L}+2\mathrm{M}+\mathrm{N})x''. \quad \ldots \ldots \quad (16)$$

The whole effect of this commutator is therefore to multiply the current by the ratio

$$\frac{\mathrm{L}^2-\mathrm{M}^2}{\mathrm{L}(\mathrm{L}+2\mathrm{M}+\mathrm{N})}.$$

The whole effect of the semirotation is to multiply the current by the ratio

$$\frac{\mathrm{L}+2\mathrm{M}+\mathrm{N}}{\mathrm{L}-2\mathrm{M}+\mathrm{N}}.$$

The total effect of a semirevolution supposed instantaneous is to multiply the current by the ratio

$$r=\frac{\mathrm{L}^2-\mathrm{M}^2}{\mathrm{L}(\mathrm{L}-2\mathrm{M}+\mathrm{N})}.$$

If p and q be the number of windings in the first and second coils respectively, the ratio r becomes

$$r=\frac{l^2p^2-m^2q^2}{l(lp^2-2mpq+nq^2)};$$

which is greater than 1, provided $2lmp$ is greater than $(ln+m^2)q$. When

$$\frac{p}{q}=\frac{1}{2}\left(\frac{n}{m}+\frac{m}{l}\right)+\frac{1}{2}\sqrt{\frac{n^2}{m}+2\frac{n}{l}-3\frac{m^2}{l^2}},$$

we have for the maximum value of r,

$$r=1+\frac{2\frac{m}{l}}{\sqrt{\frac{n^2}{m^2}+2\frac{n}{l}-3\frac{m^2}{l^2}+\frac{n}{m}-\frac{m}{l}}}.$$

In the experiment of Professor Wheatstone, in which the ends of the primary coil were put in permanent connexion by a short wire, the equations are more complicated, as we have three currents instead of two to consider. The equations are

$$\mathrm{R}x+\frac{d}{dt}(\mathrm{L}x+\mathrm{M}y)=\mathrm{S}y+\frac{d}{dt}(\mathrm{M}x+\mathrm{N}y)=\mathrm{Q}z+\frac{d}{dt}\mathrm{K}z, \quad . \quad (17)$$

$$x+y+z=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

where Q, K, and z are the resistance, self-induction, and current in the short wire. The resultant equations are of the second degree; but as they are only true when the magnetism of the cores is considered rigidly connected with the currents in the coils, an elaborate discussion of them would be out of place in what professes to be only a rough explanation of the theory of the experiments.

Such a rough explanation appears to me to be as follows:—

Without the shunt, the current in the secondary coil is always in rigid connexion with that in the primary coil, except when the commutator is changing. With the shunt, the two currents are in some degree independent; and the secondary coil, whose electric inertia is small compared with that of the primary, can have its current reversed and varied without being clogged by the sluggish primary coil.

On the other hand, the primary coil loses that part of the total current which passes through the shunt; but we know that an iron core, when highly magnetized, requires a great increase of current to increase its magnetism, whereas its magnetism can be maintained at a considerable value by a current much less powerful. In this way the diminution in resistance and self-induction due to the shunt may more than counterbalance the diminution of strength in the primary magnet.

Also, since the self-induction of the shunt is very small, all instantaneous currents will run through it rather than through the electromagnetic coils, and therefore it will receive more of the heating effect of variable currents than a comparison of the resistances alone would lead us to expect.

III. "On certain Points in the Theory of the Magneto-electric Machines of Wilde, Wheatstone, and Siemens." By C. F. Varley, Esq. In a Letter to Professor Stokes, Sec. R.S. Received February 26, 1867.

Fleetwood House, Beckenham, S.E.,
February 23, 1867.

My dear Sir,—Professor Wheatstone showed that a shunt put into the circuit of the electromagnet increased the power greatly, but the explanation that it increased the power by equalizing the resistance of the armature and that of the electromagnet is either wholly incorrect, or very nearly so.

Yesterday I had an opportunity afforded me by Mr. C. Siemens of experimenting with his machine, in which the electromagnets have each a resistance of about 250 Ohms = 500 Ohms, the armature 400 Ohms.

On adding a shunt to the electromagnet the flame was greatly increased.

The two electromagnets when connected in series had a resistance of about 500 Ohms. I then connected them in a double circuit, the resistance in this case being about 125 Ohms. By this means the same result as regards resistance could be obtained as by a shunt, with the difference that the power expended in the shunt is lost in heat; while reducing the resistance by the double circuit caused the whole force to be expended on the electromagnet.

The results of the experiment were—

1st. The shunt invariably *increased* the power.

2ndly. When the magnets were joined in double circuit the power was greatly *reduced.*

The explanation is to me obvious. In a Ruhmkorff's coil, where the iron core is divided into fine wire, so that the dying magnetism cannot set up currents in the iron core to prolong its existence, the magnetism is very rapidly lost, and the make-and-break hammer works very rapidly, sometimes as fast as sixty beats per second.

If the secondary circuit be closed so that the currents can flow, the make-and-break hammer works very slowly, indeed one or two beats per second; and in 1856 I published a description of electromagnets whose action was very slow, and which were rendered sluggish by a copper cylinder around them.

Wilde's armature, when revolving, sends intermittent currents around the electromagnet, whose circuit is broken at every half revolution of the armature.

Were the magnets composed of fine iron wire, the magnetism would die away rapidly, producing a violent current by its efforts to maintain itself, as in the Ruhmkorff's coils. (This current is called by foreigners the extra-current.)

The shunt which Wheatstone inserted carries this current across, and so maintains the magnetism of the electromagnets until the armature gives a second impulse. The current in this shunt will be found to travel in alternate directions; not so that on the electromagnet.

When the armature is discharging its current into the electromagnets, the current in the shunt is in the direction it would have if the shunt were in circuit solely with the armature.

When the armature is changing poles and is disconnected, the secondary current is in full play, and the current in the shunt is in the direction of the current prolonged in the electromagnet, that is, of the extra current.

The force expended in the shunt is wasted in heat; but a secondary wire on the electromagnet or a copper cylinder would very greatly add to the power by maintaining the magnetism, and not consume uselessly the force now wasted in the shunt.

The overlapping of the armature and the solid mass of the electromagnets tends to maintain imperfectly the magnetism during the intervals of *no* current from the armature; and but for this the machines, whether they be Wilde's, Wheatstone's, or Siemens's, would none of them work.

In 1860 I published a description of two machines I had constructed, and in 1862, at the Universal Exhibition, I exhibited a machine for adding mechanical force to static electricity without friction. A machine similar in principle, but a little different in construction, has been exhibited recently under the name of Holtz.

One of my machines bears to the other precisely the same relation that Siemens's or Wheatstone's does to Wilde's.

If these be of sufficient interest to the Royal Society, I shall be happy to exhibit them.

I am, my dear Sir,
Very truly yours,
C. F. Varley.

IV. "On a Magneto-electric Machine." By William Ladd, F.R.M.S. Communicated by Professor Stokes, Sec. R.S. Received March 14, 1867.

In June 1864 I received from Mr. Wilde a small magneto-electric machine, consisting of a Siemens's armature and six magnets. This I endeavoured to improve upon, my object being to get a cheap machine for blasting with Abel's fusees. This was done by making one of circular magnets, and a Siemens's armature revolving directly between the poles, the armature forming the circles; with this I could send a very considerable power into an electro-magnet, &c. It was then suggested to me by my assistant, that if the armature had two wires instead of one, the current from one being sent through a wire surrounding the magnets, their power would be augmented, and a considerable current might be obtained from the other wire available for external work; or there might be two armatures, one to exalt the power of the magnets, and the other made available for blasting or other purposes. Want of time prevented me carrying this out until now; but since the interesting papers of C. W. Siemens, F.R.S., and Professor Wheatstone, F.R.S., were read last month, I have carried out the idea as follows:—Two bars of soft iron, measuring $7\frac{1}{2}$ in. × $2\frac{1}{2}$ in. × $\frac{1}{2}$ in., are each wound, round the centre portions, with about thirty yards of No. 10 copper wire; and shoes of soft iron are so attached at each end, that when the bars are placed one above the other there will be a space left between the opposite shoes in which a Siemens's armature can rotate: on each of the armatures is wound about ten yards of No. 14 copper wire cotton-covered. The current generated in one of the armatures is always in connexion with the electro-magnets; and the current from the second armature, being perfectly free, can be used for any purpose for which it may be required. The machine is altogether rudely constructed, and is only intended to illustrate the principle; but with this small machine three inches of platinum wire ·01 can be made incandescent.

March 21, 1867.

Lieut.-General SABINE, President, in the Chair.

Dr. Thomas Sterry Hunt and Dr. Thomas Richardson were admitted into the Society.

The following communications were read:—

I. "Computation of the Lengths of the Waves of Light corresponding to the Lines in the Dispersion-Spectrum measured by Kirchhoff." By GEORGE BIDDELL AIRY, F.R.S., Astronomer Royal. Received March 2, 1867.

(Abstract.)

The author, after adverting to the excellence and importance of the spectral measures made by Professor Kirchhoff, points out that these measures are not available for physical inquiry until we have deduced from them the length of the light-wave corresponding to each line. The author therefore undertook the work of computing the lengths of the light-waves. For this purpose, he referred to Fraunhofer's direct measures of the lengths of the waves corresponding to certain lines, and, ascertaining the numerical measures in Kirchhoff's scale corresponding to the same lines, he expressed Fraunhofer's wave-lengths by an algebraical formula, in which the variable quantity was Kirchhoff's measure. This formula was applied to each of the lines (about 1600 in number). The wave-lengths were at first obtained in parts of the Paris inch; but all were ultimately converted into parts of the millimetre.

The author then adverts to the suspicion of inaccuracy in some parts of these results, arising from the circumstance that Kirchhoff's apparatus was not always in precisely the same state of adjustment. After expressing his own *à priori* belief that the error, if any, must be extremely small, he adverts to the comparison which he was now enabled to make between direct measures of wave-length by Ångström and Ditscheiner, and his own computations. Admitting the systematic errors of Fraunhofer which the later philosophers have indicated, and the errors incidental to interpolation and extrapolation, the remaining discrepance is very small. Its progress is so easy that there is no difficulty in interpolating its value for any one line; and thus, using the computed wave-lengths of this memoir, the wave-length for any line may be found as it would have been measured by Ångström or Ditscheiner.

In the tabular part of the communication, the principal Table contains Kirchhoff's measures and symbols, extracted from the Berlin Memoirs 1861 and 1862, with the addition throughout of one column containing the author's computed wave-lengths expressed in parts of the millimetre. This is followed by a special Table, in the same form, for the lines produced by certain metals not included in the general Table. There is then given a Table of the wave-lengths corresponding to the lines produced by different metals, extracted by the author from the general Table. And finally there are given two Tables containing respectively the comparisons of Ångström's and Ditscheiner's direct measures of wave-lengths with the wave-lengths computed by the author.

II. "On a remarkable Alteration of Appearance and Structure of the Human Hair." By Erasmus Wilson, F.R.S. Received March 12, 1867.

I have the honour of submitting to the Royal Society a specimen of human hair of very remarkable appearance. Every hair is brown and white in alternate bands, looking as if encircled with rings; and this change of aspect extends throughout the whole length of the hair, and gives to the general mass a curiously speckled character. The brown segment of the hair, which represents its normal colour, measures about $\frac{1}{50}$ of an inch in length, or something less than a quarter of a line; the white, or abnormal segment about half that length, namely $\frac{1}{100}$ of an inch; and the two together about $\frac{1}{36}$ of an inch, or one-third of a line.

The hair was taken from a lad aged seven years and a half, a gentleman's son; he is reported as being "an active, healthy boy, quick and intelligent." He was delicate up to the age of four, having suffered in quick succession the diseases of childhood, a severe attack of croup, and several attacks of convulsions. The change in the appearance of the hair was first noticed when he was between two and three years old, and has increased perceptibly during the last two years. There is no similar alteration of structure of the eyebrows and eyelashes. His complexion is dark, while that of a younger brother is fair; and the latter is free from any alteration of the hair.

Examination of the hair with a lens shows that the cylinder of the hair is perfectly uniform, that the white portion is contained within the cuticle and occupies the whole breadth of the cylinder; whilst it frequently presents a rounded cone at the central extremity, and breaks up into fibres at the opposite or distal end; and in some instances this fibrous structure is apparent at both ends of the white segment. Moreover, by transmitted light, the white segment is found to be opake, and consequently presents a dark shade, while the intermediate or brown portion has the transparency of normal hair.

When the transparency of the hair is increased by immersion in Canada balsam slightly diluted with spirits of turpentine, the white and opake segment is reduced in dimensions, and is rendered more or less transparent by imbibition of the volatile fluid; moreover it is clearly demonstrated by this process that the opacity of the segment its whiteness when seen by reflected light, and its darkness by transmitted light, are all due to the presence, in the fibrous portion of the hair, of spaces filled with air-globules. The air-spaces are necessarily very numerous and assembled closely together; while at the ends of the white segment they have more or less of a linear arrangement, and give a fibrous appearance to the opake mass. Moreover the partial transparency of the hair caused by the balsam demonstrates that, besides the air-spaces, large and small, contained in the opake portion, minute air-spaces, sometimes arranged in linear order, and sometimes communicating and forming short irregular canals, are also met with in the transparent part of the hair. And, in addition to the minute air-spaces of the plates of the fibrous portion of the hair, an accumulation of air-globules is also very apparent in the cells of the medulla.

It is evident from this examination of the hairs, that they are imperfect in structure and development, and that their imperfection indicates a weak producing organ, and probably a weakly constitution of the individual; that the cells of which the fibrous portion of the hair is composed, instead of being filled with a horny plasma, are tinged with aqueous fluid, and the desiccation of this fluid leaves behind it vacuities which in the subsequent growth of the shaft become filled with air. The most remarkable phenomenon in connexion with the case, however, is the alternation of imperfect and perfect cells, the period of continuance of the two processes (supposing them to be equally active in point of time) being twice as long for the perfect as for the imperfect structure.

Since the publication of the observations of Berthold in Müller's 'Archiv' for 1850, it is generally believed that the hair grows faster during the day than during the night; hence the first suggestion that occurred to me in connexion with the present case, seeing that the white or opake segment was shorter by one-half than the brown, was that the former represented the slower growth by night, and the latter the quicker growth by day—the white and the brown together representing an entire day of twenty-four hours. But other observations by myself have given, as the average growth of the hair of the head in persons who had been shaved, $\frac{1}{8}$ of an inch for the week, and consequently $\frac{1}{56}$ of an inch for the twenty-four hours. Now the length of hair comprehended by the white and the brown in the present case is $\frac{1}{36}$ of an inch, and consequently a much more active growth than is normally met with—corresponding, in fact, in a similar ratio, with thirty-seven hours instead of twenty-four.

I therefore refrain from speculating upon the cause of alternation of the healthy and morbid structure presented by this case, and restrict myself to the narration of the fact that during a certain space of time, amounting to a day or more, the hair is produced of normal structure, while during another space of time of undetermined extent the hair is produced unhealthily,—that the periods of healthy formation correspond pretty accurately in extent, as do those of unhealthy formation, while the latter, in measurement, are only half as extensive as the former,—moreover, that the differences of the pathological operation are, the production of a horny plasma in the normal process, and of serous and watery cell-contents in the abnormal process.

I may further observe that it is by no means improbable that the "dead" and faded hair which is met with after some illnesses and in instances of debilitated health may be due to a similar pathological process, although wanting in the periodicity and alternation which render the present case so remarkable.

III. "Remarks on the Nature of Electric Energy, and on the Means by which it is transmitted." By Charles Brooke, M.A., F.R.S., P.M.S., &c. Received March 19, 1867.

The writer has clearly shown the interchange of thermic and dynamic energy at the point of junction of the bars of a thermo-electric element of antimony and bismuth*, and he has also pointed out† that the dynamic nature of electric energy is not less clearly indicated by the long-known fact that an ordinary voltaic current always commences with a rush, as it were, the instant that the circuit is closed. The dynamic cause of this is clearly pointed out by an experiment due to the genius of Prof. Wheatstone. If a tuning-fork, the tail of which is inserted longitudinally into a wooden handle, like a file or chisel, be made to vibrate, and the end of the handle rested obliquely on a table, the resonance of the table will instantly be heard; but on moving the diapason parallel to itself in any

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* Phil. Mag. Nov. 1866.

† Ibid. Dec. 1866.

direction on the table, the resonance ceases, from the perpetual interference of the successive planes of vibration with each other. But now comes the illustration:—On arresting the motion of translation, the resonance immediately recommences, but with a rush or momentary increase of sound: this must unquestionably arise from the resistance offered by the inertia of the molecules of wood to the recommencement of wave-motion; and the parallel phenomenon in electricity may undoubtedly be similarly accounted for. And the reflex momentary current (the terminal extra-current of Faraday), which is well known to take place at the instant of opening the circuit, is equally susceptible of a dynamic interpretation: it is the analogue of the wave reflected from the fixed end of a stretched cord, after having been imparted by the hand to the free end.

The dynamic nature of electric energy is clearly indicated by the dynamo-electric* machine of Holtz, in which dynamic is directly converted into electric energy,—and by the cognate machines of Wilde, Wheatstone, Siemens, and Ladd, in all of which alike there is an intervening conversion of dynamic into magnetic energy. The enormous amount of current-energy evolved in Mr. Wilde's machine when the power of a steam-engine is employed to rotate the armatures may be judged of by the fact that a long piece of platinum wire 0·2 inch in thickness was seen to be disintegrated and partially fused. It is difficult to conceive that in these instances dynamic energy can be converted into magnetic "fluid," and that again into thermic energy: the conversion of motion into matter, and the subsequent reconversion of matter into motion, are obviously impossible.

Some further consideration of the effects of electric energy may serve to indicate the probable nature of the wave-motion. The facts of electric and magnetic polarity imply and necessitate a polarity or directionality in the motion itself, which has no analogue in the waves of sound, light, or heat. This requirement is fully met by the hypothesis of a circular spiral wave, the motion of which is direct or positive if viewed from one end, and retrograde or negative if from the other; and this suffices to explain the well-known polarity of electric and magnetic induction.

Thus far the spiral hypothesis is merely inferential; but in regard to magnetic wave-motion some strong presumptive evidence may be adduced. It appears from the experiments of Mr. Joule, made more than twenty years ago, that if a suspended mass of copper be, by twisting the suspension, made to rotate between the poles of an unexcited electro-magnet, the

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* The writer has elsewhere applied (Elements of Natural Philosophy, p. 550, *note*) a definite and intelligible meaning to the construction of those compound terms which must be constantly employed in relation to the conversions of energy. This may be accomplished by taking the first section of the term to mean the *acting cause*, the second, the *resulting effect*: thus a dynamo-electric machine will be one in which dynamic energy is employed to produce an electric current; and an electro-dynamic engine, one in which a current is employed to evolve dynamic energy.

rotation of the mass is arrested the instant the magnet is excited; and furthermore, if the mass be forcibly rotated, heat is developed in it. And it has since been ascertained that if two cylindrical magnets be so placed that their axes lie in the same straight line, their contrary poles being opposed to each other, then, if a cylinder of copper be made to rotate on its own axis, coinciding with the common axis of the magnets, no heat will be evolved by its rotation.

Now these phenomena must alike be the necessary consequences of the assumed dynamical theory; for if the copper molecules be thrown into spiral-wave motion, analogous to that of a pencil of circularly polarized light, then the motion of all the disturbed particles will be one of revolution in planes to which the lines of magnetic force are normals: and the inertia or energy of rotation (as it has been variously termed), the resistance offered by each revolving particle to any change in the direction of its axis of revolution (as exemplified by the gyroscope), will resist the rotation of the mass in any direction perpendicular to that of the axes of molecular revolution, and arrest its motion. And conversely, if the mass be forcibly rotated in the above direction, or in any other direction at right angles to the lines of magnetic force, heat will be freely developed, doubtless by internal friction arising from the perpetual displacement of the planes of molecular revolution. But in the second case, the axis of rotation of the mass coincides in direction with those of the axes of molecular revolution; hence there is no displacement of the molecular orbits, and consequently no internal friction, and very little if any heat is generated.

The rotatory character of the magnetic wave is further confirmed by the known fact that, if a plane-polarized beam pass through a transparent solid in the direction of the lines of force of a powerful electro-magnet, the plane of polarization will be rotated the instant that the magnet is excited. The truth of a theory can be established only by the verification of its necessary consequences; and it may not be too much to assume that in the present case the evidence already adduced by the writer is, in the entire absence of all contradictory evidence, strongly presumptive of the reality of the hypothesis.

It has been authoritatively stated that ordinary electric and magnetic waves *cannot both* be assumed to be spirals, because each of these forms of energy notoriously evolves the other in a direction perpendicular to its course; and the question is not without grave dynamical difficulties; but they may perhaps not be insuperable. It may possibly be that, from some unknown constraining condition or property inherent in magnetic bodies, a spiral wave, on being constrained into a spiral course, may lose its original spirality, and become a secondary spiral, having molecular motion in a direction perpendicular to that of the primary spiral.

The relations between electric energy and some of its observed physical results having thus been inferred, the question next arises as to the nature of the media by which the several modes of motion are transmitted. It is unquestionable that sound-waves are transmissible by all kinds of matter; but can any valid reason be assigned in favour of the still prevalent opinion that other modes of wave-motion are incapable of transmission by ordinary matter?—this incapacity being implied in the adoption of the self-contradictory hypothesis of an imaginary medium, not cognizable by any known means of perception.

It is a remarkable fact that in all the superseded crude notions of physical causation, each phase of physical energy has been presented in the garb either of impalpable, imponderable (in fact *immaterial*) matter itself, or of the vibrations thereof; and to some of these hypotheses have been successively added some violent supplementary hypothesis, in order adequately to meet the requirements of advancing knowledge.

To begin with chemical action:—What are now universally recognized as simple metals were once supposed to consist of some earthy matter (their oxides) combined with "Phlogiston,"—the *material* principle of brilliancy. But, unfortunately for the theory, it was soon found that the metals, on parting with their share of phlogiston (*i.e.* becoming oxidated), not only did not *lose* any, but actually *acquired* weight; therefore phlogiston was assumed to be not only *imponderable*, but *hyper*-imponderable—*i. e.* endowed with the property of absolute levity, or negative weight!

In the next place, the Newtonian theory of light assumed light to consist of molecules (of course *imponderable*) emanating from the source of light, and impinging on the perceptive organs of vision. But this hypothesis would not fit the phenomena of diffraction and interference; and to suit these physical facts the molecules must either be thrown into periodical "fits" of transmission or reflection, or the ray must be a row of egg-shaped molecules perpetually making isoperiodic somersaults, and plunging into a medium if they come on their heads, or bounding off if they fall against it sideways. Then, again, heat was supposed to consist of material particles emanating from the source of heat; and as a ball of ice placed in one focus of a concave mirror was found to lower the temperature of a thermometer placed in the conjugate focus, there were assumed to be particles of *cold*, as well as of *heat*: it is needless to add how completely the theory of exchanges accounts for these facts. At length these wild speculations were superseded, and light and heat were admitted into the category of wave-motion; but electricity and magnetism were still supposed to be either single or dual forms of "fluid" matter; and

("Saxa etiam molli dura teruntur aquâ")

these "fluids" are probably still running in the deep channels they have worn in some philosophic minds.

But the principle of admitting imponderability into the category of legitimate physical hypotheses had become tacitly accepted; and the conclusion was at once jumped at by the authors of the undulatory theory that the wave-motions of light and heat take place in an *imponderable* highly elastic fluid medium, pervading all space, *and all matter*, denominated "æther:" and this theory, with all its inconsistencies and inconsequences, is still in all probability generally entertained.

That some highly elastic and attenuated medium pervades infinite space, as the medium of transmission of the energies of light and heat (the very main-springs of organic existence) from the centre of each solar system to its dependent satellites, is a necessary consequence of the undulatory theory: its existence is, in fact, demonstrated by the periodic retardation of Encke's Comet. But the *remainder* of the hypothesis, namely that all palpable matter is pervaded by æther for the purpose of transmitting light- and heat-waves, is by no means equally necessary, or even tenable; for not a shadow of evidence of the inadequacy of all matter to transmit these motions has ever been produced, and in default of such evidence, the contrary hypothesis is at least equally tenable: and moreover the *interstitial-æther* theory (in common with all preceding physical theories involving imponderability) is burdened with grave inconsistencies. In the first place the well-known phenomena of single and double refraction and polarization, whether of light or heat, necessitate the somewhat violent hypothesis that the elasticity of the supposed transmitting medium, æther, is not, as it is in all cognizable fluids, a fixed and certain quality capable of numerical estimation, but an ever-varying quality, depending quantitatively on the elasticity of adjacent matter, and even varying in *two* or *three* directions within the same body: it would be not more repugnant to reason to assume that the elasticity of a gas is one thing in a glass bottle and another in one of brass, or that the specific gravity of silver is a function of the moon's age, or the melting-point of gold dependent on the sun's zenith-distance. Secondly, the fundamental ideas of inertia, energy, and "work" are inseparably associated with gravitation; and it seems to imply a contradiction of terms, to impute either inertia or energy (*i.e.* the capability of doing work) to an *imponderable* particle, which is consequently destitute of attraction for any other particle in the universe.

The known enormous velocity (of probably not less than 250,000 miles in a second) at which electricity travels through a copper conductor is complete evidence that ordinary matter is capable of transmitting *something* (whether matter or motion it signifies nothing for the present argument) at a considerably greater velocity than the waves of light and heat; why should not appropriate kinds of matter be assumed capable of transmitting these also? And if so, the need of the interstitial presence of æther ceases altogether; and it may with great advantage be excluded from the domains of ponderable palpable matter by the very mild hypothesis that it is *not miscible* with air, any more than oil, or *palpable* ether, with water, but that it floats above the boundary surface of our atmosphere. This hypothesis is not repugnant to reason, nor adverse to physical experience. On this supposition it is no longer needed to impute to æther imponderability; it will then be competent to fulfil its divine mission of transmitting light and heat, without doing any violence to some of the most fundamental notions of dynamics; and thus imponderability may cease to be reckoned amongst the physical attributes of matter.

March 28, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "A Comparison between some of the simultaneous Records of the Barographs at Oxford and at Kew." By BALFOUR STEWART, LL.D., F.R.S., Superintendent of the Kew Observatory. Received March 4, 1867.

Through the kindness of the Rev. Robert Main, director of the Radcliffe Observatory, Oxford, certain marked features of the curves produced by the barographs at Oxford and at Kew were compared together on four separate occasions in the year 1863.

These comparisons are the more interesting that they were all made during squalls or storms; for on such occasions it is found that the barograph curves exhibiting the height of the barometer from moment to moment present curious characteristic points, without which indeed no such comparisons could be made.

The result for these four occasions in 1863 was as follows:—

Nature of disturbance.	Date G. M. T. Oxford.	Kew.	Oxford is before Kew.
Sudden increase of pressure during squall of 30th October 1863	2.30 P.M.	3.9 P.M.?	39 minutes (probably).
Sudden increase of pressure during squall of 21st November 1863	4.0 P.M.	4.45 P.M.	45 minutes.
Peculiar points in the curves of December 3, 1863 (a stormy day)	2.40 A.M. 6.50 A.M.	3.35 A.M. 7.40 A.M.	55 minutes. 50 minutes.

Mr. Main has kindly called my attention to a well-marked minimum in the Oxford curve for February 6, 1867, which was also a stormy day. This minimum occurred at Oxford at 2.20 A.M. of that day, while at Kew it did not occur until 3.15 A.M. Oxford was thus on this occasion 55 minutes before Kew.

The peculiarity of this last occasion is the singular likeness between the two curves. I have not compared together any other features of these curves, nor perhaps could this be done with exactness; but the general impression is that the changes of pressure at Oxford were followed by similar changes at Kew, only nearly an hour later.

It is premature (until we obtain more information) to enter into a discussion of the rate of progress of storms; but we are quite justified in considering the barograph an instrument extremely well adapted to extend our knowledge of atmospheric disturbances.

We see that on those very occasions when this knowledge is most interesting the barograph comes forward to our assistance, and presents us with results which could not possibly be obtained otherwise than by a system of continuous registration. It does not, however, follow that, while a continuous record is by far the best, other records are of no value; for should an observer be placed beside an ordinary barometer during the crisis of a storm, observations made in rapid succession and accurately timed would be of very great assistance. Such an observer would in fact produce approximately a record similar in kind to that of a barograph, although inferior in value.

It ought here to be noticed that two stations are not enough to enable us to determine either the direction in which an atmospheric disturbance is propagated or the rate of propagation. It is only on the improbable supposition that all such disturbances travel in a direct line from Oxford to Kew that barographs at these two places might be deemed enough. In order to obtain the greatest amount of information which such instruments are capable of affording, it is evidently necessary to multiply our stations and to distribute them judiciously over the surface of the country. Nor is it desirable to confine ourselves to one meteorological element, but the barograph should be accompanied by a thermograph and a self-registering anemometer. As this is the system about to be pursued by the Board of Trade in their chief meteorological stations in the British Isles, we may reasonably hope that before long we may by this means receive a large accession to our knowledge of the laws which regulate atmospheric disturbances.

II. "On the Lunar-diurnal Variation of the Magnetic Declination, with special regard to the Moon's Declination." By G. NEUMAYER. Communicated by the President. Received March 11, 1867.

(Abstract.)

The hourly records of the magnetic declination systematically kept at the Flagstaff Observatory at Melbourne, Victoria, during the period from the 1st of May 1858 to the 28th of February 1863, have been discussed by the author, with a view to determine the lunar-diurnal variation to which that magnetic element is subject. The results arrived at in the course of this discussion elicit, he believes, facts hitherto unnoticed, to which it seems desirable that the attention of scientific men should be directed.

The process employed in reducing the observations was identical with that generally adopted in such cases. The disturbed observations were first eliminated, by rejecting all that differed from the final normal belonging to the same solar hour by more than a certain separating value, which was taken at 3·61 minutes of arc. The elimination of the larger disturbances having been thus effected, from every remaining reading (R) of the magnet's direction the final normal (N) belonging to that solar hour was subtracted, so that the residue R—N is devoid of the influence of the solar-diurnal variation. This residue is positive when the north end of the needle is to the east of its mean position, and negative in the contrary case. The number of observations at command amounted to 38,194, of which 4178 single observations were excluded from the discussion as being beyond the assumed limit used for separating the greater magnetic disturbances, leaving 34,016 available for the purpose of determining the lunar-diurnal variation.

The treatment of the residues with a view to classification according to lunar hours presented no particular novelty. It may be mentioned, however, that before entering on any general discussion, every month's result was calculated separately. The values for the various months were afterwards arranged, irrespectively of the year, in two groups, viz. Sun South (October to March) and Sun North (April to September). Thus the mean lunar-diurnal variation was obtained separately for each half of the year, as well as for the whole year. On examining the results, irregularities in the lunar-diurnal variation presented themselves which seemed to show that that variation depended in some degree on the moon's position with reference to the equator, on the circumstance whether her declination were north or south.

Accordingly the whole series of observations were rearranged in groups, "Declination of the Moon South" and "Declination of the Moon North," so as to cause her declination to be divided between the hours of the day, all those days being rejected on which the moon was close to the equator. The 118 groups of lunar-diurnal variation thus formed were subsequently classified according to whether the sun's declination was south or north.

A Table was thus formed giving for each lunar hour the lunar-diurnal variation of the magnetic declination separately under each of nine conditions formed by combining each of the three conditions, Sun South and North, Sun South, Sun North, with each of the three Moon South and North, Moon South, Moon North. The results given numerically in the Table were also laid down graphically in curves.

A glance at the curves shows that the lunar-diurnal variation must be regarded as being influenced by both the sun and the moon; for it is seen that in case the declinations of the two bodies are of the same name, the curves show greater regularity than in the contrary case.

The question whether during the winter season any lunar-diurnal variation can be traced at all can, the author conceives, no longer be entertained if we pay due attention to the facts which may be gleaned from the Table. Indeed it is afterwards shown, in discussing the individual years of observation, that in some cases the lunar-diurnal variation manifests itself in a very striking manner during the winter months.

On examining the results of the inquiry for the several years of observation, obvious differences are recognized on comparison. This was especially remarkable as regards the year 1861, which was the more striking as the year 1860 did not exhibit any such extraordinary deviations from the mean values. At first some error was suspected in the deduction of the results; but a perfectly independent and fresh discussion gave results agreeing in the main points with those previously arrived at.

In the course of the year 1861 the instruments previously in use were replaced by fresh ones obtained from Munich, which came into use for regular registration at the beginning of June. Though every care was taken to ensure uniformity of registration, it was deemed satisfactory to repeat the discussion, including that part only of 1861 in which the old instruments were employed; and accordingly the lunar-diurnal variation was discussed, with special regard to the moon's declination, for the year May 1860 to April 1861, and likewise for the year May 1858 to April 1859; but it was still found that towards the latter part of the former period the anomalies above pointed out made themselves clearly felt.

The results for the years 1860 and 1861 were given numerically in a special Table, and delineated in curves.

In conclusion, the author expressed a hope that the few facts he had brought forward might operate as an inducement to those who are engaged in similar pursuits, to enter upon such a laborious task as that of classifying magnetic observations for the purpose of examining into the law and nature of the lunar-diurnal variation, according to the moon's position north or south of the equator.

April 4, 1867.

Lieutenant-General SABINE, President, in the Chair.

The following communications were read :—

I. "Researches on Gun-cotton.—Second Memoir. On the Stability of Gun-cotton." By F. A. Abel, F.R.S., V.P.C.S. Received March 9, 1867.

(Abstract.)

The results of the many observations which had been instituted prior to 1860 upon the behaviour of gun-cotton when exposed to diffused or strong daylight, or to heat, although they agree generally with those of the most recent investigations on the subject, as far as relates to the nature of the products obtained at different stages of its decomposition, cannot be regarded as having a direct bearing upon the question of the stability of gun-cotton produced by strictly pursuing the system of manufacture prescribed by von Lenk, inasmuch as it has been shown that the products formerly experimented upon by different chemists varied very considerably in composition.

The investigations recently published by Pélouze and Maury*, into the composition of gun-cotton, and the influence exerted by light and heat upon its stability, are described as having been conducted with gun-cotton prepared according to von Lenk's system. The general conclusion arrived at by those chemists with reference to the latter branch of the subject was to the effect that the material is susceptible of spontaneous decomposition, under conditions which may possibly be fulfilled in its storage and application to technical and warlike purposes; and the inference is drawn, partly from the results of earlier investigators, and partly from the exceptional behaviour of one or two specimens, that gun-cotton is liable to explode spontaneously at very low temperatures when stored in considerable quantities.

It has been shown, in the memoir on the Manufacture and Composition of Gun-cotton, published last year†, that modifications in the processes of conversion and purification, which appear at first sight of very trifling nature, exert most important influences upon the composition and purity of the product. Gun-cotton of quite exceptional character has been discovered, in several instances, among samples received from Hirtenberg and among the first supplies obtained from Stowmarket; other exceptional products have also been produced by purposely modifying, in several ways, the system of manufacture as pursued at Waltham Abbey. The very considerable difference exhibited between some of these and the ordinary products in their behaviour under equal conditions of exposure to heat and light, affords good grounds for the belief that the attainment of certain exceptional results, upon which the conclusions of Pélouze and Maury's report,

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[*] Comptes Rendus.

[†] Trans. Royal Society.

condemnatory of gun-cotton, have been principally founded, are to be ascribed to such variations in the nature of the material operated upon.

Very numerous and extensive experiments and observations have been carried on during the last four years at Woolwich, both with small and large quantities of gun-cotton, for the purpose of completely investigating the conditions by which the stability of this substance, when under the influence of light and heat, may be modified, and with the view of ascertaining whether results recently arrived at in France apply to gun-cotton as manufactured in this country.

The principal points which have been established by the results arrived at in these investigations may be summed up as follows:—

1. Gun-cotton produced from properly purified cotton, according to the directions given by von Lenk, may be exposed to diffused daylight, either in the open air or in closed vessels, for very long periods without undergoing any change. The preservation of the material for 3½ years under those conditions has been perfect.

2. Long-continued exposure of the substance in a condition of ordinary dryness to strong daylight and sunlight produces a very gradual change in gun-cotton of the description defined above; and therefore the statements which have been published regarding the very rapid decomposition of gun-cotton when exposed to the sunlight do not apply to the nearly pure trinitrocellulose obtained by strictly following the system of manufacture now adopted.

3. If gun-cotton in closed vessels is left for protracted periods exposed to strong daylight or sunlight in a damp or moist condition, it is affected to a somewhat greater extent; but even under these circumstances the change produced in the gun-cotton by several months' exposure is of a very trifling nature.

4. Gun-cotton which is exposed to sunlight until a faint acid reaction has become developed, and is then immediately afterwards packed into boxes which are tightly closed, does not undergo any change during subsequent storage for long periods. (The present experience on this head extends over 3½ years.)

5. Gun-cotton prepared and purified according to the prescribed system, and stored in the ordinary dry condition, does not furnish any indication of alteration, beyond the development, shortly after it is first packed, of a slight peculiar odour and the power of gradually imparting to litmus, when packed with it, a pinkish tinge.

6. The influence exercised upon the stability of gun-cotton of average quality, as obtained by strict adherence to von Lenk's system of manufacture, by prolonged exposure to temperatures considerably exceeding those which are experienced in tropical climates is very trifling in comparison with the results recently published by Continental experimenters relating to the effects of heat upon gun-cotton; and it may be so perfectly counteracted by very simple means which in no way interfere with the essential qualities

of the material, that the storage and transport of gun-cotton presents no greater danger, and is, under some circumstances, attended with much less risk of accident than is the case with gunpowder.

7. Perfectly pure gun-cotton, or trinitrocellulose, resists to a remarkable extent the destructive effects of prolonged exposure to temperatures even approaching 100° C.; and the lower nitro- products of cellulose (soluble gun-cotton) are at any rate not more prone to alteration when pure. The incomplete conversion of cotton into the most explosive products does, therefore, not of necessity result in the production of a less perfectly perfectly permanent compound than that obtained by the most perfect action of the acid mixture.

8. But all ordinary products of manufacture contain small proportions of organic (nitrogenized) impurities of comparatively unstable properties, which have been formed by the action of nitric acid upon foreign matters retained by the cotton fibre, and which are not completely separated by the ordinary, or even a more searching process of purification.

It is the presence of this class of impurity in gun-cotton which first gives rise to the development of free acid when the substance is exposed to the action of heat; and it is the acid thus generated which eventually exerts a destructive action upon the cellulose-products, and thus establishes decomposition which heat materially accelerates. If this small quantity of acid developed from the impurity in question be neutralized as it becomes nascent, no injurious action upon the gun-cotton results, and a great promoting cause of the decomposition of gun-cotton by heat is removed. This result is readily obtained by uniformly distributing through gun-cotton a small proportion of a carbonate,—the sodic carbonate, applied in the form of solution, being best adapted to this purpose*.

9. The introduction into the finished gun-cotton of 1 per cent. of sodic carbonate affords to the material the power of resisting any serious change, even when exposed to such elevated temperatures as would induce some decomposition in the perfectly pure cellulose-products. That proportion affords, therefore, security to gun-cotton against any destructive effects of the highest temperatures to which it is likely to be exposed even under very exceptional climatic conditions. The only influences which the addition of that amount of carbonate to gun-cotton might exert upon its properties as an explosive would consist in a trifling addition to the small amount of smoke attending its combustion, and in a slight retardation of its explosion, neither of which could be regarded as results detrimental to the probable value of the material.

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* The deposition of calcic and magnesian carbonates upon the fibre of gun-cotton, either by its long-continued immersion in flowing spring water, or by its subjection to the so-called "silicating" process adopted by von Lenk, produces a similar protective effect, which, however, is necessarily very variable in its extent, as the amount of carbonate thus introduced into a mass of gun-cotton is uncertain; moreover, as it is only loosely deposited between the fibres, the proportion is liable to be diminished by any manipulation to which the gun-cotton may be subjected.

10. Water acts as a most perfect protection to gun-cotton (except when it is exposed for long periods to sunlight), even under extremely severe conditions of exposure to heat. An atmosphere saturated with aqueous vapour suffices to protect it from change at elevated temperatures; and wet or damp gun-cotton may be exposed for long periods in confined spaces to 100° C. without sustaining any change.

Actual immersion in water is not necessary for the most perfect preservation of gun-cotton; the material, if only damp to the touch, sustains not the smallest change, even if closely packed in large quantities. The organic impurities which doubtless give rise to the very slight development of acid observed when gun-cotton is closely packed in the dry condition, appear to be equally protected by the water; for damp or wet gun-cotton, which has been preserved for three years, has not exhibited the faintest acidity. If as much water as possible be expelled from wet gun-cotton by the centrifugal extractor, it is obtained in a condition in which, though only damp to the touch, it is perfectly non-explosive; the water thus left in the material is sufficient to act as a perfect protection, and consequently also to guard against all risk of accident. It is therefore in this condition that all reserved stores of the substance should be preserved, or that it should be transported in large quantities to very distant places. If the proper proportion of sodic carbonate be dissolved in the water with which the gun-cotton is originally saturated for the purpose of obtaining it in this non-explosive form, the material, whenever it is dried for conversion into cartridges, or employment in other ways, will contain the alkaline matter required for its safe storage and use in the dry condition in all climates.

Although some experiments, bearing upon the different branches of inquiry included in this memoir, are still in progress with a view to the attainment of additional knowledge of the conditions which regulate the stability of gun-cotton, it is confidently believed that the results arrived at amply demonstrate that the objections which have been of late revived, especially in France, against the employment of gun-cotton, on the ground of its instability, apply only in a comparatively slight degree to the material produced by strictly pursuing the system of manufacture perfected by von Lenk—that, as far as they do exist, they have been definitely traced to certain difficulties in the manufacture of pure gun-cotton which further experimental research may, and most probably will, overcome—but that, in the meantime, these objections are entirely set aside by the adoption of two very simple measures, against the employment of which no practical difficulties can be raised, and which there is every reason to believe must secure for this material the perfect confidence of those who desire to avail themselves of the special advantages which it presents as an explosive agent.

The nature of the decomposition of gun-cotton when exploded under different conditions is now under investigation by me; and the results arrived at will, I trust, be communicated before long to the Royal Society.

II. “Observations of Temperature during two Eclipses of the Sun (in 1858 and 1867).” By JOHN PHILLIPS, M.A., LL.D., D.C.L., F.R.S., Professor of Geology in the University of Oxford. Received April 3, 1867.

On the 15th of March 1858, occurred an annular eclipse of the sun, whose central line of shadow passed near the village of Steeple Aston, a few miles north of Oxford. Ample preparations were made for observing it by residents in Oxford, and they were met on the ground by many persons from a distance; Mr. Lassell being one of the party, there was no lack of telescopic power. The day was unfavourable—cold and cloudy, with some occasional feeble and delusive gleams, scarcely permitting a sight of the progress of the eclipse, which, however, was obvious enough by the growing and diminishing darkness. Under these circumstances I devoted my principal attention to three thermometers, carefully selected and compared beforehand—one mercurial with blackened bulb, another mercurial with clear bulb (these were placed in an open space exposed to the sun); the third, a minimum- spirit thermometer, tint red, was placed in a shaded situation. The observations began at $11^h\ 30^m$ and lasted till $2^h\ 30^m$, thus including the whole period of the eclipse, which began at $11^h\ 35^m$, reached the maximum of obscuration at $0^h\ 54^m$, and ended at $2^h\ 11^m$. The apparent semidiameters of the sun and moon were so nearly equal that the eclipse was almost total $\left(\frac{997}{1000}\right)$. The observations were recorded as follows:—

Hour.	Thermometer in shade.	Clear thermometer in sun.	Dark thermometer in sun.	Remarks.
h m	°	°	°	
11 30	49·0	51·6	58·0	Gleams.
35	Beginning of eclipse.			
45	49·0	50·0	55·0	Gleams.
12 0	48·5	49·5	54·5	
15	48·5	49·0	52·0	
30	48·1	48·5	50·0	
45	48·1	48·5	49·2	
54	This was the moment of greatest obscuration.			
1 0	47·5	47·5	47·5	Lowest temperature.
15	47·5	47·5	48·0	
30	47·6	48·0	48·8	
45	48·0	49·4	51·0	
2 0	48·3	49·8	51·7	
11	End of eclipse.			
15	49 0	50·5	53·1	Rain began.
30	48·0	49·5	51·2	Rain continued.
Mean ...	48·3	49·6	51·5	
Max. ...	49·0	51·6	58·0	
Min. ...	47·5	47·5	47·6	
Range ...	1·5	4·1	10·4	

During the late partial eclipse of the sun on the 6th of March 1867, observations of the ingress of the moon were favoured at Oxford by brilliant weather; within five minutes after the moment of maximum obscuration $\left(\frac{742}{1000}\right)$ clouds appeared; and from this time till the end of the eclipse they never wholly disappeared, but did not prevent the progress of the moon and the degrees of obscuration from being correctly marked. At the very end it was only just possible to observe the egress by a momentary attenuation of the clouds; the remainder of the day was cold, cloudy, and finally snowy. The observations began at 8^h and ended at $10^h\ 50^m$, thus including the whole period of the eclipse, which began at $8^h\ 12^m\ 15^s$, reached the greatest obscuration at $9^h\ 26^m$, and ceased at $10^h\ 45^m\ 8^s$. At the moment of greatest obscuration the light-giving area was reduced to one-third of the solar disk.

The observations comprised—

(1) Temperature in the shade, by the mean of one mercurial and one spirit thermometer very nearly agreeing throughout.

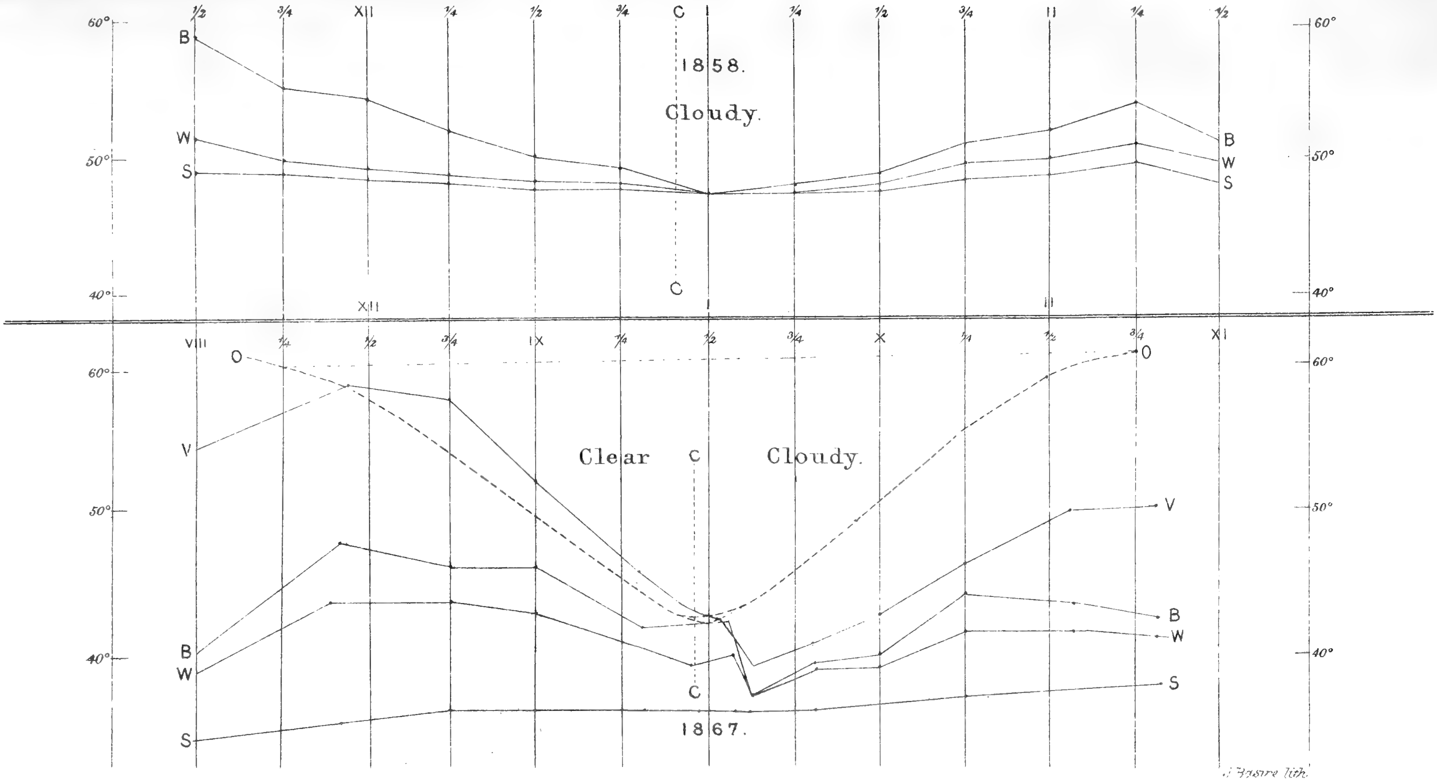
(2) Temperature in the sunlight, by a clear mercurial bulb.

(3) Temperature in the sunlight, by a dark mercurial bulb.

(4) Temperature in the sunlight, by a dark mercurial bulb enclosed in a glass tube exhausted of air.

The observations were recorded at intervals according to convenience (3^m to 20^m), the shorter intervals being purposely chosen about the time of maximum obscuration. The results are in the following Table:—

Hour.	Thermometer in shade.	Clear bulb in sun.	Dark bulb in sun.	Dark bulb in glass tube.	Remarks.
h m					
8 0	35°·5	39°·5	41°·0	54°·0	Sky always clear till the middle of the eclipse.
12	Beginning of eclipse.				
25	36·2	44·0	48·0	58·8	
45	36·7	44·0	46·5	57·8	
9 0	36·7	43·6	46·5	52·0	
18	36·7	41·5	43·0	46·5	
25	36·7	40·5	43·0	43·5	
26	This was the moment of greatest obscuration.				
32	36·7	41·5	43·2	42·9	Clouds gathered at $9^h\ 30^m$ and continued to the end of the eclipse.
35	36·7	38·0	38·0	41·0	
50	……	40·0	40·5	42·0	
10 0	……	40·0	41·0	43·3	
15	37·7	42·5	44·5	46·8	
35	……	42·7	44·0	49·9	
45	End of eclipse.				
50	38·8	42·0	43·0	50·0	
Mean…	37·2	41·5	43·2	48·4	
Max. …	38·8	44·0	48·0	58·8	
Min. …	35·5	38·0	38·0	41·0	
Range ..	3·3	6·0	10·0	17·8	



J. Basire lith.

On considering the columns of figures with attention, it will be perceived that on each occasion all the thermometers in the sunshine sank as the eclipse advanced, so as to reach the greatest depression not at, but after the epoch of greatest obscuration, and from this point rose again as the obscuration diminished, but in neither case arrived at the same elevation as in the beginning of the eclipse. In each case the eclipse began with fair prospects, but was followed by rain or snow.

In the annular (almost total) eclipse three thermometers, two in the sunshine and one in the shade, reached the very same point (47°·5), that being the lowest observed; in the partial eclipse, three thermometers corresponding to the above reached nearly the same point (36°·7, 38°, 38°), the lowest observed. The lowest point was not reached on either occasion by these instruments till some minutes after the moment of greatest obscuration (6 minutes in the annular and 9 minutes in the partial eclipse); while the thermometer enclosed in a tube did not sink below 41° at the same time. The later occurrence of the extreme depression in the partial eclipse was occasioned by the additional cooling influence of the clouds which gathered five minutes after the epoch of greatest obscuration.

By representing the observations in curves with ordinates proportioned to the depressions at the successive epochs, the circumstances which have been referred to are clearly seen,—the convergence of all the lines beyond the time-point of greatest obscuration—the exactitude of this convergence in the uniformly clouded sky of 1858, and the comparative confusion of the lines in the suddenly altered sky of 1867, where the effect of the access of cloud is 1°·9 on the enclosed thermometer, 3°·5 on the clear exposed bulb, and 5°·2 on the black exposed bulb (see Plate IX.).

The effect of the cloud on the instruments employed in the latter half of the eclipse is to reduce the temperatures at the end of the eclipse, as compared with the beginning, more than 8° in the enclosed thermometer, 5° in the dark-bulb exposed, and 2° in the clear-bulb; but in the shaded instruments the effect is contrary, for they gained 2°·6 between beginning and end.

Finally, if the areas of obscuration be calculated for the several epochs of observation (in 1867), and the proportions be represented by a curve adapted to the scale used for temperature, the fact of the postponement of the radiation-effect will appear, as well as the conformity with which the temperatures follow the curve, in the bright half of the eclipse, and fall away from it, but still proportionately, in the clouded half.

REFERENCE TO PLATE IX.

In the diagram for 1858, the temperatures observed at the several epochs are marked by the crossing of the lines S S for shade, W W for clear bulb, and B B for black bulb. The central time of the eclipse is marked C C.

In that for 1867, similar letters mark similar observations; and, in addition, V V shows the temperatures of the black bulb *in vacuo*, and O O the curve of relative obscuration at the several epochs of observed temperature.

April 11, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

I. “A new fact relating to Binocular Vision.” By A. Claudet, F.R.S. Received March 20, 1867.

The persistence of the impression made by light on the retina is demonstrated by many experiments ; but one of the most convincing, which is also very easy to try, is that which is known under the name of the thaumatrope.

Let us write the letters composing a word of eight letters, say “Victoria,” on the two sides of a small card, in such a manner that one surface shall contain the 1st, 3rd, 5th, and 7th letters, and the other surface the 2nd, 4th, 6th, and 8th, with a space between them sufficient to complete the word on each surface, which blank spaces are in fact to appear filled up during the experiment by an artificial means to be explained.

Fig. 1 shows the arrangement on the two sides of the card (section view).

Fig. 1.

Fig. 2 shows the plan of the card. The white letters are those written on one surface, and the dotted lines those written on the other.

Fig. 2.

Now by means of two strings fixed on the two sides A, B the card may be made to revolve on its axis by turning the string between the thumb and finger of each hand. By this means a very rapid motion may be communicated to the card, and while it is revolving both surfaces are alternately seen in quick succession, and the perception of the two is so simultaneous that the two sets of letters appear as one, and the whole word is read as distinctly as if it were written on one surface only.

This is easily explained. It is known that the persisting action of light on the retina has a duration of about one-eighth of a second ; so that if the card makes at least eight revolutions in a second (it may make considerably more), before one impression has vanished another produces its effect on the next part of the retina, in such a way that they are intermixed and simultaneously visible, producing an uninterrupted sensation.

The means by which this illusion is produced has been called the "thaumatrope," from two Greek words meaning "wonder" and "turn." It is difficult to trace the history of this discovery; but it is certain that it has been the result of a very old, simple, and well-known experiment.

From time immemorial schoolboys have amused themselves by holding a coin between two pins and making it revolve rapidly by blowing upon it, when to their surprise the coin showed the head mixed with the device on the other side. I have been told that as Sir John Herschel was one day making this experiment to amuse his children, in the presence of the late Dr. Paris, this gentleman was struck with the idea that if, instead of a coin, a white card was employed on each side of which one part of a design was properly arranged, the two might complete the subject during the revolution. Accordingly he made the experiment, which succeeded perfectly well. If the story is true, certainly Dr. Paris may be regarded as the inventor of the thaumatrope, which he has so well and so fully elucidated in his very interesting and instructive work entitled 'Philosophy in Sport made Science in earnest.'

This philosophical toy may be employed to show another effect of the persistence of the retinal image. If complementary colours are fixed on the two opposite sides of the card, they will become superposed during the revolution, and white light will be the result. By the same means, other curious effects of the mixture of various colours might be tried.

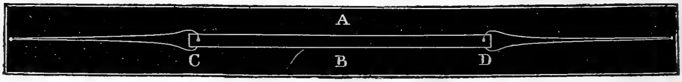
All these experiments present no difference, whether they are made looking with the two eyes or only with a single eye: the effect is the same in both cases. Therefore the illusion is equally monocular and binocular.

But I was not a little surprised to find that the thaumatrope is capable of producing another phenomenon, elucidating very forcibly the principles by which binocular vision is the only real and effective means of showing the distances of objects, which are determined by the degree of the angle of convergence of the optic axes and by one of its corollaries, the sensation of double images for all the points which are not exactly on the plane of vision.

The thaumatrope is capable of showing that binocular vision can detect to a degree hardly conceivable the most minute difference in the distances of objects, such as the distance between the planes of the two surfaces of a card, which distance is nothing more than the thickness of the card. Therefore, supposing that the thickness of the card is $\frac{1}{80}$ of an inch and the distance from the eyes 15 in, there is not a difference greater than the $\frac{1}{1200}$ part of the whole distance from the eyes to the two planes of the card; and still the difference of the degree of convergence for two planes so near each other is sufficient to excite the action of binocular vision, and by it to enable us to detect that infinitesimal difference in their distances. But that such an effect of binocular vision could possibly be displayed while looking at two planes so nearly intermixed as the surfaces of a card revolving upon its axis with such a wonderful velocity is the very extraordinary phenomenon I have discovered, and which I am about to describe and endeavour to explain.

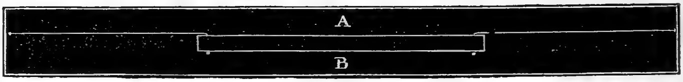
If the thickness of the card is A B, fig. 3, and if the two ends of each string, passing through the holes C and D in the card, are brought together and turned between the thumb and finger, the card will whirl exactly on its axis, and during the revolution the two surfaces A and B will be at the same distance from the eyes.

Fig. 3.



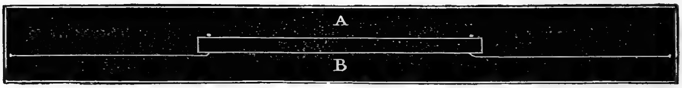
But if the two strings are drawn so that one of their knots is as in fig. 4, the surface B will revolve round the plane of the surface A corresponding with the axis of the string, and, during the revolution, every time that it is made visible to the eyes it will appear as if it were nearer than the surface A.

Fig. 4.



By reversing the position of the knots, as in fig. 5, instead of the surface B revolving round the plane of A, it will be A that will revolve round the plane of B.

Fig. 5.



These three different positions of the strings will produce three different effects.

In the position of fig. 3 the effect will be normal; that is to say, the two surfaces coming alternately at the same distance, we shall see the whole word as if the letters were on the same surface.

In the position of figs. 4 and 5 we have a very strange illusion. One half of the letters composing the word will appear before or behind the other half, according to the surface upon which they are written and the position of the knots upon that or the other surface.

In fig. 4 the letters written on the surface B will appear before the letters on the surface A; and in fig. 5 the letters on the surface A will appear before the letters on the surface B.

The cause of the anomaly resulting from the two different experiments is entirely and positively due to a sensation of binocular vision; and we may easily satisfy ourselves that it is so; for, looking with a single eye in both cases, all the letters appear on the same plane, notwithstanding the different distances of the two surfaces given by the position of the knots: and we may add another convincing proof, which is that the pseudoscope inverts the distances of the surfaces.

At first it is rather difficult to understand how the phenomenon can take place; for as the perception of the two surfaces is simultaneous, how is it possible that during such a rapid revolution the optic axes can be made to converge alternately on each surface while it is passing so quickly, and that they should be made suddenly to converge on the other in its turn?

However, there cannot be any question that in reality the phenomenon takes place, and that it is decidedly an effect of binocular vision; therefore it only remains to be explained how it can be produced. In endeavouring to arrive at the true cause of the phenomenon, we shall have to bring to mind various physiological sensations which concur in producing the effect. One is the effort we make to obtain distinct vision, and the other the effort we make to obtain single vision. These two efforts act in unison; for it is impossible not to admit that the two muscular processes by which both the angle of convergence is directed to the object and the focus of the eyes is adapted to its distance, for the double purpose of having at once single and distinct vision of every object, are two actions necessarily simultaneous and inseparably connected. They are therefore both, each in its way, criteria of the distances of objects; but they give rise to certain indirect and additional criteria for other distances, in two ways: one, the most important, is the double images of the objects situated before and behind the point of convergence; and the other, but only in a subsidiary way, the degree of confusion of the objects situated before and behind the point of convergence and which are not in focus.

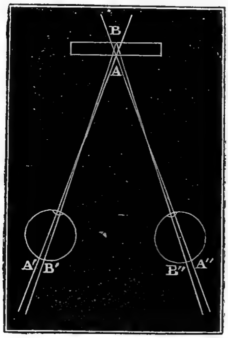
The comparison of two points, one of which is in focus and well defined, and the other out of focus and confused, helps considerably in forming a judgment that they are on different planes. But in a question of binocular vision, perhaps we ought not strictly to take into account this last criterion, which belongs equally to monocular and binocular vision; and if we allude to it, it is only because, although it does not produce the real stereoscopic effect, still it contributes to give that sort of illusion of relief which by various means may be evinced by monocular vision. Therefore it is particularly the sensation of double images, the degree of their separation, and their respective positions either outside or inside from the centres of the two retinæ, which indicate more powerfully the exact distance of the object from the point of single vision either before or behind.

When we look fixedly on a point of one surface of the revolving card, that point appears single, and we see at the same time another point on the other surface which appears double, although we hardly feel that we notice its doubleness; and from the position or distribution of the double images, either on the right or on the left of the central point, we have at the same glance the perception of the respective distances. Therefore, to judge of the distances of certain objects in the direction of the line of vision, we are not absolutely obliged to alter constantly the angle of convergence. This is proved by our perception of the two distances of the surfaces of the card while it is revolving; for it would be impossible that we should alter the angle of convergence to adapt it alternately to the two surfaces while they are turning so rapidly.

The same angle of convergence kept on one or the other surface is no impediment to our seeing both in a sufficiently distinct manner.

The whole phenomenon may be better understood by the illustration given in fig. 6.

Fig. 6.



When we converge the optic axes on B, this point, being represented on the centre of both retinæ at B' B'', is single, but A being nearer is represented on the left of the centre of the left retina at A', and on the right of the centre of the right retina at A''; therefore it appears double.

For the same reason, converging on A, this point is single, but B is double, with this difference—that one image is on the right of the left retina, and the other on the left of the right retina; so that the double images of nearer objects situated at A are represented outside the centres of the two retinæ, and those of further objects situated at B are represented inside the centres of the retinæ, and each of these two different sensations brings to our mind the perception of the distance which has produced it. During the revolution of the card we may adapt the convergence either to one or to the other surface and keep it so; but in every case the letters on that surface will appear single and a little better defined; and this, with the sensation of double images of the letters on the other surface, will be an indication of their respective distances.

As I am not aware that the illusion I have described in this paper has ever been noticed before, it has appeared to me that its publication would excite the interest of all those who look for any new fact capable of illustrating the principles of binocular vision, and showing the wonderful property of the angle of convergence, by which the most minute differences in the distances of objects and the slightest relief on their surfaces can be detected, and by which also in the abnormal conversion introduced in its action by the pseudoscope all our sensations are reversed. Therefore the pseudoscope is the great test of the phenomena of binocular vision; for by reversing certain sensations which by constant habit we may hardly notice, it renders them more conspicuous by the comparison of the abnormal state brought out by its action, and proves the theory of binocular vision in the most effective manner.

A truth is never better established than when it can be shown that the same principles are capable of producing contrary effects when they are applied in a contrary way.

Professor Wheatstone, by adding the pseudoscope to the stereoscope, has thus in the most scientific and ingenious manner completed his splendid discovery, and left very little (we might almost venture to say that he has left nothing) for further investigations in the physiology of binocular vision.

II. "On the Calculation of the Numerical Value of Euler's Constant, which Professor Price, of Oxford, calls E." By William Shanks, Esq., Houghton-le-Spring, Durham. Communicated by the Rev. B. Price, F.R.S. Received March 28, 1867.

In the year 1853 Dr. Rutherford, of the Royal Military Academy, Woolwich, sent a paper on the Computation of the value of π to the Royal Society, and the paper was published in the 'Proceedings' of that learned body*. The value of π is there given to 607 decimals, the first 440 being the joint production of Dr. Rutherford and the author of this paper, and the remaining 167 decimals having been calculated by the present writer, for the accuracy of which he alone is responsible. Subsequently, the Astronomer Royal, G. B. Airy, Esq., kindly presented the author's paper on the Calculation of the value of e, the base of Napier's logarithms, to upwards of 200 decimals; the aforesaid paper also contained the Napierian logarithms of 2, 3, and 5, as well as the modulus of the common system, all to upwards of 200 places of decimals. This paper was not, however, published, but deposited in the Archives of the Royal Society; but an abstract, containing the numerical results, was printed in the 'Proceedings'†. In a paper sent by the author to the Astronomer Royal, and forwarded by him to the Royal Society, will, the author believes, be found the reciprocal of the prime number 17389, consisting of a circulating period of no less than 17388 decimals, the largest on record. Some few remarks are also given touching circulates generally, and the easiest modes of obtaining them. The writer now desires to supplement what he then did, by giving the

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* Vol. vi. p. 273.

† Vol. vi. p. 397.

numerical value of Euler's constant, which is largely employed in "Infinitesimal Calculus," to a greater extent than has hitherto been found, and free from error.

In Crelle's Journal for 1860, vol. lx. p. 375, M. Oettinger has contributed an article on Euler's constant, and especially on "certain discrepancies" in the value given by former mathematical writers. Adopting the formula there employed, as being well adapted for the purpose, the writer of this paper has both corrected and extended what has been previously done; and as very great care has been bestowed upon the calculations, so as to exclude error, he confidently believes that his results are, as far as they go, absolutely correct. He may remark that, since the separate values of *n* in the formula (which, see below) produce identical results as far as they go, and the higher the value of *n* the more nearly we can approximate to the value of the constant, we thus have sufficient proof afforded of the correctness of the value found when *n* is 10, 20, 50, or 100. If the writer can command sufficient leisure, he may resume the calculation by and by, and, making *n* 1000, he may thus verify, as well as extend, the value of Euler's constant given in this paper. The numbers 10, 20, 50, 100, 200, and 1000, especially 10 and its integral powers, are more easily handled than others, particularly in those terms of the formula which contain Bernoulli's numbers. The harmonic progression is here "summed" much further than was requisite for finding E to 50 or 55 decimals; but this was of some importance in ensuring correctness in the decimal expression of each of the higher terms of S_{100} and S_{200}. It may be observed that the numbers of decimal places in E, obtained from *n* being 10, 20, 50, 100, and 200, are nearly proportional to $10^{\frac{1}{3}}$, $20^{\frac{1}{3}}$, $50^{\frac{1}{3}}$, $100^{\frac{1}{3}}$, and $200^{\frac{1}{3}}$—a rather curious coincidence.

The formula for Euler's constant, employed by M. Oettinger, as above stated, is—

$$\text{Constant} = Sn - \log_{\varepsilon} n - \frac{1}{2n} + \frac{B_1}{2n^2} - \frac{B_2}{4n^4} + \frac{B_3}{6n^6} - \frac{B_4}{8n^8} + \ldots\text{\&c., where}$$

$Sn = 1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \ldots \frac{1}{n}$, and B_1, B_2, B_3, &c. are Bernoulli's numbers.

$$1 + \tfrac{1}{2} + \tfrac{1}{3} + \ldots \tfrac{1}{10} = 2{\cdot}928\dot{9}6825\dot{3}$$

$$\begin{aligned} 1 + \tfrac{1}{2} + \tfrac{1}{3} + \ldots \tfrac{1}{20} = 3{\cdot}5977\dot{3}\ \ & 96571\ \ 43681\ \ 91148\ \ 37690\ \ 68908\ \ 38779\ \ 38367 \\ 10245\ \ & 37897\ \ 60291\ \ 30827\ \ 89243\ \ 77995\ \ 58542\ \ 59259 \\ 83201\ \ & 52499\ \ 71906\ \ 31865\ \ 55446\ \ 61736\ \ 61220\ \ 10084 \\ 85838\ \ & 20720\ \ 04363\ \ 02603\ \ 48526\ \ 60\dot{2}. \end{aligned}$$

$$\begin{aligned} 1 + \tfrac{1}{2} + \ldots\ldots \tfrac{1}{25} = 3{\cdot}8159\dot{5}\ \ & 81777\ \ 53506\ \ 86913\ \ 48136\ \ 76474\ \ 73449\ \ 06181 \\ 89635\ \ & 55401\ \ 83780\ \ 86220\ \ 32609\ \ 11027\ \ 77063\ \ 20242 \\ 32778\ \ & 03544\ \ 32662\ \ 95332\ \ 52228\ \ 09675\ \ 62970\ \ 52433 \\ 81377\ \ & 45056\ \ 57669\ \ 24455\ \ 54624\ \ 85197\ \ 30818\ \ 82894 \\ 77830\ \ & 46585\ \ 03877\ \ 77968\ \ 87905\ \ 92007\ \ 71409\ \ 68243 \end{aligned}$$

+ (circulating period consists of 1584 decimal places).

$1+\frac{1}{2}+\ldots\ldots\frac{1}{50}$ =4·49920 $\dot{5}$3383 29425 05756 04717 92964 76909 19706
01823 96745 38296 58902 43217 68318 06557 86735
78157 82663 88434 12900 97472 82033 55398 82942
63084 20093 94581 03200 90115 13091 15572 24477
64889 95794 59834 14243 62248 23530 45299 19591
+ (circulating period consists of 1,275,120 decimal places).

$1+\frac{1}{2}+\ldots\ldots\frac{1}{100}$ =5·18737 7$\dot{5}$176 39620 26080 51176 75658 25315 79089
72126 70845 16531 76533 95658 72195 57532 55049
66056 87768 92312 04135 49921 06986 97779 79182
73403 18617 00828 94825 42444 49096 57618 56974
16326 13467 07313 21114 47132 49733 09103 51089
+ (circulating period consists of 39,419,059,680 decimal places).

$1+\frac{1}{2}+\ldots\ldots\frac{1}{200}$ =5·87803 09$\dot{4}$81 21444 47605 73863 97130 86163 68374
00246 53027 30844 64971 94472 28783 30029 84018
15499 64301 86679 89238 37326 83211 85439 05911
76542 77855 27568 86559 30203 06049 25715 75389
22254 75748 47845 75246 64079 54805 61627 08837
+ (circulating period consists of 2,498,236,128,143,832,017,541,600 decimal places).

The following value of Euler's constant has been found from the respective sums given above, of the 10, 20, 50, 100, and 200 first terms of the Harmonic Progression :—

E or Eul. const. = ·57721 56649 01532 86060 6 $\left(\text{last term employed is} -\frac{B_{12}}{24.10^{4}}\right)$.

E= ·57721 56649 01532 86060 65120 900 $\left(\text{last term is} -\frac{B_{12}}{24.20^{24}}\right)$.

E= ·57721 56649 01532 86060 65120 90082 40243 1042 $\left(\text{last term is} + \frac{B_{13}}{26.50^{26}}\right)$.

E= ·57721 56649 01532 86060 65120 90082 40243 10421 59335 9 $\left(\text{last term is} -\frac{B_{12}}{24.100^{24}}\right)$.

E= ·57721 56649 01532 86060 65120 90082 40243 10421 59335 93995 35989 $\left(\text{last term is} + \frac{B_{13}}{26.200^{26}}\right)$.

Certainly 50 decimals are correct, and probably 55, in the value last given.

March 2, 1867.

Supplementary Paper to that of March 2, 1867, "On the Calculation of the Numerical Value of Euler's Constant." By WILLIAM SHANKS, Esq., Houghton-le-Spring, Durham. Received April 9, 1867.

When $n=500$, we have

$1+\frac{1}{2}+\frac{1}{3}\ldots\ldots\frac{1}{500}$ =6·79282 34299 90524 60298 92871 45367 97369 48198
13814 39680 91166 43088 89685 43566 23790 55049
24576 49403 73586 56039+

$$E = {}\cdot 57721\ 56649\ 01532\ 86060\ 65120\ 90082\ 40243\ 10421$$
$$59335\ 93995\ 35988\ 05771\ 53865\ 48677\ \left(\text{last term } -\frac{B_{14}}{28.500^{28}}\right).$$

When $n=1000$, we have

$$1+\tfrac{1}{2}+\tfrac{1}{3}+\ldots\tfrac{1}{1000} = 7\cdot 48547\ 08605\ 50344\ 91265\ 65182\ 04333\ 90017\ 65216$$
$$79169\ 70883\ 36657\ 73626\ 74995\ 76993\ 49165\ 20244$$
$$09599\ 34437\ 41184\ 50813+$$
$$E = {}\cdot 57721\ 56649\ 01532\ 86060\ 65120\ 90082\ 40243\ 10421$$
$$59335\ 93995\ 35988\ 05772\ 02455\ 61942\ 00508\ 15825$$
$$\left(\text{last term } +\frac{B_{15}}{30.1000^{30}}\right).$$

Hence we see that 54 decimal places are correct in the value of E (n being 200) last given in the paper dated March 2, 1867,—also that 59 decimals are correct in the value of E when $n=500$. When $n=1000$, probably 65 decimals in the value of E are correct.

When $n=1$, we readily find $E = {}\cdot 57 \left(\text{last term } -\frac{B_2}{4}\right)$.

,, $=2$, ,, $E = {}\cdot 57721 \left(\text{last term } -\frac{B_4}{8.2^8}\right)$.

,, $=5$, ,, $E = {}\cdot 57721\ 56649\ 015 \left(\text{last term } +\frac{B_{11}}{22.5^{22}}\right)$.

When $n=$	1,	E consists of	2 decimals.
,, $=$	10,	,,	21 decimals.
,, $=$	100,	,,	46 decimals.
,, $=$	1000,	,,	65 decimals, probably.
,, $n=$	2,	,,	5 decimals.
,,	20,	,,	28 decimals.
,,	200,	,,	54 decimals.
,, $n=$	5,	,,	13 decimals.
,,	50,	,,	39 decimals.
,,	500,	,,	59 decimals.

From the above we may fairly infer that when n is increased in a *geometrical* ratio, the corresponding number of decimals obtained in the value of E increases only in something like an *arithmetical* one, and that probably from 50,000 to 100,000 terms in the Harmonic Progression would require to be summed in order to obtain 100 places of decimals in the value of E, Euler's constant.

III. "On a Definite Method of Qualitative Analysis of Animal and Vegetable Colouring-matters by means of the Spectrum Microscope." By H. C. Sorby, F.R.S., &c. Received April 10, 1867.

CONTENTS.

1. *History.*

My attention was first directed to this subject by reading a report of Professor Stokes's very excellent lecture at the Royal Institution, Friday, March 4th, 1864. It immediately occurred to me that a spectroscope might be combined with a microscope, and employed to distinguish coloured minerals in thin sections of rocks and meteorites. I was soon led to examine many other coloured substances, and found that the instrument is more useful in connexion with qualitative analysis, when only very small quantities of material can be obtained. At first I employed the imperfect apparatus described in my Paper in the 'Quarterly Journal of Science'*, but afterwards, along with Mr. Browning, I constructed that described in my Paper in the 'Popular Science Review'†. For general purposes I do not think this could be much improved; but for chemical testing it is much less fatiguing to use a binocular instrument. There were many difficulties to contend with, but at length I constructed one which appears to answer all the requirements of the case.

2. *Apparatus.*

I have an ordinary large binocular microscope, and use an object-glass of about three inches focal length, corrected for looking through glass an inch thick; the lenses being at the top, so as to be as far as possible from the slit. This is placed at the focus, and between it and the lenses, at a distance of about half an inch from them, is a compound prism, composed of a rectangular prism of flint-glass, and two of crown-glass of about 61°, one at each end. This arrangement gives direct vision and a spectrum of the size most suitable for these inquiries, since a wide dispersion often makes the absorption-bands far too indistinct. In order to be able to compare two spectra side by side, a small rectangular prism is fixed over

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* April 1865, vol. ii. p. 198.

† Vol. v. p. 66.

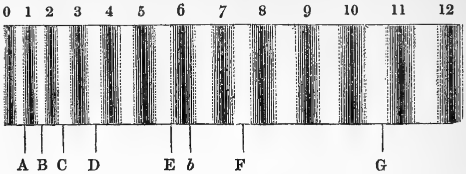
half the slit, and with the acute angle parallel to and just passing beyond it. This gives an admirable result, the only defect being that, when the spectra are in focus, their line of junction is some distance within it; and therefore to correct this I use a cylindrical lens of about two feet focal length, with its axis in the line of the slit, which can easily be fixed at such a distance between the slit and the prisms, as to bring the spectra and their line of contact to the same focus. In front of the slit, close to the small rectangular prism, is a stop with a circular opening, to shut out lateral light, and a small achromatic lens of about half an inch focal length, which gives a better field, and counteracts the effect of the concave surface of the liquid in the tubes used in the experiments, if they are not quite full. These are cut from barometer-tubes, having an internal diameter of about one-seventh of an inch, and an external diameter of about three-sevenths of an inch. They are made half an inch long, ground flat at each end, and fixed with Canada balsam on slips of glass two inches long and about six-tenths of an inch wide, so that the centre of the tube is about one-fourth of an inch from one edge. By this arrangement the liquid may be examined through the length of the tube by laying the slip of glass flat on the stage of the microscope, or through the side of the tube, by placing the slip vertical and the tube horizontal. Cells of this size can be turned upside down and deposits removed without any liquid being lost; and the upper surface of the liquid is sufficiently flat, even when inclined at a considerable angle. If requisite, small bits of thin glass can be laid on the top, which are held on by capillary attraction, or may be fastened with gold-size, if it be desirable to keep the solution for a longer time. When the depth of colour is too great in the line of the length of the cell, we can at once see what would be the effect of about one-fourth of the colour by turning it sideways; and thus we can save much time, and quickly ascertain what strength of solution would give the best result. Very frequently we obtain an excellent spectrum in one direction with one reagent, and in the other with another, without further trouble. I have constructed a small stage, too complicated to describe in writing, which enables me at once to examine solutions in two such tubes, either endways or sideways, and compare their spectra side by side, or to use test-tubes, or to fix the small apparatus which I have contrived for accurately measuring the spectra. This is of such great importance in these inquiries that I must describe it in some detail.

3. *Scale of Measurement.*

It consists of two small Nicol's prisms, and an intermediate plate of quartz. If white light, passing through two such prisms, without the plate of quartz, be examined with the spectrum-microscope, it of course gives an ordinary continuous spectrum; but if we place between the prisms a thick plate of quartz or selenite, with its axis at 45° to the plane of polarization, though no difference can be seen in the light with the naked eye, the spectrum is entirely changed. The light is still white, but it is made up of alternate black and coloured bands, evenly distributed over the whole spectrum. The number of these depends on the thickness of the depolarizing plate, so that we may have, if we please, almost innumerable fine black lines, or fewer, broader bands, black in the centre and shaded off at each side. These facts are of course easily explained by the interference of waves. It would, I think, be impossible to have a more convenient or suitable scale for measuring the spectra of coloured solids and liquids. If we use a micrometer in the eyepiece, an alteration in the width of the slit modifies the readings, and the least movement of the apparatus may lead to error, whereas this scale is not open to either objection. Besides this, the unequal dispersion of the spectrum makes the blue end too broad, so that a given width, as measured with a micrometer in the eyepiece, is not of the same optical value as the same width in the red. The divisions in the interference-spectrum bear, on the contrary, the same relation to the length of the waves of light in all parts of the spectrum, and no want of adjustment in the instrument alters their position. As will be seen from the drawing (fig. 1), the unequal dispersion makes the distance betweeen the bands in the blue about twice as great as in the red. The perfection of a spectrum would be one in which they were all at equal intervals; but possibly no such uniform dispersion could be produced. By having a direct-vision prism, composed of one of flint-glass of 60°, and two of crown-glass of suitable angle, we can place it over the eyepiece, and may diminish the dispersion at the blue end, or increase that at the red end, by turning it in one position or the other, and thus see either end to the greatest advantage. It is, of course, very easy to draw spectra on this principle, and give all parts equal prominence, and not an unduly compressed red, and an unduly expanded blue end. Thus drawn, the spectra are far more uniform in many of their characters, and some general laws are at once apparent that otherwise might have been entirely overlooked; and on this account I shall adopt this system in those figured in this paper. It is, in fact, merely representing the actual measurements by drawings, without being at the trouble of distorting them, so as to be like naturally distorted spectra.

Since the number of divisions depends on the thickness of the interference-plate, it became necessary to decide what number should be adopted. At first I thought that ten would be most suitable; but, on trying, it appeared to me too few for practical work. Twenty is too many, since it then becomes extremely difficult to count them. Twelve is as many as can be easily counted; it is a number easily remembered, gives sufficient accuracy, and has a variety of other advantages. With twelve divisions the sodium-line D comes very accurately at $3\frac{1}{2}$; and thus, by adjusting the plate so that a bright sodium-line is hid in the centre of the band, when the Nicol's prisms are crossed, it is accurately at $3\frac{1}{2}$, when they are arranged parallel, so as to give a wider field. The general character of the scale will be best understood from the following figure, in which I have numbered the bands, and given below the principal Fraunhofer lines.

Fig. 1.



The centre of the bands is black, and they are shaded off gradually at each side, so that the shaded part is about equal to the intermediate bright spaces. Taking, then, the centres of the black bands as 1, 2, 3, &c., the centres of the bright spaces are $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c., the lower edges of each $\frac{3}{4}$, $1\frac{3}{4}$, &c., and the upper $1\frac{1}{4}$, $2\frac{1}{4}$, &c. We can easily divide these quarters into eighths by the eye; and this is as near as is required in the subject before us, and corresponds as nearly as possible to $\frac{1}{100}$ part of the whole spectrum, visible under ordinary circumstances by gaslight and daylight. Absorption-bands at the red end are best seen by lamplight, and those at the blue end by daylight.

On this scale the position of some of the principal lines of the solar spectrum is about as follows:—

A	$\frac{3}{4}$	B	$1\frac{1}{2}$	C	$2\frac{3}{8}$	D	$3\frac{1}{2}$
E	$5\frac{11}{16}$	*b*	$6\frac{3}{16}$	F	$7\frac{1}{2}$	G	$10\frac{5}{8}$

At first I used plates of selenite, which are easily prepared, because they can be split to nearly the requisite thickness with parallel faces; but I found that its depolarizing power varies so much with the temperature, that even the ordinary atmospheric changes alter the position of the bands. Quartz cut parallel to the principal axis of the crystal is so slightly affected in this manner, as not to be open to this objection, but is prepared with far greater difficulty. The sides should be perfectly parallel, and the thickness about ·043 inch, and gradually polished down with rouge until the sodium-line is seen in its proper place. This must be done carefully, since a difference of $\frac{1}{10000}$ inch in thickness would make it decidedly incorrect. I have prepared such plates, corresponding to my own, and placed them in the hands of Mr. Browning and of Messrs. Becks, so that any one wishing to adopt a similar scale may be able to do so more accurately.

The two Nicol's prisms and the intervening plate are mounted in a tube and attached to a piece of brass in such a manner that the centre of the aperture exactly corresponds to the centre of any of the cells used in the experiments, which are all made to correspond in such a manner that any of them, or this apparatus, may be placed on the stage and be in the proper place without further adjustment, which, of course, saves much time and trouble.

4. *Symbols used to describe Spectra.*

In order to describe spectra in my note-book or in print, I have devised a simple notation, employing types in constant use. Instead of writing an account of this system, I here give a printed illustration, which will show that by this means it is easy to give in a single line all the essential particulars which would otherwise require a long and tedious description, or a number of drawings and woodcuts. Without some such method of measuring and recording spectra it would be almost impossible to carry on extensive inquiries.

The intensity of the absorption is expressed by the following types:—

Not at all shaded		Blank space.
Very slightly shaded	. . .	Dots with wide spaces.
Decidedly shaded	. . .	Dots closer together.
More shaded	...	Very close dots.
Strongly shaded, but so that a trace of colour is still seen	---	Three hyphens close.
Still darker	—	Single dash.
Nearly black	——	Double dash.

Except when specially requisite, only the symbols . . . --- — are employed for the sake of simplicity, and then as signs of the relative, rather than of the absolute, amount of absorption; and it is assumed that there is a gradual shading off from one tint to the other, unless the contrary is expressed. This is done by means of a small vertical line over the figure (see No. 11), which shows that there is a well-marked division between them. Definite narrow absorption-bands are indicated by * printed over their centre. This will be better understood by a description of the spectrum of deoxidized hæmatin.

Clear Red, Orange, and Yellow.	* Very dark band.	Clear Green.	* Much less dark band.	Clear Green and Blue.	Gradually shaded to very dark at the extreme blue end.
	4¼ — 5		5½ ... 6⅜		9 . . 10 --- 11 —

The following examples will show how simple or more complicated spectra may thus readily be printed and compared. I have chosen solutions of similar tint, in order to show that the spectra of those of nearly the same colour may be very different, or, if analogous, may differ in details easily expressed by the symbols. The colour of each is given after the name. Nos. 1, 8, 9, 10, 11, 12, and 13 can be kept for a long time sealed up in tubes, and the rest are easily prepared. I have in all cases chosen that strength of solution which gives either the most characteristic spectra, or those best suited for comparison with other allied colours.

1.	Cudbear in alum	(*Pink*)	3 . . . . . 8 11 . —
2.	Colour of Elder berries with citric acid.	(*Red Pink*)	4 . - 5¼—8 - - 9 . . . 11 - —
3.	Brazil-wood, with bicarbonate of ammonia.	(*Pink*)	4½ $\overset{*}{—}$ 5¾ 8
4.	Logwood, with bicarbonate of ammonia.	(*Pink*)	3⅝ $\overset{*}{—}$ 5¼ 7

The next four are spectra of blood, produced by the successive addition of the various reagents, as in detecting fresh stains.

5. Fresh Blood. (*Pale Scarlet*) $3\frac{1}{2} \overset{*}{\text{—}} 4\frac{3}{8} \quad 4\frac{3}{4} \overset{*}{\text{—}} 5\frac{3}{4} \quad 7\,.\,.\,8 \text{ - - } 9\text{—}$

6. Citric Acid then added. (*Pale Brown*) $1\frac{5}{8} \overset{*}{\ldots} 2\frac{1}{4} \quad 4 \ldots 8 \ldots 9 \text{ --- } 10 \text{ —}$

7. Ammonia then added. (*Pale Brown*) $3\frac{5}{8} \overset{*}{\ldots} 4\frac{3}{8} \quad 4\frac{7}{8} \overset{*}{\ldots} 5\frac{5}{8} \quad 7\,.\,.\,8 \text{ --- } 10\text{—}$

8. Deoxidized hæmatin, from blood-stain 2 years old. (*Pink*) $4\frac{1}{4} \overset{*}{\text{—}} 5 \quad 5\frac{1}{2} \overset{*}{\ldots} 6\frac{3}{8} \quad 9\,.\,.\,10 \text{ --- } 11 \text{ —}$

With these may be compared the two spectra which more nearly resemble those produced by blood than any I have yet seen.

9. Cochineal in alum. (*Pink*) $3\frac{3}{8} \overset{*}{\text{—}} 4\frac{1}{2} \ldots 5\frac{1}{8} \overset{*}{\text{---}} 6\frac{1}{8} \ldots 7\frac{1}{2}$

10. Alkanet-root in alum. (*Pink*) $3\frac{1}{2} \overset{*}{\text{—}} 4\frac{3}{8} \quad 5\frac{1}{4} \overset{*}{\ldots} 5\frac{3}{4}$

The following spectra of compounds derived from chlorophyll are as complicated as any I have met with.

11. Normal chlorophyll in alcohol. (*Deep Green*) $\frac{7}{8} \overset{*}{\text{—}} 2\frac{3}{8} \text{ --- } \overset{|}{3\frac{1}{4}} \ldots 4\frac{1}{2} \quad 6\frac{3}{4} \text{ . - - } 7\frac{1}{2}\text{—}$

12. Ditto, as decomposed by acids, or as found in some leaves. (*Olive Green*) $1 \overset{*}{\text{—}} 2\frac{1}{8} \quad 2\frac{3}{4} \overset{*}{\text{---}} 3\frac{3}{8} \quad 4\frac{1}{4} \ldots 5\frac{1}{4} \overset{*}{\text{---}} 5\frac{3}{4} \ldots 6\frac{3}{8} \overset{*}{\text{---}} 7\frac{3}{8} \quad 8\frac{1}{2} \text{ . - - } 9\frac{1}{2}\text{—}$

13. Ditto, as decomposed by caustic potash, and then by hydrochloric acid. (*Red-Green, Neutral Tint*) $\frac{1}{4} \overset{*}{\text{—}} \frac{3}{4} \quad 1\frac{1}{8} \overset{*}{\text{—}} 1\frac{3}{8} \quad 1\frac{7}{8} \overset{*}{\text{—}} 2\frac{1}{8} \quad 4\frac{1}{2} \overset{*}{\text{—}} 5\frac{1}{4} \ldots 9 \text{ - - } 10\text{—}$

5. *General Remarks on Absorption, &c.*

It appears to me that in adopting the undulatory theory of light it greatly simplifies the subject before us if we, to some extent, make use of the phraseology of acoustics. And thus, for example, I shall speak of two absorption-bands that occur, one nearer the *red*, and the other nearer the *blue*, end of the spectrum, as being relatively *lower* and *higher*. In a similar manner, if the addition of some reagent cause the absorption to increase towards the blue, and decrease towards the red, end, I shall describe it as *raising* the position of the absorption. We may also make some facts more intelligible by comparing them with the analogous phenomena of sound, and thus, for instance, may suppose that very narrow absorption-bands indicate that the ultimate particles of the substance will only take up vibrations of light of nearly one particular velocity, and that broad absorption-bands show that the particles have a much less definite rate of movement. Analogy would also lead us to infer that, when two spectra differ very decidedly, they must be due to different substances, or to the same in a different condition; whereas, if two spectra agree, they may be the same substance, or two distinct substances, whose different actions are made equal by particular circumstances. As an illustration, we may refer to a short string, which may give the same note as a longer whose tension is greater. For this reason we should be careful not to rely too much on one spectrum. If, however, we can produce some great physical change in both substances, and still their spectra remain the same under equal conditions, and if this occur uniformly in several different changes, we may conclude that they are identical. Hence the value of the various reagents named below. Many excellent illustrations of these principles could easily be given.

6. *General Method of Experiments.*

Since the spectrum-microscope enables us to use very small quantities, it appeared desirable to adopt such a method of research as would enable us to take full advantage of this circumstance, and to avoid as much as possible previous chemical manipulations. On this account I shall say nothing about modified chemical methods, which may, of course, be also employed when sufficient material is at command. My aim has been to contrive a special system of qualitative analysis of coloured substances applicable to minute quantities, and as independent of general chemistry as the blowpipe method is in the case of minerals. I may here say that in some very important practical applications to the detection of blood-stains not above $\frac{1}{100}$ of a grain was at disposal, and yet perfectly satisfactory results were obtained.

I was led to study the colouring-matters of flowers, leaves, fruits, woods, and roots, because it appeared a most admirable field of inquiry to teach the general principles of the subject. The colours being so various, and occurring under such complicated conditions, I thought that if methods could be devised to distinguish those that are dissimilar and to prove the identity of those that are alike, even when mixed with coloured impurities, such principles could easily be applied to other inquiries. If the question were merely to distinguish or compare absolutely pure colouring-matters, there would be little or no difficulty; but it appeared to me that one great value of the method would be to be able to apply it at once to very impure and mixed materials. In such cases mere colour is of very secondary importance, since that may be totally changed by a very small amount of impurity.

7. *Preparation of Colours.*

If the petals, leaves, &c. of plants be crushed in water, it very commonly happens that the colour is rapidly decomposed and no clear solution can be obtained; but if crushed in a moderate quantity of spirits of wine, and the solution squeezed out, filtered, and evaporated to dryness at a gentle heat, the colouring-matter does not decompose, even when redissolved in water and filtered to remove anything not soluble in that liquid. This clear solution should then be evaporated to dryness at a gentle heat in a small saucer, and kept *dry*; for then the colours often undergo no important change in the course of many months, whereas, when kept dissolved in water or alcohol, they may quickly decompose. I have thus prepared the colouring-matter of above a hundred different vegetable substances, some of which have become entirely changed, but a large number are apparently still unaltered. I have also kept a number of colours, sealed up in glass tubes, ready for direct examination, dissolved in alcohol, in strong syrup, or in alum. Many have decomposed, but many have kept perfectly well, or have merely faded, and still give excellent spectra after above a year. I have also prepared and kept in the same manner some animal colouring-matters, but comparatively few.

8. *Method of examination.*

The coloured substances are examined, when dissolved in water, alcohol, or other solvent, in the small glass cells already described, and the various reagents are added and mixed by means of a moderately stout platinum wire, flattened at one end and turned up square, like a little hoe. I have made many experiments in order to ascertain what reagents are most serviceable in developing characteristic spectra, and have at length concluded that for general purposes the following are the most convenient. Those which are solid are best kept in small bottles as coarse powder, and added to the small cells in a solid state, so that the quantity used may be more readily known.

9. *Reagents.*

Hydrochloric acid.
Citric acid.
Benzoic acid.
Boracic acid.
Bicarbonate of ammonia.
Carbonate of soda.
Diluted solution of ammonia.
Caustic potash.
Sulphite of soda.
Sulphate of protoxide of iron.
Alum.
Iodine dissolved in alcohol.
Bromine dissolved in water.
Solution of hypochlorite of soda.
Permanganate of potash.

This list might of course be very much extended, if we were to include such reagents as may be used in separating or decomposing colours by the ordinary chemical methods. In describing the effect of those named in this list, I feel that I could not avoid mentioning some well-known facts without breaking the thread of my argument; and therefore I trust it may not be thought out of place if I give a general account of the whole from the particular point of view required by the subject more especially before us.

The action of many reagents is so intimately related to different parts of the spectrum, as to show that there must be some connexion between so-called chemical reactions and optical phenomena. Not that their effect is absolutely the same in the case of all coloured substances, but generally only the *extent* differs, whilst the *character* of the change is uniform; unless, indeed, decomposition take place; and even then it has a tendency to conform to a general law.

10. *Solvents.*

Water and alcohol are the most useful solvents, and the spectra of the two solutions of the same substance often differ most strikingly; in fact they often behave in other respects as if they were solutions of different substances. Sometimes the spectra are absolutely identical, but often well-marked narrow absorption-bands are seen in the alcoholic solution, where they are almost, or quite, invisible in the aqueous. Very commonly the same bands are seen in both, but not exactly in the same place, alcohol sometimes raising them to a higher part of the spectrum, and sometimes depressing them. Occasionally the spectrum of the dry material is like that of the alcoholic solution, and unlike that of the aqueous, as if the difference were due to the presence of water, but in other cases it is unlike both. At all events the facts clearly show that a solvent has a most important action on the ultimate particles of the substance in solution, since it may produce a greater change in optical phenomena than even chemical combination. Undistilled hard water may act like a weak alkali.

11. *Acids and Alkalies.*

As far as optical phenomena are concerned, there is no absolute division between acids and alkalies; for we have every connecting link from the strongest acids to the strongest alkalies. In order to understand their action, it is most essential to distinguish between what may be called "general absorption" and "local absorption-bands." There may, perhaps, be no absolute line of division, but when seen to advantage they are affected in such a different manner that it is desirable to treat of each separately.

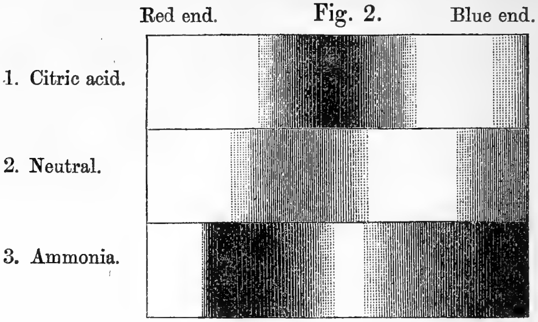
12. *General Absorption.*

As a good example of simple general absorption, we may take the crimson colouring-matter of the common Wallflower (*Cheiranthus Cheiri*), which is soluble in water, and, along with a yellow only soluble in alcohol, gives rise to the varied colours of the flowers.

When neutral, it is crimson.... $2\frac{3}{4}$....7 10..11-—
With ammonia, fine green .... $1\frac{3}{4}$—$4\frac{1}{2}$-..6 7.. 8-- 9—
With citric acid, deep pink .... $3\frac{1}{2}$.-$4\frac{1}{2}$—6-..$8\frac{1}{2}$ 11...

These facts will be better understood by means of the following drawing:—

Fig. 2.



Whence it will be seen that citric acid raises and greatly increases the central absorption, and ammonia lowers and also increases it. At the same time the absorption at the extreme blue end of the spectrum is raised by the acid almost to beyond the range of vision, but lowered to the centre of the spectrum by ammonia. Acids and alkalies of intermediate character, as, for example, boracic acid and bicarbonate of ammonia, produce intermediate effects. These well-known phenomena may be looked upon as typical of acids and alkalies, but the extent of their action varies for each particular colouring-matter, so that in some cases it is slight, and sometimes neither acids nor alkalies produce any effect. Their relative action on the central and upper absorption also varies very greatly in different colours. If there is no general absorption in the centre of the spectrum, when the colour is neutral, but only an absorption at the blue end, acids and alkalies act on it in precisely the same manner as on the absorption at the blue end in the case just described, raising or lowering it to an extent varying greatly according to the substance; and the same may be said of any general absorption at the red end. The reverse certainly occurs when an acid is added to chromate of potash, or excess of ammonia to a salt of copper; and, according to Stokes (Phil. Trans. 1862, p. 609), alkaloid bases usually show this reverse action. It may depend on the different properties of two distinct *compounds*, which does not appear to be the cause of the phenomena now under consideration. In the case of all the vegetable colouring-matters which I have examined, the tendency of acids is to raise, and of alkalies to depress, the general absorption in each part of the spectrum; the extent of this action depending on the strength and quantity of the reagents, and on the nature of each colouring-matter; and thus we have a general rule, and not several, as commonly adopted by chemists, each of very limited application; for instance, that vegetable blues are turned red by acids, and green by alkalies; and that vegetable yellows are reddened by alkalies. I may here remark that some colours would appear to be exceptions, if we did not remember that waves of light, or waves analogous to them, exist beyond the visible spectrum. Thus, for example, when alkalies are added to the yellow solution of Brazil-wood (*Cæsalpinia crista*), it is changed to pink, the absorption being so much lowered that the blues are transmitted; this clear space corresponding to what was probably a clear space beyond the blues visible under ordinary circumstances, but which would perhaps be seen, if examined in the manner described by Stokes in his paper on the long spectrum of electric light*.

13. *Fading of Solutions.*

One striking peculiarity in the action of acids on the solutions of many vegetable colours is, that, when they are in a particular state of acidity, they fade to nearly or quite colourless, without there being any decomposition. This is especially the case with pink colours dissolved in alcohol. It occurs slightly with blue colours, and little, if at all, with yellows. The aqueous solutions change much more slowly, but more and more rapidly the more they are diluted; and frequently attain a permanent depth of colour, which is dark or pale according as the solution is strong or dilute. Of course I here allude to the effect of the *same total amount of colour*, and not to the different effect of the same *quantity* of a strong or dilute *solution*. The alcoholic solutions obtained direct from the flowers often fade so rapidly, and become so nearly colourless, that any one might easily fancy that all the colour was lost by decomposition; and an evaporating dish containing it, might appear merely filled with brownish alcohol, and yet on evaporation the whole dish might be covered with a fine deep colour. The same change may occur over and over again, the deep-coloured solution first obtained soon fading, and the colour being restored by subsequent evaporation.

When such a colour is dissolved in a little water and added to alcohol in an experiment tube, the colour may at first be very deep, but may fade so rapidly that there is scarcely time to observe the spectrum before it passes into that molecular state which does not absorb any of the rays of light. The colouring-matter of the flowers of the red Salvia (*S. splendens*) is an excellent example. Neutral solutions do not undergo this rapid change; a different condition of acidity is requisite for different colouring-matters; and some do not change at all. A large excess of citric acid very often restores the intensity of the colour; and usually the absorption-bands are seen to the greatest advantage when the solution is in that state which rapidly fades; and by adding too much colour and watching whilst it fades, they may be seen and measured when at their best. This fading of a dark-coloured solution must not be confounded with the change which takes place on diluting some salts, as described by Dr. Gladstone in his paper on that subject†.

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* Phil. Trans. 1862, p. 599.

† Quart. Journ. Chem. Soc. vol. xi. p. 36.

14. *Absorption-bands.*

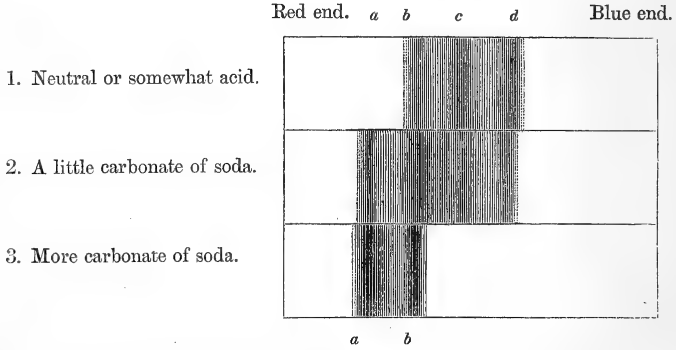
Though acids and alkalies thus, to a greater or less extent, alter the position of the general absorption, they act very differently on the special, local absorption to which it is very convenient to restrict the term "absorption-bands." Since I shall often have to speak of their being at equal intervals, it would be well to say that I have found it convenient to construct a wedge-shaped piece of quartz, cut parallel to the axis of the crystal, and to use it along with two Nicol's prisms in such a manner that the spectrum may be divided into any requisite number of equal portions, by interference-bands situated in any requisite position. This of course avoids the errors which so often happen when we compare together measurements that cannot be made with very great accuracy.

As an excellent illustration I select the colouring-matter of Alkanet-root (*Anchusa tinctoria*). It is insoluble in water, but is easily dissolved by alcohol, even when much diluted with water, and gives a clear pink solution. The spectrum is nearly the same when the colour is dissolved in absolute alcohol, as when much water is present, only each of the absorption-bands is situated rather higher. Thus, taking the centres of the bands, we have—

	b.	*c.*	*d.*
Absolute alcohol	$4\frac{3}{8}$	$5\frac{7}{8}$	$7\frac{3}{8}$
Very dilute alcohol	$4\frac{1}{4}$	$5\frac{3}{4}$	$7\frac{1}{4}$

The general spectrum of the solution in dilute alcohol will be best understood from the following figure, No. 1:—

Fig. 3.



Acids produce no important change, and the effect of alkalies is best seen by gradually adding carbonate of soda. This alters the colour to a more and more blue-purple, and the spectrum is changed in the manner shown in fig. 3. The three bands seen in the neutral solution may be referred to as *b*, *c*, and *d*, and their centres occur at equal intervals of about $1\frac{1}{2}$. When enough carbonate of soda has been added to make it slightly purple, a fourth band, *a*, makes its appearance, separated from *b* by the same equal interval of $1\frac{1}{2}$, whilst the other bands remain in the same position as at first, only modified in intensity. The band *a* becomes darker and darker as more carbonate is added, until, when the solution is a fine purple, it is as dark as the others (see No. 2); and on adding more carbonate it becomes still darker, and the bands *c* and *d* more faint, until the solution is a purple-blue; and the spectrum has only the two well-marked bands *a* and *b*, shown by No. 3.

The bright blue colouring-matter of the flowers of *Lobelia speciosa* gives, when *neutral,* almost exactly the same spectrum as that of Alkanet-root when *alkaline,* No. 3, having two well-marked absorption-bands, whose centres are at $2\frac{3}{4}$ and $4\frac{1}{4}$; and on adding carbonate of soda, the upper one is gradually removed, and the centre of the lower is depressed to near $2\frac{1}{2}$. More or less similar results occur in the case of many other blue colouring-matters; and on adding a slight excess of acid the general absorption is raised, and other bands may be developed higher up, at equal intervals; but when a large excess has been added, they are lost in a strong general absorption. Too strong an alkali may also destroy narrow bands in a similar manner, as is well seen in the case of Brazil-wood. The neutral aqueous solution shows an absorption-band, made far more distinct by the addition of bicarbonate of ammonia, which makes it pink and very fluorescent. The spectrum is then

$$\overset{*}{4\tfrac{3}{4}}\text{—}5\tfrac{3}{4}\;.\;.\;.\;.\;.\;7\,;$$

but on adding excess of ammonia the solution ceases to be fluorescent, the narrow absorption-band is lost, and the spectrum becomes

$$3\tfrac{1}{2}\;.\;\text{-}\;4\tfrac{1}{4}\text{—}8\;\text{-}\;.\;.\;9\tfrac{1}{2}.$$

Ammonia does not produce this effect when the colour is dissolved in alcohol, the solution remaining fluorescent and still giving the narrow band; and, as a general rule, that solvent greatly impedes or entirely prevents such changes, and on this account almost invariably shows absorption-bands to the greatest advantage.

We therefore see from the above examples that the absorption follows the general rule, and is raised by acids, and depressed by alkalies; but this only applies to the absorption when viewed as *a whole,* and not to the separate bands; for those reagents change their *intensity,* but not their *position.* In some cases, indeed, their position is slightly altered, so that perhaps it would be more correct to say that acids and alkalies may raise and depress the general absorption to an extent equal to a considerable fraction of its own great breadth; whereas they either do not change the position of narrow absorption-bands, or merely raise and depress them by a fraction of their own narrow width. It is their very definite position that makes them so useful in this method of analysis.

Unfortunately I have not hitherto been able to find a sufficient number of colouring-matters giving rise to three or more well-marked absorption-bands, to warrant a general conclusion; and therefore it is perhaps premature to conclude that their centres always occur at *equal intervals.* At the same time it is certainly a very common fact. When the maximum point of transparancy occurs between the different bands, there may be, as it were, a double interval; but then, sometimes, even this missing band may be seen under favourable conditions. A difference in the general absorption may also somewhat alter the apparent position of a band, if it is strongly shaded on one side, and not on the other; and the presence of impurities may also modify the results, so that absolute accuracy cannot be expected in all cases; and occasionally very narrow bands occur which appear to belong to a second system. It must be borne in mind that the bands are equidistant, only when measured by means of the interference-spectrum. Thus, in the case of Alkanet-root, when measured with a micrometer, instead of the intervals *a* to *b* and *c* to *d* being equal, they are related to one another about as $1\frac{1}{2}$ and 2, and thus the general law is entirely obscured. If subsequent research should prove that the bands are normally at equal intervals, it will be a fact of great value in deciding whether certain spectra are or are not due to a *mixture of colours*; since, if a band occurred at a perfectly unequal interval, it would show that there must be at least two substances. Even in the present state of our knowledge, any inequality should make us carefully search for some satisfactory reason for such a divergence from a common rule. My meaning will be better understood from the following examples.

If a little of the colour of Brazil-wood be added to the solution of Alkanet-root, the bands are not altered, and are seen at $4\frac{1}{4}$, $5\frac{3}{4}$, and $7\frac{1}{4}$; but a little bicarbonate of ammonia developes a well-marked band, whose centre is at $5\frac{1}{4}$, and therefore at an interval of 1, instead of $1\frac{1}{2}$. The same is also well seen in the case of a mixed solution of Brazil-wood and blue *Lobelia.* I therefore argue that if an unknown substance gave rise to similar spectra, with bands at unequal intervals, we ought strongly to suspect that it was either naturally composite, or that some new compound had been formed by decomposition. As a very good illustration I may refer to the product of the action of acids on chlorophyll. The band in the red is not at an equal interval; but, on careful examination, it is seen to be made up of two bands, the upper of which is at an equal interval. I was not aware that these were due to two different substances, but was led to think it very probable; and Professor Stokes informs me that he has proved it to be the case. As an illustration of another kind of exception, I refer to the colouring-matter of the pink Stock (*Matthiola annua*). The aqueous solution shows two bands, whose centres are at about $3\frac{5}{8}$ and $5\frac{1}{4}$; and on adding ammonia the upper is removed, and the lower depressed to $3\frac{1}{4}$. In spirit of wine they are at 4 and $5\frac{1}{2}$, and ammonia developes a third at 3, which are *not* equal intervals. However, if absolute alcohol be used, the bands are at $2\frac{3}{4}$, $4\frac{1}{8}$, and $5\frac{1}{2}$, which are *equal* intervals; and thus we see that the abnormal inequality is due to the presence of water, which causes the spectrum to be as if due to a mixture of two colours, when in reality it is the same colour dissolved in two solvents.

In spectra showing one absorption-band, there is very commonly a general absorption, extending from it towards the blue end; whereas it so seldom extends towards the red end that it is doubtful if it ever occurs in substances, undoubtedly not a mixture of two colours. It can, however, so easily occur in mixed colours, that any substance giving rise to such a spectrum is probably a mixture. Many illustrations might be given, but I will select Brazil-wood, and the same artificially mixed with the colour of beet-root. Adding bicarbonate of ammonia to both, we have—

Brazil-wood alone ...... $4\frac{3}{4}\overset{*}{—}5\frac{3}{4}\ldots 7$

Brazil-wood and beet-root $3\frac{1}{2}\ldots 4\frac{5}{8}\overset{*}{—}5\frac{3}{4}\ldots\ldots 8$

Here, then, the shading below the absorption-band from $3\frac{1}{2}$ to $4\frac{5}{8}$ is evidence of the second colour, and if such a mixture had occurred naturally its mixed character might easily have been overlooked. I have found many cases similar to this, and had proved that they were mixtures, before I was aware that the spectra indicated it. If these very common facts turn out to be general laws, we might thus detect at once the presence of as many as three different substances, or at all events might learn what further examination was desirable.

15. *Sulphite of Soda.*

Sulphite of soda is a most valuable reagent, and its action very remarkable. It enables us to divide colours into three groups, according as it produces a change in an ammoniacal, or acid solution, or in neither. The action is related in a very simple manner to the spectra. Having added an excess of ammonia, there may be a well-marked broad absorption over more or less of the red, orange, yellow, and upper green; and above this a clear transparent space, limited by a variable amount of absorption, extending downwards from the extreme blue. Fig. 2 will illustrate my meaning. In the case of one group of colours, the addition of sulphite of soda almost immediately removes the detached, broad absorption in the lower part of the spectrum, but leaves that at the blue end quite unchanged, or only slightly modified by the solution being made more alkaline. If, then, as in the case of magenta, there is no absorption at the blue end, sulphite of soda makes the solution quite colourless; whereas if the blues are absorbed, as in the case of the ammoniacal solutions of the colour of red roses, and some species of *Dianthus*, it changes the colour from green to yellow. If the absorption extends continuously down from the extreme blue to the orange, as often happens when ammonia is added to yellow colours, sulphite of soda produces no change. It is only when there is a more or less perfect *division* between the upper and lower absorption, that it has any effect; and then it simply and entirely removes the lower absorption. Some colours are changed immediately, even when a very small quantity of sulphite is added; but others require more and change gradually, though still very soon.

16. *Groups A, B, and C.*

Colours which are thus altered when the solution is ammoniacal, constitute my group A. Frequently, however, sulphite of soda does not remove the detached absorption when excess of ammonia is present, but does so when there is an excess of citric acid. These constitute my group B. As in the other group, any absorption which extends continuously from the extreme blue end is not altered, but the detached absorption in the green is almost immediately removed; and therefore a deep pink or red solution may at once become quite colourless, or only a very pale yellow; and in many cases this residual colour is due to some yellow colouring-matter mixed with the other. I have never seen a colour which was changed by sulphite when alkaline, and not when acid; and thus citric acid never restores the colour when it is added to the changed ammoniacal solution. Excess of ammonia usually restores the faded acid solution to nearly the original colour, and it is therefore not a case of actual decomposition, but merely the result of some remarkable molecular change. A third group of colours consists of those which are not almost immediately changed by sulphite of soda, when either alkaline or acid; and these I call group C. Some of them may fade on keeping several hours, and some do not fade even in several days, but they cannot thus be divided into two definite groups. When thus faded, ammonia does not restore the colour; and therefore it is evidently the effect of decomposition, and not like the mere molecular change met with in group B.

On the whole, the groups A, B, and C are remarkably distinct. There are, indeed, a few cases where the change takes place somewhat slowly; and a few scarlet colours do not show very distinctly the characteristic peculiarities of either B or C; but there are other very strong reasons for believing that some of these are really mixtures of different groups. Even if it should be found that perfectly simple colouring-matters may have, as it were, intermediate characters, such appear to be so rare that practically they may be classed with mixtures, until some reason be found for classing them together as exceptions.

These reactions of sulphite of soda are so much interfered with by the presence of alcohol, that it should never be employed as a solvent, unless the substance is insoluble in water; and then it should be diluted as much as possible, since the ordinary spirit of wine with an equal quantity of water is the extreme strength admissible, and even that very much delays the reaction. The effect of various other reagents is also sometimes very different, according to the nature of the solvent.

The three groups A, B, and C differ in other particulars. It is easy to change A or B into C by various reagents which produce decomposition, but I do not know a case where C can be changed into A or B. Caustic alkalies usually soon decompose colours belonging to group A, when dissolved in water, but act slowly on those of groups B and C. Usually colours of group C are far more permanent than those of groups A and B, and to it belong most of the vegetable colours used in dyeing, and nearly all yellows.

17. *Other Reagents.*

Boracic Acid.—The chief value of this reagent is that it gives nearly the same spectrum as that of a neutral solution when added after the addition of a slight excess of ammonia. It should therefore be well fused in a platinum crucible and recrystallized, so as to be quite free from any stronger acid.

Sulphate of Iron.—Sulphate of the protoxide of iron is chiefly useful as a deoxidizing agent, in the case of blood and a few analogous substances, taking care to have citric acid present to prevent the precipitation of the oxide by ammonia *.

Alum.—Alum has a remarkable influence on some colours, and it has the property of gradually restoring many after they have passed into the faded modification. Many colours also may be kept for a long time dissolved in a strong solution, sealed up in tubes; and it is occasionally an excellent solvent for substances insoluble in either water or alcohol. The chief objection to it as a reagent is that the spectra are so much influenced by the presence of ammonia, even when neutralized by an acid, that it is almost impossible to compare together different substances under exactly the same conditions.

Iodine and Bromine.—Iodine dissolved in alcohol, and bromine in water, are useful in producing decompositions, which may differ very considerably in colours which are otherwise very similar; as, for example, the yellow colouring-matters of the root of rhubarb and of turmeric. The iodine or bromine should be added in sufficient quantity, and then ammonia used to remove the excess, and thus avoid the effect of their own colour. The solution may then be made acid with citric acid, and should in both cases be compared with another tube to which no iodine or bromine has been added.

Hypochlorite of Soda.—This reagent, with or without the addition of citric acid, is sometimes useful, as for instance in detecting the adulteration of rhubarb with turmeric; but generally its action is too powerful and too uniform.

Permanganate of Potash.—This also usually acts too powerfully on colouring-matters. The excess can easily be removed by sulphite of soda, which makes an alkaline solution pale yellow, but an acid solution quite colourless.

18. *Grouping of Colours.*

Having now considered some of the chief peculiarities of the most useful reagents, I proceed to describe what appears to me to be the most convenient method of dividing colouring-matters into groups and subgroups, so as to enable us to ascertain the nature of any particular substance under

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* See Stokes's Paper, Proceed. Roy. Soc. vol. xiii. (1864) p. 355.

examination. The number of distinct coloured compounds met with in different plants is so great, that some such classification is imperative. In the first place, we cannot do better than divide them according as they are soluble in water or alcohol. This may be looked upon as a *chemical* division, and is very useful in practice. Thus—

Soluble in water and not precipitated by alcohol....	Division	1.
Soluble in water and precipitated by alcohol	„	2.
Insoluble in water but soluble in alcohol	„	3.
Insoluble both in water and alcohol	„	4.

Of course cases occur which cannot be unhesitatingly classed with any one of these; but they often form good practical divisions, and necessarily modify the methods requisite for further examination.

19. *Method and Order of Experiment.*

If a colour belongs to division 1, a small quantity, sufficient for three or four experiments, should be exposed to the vapour of ammonia in a watch-glass, until there is certainly no longer any *free acid*, and then gently evaporated, so that all excess of ammonia may be lost. If not thus made neutral we might be entirely misled; for some pink colours are blues reddened by an acid. A small quantity should then be dissolved in water in one of the small experiment-tubes and the spectrum observed. If too little colour has been added to give the characteristic spectrum, more should be introduced; but if any part is entirely absorbed, the cell should be turned sideways, in order to see whether or no some narrow absorption-band occurs there; and then it may be desirable to remove some of the solution, and fill up the cell with water. As a general rule, so much colour should be added as to make the darkest part of the spectrum decidedly shaded, but yet not so black as to hide any narrow bands; and if any occur, the solution should be made of such a strength as to show them to the greatest advantage. This can easily be done, after a little practice, and is made much easier by being able to turn the tubes sideways. Having noted the spectrum of the neutral solution, a very small quantity of ammonia should be added, and then a decided excess, the spectra being examined to see if there be any difference; for this is very often the case and very important facts may be overlooked if too great an excess be added at first. The addition of a small bit of sulphite of soda then shows at once whether or no a colour belonging to group A is present; and on adding excess of citric acid we may also determine whether it chiefly belongs to groups B or C. Ammonia should then be added in excess, which may or may not restore it to the same state as before the addition of the acid. To another portion of colour carbonate of soda should be added, and then excess of citric acid, both spectra being carefully observed; and finally sulphite of soda, which definitely shows whether or no there is any other colour than one belonging to group C. Combining the results of the two sets of experiments, we may decide whether it belongs to groups A, B, or C, or is a mixture of any of them. If the substance is insoluble in water but soluble in alcohol, the same experiments should be made, only we must add the colour dissolved in alcohol to as much water as can be used without making the solution turbid, and must remember how much the presence of alcohol may interfere with the action of some of the reagents.

Another portion of the neutral colour should then be dissolved in as strong alcohol as will give a clear solution, and ammonia, benzoic acid, a little citric acid, and much of it added one after the other; and all the spectra carefully observed, as well as any other facts which may present themselves.

By thus using three separate quantities of colour, and adding reagents one after the other, we may obtain about a dozen spectra, which may differ from one another in important particulars, or in some few cases may be all alike. The experiments are so easily made, that the whole series of twelve spectra may be seen in the space of five minutes; and the total quantity of material need not in some cases be more than $\frac{1}{1000}$ of a grain. The facts thus learned may show that for particular practical purposes some different method could be employed with advantage, and that only one or two simple experiments are needed. For example, suspected blood-stains should be treated in an entirely different manner, as described in my Paper on that subject*; and in examining dark-coloured wines, in order to form some opinion of their age from the relative quantity of the colour belonging to group C, gradually formed by the alteration of the original colouring-matter of the grape belonging to group B, it is only requisite to observe the effect of sulphite of soda after the addition of citric acid. It would, however, extend this Paper beyond the limits I have prescribed to myself, if I were to enter into practical applications, and I shall therefore merely give a description of a convenient method of grouping the various colours.

20. *Subgroups.*

Since the narrow absorption-bands are decidedly the most important means of identification, it appears to me that we cannot do better than adopt subdivisions founded on *their number*. We may thus divide each group A, B, and C into subgroups, in which the neutral aqueous solutions exhibit 0, 1, 2, 3, &c. decided absorption-bands. Sometimes one of them may be so obscure that we may hesitate whether it should be counted or not; but practically this is no very serious objection, if we decide to reckon only distinct bands, and to look on the fainter as important merely in identifying individual colours. If no absorption-band can be seen in the neutral solution, we may take into account those seen when more or less ammonia is added; and if none occur in either case, we may make use of those seen in the alcoholic solution when neutral, and after the addition of ammonia. Whenever in this order of experiments the solution gives

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* Quarterly Journal of Science, vol. ii. p. 205.

any decided absorption-band the subgroup is determined; and it is only when none has been produced that the process must be carried further.

The general connexion of the subgroups will be best seen from the following Table:—

$$1,\ \mathrm{A}\begin{cases} aq_0 \\ aq_1 \\ aq_2, \text{ \&c.} \end{cases} \begin{cases} am_0 \\ am_1 \\ am_2, \text{ \&c.} \end{cases} \begin{cases} al_0 \\ al_1 \\ al_2, \text{ \&c.} \end{cases} \begin{cases} am_0 \\ am_1 \\ am_2, \text{ \&c.} \end{cases}$$

The same system is applicable to each division, 1, 2, and 3, and to each group A, B, and C. We can easily express the subgroups by using one or more of the signs *aq*, *am*, *al*, *am*, with a figure to indicate the number of bands in the first term that contains any; those before it being given to show the facts more clearly.

Each colour can be indicated by writing after the subgroup the characteristic spectrum, or, for the sake of simplicity, merely the position of the *centres* of the bands, when they are seen as independent as possible of general absorption. If the centres of the bands are in different positions the colours cannot be the same; but if they agree it does not necessarily follow that they are the same. It is probable, but must be further proved by the correspondence of other spectra. As examples I give a few well-marked cases.

Purple Pansy	1, A, aq_0 am_1 (4).
Crimson Rose	1, A, aq_0 am_0 al_0 am_1 ($2\frac{1}{2}$).
Blue Lobelia (*L. speciosa*)	1, B, aq_2 ($2\frac{3}{4}$, $4\frac{1}{4}$).
Pink Stock (*Matthiola annua*)	1, B, aq_2 ($3\frac{5}{8}$, $5\frac{1}{4}$).
Several blue species of *Campanula*	1, B, aq_4 ($2\frac{3}{8}$, 4, $5\frac{5}{8}$, $7\frac{1}{4}$).
Brazil-wood (*Cæsalpinia crista*)	1, C, aq_1 ($5\frac{1}{4}$).
Logwood (*Hæmatoxylum campechianum*)	1, C, aq_1 ($4\frac{3}{8}$).
Sandalwood (*Pterocarpus santalinus*)	3, C, aq_2 (6, $7\frac{1}{2}$).
Alkanet-root (*Anchusa tinctoria*)	3, C, aq_3 ($4\frac{1}{4}$, $5\frac{3}{4}$, $7\frac{1}{4}$).

21. *Individual Colours.*

Having then ascertained to which subgroup any particular colour belongs, it is in the next place requisite to determine what particular substance it is. When it gives rise to well-marked absorption-bands, this may be more or less definitely decided by their exact position and character; since they may of course occur in different situations, or vary much in absolute and relative breadth and in intensity. Thus, choosing closely related spectra, we have, for example,—

1, B, aq_2

Blue *Lobelia speciosa*.......... $2\frac{1}{4}\overset{*}{\text{—}}3\frac{1}{4}\ldots3\frac{3}{4}\overset{*}{\text{—}}4\frac{5}{8} \qquad 11\ldots$

Pink *Matthiola annua* .... $2\frac{3}{4}\overset{*}{\text{---}}4\frac{1}{4}\ldots4\frac{1}{2}\overset{*}{\text{---}}5\frac{1}{2}\ldots8 \quad 10..11\text{—}$

1, C, aq_1

Logwood (*Hæmatoxylum campechianum*) $3\frac{5}{8}\overset{*}{\text{—}}5\frac{1}{4}\ldots7\text{--}8\text{—}$

Brazil-wood (*Cæsalpinia crista*) ...... $4\frac{1}{2}\overset{*}{\text{—}}5\frac{3}{4}..7\text{--}8\text{—}$

Such spectra are at once seen to differ most decidedly when compared side by side; and that the colouring-matters are entirely different is proved by other facts. If the absorption-bands agree very closely, we ought to compare other spectra before concluding that the substances are the same.

22. *Mixed Colours.*

Of course, if any impurity is present which absorbs that part of the spectrum where the characteristic bands occur, it may be difficult, or even impossible, to determine the nature of the substance; but the rest of the spectrum may be obscured, and the general colour entirely changed, without the least difficulty being thereby produced. Thus, for example, on adding a solution of Saffron (*Crocus sativus*) to that of the blue *Lobelia*, the colour is changed from blue to a curious olive, and the spectrum becomes—

Lobelia and Saffron........	$2\frac{1}{4}\overset{*}{\text{—}}3\frac{1}{4}\ldots3\frac{3}{4}\overset{*}{\text{—}}4\frac{5}{8}$	$6\frac{1}{2}$ . . 7- —
Lobelia	$2\frac{1}{4}\overset{*}{\text{—}}3\frac{1}{4}\ldots3\frac{3}{4}\overset{*}{\text{—}}4\frac{5}{8}$	11...
Difference		$6\frac{1}{2}$ . . 7- —

If we did not know it, we might thus infer that they were the same substance, and only differed because one contained a yellow colour; and this conclusion would be borne out by adding to each citric acid and sulphite of soda, which make the *Lobelia* colourless, and leave the residual yellow colour, $6\frac{1}{2}$ . . 7-—, in the case of the mixture. The petals of very many flowers do really contain more or less of such a yellow, which appears to be that developed to a much greater extent in the stamens, &c.; and though this often modifies the general colour and the spectra, its presence may be recognized in a similar manner. Different species of *Dianthus*, various kinds of Roses, and *Digitalis purpurea* are good examples of one general colouring-matter modified in this manner. Its normal character is

$$1,\ \mathrm{A},\ aq_0\ am_0\ al_0\ am_1\ (1\tfrac{3}{4}\overset{*}{\text{--}}\,2\tfrac{1}{4}\ldots4\tfrac{1}{2}\quad 11\,..).$$

In studying mixed colours, so much depends on their special characters, that it would be difficult to give any other general rules; and particular cases do not form part of the plan of the present paper.

23. *Spectra with no Bands.*

The principal difficulty to be contended with in this method of qualitative analysis, is in the case of the subgroups where no decided absorption-bands can be developed by any of the reagents. They can be easily divided into the groups A, B, and C, but the difficulty is to distinguish the separate colours, if we are not sure that they are pure and simple. Sometimes special facts may be of use; but, as a general rule, we are compelled to have recourse to the position and character of the general absorption. This requires a good deal of care, since a difference in the state of the solution may make the same colour differ more than two quite distinct colours. After trying a number of experiments, I find that the best spectra for comparison are those obtained by adding first a moderate excess of carbonate of soda, and then a considerable excess of citric acid. Both of these solutions change very slowly, and give well-marked spectra; whereas ammonia often causes decomposition, and weaker alkalies or acids give much more faint spectra, or such as rapidly fade. Closely related colours should be compared together, and made as nearly equal as possible after the addition of the carbonate, and then citric acid added in considerable, and nearly equal, excess. We thus can compare two different spectra; and even if the position of the absorption is the same in both cases, the relative intensity may vary considerably. Very closely allied colours may often be easily distinguished in this manner, and the only great difficulty is when coloured impurities are present. As an example, I give some colours belonging to subgroup 1, B, $aq_0\ am_0\ al_0\ am_0$.

	Carbonate of soda.		Citric acid.
Petals of Wallflower (*Cheiranthus Cheiri*)	$2\frac{1}{4}$.---.5	8..9--10—	$3\frac{3}{4}$.----.7
Dark grapes	$2\frac{1}{4}$..--$5\frac{3}{4}$...9	10..11--	$4\frac{1}{4}$..---..$8\frac{1}{2}$
Fruit of Elder (*Sambucus nigra*)	$2\frac{1}{2}$.--6..9	11...	$4\frac{1}{2}$..---..$8\frac{1}{2}$

The first differs entirely from the latter two, but they are so similar that it requires great care to be sure that they differ essentially. If it were quite certain that such colours were pure, it would not be difficult to distinguish them with confidence; but since they may contain coloured impurities, we must occasionally be content with results somewhat doubtful in questions of minute detail, which might not be of the least importance in some practical investigations.

24. *Yellow Colours.*

One of the best general methods of distinguishing yellow colours belonging to subgroup C, $aq_0\ am_0\ al_0\ am_0$, or those with bands which are much alike, is to make them as nearly as possible of the same tint when neutral, and then to add excess of ammonia, which may make them very unequal. For example—

	Neutral.	Ammonia.
Yellow Dahlia (*D. variabilis*)	8..9--10—	3...4-.-$4\frac{1}{2}$—
Yellow Calceolaria (*C. aurea-floribunda*)	7...9--11—	$6\frac{1}{4}$..$6\frac{3}{4}$--7—
Saffron (*Crocus sativus*)	7...8--11—	7..8--11—

The action of ammonia thus shows that they differ very much, but at the same time the *Calceolaria* might be a mixture of the other two, and this would have to be decided by other facts.

25. *Fading of Group C.*

Sometimes in examining colours of group C, advantage may be taken of the different rate at which their acid solutions decompose and fade, when a considerable quantity of sulphite of soda has been added to an acid solution. The two solutions should be made as nearly equal as possible in all respects, and then the rate of fading may prove that they are very different, or may show that one is a mixture. After fading, the addition of excess of ammonia may show valuable facts. For example, the colour of the root of the red beet (*Beta vulgaris*) is pink, but that of the leaves is red, the spectrum differing from that of the root merely in having the blue end much absorbed. On keeping acid solutions of both to which sulphite of soda has been added, that of the root becomes colourless, and that of the leaves yellow; and thus, considering that acid solutions of colours belonging to group C are very rarely pink, it is almost certain that the colour of the leaves is the same as that of the root, but modified by the yellow colour so common in leaves.

26. *Conclusion.*

Such, then, is a general outline of the method which I have hitherto found the most convenient in studying different colouring-matters, and for determining to what individual species any particular colour may belong. I need hardly say that it is just the sort of qualitative analysis to employ in detecting adulterations in many substances met with in commerce, as well as in inquiries where very small quantities of material are at command. By this method we might be able in a few minutes to form a very satisfactory opinion, or at least one that might meet all practical requirements, and even under unfavourable circumstances we might narrow the inquiry to a surprising extent; and if this can be said even now, surely further research cannot fail to make it most useful in cases where ordinary chemical analysis would be of little or no use.

The Society then adjourned over the Easter recess to Thursday, May 2.

May 2, 1867.

Lieut.-General SABINE, President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

William Baird, M.D.
W. Boyd Dawkins, Esq.
Baldwin Francis Duppa, Esq.
Albert C. L. G. Günther, M.D.
Julius Haast, Esq., Ph.D.
Captain Robert Wolseley Haig, R.A.
Daniel Hanbury, Esq.
John Whitaker Hulke, Esq.
Edward Hull, Esq.
Edward Joseph Lowe, Esq.
James Robert Napier, Esq.
Benjamin Ward Richardson, M.D.
J. S. Burdon Sanderson, M.D.
Henry T. Stainton, Esq.
Charles Tomlinson, Esq.

A letter from the Foreign Office, addressed to the President, was read, communicating the following paragraph from the 'Moniteur,' transmitted by H. M. Ambassador at Paris:—

"Paris, le 27 Mars.

"L'Empereur, dans sa sollicitude pour tout ce qui intéresse la science et les relations commerciales, a décidé que des officiers de la marine et des ingénieurs hydrographes seraient envoyés sur différents points du globe, dans le but de déterminer par des observations astronomiques un certain nombre de méridiens fondamentaux qui serviront à assurer la position géographique des lieux intermédiaires.

"Ce travail important permettra de corriger la Table des latitudes et longitudes insérée dans la *Connaissance des temps,* et dont les erreurs ont été signalées dans un rapport addressé au ministre de l'instruction publique par le président du Bureau des longitudes."

The following communications were read:—

I. "Optics of Photography.—On a Self-acting Focus-Equalizer, or the means of producing the Differential Movement of the two Lenses of a Photographic Optical Combination, which is capable, during the exposure, of bringing consecutively all the Planes of a Solid Figure into Focus, without altering the size of the various images superposed." By A. Claudet, F.R.S. Received April 8, 1867.

When a solid figure is brought too near the object-glass of a camera obscura, the difference of focus for its various planes is comparatively so great, that it is impossible that all the images should be equally well defined. Hence, in the case of photographic portraiture, there is a want of harmony in the representation of the various parts; some are too sharply delineated, and some others are confused in proportion as they are more and more distant from the plane in focus. But there is another defect which is the consequence of the difference of distance of the various planes bearing too great a proportion to the distance of the whole, which is that the nearest parts of the figure are too much enlarged, and the furthest too much reduced.

In a paper I read at the British Association at Nottingham in 1866, I proposed a plan to obviate these defects, which consisted in bringing all the planes consecutively into focus, by moving, during the exposure, the tube of the lens or the back frame of the camera; the consequence of which was, of course, that the planes were also during that movement brought out of focus; so that a sharp image of every plane was impressed upon a confused image; but they were all in the same degree in that mixed state, and the result was an equality of effect producing harmony in the whole, and that kind of softness in the picture so much approved by artists, as resembling, more than the sharpest photographs, the effect that they aim at producing.

The original simple idea of equalizing the focus of the various planes by moving either the frame holding the plate, or the tube of the lens, during the exposure had, it appears, occurred to several persons engaged in photographic pursuits (of which I was not aware before reading my paper); but it is certain that the plan had never been practically and generally adopted, and that, at all events, no specimens of the process had at any time been exhibited in public, probably because it presented several difficulties which could not be easily overcome. The greatest of these difficulties I soon found during my investigations, which was that, in changing the focal distances merely by moving the frame or the tube, the size of the various superposed images was unavoidably reduced or increased according to the alteration of focus during the movement applied.

Therefore I turned my attention to the means which might be found capable of avoiding this defect, and a fortunate idea presented itself, by which I found that it was possible to preserve the size of the various images during the adaptation of the focus to the different planes of the figure.

The desideratum was, when changing the focus, to increase the power of the double lens for the planes the most distant and to reduce it for the nearest planes. At first this seemed to be an impossibility. But in considering the subject attentively, I was suddenly struck with the fact that the power of any double combination of lenses being proportionate to the distance which separates the two lenses greater when they are more separated, and smaller when they are less separated, it was possible to alter the power of the combination by changing the distance between the two lenses.

Therefore, if, instead of moving the whole tube containing the two lenses, we move only the back lens nearer the plate, when we want to focus for more distant planes, we increase at the same time the power of the double combination, and consequently the size of the image; and if we move the lens further from the plate, when we want to focus for the nearest planes, in doing so, by reducing the separation of the two lenses, we reduce the power of the combination, and consequently also the size of the image. This is a most fortunate property; for by this means it is possible not only to equalize the definition of the various planes, but at the same time to equalize the size of their images, and consequently to avoid the exaggeration of perspective by which the nearest planes are increased, and the furthest disproportionately reduced, a defect which is so detrimental to the appearance of large photographs.

I submitted my plan to M. Voigtlander, the celebrated optician, and I had the satisfaction to meet with his entire approbation. He found that I had solved the problem in a way which was perfectly correct and sufficient in practice. But wishing to investigate the question from a higher mathematical point of view, and being unable from indisposition to go himself into the subject, he charged his step-son, Dr. Sommer, Professor of Mathematics at the Carolinian College of Brunswick, well versed in all the questions of optical photography, to calculate the result of the gradual increase and reduction of the power of the double combination, in conjunction with the alteration of focus. Dr. Sommer entered thoroughly into the subject, and soon sent me a series of elaborate formulæ, showing that, although for all practical purposes in photography the movement of one of the two lenses, as I had proposed, fulfilled the object I had in view, still he found that a more scientific consideration of the subject called for a modification in my plan; which was that, instead of moving only one of the lenses, the same degree of their separation should be imparted by moving the two lenses in contrary directions from the fixed centre of the combination, and in different proportions, according to the distance of the object. These differential proportions were indicated in a table calculated by Dr. Sommer which he sent me.

This presented another difficult and unexpected problem, the solution of which was indeed most perplexing. But I did not like that it should be said that my plan was not completely in accordance with the mathematical laws of optics; and I set to work at finding a mechanical means by which I could avail myself of the scientific calculations of Dr. Sommer.

I have found such means; and it turns out indeed that by my mechanical construction the differential movement can be effected, not only as readily and easily, but with a greater command and steadiness than by moving only one lens. The following is a description of the arrangement:—

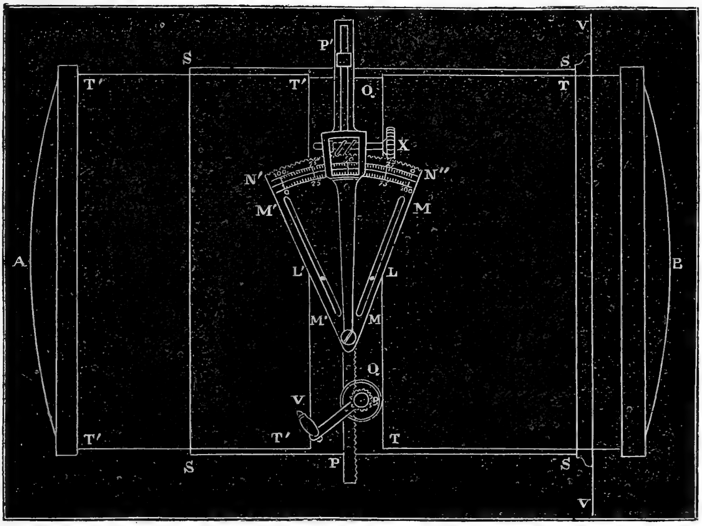
Description of the Focus-Equalizer.

The tube, containing at each end the lenses A and B, is divided into two parts, sliding in the principal tube S S S S fixed in the front of the camera at V V′.

Each tube has a strong pin, L and L′. These two pins are intended to push the tubes to and fro from the centre of the combination on the line P P′ by means of the mechanical piece N N′ N″ in the shape of a sextant, having two slits, M M and M′ M′, cut at an angle of 36°. Now the sextant, being mounted on a sliding bar *q q′*, fixed in a socket holding to the tube S S S S at P P′, can be made to move to and fro on the line P P′ by means of a rack and pinion moved by a handle V on the axis R. While the sextant moves in the line P P′, the two slits will act on the two pins, and gradually increase the separation of the tubes; and on making the sextant move back from P′ to P, the slits will bring the two pins nearer each other, and decrease the separation of the tubes.

It will thus be easily understood how we can increase and reduce the separation of the two lenses from the centre of the combination; but we have now to explain how we can produce the differential movement according to the mathematical formulæ calculated by Dr. Sommer.

The arc of the sextant is divided into 100 parts, in two rows one against the other. The divisions on the outer limb have their zero on the left, and the 100 division on the right; on the inside limb the divisions are in a contrary direction.



By means of the endless screw X acting on the toothed edge of the sextant, it can be moved on its horizontal axis, so that any of its divisions may be brought under the index fixed on the middle bar $q\,q'$.

Now, supposing that by the table of Dr. Sommer the lens A for a certain distance of the object should move 0·235, and the lens B 0·765 of the whole space by which the lenses require to be separated or approximated, we turn the endless screw until the index is on the $23\frac{1}{2}$ division of the inside scale, and of course on the $76\frac{1}{2}$ division of the outside scale.

In that position of the sextant the slits M M and M′ M′, by means of the pins attached to the tubes of the lens A and of the lens B, will make them accordingly move—A in the proportion of 0·235, and B in the proportion of 0·765 of the whole space.

If for another distance the lens A should have to move 0·333, and the lens B 0·666, setting both limbs of the sextant to these divisions, the lens A will move $\frac{1}{3}$, and the lens B $\frac{2}{3}$ of the whole space.

If we wanted to move the two lenses in the same proportion, the sextant should be set so that the 50th division of both scales should be under the index.

And, finally, if, for the sake of comparative experiments, it were wanted to move only the lens A or the lens B, the slit of the pin for either and the zero of the scale should be placed under the index, by which that lens would be completely stationary, and the whole motion imparted to the other.

II. "On the Genera *Heterophyllia, Battersbyia, Palæocyclus,* and *Asterosmilia*; the Anatomy of their Species, and their Position in the classification of the Sclerodermic Zoantharia." By Dr. P. M. Duncan, Sec. G.S. Communicated by Prof. Huxley. Received March 30, 1867.

(Abstract).

Although the practical and natural classification of the Madreporaria (Sclerodermic Zoantharia) which has been submitted by MM. Milne-Edwards and Jules Haime is very generally admitted to be the best, still there are great gaps in the succession of the genera, and, moreover, some genera cannot be placed.

The "break" between the Turbinolides and the Astræides is so great as to render the classification rather artificial; but Dr. Duncan's discovery of a genus *Asterosmilia,* comprising several species, unites these great divisions. The new genus has the peculiarities of the Trochocyathi, but in addition it is furnished with an endotheca. The species are described.

The genera *Heterophyllia,* M^cCoy, and *Battersbyia,* Milne-Edwards and Jules Haime, are amongst those *incertæ sedis.* The discovery of several new species of *Heterophyllia* enables Dr. Duncan to determine the anatomy of the genus, to offer for consideration the most extraordinary coral form he has ever seen, and to ally the genus with *Battersbyia,* which he proves had no cœnenchyma. The species of both of the genera are described shortly, and the development and reproduction of *B. gemmans* also. The genera are placed amongst the Astræidæ.

The genus *Palæocyclus,* M.-E. & J. H., supposed to be one of the Fungidæ, is proved to be a vesiculo-tubulate coral genus, and to be one of the Cyathophyllidæ.

One Mesozoic family is therefore removed from the Palæozoic coral-fauna, and two genera of a Mesozoic division are introduced. They foreshadow the Thecosmiliæ of the Trias.

III. "Contribution to the Anatomy of *Hatteria* (*Rhynchocephalus,* Owen)." By Albert Günther, M.A., Ph.D., M.D. Communicated by Prof. Owen. Received April 4, 1867.

(Abstract.)

The skull of *Hatteria* is distinguished by the following characters:—

1. Persistence of the sutures, especially of those between the lateral halves of the skull, combined with great development of its ossified parts, as it appears in the expanse of the bones forming the upper surface of the facial portion, in the completeness of an orbital ring with a temporal and zygomatic bar (Crocodilia), in the much expanded columella, in the nearly completely osseous bottom of the orbit, and roof of the palate.

2. Sutural union of the tympanic with the skull; firm and solid union of the bones of the palate with the tympanic, as shown by the sutural connexion of tympanic and pterygoid, broad sutural connexion of the columella with tympanic and pterygoid, immoveable pterygo-sphenoid joint, firm and extensive attachment of pterygoid to ectopterygoid.

3. This restriction of the mobility of the bones named is compensated by an increased and modified mobility of the lower jaw, the mandibles being united by ligament, and provided with a much elongate articular surface.

4. Displacement of the palatine bones, which are separated by the pterygoids, and replace a palatal portion of the maxillaries.

5. Perforation of the tympanic; extremely short postarticular process of the mandible.

The *vertebral* column and the *remainder of the skeleton* show the following peculiarities:—

1. Vertebræ amphicœlian; caudal vertebræ vertically divided into two equal halves. Points of minor importance are the uniform development of strong neural spines, and the direction of the caudal pleurapophyses which point forwards.

2. The costal hæmapophyses are modified, first, into a series of appendages identical in position with the uncinate processes of birds; and secondly, into a double terminal series connecting the ribs with the thoracic and abdominal sterna, the distal pieces being much dilated and forming the base of a system of muscles (retractors of the abdominal ribs).

3. The development of a system of abdominal ribs, standing in intimate and functional relation to the ventral integuments.

4. Continuity of the ossification of the coracoid; presence of an acromial tuberosity of the scapula; subvertical direction of the os ilium.

5. The arrangement of the bones of the limbs and their muscles does not show any deviation from the Lacertian type.

The *dentition* of *Hatteria* is unique. That of young examples differs scarcely from the dentition of other acrodont lizards. In adult examples the intermaxillaries are armed with a pair of large cutting-teeth; a part of the lateral teeth are lost, and the alveolar edges of the jaws are cutting and highly polished, performing the function of teeth. A series of palatine teeth is in close proximity and parallel to the maxillary series, both series receiving between them in a groove the similarly serrated edge of the mandible.

As regards the *organs of sense*, the absence of the pecten of the eye and of the tympanic cavity, the commencement of a spiral turn of the cochlea, and the attachment of the hyoid bone to the terminal cartilage of the stapes—are to be noticed.

The structure of the *heart* and of the organs of *respiration* and *circulation* are of the Lacertian type.

The *absence of a copulatory organ* is a character by which *Hatteria* is distinguished from all other Saurians. Thus *Hatteria* presents a strange combination of elements of high and low organization, and must be regarded as the type of a distinct group. Its affinities and systematic position may be indicated in the following synopsis of RECENT REPTILIA :—

- I. SQUAMATA.
  - First order. *Ophidia.*
  - Second order. *Lacertilia.*
    - Suborder A. *Amphisbænoidea.*
    - Suborder B. *Cionocrania.*
    - Suborder C. *Chamæleonoidea.*
    - Suborder D. *Nyctisaura.*
  - Third order. *Rhynchocephalia.*
- II. LORICATA.
  - Fourth order. *Crocodilia.*
- III. CATAPHRACTA.
  - Fifth order. *Chelonia.*

IV. "On the Curves which satisfy given conditions." By Prof. CAYLEY, F.R.S. Received April 18, 1867.

(Abstract.)

The present memoir relates to portions only of the subject of the curves which satisfy given conditions; but any other title would be too narrow: the question chiefly considered is that of finding the number of the curves which satisfy given conditions; the curves are either curves of a determinate order r (and in this case the conditions chiefly considered are conditions of contact with a given curve), or else the curves are conics; and here (although the conditions chiefly considered are conditions of contact with a given curve or curves) it is necessary to consider more than in the former case the theory of conditions of any kind whatever. As regards the theory of conics, the memoir is based upon the researches of Chasles and Zeuthen, as regards that of the curves of the order r, upon the researches of De Jonquières: the notion of the quasi-geometrical representation of conditions by means of loci in hyper-space is employed by Salmon in his researches relating to the quadric surfaces which satisfy given conditions. The papers containing the researches referred to are included in a list subjoined. I reserve for a separate second memoir the application to the present question, of the Principle of Correspondence.

V. "Second Memoir on the Curves which satisfy given conditions; the Principle of Correspondence." By Professor Cayley, F.R.S. Received April 18, 1867.

(Abstract.)

In the present memoir I reproduce with additional developments the theory established in my paper "On the Correspondence of two points on a Curve" (London Math. Society, No. VII., April 1866); and I endeavour to apply it to the determination of the number of the conics which satisfy given conditions; these are conditions of contact with a given curve, or they may include arbitrary conditions Z, 2Z, &c. If, for a moment, we consider the more general question where the Principle is to be applied to finding the number of the curves C^r of the order r, which satisfy given conditions of contact with a given curve, there are here two kinds of special solutions; viz., we may have proper curves C^r touching (specially) the given curve at a cusp or cusps thereof, and we may have improper curves, that is, curves which break up into two or more curves of inferior orders. In the case where the curves C^r are lines, there is only the first kind of special solution, where the sought for lines touch at a cusp or cusps. But in the case to which the memoir chiefly relates, where the curves C^r are conics, we have the two kinds of special solutions, viz., proper conics touching at a cusp or cusps, and conics which are line-pairs or point-pairs. In the application of the Principle to determining the number of the conics which satisfy any given conditions, I introduce into the equation a term called the "Supplement" (denoted by the abbreviation "Supp."), to include the special solutions of both kinds. The expression of the Supplement should in every case be furnished by the theory; and this being known, we should then have an equation leading to the number of the conics which properly satisfy the prescribed conditions; but in thus finding the expression of the Supplements, there are difficulties which I am unable to overcome; and I have contented myself with the reverse course, viz., knowing in each case the number of the proper solutions, I use these results to determine *à posteriori* in each case the expression of the Supplement; the expression so obtained can in some cases be accounted for readily enough, and the knowledge of the whole series of them will be a convenient basis for ulterior investigations.

May 9, 1867.

Lieut.-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Mr. Webster proposed, and Mr. Heywood seconded, the Right Hon. Sir William Bovill, Lord Chief Justice of the Common Pleas, for election and immediate ballot.

The ballot having been taken, the Lord Chief Justice Bovill was declared duly elected a Fellow of the Society.

The following communications were read:—

I. "On the Development and Succession of the Teeth in the Marsupialia." By William Henry Flower, F.R.S., F.R.C.S., &c., Conservator of the Museum of the Royal College of Surgeons of England.

(Abstract.)

Although the dentition of adult individuals of all the animals which constitute the remarkable Order or, rather, Subclass Marsupialia, has been repeatedly subjected to examination, and described with exhaustive minuteness of detail, it is a singular circumstance that most of those peculiarities in the succession of their teeth which distinguish them from other mammals appear hitherto to have escaped observation. To supply this blank is the object of the present communication. Fortunately the materials at my disposal, although not quite so complete as might be desired, are yet amply sufficient to illustrate the main aspects of the question, and to supply a result as interesting as it was unexpected.

Descriptions are given in the paper, accompanied by drawings, of several stages of the dentition of members of each of the six natural families into which the order is divided.

1. *Macropodidæ.*—The dentition of the Kangaroo (genus *Macropus*), from the completely edentulous fœtus to adult age, is described in detail. Contrary to what has been specially stated with regard to this genus, there are no deciduous or milk-incisors, the teeth of this group which are first formed and calcified in both jaws being those which are retained throughout the life of the animal. The rudimentary canine and first premolar have also no deciduous predecessors. The second tooth of the molar series (a true molar in form) is vertically displaced by a premolar. The four true molars have, as has long been known, no deciduous predecessors. There is thus but one tooth on each side of each jaw in which the phenomenon of diphyodont succession occurs. The period at which this takes place varies in different species of the family. In some forms of *Hypsiprymnus*, the successional premolar is not cut until after the last true molar is in place and use,—this probably having relation to the extraordinary size of the tooth, and the time consequently required for its development. A special characteristic of this family is the tendency to lose the canine and one or both premolars at a comparatively early period of life.

2. *Phalangistidæ.*—Several early stages of the dentition of *Phalangista vulpina* are described and figured. In a young specimen in which no teeth had cut the gum, the crowns of the permanent incisors, canine, and first two molars were found to be calcified, and the germ of the permanent premolar was already formed beneath the milk- or deciduous molar, which, as in *Macropus*, is the only tooth which is shed and replaced by a successor. The change takes place at an earlier period than in the last family.

3. *Peramelidæ.*—No very early stages of *Perameles* were examined; but

adolescent specimens of this genus and of *Chæropus* show that a very minute, compressed, molariform tooth is replaced by the triangular, pointed, third or posterior premolar. No other signs of vertical displacement and succession were observed.

4. *Didelphidæ.*—In the American genus *Didelphys*, the observations are complete from the earliest stage, and show that, as in the Australian *Macropodidæ* and *Phalangistidæ*, none of the teeth of the permanent series have predecessors except the compressed pointed last premolar, which replaces a tooth having the broad multicuspidate crown of a true molar. This change does not occur until the animal approaches the adult age.

5. *Dasyuridæ.*—In a fœtal *Thylacinus*, in which no teeth had cut the gum, the crowns of the permanent incisors, canines, premolars, and anterior true molars were partially calcified, and necessarily much crowded together in the jaw. A very minute rudimentary molar was situated just beneath the alveolar mucous membrane, superficially to the apex of the hindermost premolar, and was evidently its milk-predecessor.

6. *Phascolomyidæ.*—This family is placed last because the observations regarding it are less complete than in the case of any of the others. The youngest Wombat available presented no evidence of succession of any of the teeth; but it is probable that the single premolar is preceded by a milk-molar, at a still earlier period than any examined.

From the foregoing observations it may be concluded with tolerable safety that the animals of the Order Marsupialia present a peculiar condition of dental succession, uniform throughout the order, and distinct from that of all other mammals. This peculiarity may be thus briefly expressed. The teeth of Marsupials do not vertically displace and succeed other teeth, with the exception of a single tooth on each side of each jaw. The tooth in which a vertical succession takes place is always the corresponding or homologous tooth, being the hindermost of the premolar series*, which is preceded by a tooth having the characters, more or less strongly expressed, of a true molar.

It has been usual to divide the class Mammalia, in regard to the mode of formation and succession of their teeth, into two groups—the *Monophyodonts*, or those that generate a single set of teeth, and the *Diphyodonts*, or those that generate two sets of teeth; but even in the most typical diphyodonts the successional process does not extend to the whole of the teeth, always stopping short of those situated most posteriorly in each series. The Marsupials occupy an intermediate position, presenting as it were a rudimentary diphyodont condition, the successional process being confined to a single tooth on each side of each jaw. This position, however, is by no means without analogy among the mammals of the placental series. In the Dugong and the existing Elephants the successional process is limited

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* The convenient distinction between false molars or premolars and true molars, is always well marked in the form of the crown, especially in the upper jaw, in the Marsupials.

to the incisor teeth. It is questionable whether the first premolar of those animals of this group which have four premolar teeth, as the Hog, Dog (mandible), &c., ever has a deciduous predecessor, at all events so far advanced as to have reached the calcified stage. But the closest analogy with the marsupial mode of succession is found among the Rodents. Here the incisors appear to have no deciduous predecessors; and in the Beaver, Porcupine, and others, which have but four teeth of the molar series, *i. e.* three true molars and one premolar, the latter is, exactly as in the Marsupials, the only tooth which succeeds a deciduous tooth. The analogy, however, does not hold in those Rodents which have more than one premolar, as the Hare; for in this case each of these teeth has its deciduous predecessor.

In the preceding account I have used the term "permanent" for those teeth which remain in use throughout the animal's life, or, if they fall out (as do the rudimentary canines and the premolars of the *Macropodidæ*), do not give place to successional teeth; and I have therefore assumed that the milk or temporary dentition of the typical diphyodont mammals is represented in the Marsupials only by the deciduous molars. It may be held, on the other hand, that the large majority of the teeth of the Marsupials are the homologues of the milk or first teeth of the diphyodonts, and that it is the permanent or second dentition which is so feebly represented by the four successional premolars. This view is supported by many general analogies in animal organization and development, such as the fact that the permanent state of organs of lower animals often represents the immature or transitional condition of the same parts in beings of higher organization.

Looking only to the period of development of the different teeth in some of the marsupial genera, we might certainly be disposed to place the successional premolar in a series by itself, although, indeed, all its morphological characters point out its congruity with the row of teeth among which it ultimately takes its place, the reverse being the case with its predecessor. It is, however, almost impossible, after examining the teeth of the young Thylacine described and figured in the paper, to resist the conclusion originally suggested. The unbroken series of incisors, canines, premolars, and anterior true molars of nearly the same phase of development, with posterior molars gradually added as age advances, form a striking contrast to the temporary molar, so rudimental in size, and transient in duration. I can scarcely doubt that the true molars of this animal would be identified by every one as homologous with the true molars of the diphyodonts, which are generally regarded as belonging to the permanent series, although they never have deciduous predecessors. Now, if the homology between the true molars of the Thylacine and those of a Dog, for instance, be granted, and if the anterior teeth (incisors, canines, and premolars) of the Thylacine be of the same series as its own true molars, they must also be homologous with the corresponding permanent teeth of the Dog.

It may be objected to this argument, that the true molars of the diphyodonts, not being successional teeth, ought to be regarded as members of the first or milk- series; but, in truth, the fact that they have themselves no predecessors does not make them serially homologous with the predecessors of the other teeth, while their morphological characters, as well as their habitual persistence throughout life, range them with the second or permanent series.

We have been so long accustomed to look upon the second set of teeth as an after-development or derivative from the first, that it appears almost paradoxical to suggest that the milk- or deciduous teeth may rather be a set superadded to supply the temporary needs of mammals of more complex dental organization. But it should be remembered that, instead of there being any such relation between the permanent and the milk-teeth as that expressed by the terms "progeny" and "parent" (sometimes applied to them), they are both (if all recent researches into their earlier development can be trusted) formed side by side from independent portions of the primitive dental groove, and may rather be compared to twin brothers, one of which, destined for early functional activity, proceeds rapidly in its development, while the other makes little progress until the time approaches when it is called upon to take the place of its more precocious *locum tenens.*

Many facts appear to point to the milk-teeth as being the less constant and important of the two sets developed in diphyodont dentition. Among these the most striking is the frequent occurrence of this set in a rudimentary and functionless or, as it were, partially developed state. The milk-premolars of some Rodents (as the Guinea-pig), shed while the animal is *in utero,* the simple structure and evanescent nature of the milk-teeth of the Bats, Insectivores, and Seals, the diminutive first incisors of the Dugongs and Elephants, all appear to be cases in point. On the other hand, examples of the commencing or sketching out, as it were, of the successors to a well-formed, regular, and functional first set of teeth, are rarely, if ever, met with. Occasional instances of the habitual early decadence, or, perhaps, absence of some of the second or so-called permanent teeth occur in certain animals; but these are rather examples of the disappearance or suppression of organs of which there is no need in the economy, and chiefly occur in isolated and highly modified members of groups in the other members of which the same phenomenon does not occur, as in *Cheiromys* among the Lemurs, *Trichechus* among the Seals, and the recent Elephants (as regards the premolars) among the Proboscideans. They form no parallel to the cases mentioned above of the rudimentary formation of an entire series of teeth of the temporary or milk-set.

To return to the marsupials:—If this view be correct, I should be quite prepared to find, in phases of development earlier than those yet examined, some traces either of the papillary, follicular, or saccular stages of milk-predecessors to other of the teeth besides those determinate four in which, for some reason at present unexplained, they arrive at a more mature growth*. Such proof as this would alone decide the truth of these specu-

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* It may be remarked that the milk-tooth, which alone is developed in the Marsu-

lations; and I have not at present either the requisite leisure or materials for following out so delicate an investigation. I trust that the facts already elicited are sufficiently novel and important to justify my bringing them, as they now stand, before the Society.

II. "On a Property of Curves which fulfil the condition $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$." By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS.L. & E. Received April 9, 1867.

1. In a paper "On Stream-Lines," published in the Philosophical Magazine for October 1864, I stated, and, in a Supplement to the same paper, published in the Philosophical Magazine for January 1865, I proved the proposition that "all waves in which molecular rotation is null begin to break when the two slopes of the crest meet at right angles."

2. I have now to state the purely geometrical proposition of which that mechanical proposition is a consequence. *If a plane curve which fulfils the condition* $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$ *cuts itself in a double point, it does so at right angles.*

3. The following is the demonstration. It is well known that the inclination of any plane curve to the axes at an ordinary point is given by the equation

$$\frac{d\phi}{dx}dx+\frac{d\phi}{dy}dy=0;$$

also that at a double point $\frac{d\phi}{dx}$ and $\frac{d\phi}{dy}$ both vanish, so that the inclinations of the two branches to the axes are given by the two roots of the quadratic equation

$$\frac{d^2\phi}{dx^2}.dx^2+2\frac{d^2\phi}{dx\,dy}.dx\,dy+\frac{d^2\phi}{dy^2}.dy^2=0;$$

whence it follows that the product of the two values of $\frac{dy}{dx}$, which are the two values of the tangent of the inclination to the axis of x, is $=\frac{\frac{d^2\phi}{dx^2}}{\frac{d^2\phi}{dy^2}}$. In a curve which fulfils the before-mentioned condition, the value of that product is -1; and when such is the case with the product of the tangents of two angles, the difference of those angles is a right angle; therefore the two branches cut each other at right angles. Q.E.D.

4. The proposition just demonstrated is so simple and so obvious, that

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pials, corresponds homologically with that which, as a general rule, is most persistent in the typical diphyodonts, including Man, viz. the posterior milk-molar, replaced by the posterior permanent premolar.

I was at first disposed to think it must have been known and published previously; and had I not been assured by several eminent mathematicians that it had not been previously published to their knowledge, I should not have ventured to put it forth as new.

Supplement to the preceding Paper. Received April 23, 1867.

Professor Stokes, D.C.L., has pointed out to me an extension of the preceding theorem, viz. *that at every multiple point in a plane curve which fulfils the condition* $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$, *the branches make equal angles with each other*; so that, for example, if n branches cut each other at a multiple point, they make with each other $2n$ equal angles of $\frac{\pi}{n}$.

The following appears to me to be the simplest demonstration of the extended theorem. At a point where n branches cut each other the following equation is fulfilled by all curves:

$$\left(dx\frac{d}{dx}+dy\frac{d}{dy}\right)^n\phi=0.$$

Let θ be the angle made by any branch with the axis of x; then

$$\left(\cos\theta\frac{d}{dx}+\sin\theta\frac{d}{dy}\right)^n\phi=0.$$

But in a curve which fulfils the equation $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}=0$, we have

$$\frac{d}{dy}=\sqrt{-1}\,.\frac{d}{dx};$$

whence it follows that in such a curve the equation of a multiple point of n branches is

$$\left\{(\cos\theta+\sqrt{-1}\,.\sin\theta)\frac{d}{dx}\right\}\phi=0.$$

Choose for the axis of x a tangent to one of the branches at the multiple point. Then it is evident that the preceding equation is satisfied by the $2n$ values of θ corresponding to the $2n$th roots of unity, that is to say, by

$$\theta=0,\quad\frac{\pi}{n},\quad\frac{2\pi}{n},\ \&c.,\ldots\frac{(2n-1)\pi}{n};$$

therefore the n branches make with each other $2n$ equal angles of $\frac{\pi}{n}$. Q.E.D.

III. "A Tabular Form of Analysis, to aid in tracing the Possible Influence of Past and Present upon future states of Weather." By S. ELLIOTT HOSKINS, F.R.S., &c. Received March 28, 1867.

The data upon which the present communication is founded are derived from the 'Greenwich Reports,' from Mr. Glaisher's papers in the Philosophical Transactions, and from my own observations at Guernsey. The latter were commenced in the autumn of 1842, in accordance with the recommendations of the Committee of Physics of the Royal Society, and were taken at the request of Professor Daniell, by whom the instruments employed were selected. These instruments, made by Newman, were after a time replaced by others, at the suggestion of Mr. Glaisher, by whom they were compared with the standards at the Royal Observatory.

For my own guidance in the first instance, I sought to arrange the results thus obtained in such a manner as to discover, if possible, whether any month or class of months stood to each other in the relation of cause and effect; in other words, whether the atmospheric conditions of autumn exercised any distinguishable influence upon the fruitful or unfruitful character of ensuing seasons.

In order to attain this object, the principle seemed to be that of condensing within narrow limits, by means of intelligible symbols, as many elements of weather, in the popular acceptation of the word, as might be required. But the ordinary curvilinear form of diagram could not be so modified as to answer this purpose, and I therefore availed myself of a plan suggested by Mr. Galton:—that of converting the records of observations into appropriate signs, and placing them compactly in a series of squares.

Upon this principle the annexed diagrams* are constructed, comprising those elements of weather which more directly affect vegetation; viz. heat, cold, dryness, moisture, and their combinations. The same kind of preparatory steps were taken for the compilation of the Greenwich as for the Guernsey diagram, so as to render the results comparable—less, perhaps, for the sake of mere comparison, than for the purpose of testing the value of the latter by means of an accredited standard.

The first process consisted in copying out the degrees of monthly mean temperature, the number of rainy days, and the days of wind, from four directions, intermediate to the cardinal points. These several copies being verified, the monthly average of each of the above elements for twenty years, from 1843 to 1862, was taken.

The next step was to obtain the difference between the adopted average and the mean of each month in every year. By prefixing the plus and minus signs to the resulting figures, the excess and defect of each element is shown.

The third process was to separate the above-mentioned series of years

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* [The diagrams are not published, but are preserved for reference in the Archives of the Society.]

TAB

[*To face* p. 470.

ays in Excess and Defect of an average of Twenty Years, at Guernsey.

Average years	pril.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
	13	11	10	11	11	12	18	17	17	
First decade.	+7	+9	+ 7	−2	− 0	− 6	+ 6	+4	− 1	
	−9	−7	− 4	−0	+ 3	− 0	+ 1	−1	− 8	
	−3	−1	− 2	+4	− 0	+ 3	− 9	−0	+ 7	
	+3	+1	− 2	+1	+ 5	− 0	+ 7	−4	− 0	
	+2	+6	− 3	−4	− 3	+ 1	− 2	−1	+ 4	
	+6	−8	+10	+2	+13	− 3	+ 5	+2	− 0	
	+8	+1	− 7	+2	− 7	+ 2	+ 2	−0	+ 5	
	+9	+5	− 4	+2	+ 2	− 1	− 0	+4	−12	
	+2	−3	− 5	−0	− 2	− 5	− 0	+4	− 6	
	−6	+2	+ 7	−5	− 1	+ 2	− 0	+7	+ 8	
Months {	3	4	7	5	6	6	5	5	6	62
Deg	7	6	3	5	4	4	5	5	4	58
Second decade.	−1	−4	+ 9	+2	− 2	+ 1	+ 1	−7	− 1	
	−8	+7	+ 2	−2	− 5	− 8	− 1	+4	+ 4	
	−7	+3	− 1	+3	− 5	− 6	+ 3	−3	− 4	
	+2	−1	− 4	+1	+ 3	+ 2	− 9	−3	+ 6	
	+5	−2	− 2	−6	− 0	+ 4	+ 1	−8	− 4	
	−3	−3	− 1	−0	− 4	+ 1	− 2	−1	+ 3	
	+2	+5	+ 2	−5	− 1	+ 7	+ 6	−1	+ 1	
	−1	+6	+11	−3	+12	+ 1	− 4	−3	+ 6	
	−6	−7	+ 3	+8	− 1	+ 4	− 8	+7	− 6	
	−2	+1	− 0	+1	− 2	+ 1	+ 2	−1	+ 2	
Months {	7	5	5	5	8	2	5	8	4	66
Deg	3	5	5	5	2	8	5	2	6	54
	−7	−4	+4	−7	+2	+ 5	+ 1	+1	− 4	
	−4	−8	+3	−3	−6	+ 9	−10	+4	− 4	
	−4	+5	−6	−4	+3	−10	+ 2	+2	− 7	
	+1	+3	−1	−3	+2	+16	−10	+1	+ 1	

TABLE I.—Monthly Mean Temperature in Excess and Defect of an average of Twenty Years, at Guernsey.

		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
Average of 20 years ……		43·5	42·5	43·7	47·2	51·9	56·7	60·5	60·7	58·4	54·2	48·3	45·4	
First decade.	1843.	+ 0·8	− 2·0	+ 1·0	+ 1·1	− 0·2	− 1·7	+ 0·3	+ 0·9	+ 3·1	−0·2	+ 1·3	+ 2·9	
	44.	+ 2·3	− 0·6	+ 1·7	+ 4·6	+ 1·0	+ 2·2	+ 0·3	− 0·7	+ 1·8	+0·2	+ 1·0	− 4·6	
	45.	− 0·4	− 2·6	− 4·5	− 0·3	− 0·3	+ 2·4	− 0·5	− 1·3	− 0·9	−0·1	+ 2·5	+ 1·4	
	46.	+ 3·1	+ 4·5	+ 2·9	+ 1·7	+ 2·9	+ 6·5	+ 2·6	+ 2·4	+ 3·8	+0·7	+ 1·3	− 3·7	
	47.	− 1·4	− 1·4	− 0·3	− 0·6	+ 2·7	+ 0·3	+ 3·0	+ 1·0	− 0·2	+0·9	+ 3·1	+ 1·8	
	48.	− 2·8	+ 3·3	+ 2·3	+ 2·0	+ 5·8	+ 0·1	+ 0·4	− 1·2	+ 0·9	+0·1	− 0·4	+ 2·8	
	49.	+ 2·2	+ 4·4	+ 1·8	− 1·1	+ 1·6	+ 2·2	+ 1·0	+ 2·1	+ 0·5	+0·2	+ 1·3	− 0·9	
	50.	− 4·0	+ 3·6	− 0·3	+ 0·7	− 0·6	+ 1·9	+ 0·7	− 0·1	− 0·1	−3·7	+ 1·5	+ 0·8	
	51.	+ 3·2	+ 1·1	+ 1·8	+ 0·1	− 0·4	+ 1·9	+ 4·5	+ 2·1	+ 0·4	+0·1	− 3·8	− 0·5	
	52.	+ 1·9	+ 1·1	− 0·5	− 0·9	− 0·3	− 1·3	+ 3·4	+ 0·2	− 0·8	−3·0	+ 3·7	+ 4·6	
Months	Warm	6	6	6	6	5	8	9	6	6	6	8	6	78
	Cold	4	4	4	4	5	2	1	4	4	4	2	4	42
Degrees		+13·5	+18·0	+11·5	+10·2	+14·0	+17·5	+16·2	+ 8·7	+10·5	+3·2	+15·7	+14·3	+153·3
		− 8·6	− 6·6	− 5·6	− 2·9	− 1·8	− 3·0	− 0·5	− 3·3	− 2·0	−7·0	− 4·2	− 9·7	− 55·2
Second decade.	1853.	+ 2·5	− 5·4	− 4·9	− 0·7	− 1·4	− 2·2	− 2·5	− 2·1	− 2·5	−0·8	− 0·2	− 4·3	
	54.	− 0·6	− 0·1	+ 0·9	+ 0·8	− 1·2	− 4·0	− 1·9	− 0·3	+ 0·1	−0·7	− 1·8	+ 1·0	
	55.	− 3·4	− 6·0	− 2·3	− 2·8	− 3·1	− 2·9	− 1·3	− 0·2	− 0·9	−0·5	− 2 2	− 2·5	
	56.	+ 0·5	+ 1·5	− 1·5	− 0·5	− 2·6	− 1·4	− 1·6	+ 0·9	− 2·1	+0·7	− 0·8	+ 1·2	
	57.	− 0·8	− 0·8	− 0·5	− 1·3	− 0·2	+ 1·2	+ 1·2	+ 1·9	+ 2·1	+1·6	+ 2·6	+ 3·2	
	58.	− 0·8	− 1·5	− 0·7	+ 1·2	− 1·9	+ 1·7	− 2·8	− 0·6	+ 1·7	+0·4	− 2·6	+ 0·8	
	59.	+ 1·6	+ 3·1	+ 2·6	+ 1·0	− 1·3	+ 0·2	+ 4·6	+ 2 2	− 0·4	+0·3	+ 0·2	− 2·1	
	60.	+ 1·3	− 3·5	− 1·4	− 4·6	+ 1·8	− 4·1	− 4·3	− 4·7	− 4·1	−1·1	− 2·3	− 3·1	
	61.	− 4·7	+ 1·2	+ 1·6	− 1·0	− 1·0	− 1·4	− 2·8	− 0·4	− 0·8	+3·3	− 1·7	− 0·1	
	62.	− 0·2	+ 0·9	+ 1·2	+ 0·4	+ 0·4	− 2·5	− 3·4	− 2·1	− 0·8	+0·5	− 2·4	+ 2·0	
Months	Warm	4	4	4	4	2	3	2	3	3	6	2	5	42
	Cold	6	6	6	6	8	7	8	7	7	4	8	5	78
Degrees		+ 5·9	+ 6·7	+ 6·3	+ 3·4	+ 2·2	+ 3·1	− 5·8	+ 5·0	+ 3·9	+6·8	+ 2·8	+ 9·2	+ 60·1
		−10·5	−17·3	−11·3	−10·9	−12·7	−18·5	−20·6	−10·4	−11·6	−3·1	−14·0	−12·1	−153·0
	1863.	+ 1·4	+ 2·4	+ 1·4	+ 0·6	− 1·7	− 2·6	− 1·3	− 0·4	− 3·8	−1·0	+ 0·3	− 2·2	
	64.	− 1·8	− 3·2	+ 1·1	+ 1·1	+ 1·4	− 2·4	− 1·4	− 2·0	− 0·7	−0·9	− 0·9	− 2·5	
	65.	− 1·7	− 1·7	− 4·7	+ 2·7	+ 0·7	+ 0·8	+ 0·5	− 1·3	+ 4·4	+2·2	+ 1·2	+ 0·4	
	66.	+ 2·7	+ 2·1	− 1·4	+ 0·5	− 2·0	+ 0·4	− 2·1	− 2·2	− 2·6	+0·7	+ 1·3	+ 2·6	
	67.	− 3·5	−	−	−	−	−	−	−	−	−	−	−	

TABLE II.—Number of rainy days in Excess and Defect of an average of Twenty Years, at Guernsey.

		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
Average of 20 years ……		18	13	15	13	11	10	11	11	12	18	17	17	
First decade.	1843.	+3	+ 5	− 4	+7	+9	+ 7	−2	− 0	− 6	+ 6	+4	− 1	
	44.	−2	+12	+ 6	+9	−7	− 4	−0	+ 3	− 0	+ 1	−1	− 8	
	45.	−2	− 6	+ 2	−3	−1	− 2	+4	− 0	+ 3	− 9	−0	+ 7	
	46.	+2	− 3	+ 5	+3	+1	− 2	+1	+ 5	− 0	+ 7	−4	− 0	
	47.	−3	+ 3	− 3	+2	+6	− 3	−4	− 3	+ 1	− 2	−1	+ 4	
	48	−2	+11	+11	−6	−8	+10	+2	+13	− 3	+ 5	+2	− 0	
	49.	+2	− 0	− 7	+8	+1	− 7	+2	− 7	+ 2	+ 2	−0	+ 5	
	50.	−0	+ 3	− 6	+9	+5	− 4	+2	+ 2	− 1	− 0	+4	−12	
	51.	+3	− 6	+ 3	+2	−3	− 5	−0	− 2	− 5	− 0	+4	− 6	
	52.	+7	− 3	−11	−6	+2	+ 7	−5	− 1	+ 2	− 0	+7	+ 8	
Months	Dry	5	5	5	3	4	7	5	6	6	5	5	6	62
	Wet	5	5	5	7	6	3	5	4	4	5	5	4	58
Second decade.	1853.	+ 2	+7	+ 3	−1	−4	+ 9	+2	− 2	+ 1	+ 1	−7	− 1	
	54.	+ 2	−1	−10	−8	+7	+ 2	−2	− 5	− 8	− 1	+4	+ 4	
	55.	−10	−0	+ 5	−7	+3	− 1	+3	− 5	− 6	+ 3	−3	− 4	
	56.	+ 2	−3	−10	+2	−1	− 4	+1	+ 3	+ 2	− 9	−3	+ 6	
	57.	+ 8	−7	− 4	+5	−2	− 2	−6	− 0	+ 4	+ 1	−8	− 4	
	58.	− 7	−2	− 2	−3	−3	− 1	−0	− 4	+ 1	− 2	−1	+ 3	
	59	− 2	−0	+ 7	+2	+5	+ 2	−5	− 1	+ 7	+ 6	−1	+ 1	
	60.	+ 4	−0	+ 4	−1	+6	+11	−3	+12	+ 1	− 4	−3	+ 6	
	61.	− 9	+2	+ 3	−6	−7	+ 3	+8	− 1	+ 4	− 8	+7	− 6	
	62.	− 4	−4	+ 5	−2	+1	− 0	+1	− 2	+ 1	+ 2	−1	+ 2	
Months	Dry	5	8	4	7	5	5	5	8	2	5	8	4	66
	Wet	5	2	6	3	5	5	5	2	8	5	2	6	54
	1863.	+3	− 1	−3	−7	−4	+4	−7	+2	+ 5	+ 1	+1	− 4	
	64.	−7	+ 1	+1	−4	−8	+3	−3	−6	+ 9	−10	+4	− 4	
	65.	+6	+ 5	−2	−4	+5	−6	−4	+3	−10	+ 2	+2	− 7	
	66.	+6	+11	+1	+1	+3	−1	−3	+2	+16	−10	+1	+ 1	
	67.	+3	−											

of Twenty Years, at Guernsey.

		ugust.		September.				October.				November.				December.			
		S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.
1843.	3	10	2	12	8	7	3	6	9	11	5	7	10	7	6	7	8	10	6
44.	3	−3	+4	+3	+1	−5	+1	−4	+5	+2	−3	−0	−1	+4	−3	−0	−5	+2	+3
45.	1	+2	−2	+6	−4	−1	−1	−1	+2	+2	−3	−0	−6	+6	−0	+12	−6	−8	+2
46.	2	−1	−1	+1	+1	−0	−2	−4	+5	−1	−0	−0	−4	+2	+2	−6	+6	+6	−6
47.	2	−1	−1	+3	−0	−3	−0	−0	+2	+1	−3	+3	−4	+2	−1	+6	+5	−5	−6
48.	4	−6	−2	−9	+9	+1	−1	+3	−4	−0	+1	−5	+4	−3	+4	−3	+3	−3	+3
49.	1	+7	−1	+4	−2	−4	+2	+2	−2	+4	−4	−3	+8	−1	−4	−0	−3	+4	−1
50.	8	−5	−0	+4	−4	−2	+2	+4	−4	+2	−2	−3	−1	+4	−0	+3	+1	−2	−2
51.	3	+3	−2	+5	−3	−1	−1	+1	+4	−2	−3	−2	−0	+6	−4	−1	−1	−1	+3
52.	2	−0	−1	+7	−2	−3	−2	+4	−1	−0	−3	−2	+10	−3	−5	+11	−2	−3	−6
	5	+7	−1	−5	+6	−3	+2	+6	−0	−3	−3	−4	−3	+4	+3	−7	−3	+8	+2
1853.																			
54.	3	−3	−0	−5	+3	+3	−1	−2	−2	−1	+5	+6	−4	−4	+2	+10	−1	−9	−0
55.	1	−6	−0	+4	−4	−1	+1	−0	+5	−4	−1	−0	+4	−2	−2	−7	+17	−4	−6
56.	9	+4	+1	+6	−2	−3	−1	−6	+4	+2	−0	+6	−2	−4	−0	+1	+4	−6	+1
57.	4	−1	+2	−5	+3	+2	−0	+7	−6	−5	+4	−1	+8	−4	−3	−4	+4	−1	+1
58.	6	−1	+1	−6	−5	+8	+3	−3	−3	−1	+7	+4	−7	−2	+5	−4	−5	+4	+5
59.	8	−6	−1	−3	−4	+4	+3	+5	−4	−1	−0	+10	−8	−5	+3	−2	−3	+3	+2
60.	1	−6	+3	−8	+4	+4	−0	+1	−0	−3	+2	−1	−4	+1	+4	+1	−4	+5	−2
61.	2	+8	−0	−4	+1	+2	−1	−3	+1	−0	+2	+6	−6	−3	+3	−2	+1	−3	+4
62.	3	+4	+3	−10	+1	+6	+3	+6	−6	−5	+5	−4	−0	+7	−3	+4	+1	−4	+1
	1	−1	+1	−0	−1	+1	−0	−2	−0	+1	+1	+4	−1	−3	−0	−6	+4	+3	−1
1863.																			
64.	5	+8	+1	−11	+5	+6	−0	−2	−4	+3	+3	−4	−5	+6	+3	−5	+6	+2	−3
65.	1	−3	−1	−8	−0	+9	−1	+10	−6	−4	−0	+2	−0	−0	−0	+5	−4	−4	+3
66.	4	+4	−2	−0	−2	−2	+4	−0	−0	−0	−0	−0	−2	+2	−0	+2	−5	−1	+4
67.	1	−0	+2	−10	−2	+10	+2	+8	−4	−6	+2	−4	+1	+7	−4	−6	+1	+4	+1
		—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

cond decade.—1853 to 1862.

		January.	February.	July.	August.	September.	October.	November.	December.		Warm.		Cold.			Warm. a+b	Cold. c+d
											Dry.	Wet.	Dry.	Wet.			
											a	b	c	d			
	1843.	B	D	d	c	d	d	c	c		0	1	5	6		1	11
	44.	A	D	c	c	a	c	d	b		3	1	4	4		4	8
	45.	C	C	d	c	c	d	c	c		0	0	8	4		0	12
	46.	B	A	d	b	d	a	c	b		2	3	4	3		5	7
	47.	C	D	a	a	b	b	a	a		5	2	3	5		7	5
	48.	C	B	c	c	b	a	c	b		3	2	7	0		5	7
	49.	B	A	a	a	d	b	a	a		5	4	0	3		9	3
	50.	C	B	c	d	d	c	c	d		0	2	5	5		2	10
	51.	B	A	d	c	d	a	d	c		1	2	5	4		3	9
	52.	B	A	d	c	d	b	c	b		2	4	4	2		6	6
Warm	Dry A	1	4	2	2	1	3	2	1		21					42	
	Wet B	5	2	0	1	2	3	0	4			21					
Cold	Dry C	4	1	3	6	1	2	6	3				45				78
	Wet D	0	3	5	1	6	2	2	2					33			

		October.		November.		December.		Total.	
	l.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
First decade. A		6	4	8	2	6	4	78	42
Second ,, a		6	4	2	8	5	5	42	78

	.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
First decade. A		5	5	5	5	6	4	62	58
Second ,, a		5	5	8	2	4	6	66	54

Table III.—Direction of Wind. Number of Days in Excess and Defect of an average of Twenty Years, at Guernsey.

	January.				February.				March.				April.				May.				June.				July.				August.				September.				October.				November.				December.			
	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.
	6	9	9	7	8	8	8	4	10	9	7	5	10	9	7	4	12	8	7	4	8	9	10	3	8	10	11	2	6	13	10	2	12	8	7	3	6	9	11	5	7	10	7	6	7	8	10	6
1843.	-4	+7	+1	-4	+5	-3	-4	+2	+2	-5	-0	+3	-6	+3	+3	-0	-5	+1	+1	+3	+5	-0	-2	-3	-6	+4	+1	+1	+2	-3	-3	+4	+3	+1	-5	+1	-4	+5	+2	-3	-0	-1	+4	-3	-0	-5	+2	+3
44.	+4	+5	-3	-6	-4	+5	+2	-2	-1	-1	+4	-2	+5	+1	-2	-4	+13	-3	-6	-4	-1	-1	+1	+1	-0	+4	-3	-1	-1	+1	+2	-2	+6	-4	-1	-1	-1	+2	+2	-3	-0	-6	+6	-0	+12	-6	-8	+2
45.	-5	-0	+2	+3	+3	+3	-6	-0	+6	-0	-2	-4	+3	-2	-1	-0	-1	+4	-0	-3	+2	-0	-0	-2	-0	-5	+5	-0	-0	+2	-1	-1	+1	+1	-0	-2	-4	+5	-1	-0	-0	-4	+2	+2	-6	+6	+6	-6
46.	-4	+3	+2	-1	-3	+3	-1	+1	-7	+4	+5	-2	-1	+2	-0	-1	-0	+1	+3	-4	+12	-4	-5	-3	-3	-1	+2	+2	+4	-2	-1	-1	+3	-0	-3	-0	-0	+2	+1	-3	+3	-4	+2	-1	+6	+5	-5	-6
47.	+4	-4	-3	+3	+2	+1	-1	-2	+3	-3	-2	+2	-8	+7	+3	-2	-7	-0	+4	+3	+3	-2	-0	-1	+5	+2	-6	-1	+4	+4	-6	-2	-9	+9	+1	-1	+3	-4	-0	+1	-5	+4	-3	+4	-3	+3	-3	+3
48.	+7	-0	-5	-2	-6	+3	+7	-3	-7	+8	+1	-2	-0	-1	+2	-1	+10	-3	-5	-2	-3	-2	+7	-2	+2	-2	+1	-1	-5	-1	+7	-1	+4	-2	-4	+2	+2	-2	+4	-4	-3	+8	-1	-4	-0	-3	+4	-1
49.	-2	+4	+3	-5	-5	+4	+3	-2	+2	+1	-4	+1	-3	+3	-1	+1	+3	+1	-1	-3	+7	+6	-10	-3	+1	-0	-1	-0	-3	+8	-5	-0	+4	-4	-2	+2	+4	-4	+2	-2	-3	-1	+4	-0	+3	+1	-2	-2
50.	+6	-2	-2	-2	-5	+2	+5	-2	+5	-1	-3	-1	-3	-4	+5	+2	-1	+2	-2	+1	+3	-0	-2	-1	-1	+2	-0	-1	-4	+3	+3	-2	+5	-3	-1	-1	+1	+4	-2	-3	-2	-0	+6	-4	-1	-1	-1	+3
51.	-6	-3	+5	+4	+4	-3	-4	+3	-9	+3	+7	-1	+3	+1	+2	-4	-2	+3	-1	-0	+1	-1	+3	-3	-3	+3	-0	-0	+3	-2	-0	-1	+7	-2	-3	-2	+4	-1	-0	-3	-2	+10	-3	-5	+11	-2	-3	-6
52.	-6	-4	+6	+4	-2	+7	-2	-2	+12	-8	-5	+1	+14	-9	-5	-0	+4	-3	-2	+1	-8	-7	+13	+2	+6	-2	-6	+2	-1	-5	+7	-1	-5	+6	-3	+2	+6	-0	-3	-3	-4	-3	+4	+3	-7	-3	+8	+2
1853.	-2	+1	+2	-1	+5	-0	-6	+1	+3	-4	-0	+1	-8	+9	-2	+1	+2	+2	-2	-2	-2	+1	+2	-1	-7	-6	+12	+1	+6	-3	-3	-0	-5	+3	+3	-1	-2	-2	-1	+5	+6	-4	-4	+2	+10	-1	-9	-0
54.	-4	+2	-4	+6	-2	+6	-2	-2	+2	-2	-0	-0	+10	-2	-6	-2	-5	-3	+7	+1	-0	-2	+1	+1	+3	-2	+1	-2	+5	+1	-6	-0	+4	-4	-1	+1	-0	+5	-4	-1	-0	+4	-2	-2	-7	+17	-4	-6
55.	+9	+1	-5	-5	+8	-5	-3	-0	+3	-4	+4	-3	+4	+2	-2	-4	-1	-0	-0	+1	-1	+2	-1	-0	-3	+1	+0	+2	+4	-9	+4	+1	+6	-2	-3	-1	-6	+4	+2	-0	+6	-2	-4	-0	+1	+4	-6	+1
56.	+3	-2	-2	+1	+4	-6	+2	+1	+14	-5	-7	-2	-0	-3	-2	+5	-7	-0	+4	+3	+1	+4	-3	-2	-2	+4	-2	-0	+3	-4	-1	+2	-5	+3	+2	-0	+7	-6	-5	+4	-1	+8	-4	-3	-4	+4	-1	+1
57.	+2	+7	-3	-6	-0	-5	-1	+6	-1	-4	+3	+2	-5	+1	+2	+2	+5	-2	-4	+1	+2	-4	-3	+4	-3	-1	+2	+2	+6	-6	-1	+1	-6	-5	+8	+3	-3	-3	-1	+7	+4	-7	-2	+5	-4	-5	+4	+5
58.	+1	-2	-1	+2	+3	-6	-3	+6	+1	+3	-6	+2	-1	-3	-1	+5	-2	-2	+6	-2	-0	+5	-5	-0	-4	+7	-2	-1	-1	+8	-6	-1	-3	-4	+4	+3	+5	-4	-1	-0	+10	-8	-5	+3	-2	-3	+3	+2
59.	+1	-1	-0	-0	-6	+2	+3	+1	-9	+5	+6	-2	-5	-1	+6	-0	+8	-1	-5	-2	+2	+2	-6	+2	+5	-2	-6	-3	+2	+1	-6	+3	-8	+4	+4	-0	+1	-0	-3	+2	-1	-4	+1	+4	+1	-4	+5	-2
60.	-4	-1	+2	+3	+1	+5	-4	-1	-8	+6	+3	-1	+5	-1	-2	-2	-4	-3	+8	-1	-7	+2	+6	-1	+2	+6	-6	-2	-6	-2	+8	-0	-4	+1	+2	-1	-3	+1	-0	+2	+6	-6	-3	+3	-2	+1	-3	+4
61.	+3	-8	+2	+3	-5	-4	+5	+4	-8	+5	+4	-1	+8	-2	-6	-0	-3	+9	-4	-2	-0	+2	-2	-0	-6	-3	+6	+3	-4	-3	+4	+3	-10	+1	+6	+3	+6	-6	-5	+5	-4	-0	+7	-3	+4	+1	-4	+1
62.	-2	-2	+2	+2	+2	-6	+2	+2	-0	-5	+2	+3	-2	-2	+3	+1	-4	+2	+3	-1	-6	+7	-0	-1	-7	-0	+5	+2	+1	-1	-1	+1	-0	-1	+1	-0	-2	-0	+1	+1	+4	-1	-3	-0	-6	+4	+3	-1
1863.	-1	-1	+3	-1	-4	-1	+2	+3	-8	+6	+3	-1	-6	+9	-1	-2	+1	+3	-2	-2	-5	+1	+3	+1	+5	-1	-5	+1	-4	-5	+8	+1	-11	+5	+6	-0	-2	-4	+3	+3	-4	-5	+6	+3	-5	+6	+2	-3
64.	+3	-7	+1	+3	+3	-3	+1	-0	-2	-2	+3	+1	+4	-0	-4	-0	+2	+4	-3	-3	-6	+4	+4	-2	+3	-3	-0	-0	+5	-1	-3	-1	-8	-0	+9	-1	+10	-6	-4	-0	+2	-0	-0	-0	+5	-4	-4	+3
65.	-2	+2	+1	-1	-2	-3	+6	-1	-2	+5	-3	-0	+7	-7	-4	+4	-5	-4	+4	+5	+6	+2	-7	-1	-7	+4	-0	+3	-2	-4	+4	-2	-0	-2	-2	+4	-0	-0	-0	-0	-0	-2	+2	-0	+2	-5	-1	+4
66.	-5	-1	+7	-1	-5	-1	+5	+1	+1	-1	+1	-1	-1	-5	+5	+1	+1	-1	+1	-1	-0	-2	-1	+3	+2	-1	-1	-0	-3	+1	-0	+2	-10	-2	+10	+2	+8	-4	-6	+2	-4	+1	+7	-4	-6	+1	+4	+1
67.	+1	-2	+3	-2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

econd decade.—1853 to 1862.

	January.	February.	July.	August.	September.	October.	November.	December.	Warm.		Cold.		Warm. a+b	Cold. c+d
									Dry.	Wet.	Dry.	Wet.		
									a	b	c	d		
1843.	B	D	d	c	d	d	c	c	0	1	5	6	1	11
44.	A	D	c	c	a	c	d	b	3	1	4	4	4	8
45.	C	C	d	c	c	d	c	c	0	0	8	4	0	12
46.	B	A	d	b	d	a	c	b	2	3	4	3	5	7
47.	C	D	a	a	b	b	a	a	5	2	3	5	7	5
48.	C	B	c	c	b	a	c	b	3	2	7	0	5	7
49.	B	A	a	a	d	b	a	a	5	4	0	3	9	3
50.	C	B	c	d	d	c	c	d	0	2	5	5	2	10
51.	B	A	d	c	d	a	d	c	1	2	5	4	3	9
52.	B	A	d	c	d	b	c	b	2	4	4	2	6	6
Warm Dry A	1	4	2	2	1	3	2	1	21				42	
Warm Wet B	5	2	0	1	2	3	0	4		21				
Cold Dry C	4	1	3	6	1	2	6	3			45			78
Cold Wet D	0	3	5	1	6	2	2	2				33		

		October.		November.		December.		Total.	
	l.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
First decade. A		6	4	8	2	6	4	78	42
Second ,, a		6	4	2	8	5	5	42	78

		Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
First decade. A		5	5	5	5	6	4	62	58
Second ,, a		5	5	8	2	4	6	66	54

Table IV.—Analytical (Guernsey).

First decade.—1843 to 1852.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Warm.		Cold.		Warm. A+B	Cold. C+D
													Dry.	Wet.	Dry.	Wet.		
													A	B	C	D		
1843.	B	D	A	B	D	D	A	A	A	D	B	A	5	3	0	4	8	4
44.	A	D	B	A	A	A	A	D	A	B	A	C	7	2	1	2	9	3
45.	C	C	D	C	C	A	D	C	D	C	A	B	2	1	6	3	3	9
46.	B	A	B	B	B	A	B	B	A	B	A	C	4	7	1	0	11	1
47.	C	D	C	D	B	A	A	A	D	A	A	B	5	2	2	3	7	5
48.	C	B	B	B	A	B	B	D	A	B	D	A	3	6	1	2	9	3
49.	B	A	A	D	B	A	B	A	B	B	A	D	5	5	0	2	10	2
50.	C	B	C	B	D	A	B	D	C	C	B	A	2	4	4	2	6	6
51.	B	A	B	B	C	A	A	A	A	A	D	C	6	3	2	1	9	3
52.	B	A	C	C	D	D	A	A	D	C	B	B	3	3	3	3	6	6
Warm Dry A	1	4	2	1	2	7	5	5	5	2	5	3	42				78	
Warm Wet B	5	2	4	5	3	1	4	1	1	4	3	3		36				
Cold Dry C	4	1	3	2	2	0	0	1	1	3	0	3			20			42
Cold Wet D	0	3	1	2	3	2	1	3	3	1	2	1				22		

Second decade.—1853 to 1862.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Warm.		Cold.		Warm. a+b	Cold. c+d
													Dry.	Wet.	Dry.	Wet.		
													a	b	c	d		
1853	b	d	d	c	c	d	d	c	d	d	c	c	0	1	5	6	1	11
54.	d	c	a	a	d	d	c	c	a	c	d	b	3	1	4	4	4	8
55.	c	c	d	c	d	c	d	c	c	d	c	c	0	0	8	4	0	12
56.	b	a	c	d	c	c	d	b	d	a	c	b	2	3	4	3	5	7
57.	d	c	c	d	c	a	a	a	b	b	a	a	5	2	3	5	7	5
58.	c	c	c	a	c	a	c	c	b	a	c	b	3	2	7	0	5	7
59.	a	a	b	b	d	b	a	a	d	b	a	a	5	4	0	3	9	3
60.	b	c	d	c	b	d	c	d	d	c	c	d	0	2	5	5	2	10
61.	c	b	b	c	c	d	d	c	d	a	d	c	1	2	5	4	3	9
62.	c	a	b	a	b	c	d	c	d	b	c	b	2	4	4	2	6	6
Warm Dry A	1	3	1	3	0	2	2	2	1	3	2	1	21				42	
Warm Wet B	3	1	3	1	2	1	0	1	2	3	0	4		21				
Cold Dry C	4	5	3	4	5	3	3	6	1	2	6	3			45			78
Cold Wet D	2	1	3	2	3	4	5	1	6	2	2	2				33		

Warm and Cold Months.

		January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total.	
		Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
First decade.	A+B and C+D	6	4	6	4	6	4	6	4	5	5	8	2	9	1	6	4	6	4	6	4	8	2	6	4	78	42
Second ,,	a+b and c+d	4	6	4	6	4	6	4	6	2	8	3	7	2	8	3	7	3	7	6	4	2	8	5	5	42	78

Dry and Wet Months.

		Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
First decade.	A+C and B+D	5	5	5	5	5	5	3	7	4	6	7	3	5	5	6	4	6	4	5	5	5	5	6	4	62	58
Second ,,	a+c and b+d	5	5	8	2	4	6	7	3	5	5	5	5	5	5	8	2	2	8	5	5	8	2	4	6	66	54

Number of Days with rain in Excess and Defect of an average of Twenty Years, at Greenwich.

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
15	12	13	13	14	13	13	13	13	17	14	14	
− 3	+4	−6	+ 3	+10	+ 3	+ 4	+ 2	− 7	+ 6	+7	−4	
+ 3	+4	+7	− 8	− 5	− 3	+ 2	+ 1	− 1	+ 4	+9	−1	
+ 4	−0	−2	+ 1	+12	+ 2	+10	+ 8	+ 3	− 1	+8	+3	
− 0	−0	+8	+ 6	− 3	− 5	+ 2	+ 2	− 3	+11	−3	−0	
+ 5	+3	+1	+ 1	+ 7	+ 4	− 5	+ 3	+ 2	+ 1	+5	−1	
− 7	+7	+9	+10	− 9	+ 9	+ 5	+16	+ 1	+ 9	+5	+4	
+ 2	+7	−2	+ 7	+ 1	− 6	− 1	− 5	+ 2	+ 4	−3	+4	
− 5	+1	−8	+ 5	+ 7	− 5	+ 2	+ 1	− 0	− 9	−0	+2	
− 0	+2	+8	− 2	− 2	− 1	+ 4	− 4	+ 1	− 3	−4	−8	
+ 4	−0	−8	− 7	− 0	+10	− 9	+ 3	− 0	− 0	+9	+5	
5 5	3 7	5 5	3 7	5 5	5 5	3 7	2 8	5 5	4 6	4 6	5 5	49 71
+ 5	+1	−0	+ 1	− 3	− 0	+ 3	− 6	− 1	+ 7	−3	−6	
− 0	−3	−7	− 6	+ 3	− 1	+ 2	− 1	− 4	− 6	−1	+2	
+ 5	−1	−1	− 9	− 2	− 4	− 3	− 3	− 7	+ 5	+3	−3	
+ 3	−2	−7	− 0	+ 4	− 6	− 0	− 3	+ 4	− 7	−4	−1	
+ 5	−9	−3	+ 5	− 9	− 4	− 4	− 2	− 0	− 8	−6	−8	
−10	−6	−5	− 2	+ 3	− 8	− 1	− 5	− 3	− 8	−7	−0	
− 4	−0	−3	− 0	− 5	− 6	− 6	− 2	+ 4	+ 1	−1	+3	
+ 6	+1	+5	− 0	− 0	+10	− 3	+12	+ 4	− 7	−3	+3	
− 8	−1	+8	− 7	− 6	+ 2	+ 7	− 4	+ 2	− 7	+1	−4	
+ 2	−6	+8	− 0	+ 2	+ 4	− 1	− 2	− 1	− 0	−4	+3	
4 6	8 2	7 3	8 2	6 4	7 3	7 3	9 1	6 4	7 3	8 2	6 4	83 37
+ 1	−3	−3	− 4	− 4	+ 1	−10	+ 1	+ 1	+ 1	−3	−6	
− 4	+1	+2	− 9	− 4	− 3	−10	− 8	+ 3	−10	−1	−4	
+ 1	−2	−3	− 6	− 1	− 8	− 2	+ 4	−12	+ 2	+4	−4	
+ 2	+6	+2	+ 7	− 1	+ 1	− 4	+ 5	− 6	− 7	−1	+4	
−	−											

Second decade.—1853 to 1862.

	June.	July.	August.	September.	October.	November.	December.		Warm.		Cold.			Warm. a+b	Cold. c+d
									Dry.	Wet.	Dry.	Wet.			
									a	b	c	d			
	c	d	c	c	b	c	c		0	2	7	3		2	10
	c	d	c	a	a	c	b		6	1	3	2		7	5
	c	a	a	a	b	d	c		3	1	6	2		4	8
	c	c	a	d	a	c	a		5	1	4	2		6	6
	a	a	a	a	a	a	a		10	0	0	2		10	2
	a	c	a	a	c	c	a		4	0	7	1		4	8
	a	a	a	d	b	c	d		8	1	1	2		9	3
	d	c	d	d	c	c	d		1	1	4	6		2	10
	b	d	a	b	a	d	a		4	3	3	2		7	5
	d	c	c	a	a	c	d		4	3	3	2		7	5
Warm	3	3	6	5	5	1	4		45					58	
	1	0	0	1	3	0	1			13					
Cold	4	4	3	1	2	7	2				38				62
	2	3	1	3	0	2	3					24			

ember.	October.		November.		December.		Total.	
Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
7	4	6	9	1	7	3	65	55
4	8	2	1	9	5	5	58	62
Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
5	4	6	4	6	5	5	50	70
4	7	3	8	2	6	4	83	37

TABLE V.—Monthly Mean Temperature in Excess and Defect of an average of Twenty Years, at Greenwich.

		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
Average of 20 years………		38·6	38·7	41·6	46·4	52·7	54·0	61·9	61·3	57·0	50·8	43·0		
First decade.	1843.	+ 1·3	− 2·7	+ 1·3	+ 0·7	− 0·5	− 2·7	− 1·0	+ 0·8	+ 2·5	− 2·8	+ 0·8	+ 3·9	
	44.	+ 0·5	− 3·5	− 0·1	+ 5·3	+ 0·2	+ 1·7	− 0·5	− 3·6	− 0·1	− 1·3	+ 1·0	− 7·0	
	45.	− 0·3	− 6·0	− 6·4	− 0·1	− 3·3	+ 1·7	− 2·1	− 4·0	− 3·4	− 0·6	+ 2·8	+ 1·7	
	46.	+ 5·1	+ 5·2	+ 1·7	+ 0·7	+ 1·9	+ 6·3	+ 2·6	+ 1·9	+ 3·1	− 0·3	+ 3·0	− 7·1	
	47.	− 3·5	− 2·3	− 0·6	− 1·1	+ 3·7	− 1·0	+ 3·5	+ 0·8	− 2·7	+ 2·1	+ 3·9	+ 2·8	
	48.	− 4·0	+ 4·7	+ 2·2	+ 1·2	+ 7·0	− 0·5	− 0·4	− 2·8	− 1·2	+ 0·8	+ 0·8	+ 4·4	
	49.	+ 1·5	+ 4·5	+ 0·9	− 3·2	+ 1·3	− 1·1	+ 0·2	+ 1·6	+ 1·8	+ 0·3	+ 1·1	− 0·9	
	50.	− 4·9	+ 6·0	− 1·7	+ 2·1	− 1·4	+ 1·8	+ 0·3	− 1·1	− 0·6	− 3·8	+ 3·5	+ 0·6	
	51.	+ 4·3	+ 1·4	+ 1·0	− 1·7	− 1·8	− 0·1	− 1·8	+ 1·0	− 0·1	+ 1·8	− 5·1	+ 0·4	
	52.	+ 3·4	+ 2·1	− 0·3	− 0·5	− 1·2	− 2·9	+ 4·7	+ 0·8	− 0·2	− 2·9	+ 5·9	+ 7·6	
Months	Warm	6·	6·	5·	5·	5·	4·	5·	6·	3·	4·	9·	7·	
	Cold	4·	4·	5·	5·	5·	6·	5·	4·	7·	6·	1·	3·	
Degrees	Plus	+16·1	+23·9	+ 7·1	+10·0	+14·1	+11·5	+11·3	+ 6·9	+ 7·4	+ 5·0	+22·8	+21·4	157·5
	Minus	−12·7	−14·5	− 9·1	− 6·6	− 8·2	− 8·3	− 5·8	−11·5	− 8·3	−11·7	− 5·1	−15·0	116·8
Second decade.	1853.	+ 3·8	− 5·4	− 3·1	− 1·2	− 0·7	− 0·8	− 1·6	− 1·3	− 1·7	+ 0·1	− 0·9	− 6·0	
	54.	+ 0·4	+ 0·8	+ 2·2	+ 2·0	− 1·8	− 3·3	− 1·6	− 0·4	+ 1·1	+ 1·4	− 2·5	+ 1·3	
	55.	− 3·7	− 9·3	− 3·7	− 0·6	− 3·9	− 2·1	+ 0·2	+ 0·8	+ 0·1	+ 0·4	− 1·7	− 4·4	
	56.	+ 0·8	+ 3·3	− 2·9	+ 0·4	− 3·2	− 0·5	− 0·8	+ 2·3	− 1·8	+ 0·9	− 2·3	+ 0·2	
	57.	− 2·0	+ 0·5	+ 0·2	− 0·7	+ 1·3	+ 2·8	+ 2·6	+ 4·5	+ 2·7	+ 2·1	+ 2·8	+ 5·1	
	58.	− 1·1	− 4·1	− 0·2	− 0·2	− 1·0	+ 5·9	− 1·2	+ 0·7	+ 3·3	− 0·1	− 3·4	+ 1·0	
	59.	+ 1·8	+ 4·4	+ 4·8	+ 0·2	+ 0·4	+ 2·4	+ 6·2	+ 2·2	− 0·3	+ 0·1	− 1·1	− 3·2	
	60.	+ 1·1	− 3·0	− 0·5	− 3·5	+ 1·1	− 4·2	− 4·3	− 3·6	− 3·6	− 0·2	− 2·2	− 3·7	
	61.	− 4·8	+ 3·4	+ 2·5	− 2·1	− 0·8	+ 0·1	− 1·0	+ 1·9	+ 0·1	+ 4·1	− 2·2	+ 1·0	
	62.	+ 0·4	+ 2·4	+ 3·5	+ 2·0	+ 2·7	− 2·7	− 2·8	− 1·8	+ 0·7	+ 1·0	− 3·2	− 6·4	
Months	Warm	6·	6·	5·	4·	4·	4·	3·	6·	6·	8·	1·	5·	
	Cold	4·	4·	5·	6·	6·	6·	7·	4·	4·	2·	9·	5·	
Degrees	Plus	+ 8·3	+14·8	+13·2	+ 4·6	+ 5·5	+11·2	+ 9·0	+12·4	+ 8·0	+10·1	+ 2·8	+ 8·6	108·5
	Minus	−11·6	−21·8	−10·4	− 8·3	−11·4	−13·6	−13·3	− 7·1	− 7·4	− 0·3	−19·5	−23·7	148·4
	1863.	+ 3·2	+ 3·4	+ 2·3	+ 2·7	− 0·7	− 0·9	− 1·1	+ 0·6	− 3·3	+ 0·8	+ 2·7	+ 3·2	
	64.	− 2·1	− 2·7	− 0·3	+ 1·8	+ 1·1	− 1·6	− 0·1	− 1·7	− 0·1	− 0·3	− 1·8	− 1·5	
	65.	− 2·3	− 2·1	− 5·0	+ 5·9	+ 3·4	+ 1·2	+ 1·9	− 1·4	+ 6·9	+ 0·1	+ 1·2	+ 2·4	
	66.	+ 4·0	+ 1·8	− 1·1	+ 1·5	− 2·6	+ 1·9	− 0·9	− 1·9	− 0·6	+ 0·5	+ 1·3	+ 2·9	
	67.	−	−											

TABLE VI.—Number of Days with rain in Excess and Defect of an average of Twenty Years, at Greenwich.

		Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	
Average of 20 years ……		15	12	13	13	14	13	13	13	13	17	14	14	
First decade.	1843.	− 3	+4	−6	+ 3	+10	+ 3	+ 4	+ 2	− 7	+ 6	+7	−4	
	44.	+ 3	+4	+7	− 8	− 5	− 3	+ 2	+ 1	− 1	+ 4	+9	−1	
	45.	+ 4	−0	−2	+ 1	+12	+ 2	+10	+ 8	+ 3	− 1	+8	+3	
	46.	− 0	−0	+8	+ 6	− 3	− 5	+ 2	+ 2	− 3	+11	−3	−0	
	47.	+ 5	+3	+1	+ 1	+ 7	+ 4	− 5	+ 3	+ 2	+ 1	+5	−1	
	48.	− 7	+7	+9	+10	− 9	+ 9	+ 5	+16	+ 1	+ 9	+5	+4	
	49.	+ 2	+7	−2	+ 7	+ 1	− 6	− 1	− 5	+ 2	+ 4	−3	+4	
	50.	− 5	+1	−8	+ 5	+ 7	− 5	+ 2	+ 1	− 0	− 9	−0	+2	
	51.	− 0	+2	+8	− 2	− 2	− 1	+ 4	− 4	+ 1	− 3	−4	−8	
	52.	+ 4	−0	−8	− 7	− 0	+10	− 9	+ 3	− 0	− 0	+9	+5	
Months	Dry	5	3	5	3	5	5	3	2	5	4	4	5	49
	Wet	5	7	5	7	5	5	7	8	5	6	6	5	71
Second decade.	1853.	+ 5	+1	−0	+ 1	− 3	− 0	+ 3	− 6	− 1	+ 7	−3	−6	
	54.	− 0	−3	−7	− 6	+ 3	− 1	+ 2	− 1	− 4	− 6	−1	+2	
	55.	+ 5	−1	−1	− 9	− 2	− 4	− 3	− 3	− 7	+ 5	+3	−3	
	56.	+ 3	−2	−7	− 0	+ 4	− 6	− 0	− 3	+ 4	− 7	−4	−1	
	57.	+ 5	−9	−3	+ 5	− 9	− 4	− 4	− 2	− 0	− 8	−6	−8	
	58.	−10	−6	−5	− 2	+ 3	− 8	− 1	− 5	− 3	− 8	−7	−0	
	59.	− 4	−0	−3	− 0	− 5	− 6	− 6	− 2	+ 4	+ 1	−1	+3	
	60.	+ 6	+1	+5	− 0	− 0	+10	− 3	+12	+ 4	− 7	−3	+3	
	61.	− 8	−1	+8	− 7	− 6	+ 2	+ 7	− 4	+ 2	− 7	+1	−4	
	62.	+ 2	−6	+8	− 0	+ 2	+ 4	− 1	− 2	− 1	− 0	−4	+3	
Months	Dry	4	8	7	8	6	7	7	9	6	7	8	6	83
	Wet	6	2	3	2	4	3	3	1	4	3	2	4	37
	1863.	+ 1	−3	−3	− 4	− 4	+ 1	−10	+ 1	+ 1	+ 1	−3	−6	
	64.	− 4	+1	+2	− 9	− 4	− 3	−10	− 8	+ 3	−10	−1	−4	
	65.	+ 1	−2	−3	− 6	− 1	− 8	− 2	+ 4	−12	+ 2	+4	−4	
	66.	+ 2	+6	+2	+ 7	− 1	+ 1	− 4	+ 5	− 6	− 7	−1	+4	
	67.	−	−											

Second decade.—1853 to 1862.

	June.	July.	August.	September.	October.	November.	December.		Warm.		Cold.			Warm. a+b	Cold. c+d
									Dry.	Wet.	Dry.	Wet.			
									a	b	c	d			
	c	d	c	c	b	c	c		0	2	7	3		2	10
	c	d	c	a	a	c	b		6	1	3	2		7	5
	c	a	a	a	b	d	c		3	1	6	2		4	8
	c	c	a	d	a	c	a		5	1	4	2		6	6
	a	a	a	a	a	a	a		10	0	0	2		10	2
	a	c	a	a	c	c	a		4	0	7	1		4	8
	a	a	a	d	b	c	d		8	1	1	2		9	3
	d	c	d	d	c	c	d		1	1	4	6		2	10
	b	d	a	b	a	d	a		4	3	3	2		7	5
	d	c	c	a	a	c	d		4	3	3	2		7	5
Warm	3	3	6	5	5	1	4		45					58	
	1	0	0	1	3	0	1			13					
Cold	4	4	3	1	2	7	2				38				62
	2	3	1	3	0	2	3					24			

ember.	October.		November.		December.		Total.	
Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
7	4	6	9	1	7	3	65	55
4	8	2	1	9	5	5	58	62

Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
5	4	6	4	6	5	5	50	70
4	7	3	8	2	6	4	83	37

TABLE VII.—Analytical (Greenwich).

First decade.—1843 to 1852.

		January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Warm.		Cold.		Warm. A+B	Cold. C+D
														Dry.	Wet.	Dry.	Wet.		
														A	B	C	D		
	1843.	A	D	A	B	D	D	D	A	A	D	B	A	5	2	0	5	7	5
	44.	B	D	D	A	A	A	D	D	C	D	B	C	3	2	2	5	5	7
	45.	D	C	C	D	D	B	D	D	D	C	B	B	0	3	3	6	3	9
	46.	A	A	B	B	A	A	B	B	A	D	A	C	6	4	1	1	10	2
	47.	D	D	D	D	B	D	A	B	D	B	B	A	2	4	0	6	6	6
	48.	C	B	B	B	A	D	D	D	D	B	B	B	1	6	1	4	7	5
	49.	B	B	A	D	B	C	A	A	B	B	A	D	4	5	1	2	9	3
	50.	C	B	C	B	D	A	B	D	C	C	A	B	2	4	4	2	6	6
	51.	A	B	B	C	C	C	D	A	D	A	C	A	4	2	4	2	6	6
	52.	B	A	C	C	C	D	A	B	C	C	B	B	2	4	5	1	6	6
Warm	Dry A	3	2	2	1	3	3	3	3	2	1	3	3	29				65	
	Wet B	3	4	3	4	2	1	2	3	1	3	6	4		36				
Cold	Dry C	2	1	3	2	2	2	0	0	3	3	1	2			21			55
	Wet D	2	3	2	3	3	4	5	4	4	3	0	1				34		

Second decade.—1853 to 1862.

		January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Warm.		Cold.		Warm. a+b	Cold. c+d
														Dry.	Wet.	Dry.	Wet.		
														a	b	c	d		
	1853.	b	d	c	d	c	c	d	c	c	b	c	c	0	2	7	3	2	10
	54.	a	a	a	a	d	c	d	c	a	a	c	b	6	1	3	2	7	5
	55.	d	c	c	c	c	c	a	a	a	b	d	c	3	1	6	2	4	8
	56.	b	a	c	a	d	c	c	a	d	a	c	a	5	1	4	2	6	6
	57.	d	a	a	d	a	a	a	a	a	a	a	a	10	0	0	2	10	2
	58.	c	c	c	c	d	a	c	a	a	c	c	a	4	0	7	1	4	8
	59.	a	a	a	a	a	a	a	a	d	b	c	d	8	1	1	2	9	3
	60	b	d	d	c	a	d	c	d	d	c	c	d	1	1	4	6	2	10
	61.	c	a	b	c	c	b	d	a	b	a	d	a	4	3	3	2	7	5
	62.	b	a	b	a	b	d	c	c	a	a	c	d	4	3	3	2	7	5
Warm	Dry a	2	6	3	4	3	3	3	6	5	5	1	4	45				58	
	Wet b	4	0	2	0	1	1	0	0	1	3	0	1		13				
Cold	Dry c	2	2	4	4	3	4	4	3	1	2	7	2			38			62
	Wet d	2	2	1	2	3	2	3	1	3	0	2	3				24		

Warm and Cold Months.

		January.		February.		March.		April.		May.		June.		July.		August.		September.		October.		November.		December.		Total.	
		Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.	Warm.	Cold.
First decade.	A+B and C+D	6	4	6	4	5	5	5	5	5	5	4	6	5	5	6	4	3	7	4	6	9	1	7	3	65	55
Second „	a+b and c+d	6	4	6	4	5	5	4	6	4	6	4	6	3	7	6	4	6	4	8	2	1	9	5	5	58	62

Dry and Wet Months.

		Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.	Dry.	Wet.
First decade.	A+C and B+D	5	5	3	7	5	5	3	7	5	5	5	5	3	7	3	7	5	5	4	6	4	6	5	5	50	70
Second „	a+c and b+d	4	6	8	2	7	3	8	2	6	4	7	3	7	3	9	1	6	4	7	3	8	2	6	4	83	37

into two decennial periods or decades, and then to compute, not only the number of months above and below the average, but also the degrees of temperature. (See Tables I., II., III., V. and VI. in the Appendix.)

Lastly, the numerals thence derived were converted into simple and familiar signs, which were then delineated upon a sheet of sectional paper, accurately engraved according to scale. A square space was allotted to each of the months, and they were laid down as abscissas, with the years for ordinates. (See Diagrams I. and II. (Archives).)

The light squares in the diagrams denote warmth, the degrees being expressed by a modification of the plus sign, in red ink; the dark, or shaded squares indicate cold, and the degrees are marked in black ink with the minus sign. Black dots indicate the number of rainy days above the average, and red dots the number below it. The diagonal lines from the centre of each square show the direction and days of predominant wind, one-sixteenth of an inch being equivalent to five days beyond the mean; the red lines are associated with dryness, and the black with moisture. It may be necessary to explain that, as regards rainfall, frequency, rather than quantity, was selected as a criterion. That is to say, the number of days on which rain fell rather than the number of inches collected by the pluviometer; for it not unfrequently happens that a few heavy showers yield a greater amount of water than many days of gentle rain of long continuance.

The combined signs in the above arrangement are intended to represent four different states of weather, viz. warm + dry; warm + wet; cold + dry; and cold + wet.

If the sectional paper be large enough to admit of blank spaces being left, the signs of the weather may be delineated therein as each month elapses; and thus the diagram becomes a sort of register, and an ever-ready table of reference.

For instance, by running the eye along the vertical columns of the Guernsey diagram, to which I must confine myself for the present, the existing state of the weather can easily be compared with that of corresponding months, up to 1843; and by following each horizontal line of squares the character of each year may as readily be ascertained. Thus we shall find, on comparing the September of 1865 with that of preceding years, that it was the hottest and the driest of the whole series. On looking along the ordinate corresponding to 1846, it will be seen that during eleven months of that year the temperature was uniformly above the adopted average.

On taking a general view of this diagram, after its completion, a very cursory glance sufficed to show me that a striking difference existed in the distribution of light and shade. On closer inspection, it became manifest that the number of light squares in the one decade exactly counterbalanced the dark squares in the other; so that the warm months of the first period were in direct ratio to the cold months of the second.

These contrasts, quite unlooked for by me, were all the more surprising, as the data employed had been taken as they came, and not selected for the purpose of supporting any preconceived notion. It seemed to me, therefore, that this kind of diagram, besides serving as a convenient table of reference, was a collection of materials prepared and classified for further analysis. Under this impression I proceeded to decompose it, and to rearrange the products in a tabular form*—converting into letters of the alphabet the combined signs in the squares, so as to designate the four states of weather, before mentioned, as follows:—A=warm+dry; B=warm+wet; C=cold+dry, and D=cold+wet.

These letters were then placed in columns under the heads of months and years; the number of times in which each letter recurred was noted, and these numerals, which may be termed coefficients of the sums of the letters, were collected in lines and columns, those of the months at the foot, and those of the years at the sides of a table of analysis. See Table IV.

When the coefficients of the whole series were thus placed in juxtaposition, it was satisfactory to find that the general contrasts, noticed in the diagram, were borne out numerically; and still more satisfactory to ascertain that there was a close agreement between the ratio of the months and that of the degrees of temperature, plus and minus.

	Months.	Degrees.	
First decade.	78 warm to 42 cold.	153°·5 plus to 55°·2 minus.	Guernsey.
Second decade.	78 cold to 42 warm.	153°·0 minus to 60°·0 plus.	

The columns at the sides of the Tables of analysis, that of Greenwich as well as Guernsey, indicate that there was the intrusion of one cold year (1845) in the warm period, and of two warm years (1857 and 1859) in the cold period. A similar kind of intercalation was pointed out by Mr. Howard, in his 'Cycle of the Seasons,' from 1824 to 1841, namely, the intrusion of one cold year in the warm, and one warm year in the cold cycle.

On examining the coefficients more in detail, in the hope of being able to detect some group of months which seemed to bear a peculiar relation to the rest, I met with a frequent recurrence of an exact inverse order between the warm months of the two decades; and often a direct ratio between the wet and dry.

During the first decade, the ratio between warm and cold, in the groups of January, February, March, April, August, September, October, and December, is invariably 6 to 4. It is one of greater inequality in the Junes and Novembers, being 8 to 2 in both cases; warm Mays are equal to the cold, but warm Julys preponderate in the proportion of 9 to 1.

In the second decade, the warm Novembers are to the cold the exact reverse of what they were in the first, being 2 to 8; and the ratios of Ja-

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* I am indebted to Dr. Guy's Croonian Lectures for an insight into this method.

nuary, February, March, and April are also reversed, being 4 to 6, instead of 6 to 4.

When one decade is compared with the other, the last-named months are found to stand in exact inverse order, viz. 6 to 4 in the first, and 4 to 6 in the second.

The wet and dry groups seem, on the whole, to be more evenly balanced than the warm and cold; but the Novembers of the second decade are remarkable for dryness. The connexion between the predominant winds and the other states of weather has not as yet been traced systematically; but the diagram shows great excess of north-east wind in the spring of 1852, and a long continuance of cold weather setting in early in the following year. It is also evident that wind from south-east was more prevalent during the second than the first decade.

From the foregoing comparison of the different months, the group of Novembers seems to be the most exceptional; it may therefore be worth while to recapitulate the peculiarities that have been noticed.

1st. The ratio between the coefficients of this group in different decades is invariably one of considerable inequality.

2nd. Two cold Novembers only occur in the warm cycle, and only two warm ones in the cold cycle.

3rd. In the second decade the proportion of warm to cold Novembers is 8 to 2, and of dry and wet 2 to 8; but in the warm period warm and wet months were prettily evenly distributed.

4th. Novembers of comparatively low temperature, such for instance as those of 1851, 1853, 1854, 1855, 1856, 1860, 1861, and 1862, were in each year succeeding those enumerated, followed by Mays or Junes of a similar character. The following *résumé* shows the relations between the Novembers and the Junes.

		Months.	
		Novembers.	Junes.
Guernsey.	1st decade.	8 warm to 2 cold.	8 warm to 2 cold.
	2nd decade.	8 cold to 2 warm.	7 cold to 3 warm.

		Degrees.	
		Novembers.	Junes.
Guernsey.	1st decade.	15°·7 plus to 4°·2 minus.	17°·5 plus to 3°·0 minus.
	2nd decade.	14°·0 minus to 2°·8 plus.	18°·5 minus to 3°·1 plus.

These contrasts and analogies seem to justify the surmise that the atmospheric conditions of the former months may have exercised some influence upon those of the latter. Whether such be the case generally is not to be determined until a much longer series of results at Guernsey can be compared.

The peculiarities with respect to the Novembers may be purely accidental, or confined to the period under consideration; but that they are not restricted to locality is proved by the Greenwich Tables, in which these groups stand out still more prominently (see Table VII.); the ratios between warm and cold being 9 to 1 in the first decade, and 1 to 9 in the second. It is difficult therefore to avoid the conclusion that, during the twenty years in question, the Novembers were exceptional months at both places; although at Guernsey they were more frequently followed by unfavourable Junes.

The Greenwich diagram (Diagram II. Archives), to which I must now briefly advert, does not exhibit so striking a contrast of light and shade as was observable at first sight in the other diagram. But on further examination it will be found that the warm months of the first decade correspond nearly in number with the cold months of the second, although not so exactly as at Guernsey.

		Months.	Degrees.
Greenwich.	1st decade.	65 warm to 55 cold.	157°·5 plus to 108°·5 minus.
	2nd decade.	62 cold to 58 warm.	148°·4 minus to 116°·8 plus.

On comparing the above abstract with that in a previous page, it will be perceived that the disparity between the general results, from both places, is not very considerable; a similarity all the more remarkable, when we consider the great difference in position and latitude of the inland and the insular stations. See Diagram III.

It would be superfluous to enter into any further discussion of the various alternations which the coefficients are susceptible of, in a paper which is merely intended to direct attention to the accompanying diagrams and analytical tables. My motive for venturing to bring them under notice is a desire to place them in the hands of those better qualified than I am for conducting processes of induction; and as the modified plan I have adopted is based upon long recognized principles, which are applicable to the investigation of atmospheric phenomena in any locality, I trust that it may be deemed worthy of consideration.

IV. "Monthly Magnetic Determinations, from June to November 1866 inclusive, made at the Observatory at Coimbra," by Professor J. A. de Souza, Director of the Observatory. Communicated (with a Note) by the President. Received May 8, 1867.

[Note.—These observations contain the record of the commencement of the absolute magnetic determinations at the Coimbra Observatory, with instruments procured by M. de Souza at Kew, and on the system of observation and reduction adopted at the Kew Observatory. The employment of the photographic continuously self-recording instruments at Coimbra has hitherto been delayed by the works required for the introduction of gas into the Observatory; but this has now been accomplished; and a letter, dated April 20, 1867, from the Director states that the photographic records were at that date on the point of commencing. The latitude and longitude of the Observatory are 40° 12′·5 N., and 8° 25′ W.—E. S.]

Observations of Deflection, Vibration, and Dip taken at the Coimbra Observatory, 1866.

The horizontal, vertical, and total forces are calculated to English measure.

The vertical and total forces are obtained from the absolute measures of horizontal force and dip.

The value of log π^2 K determined at the Kew Observatory is=1·64829 at temp. 62° Fahr.

The value of log μ is =6·30487.

The values of the coefficients q and q' are respectively 0·000128, 0·00000030. The temperature correction was obtained from the formula $q(t_0-38)+q'(t_0-38)^2$. The correction for error of graduation of the deflection-bar at 1·0 foot is =−0·00006, at 1·3 is =−0·00024. The time of one vibration has been obtained from the mean of twenty-four determinations of the time of 100 vibrations.

The angles of deflection are each the mean of two determinations. The difference between the angles of every pair was found always less than 40″.

In deducing from these observations the ratio and product of the magnetic moment m of the magnet, and of the earth's horizontal magnetic intensity X, no correction has been required for the rate of the chronometer, or for the initial and terminal semiarcs of vibration, the former having been always less than 2^s·0, and the latter less than 70′ at commencement, and 30′ at end.

But the induction and temperature corrections have always been applied, and the observed time of vibration has been corrected for the effect of torsion of the suspending thread.

The torsion for 90° was found no less than 5′·18, and no greater than 8′·67.

In the calculations of the ratio $\frac{m}{X}$, the third and subsequent terms of the series $1+\frac{P}{r^2}+\frac{Q}{r^4}+$ &c. have been omitted. The value of the constant P was found to be =−0·0022317, by the mean of thirty-one determinations obtained each from two pairs of deflection observations at distances 1·0 and 1·3 foot.

The horizontal force for each day is the mean of those calculated for each pair of deflections at distances 1·0 and 1·3 foot.

June 1866. COIMBRA.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\,\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1 0 45	1·0	66·2	12 45 18·7	9·04572	1 0 40	66·7	4·24829	0·39230	0·5230	2 3 34	1	M. M.	61 15 24	4·7131	8·5938	9·8014
	1·3		5 46 31·2	9·04565				0·39230			2		61 15 36			
11 11 36	1·0	74·7	12 33 37·5	9·03980	11 11 37	76·0	4·27962	0·38653	0·5166	12 1 11	1	”	61 17 6	4·7133	8·5968	9·8041
	1·3		5 41 28·7	9·03996				0·38653			2		61 14 47			
20 0 6	1·0	74·1	12 2 8·7	9·02151	20 11 59	74·6	4·21697	0·39912	0·5132	21 2 24	1	”	61 17 9	4·8849	8·9139	10·1642
	1·3		5 27 6·2	9·02130				0·39912			2		61 16 4			
25 1 52	1·0	73·1	12 2 0·0	9·02134	25 1 46	74·5	4·21716	0·39919	0·5132	28 11 32	1	”	61 14 54	4·8863	8·9024	10·1564
	1·3		5 27 3·7	9·02116				0·39919			2		61 13 46			
												Means ………	61 15 36	4·7994	8·7517	9·9815

July 1866. COIMBRA.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\,\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1 1 1	1·0	72·6	12 1 4·4	9·02075	1 0 55	75·9	4·21717	0·39916	0·5128	2 11 47	1	M. M.	61 13 54	4·8883	8·9038	10·1574
	1·3		5 26 55·0	9·02093				0·39916			2		61 14 1			
5 0 23	1·0	72·9	12 1 12·5	9·02086	5 0 27	75·0	4·21737	0·39908	0·5128	6 11 8	1	,,	61 16 50	4·8880	8·9201	10·1715
	1·3		5 26 46·2	9·02077				0·39908			2		61 16 33			
15 1 21	1·0	77·8	11 21 55·0	8·99725	15 1 25	78·6	4·22934	0·39677	0·4977	13 10 56	1	,,	61 17 39	5·0089	9·1526	10·4335
	1·3		5 9 16·2	8·99731				0·39678			2		61 19 30			
20 0 6	1·0	79·4	11 21 13·7	8·99694	20 0 5	80·2	4·22938	0·39691	0·4976	21 0 33	1	,,	61 12 39	5·0114	9·1260	10·4115
	1·3		5 8 58·7	8·99702				0·39691			2		61 14 38			
25 11 41	1·0	77·6	11 22 1·2	8·99730	25 11 50	78·4	4·22905	0·39689	0·4978	26 11 53	1	,,	61 15 10	5·0097	9·1402	10·4230
	1·3		5 9 13·1	8·99722				0·39687			2		61 17 35			
												Means ………	61 15 51	4·9613	9·0485	10·3194

August 1866. COIMBRA.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1 11 3	1·0	78·7	11 22 41·2	8·99780	1 11 15	79·4	4·22982	0·39681	0·4981	2 0 23	1	M. M.	61 7 1	5·0062	9·1117	10·3964
	1·3		5 9 34·4	8·99780				0·39681			2		61 18 43			
5 0 0	1·0	80·1	11 32 30·0	9·00405	5 0 7	82·4	4·22529	0·39774	0·5022	6 2 5	1	,,	61 15 32	4·9758	9·0906	10·3633
	1·3		5 13 56·2	9·00399				0·39775			2		61 21 8			
10 0 12	1·0	79·8	11 21 1·9	8·99685	10 0 44	81·8	4·22972	0·39678	0·4975	11 2 37	1	,,	61 19 7	5·0113	9·1634	10·4442
	1·3		5 8 52·5	8·99691				0·39678			2		61 20 4			
15 0 40	1·0	95·1	11 18 40·0	8·99660	15 0 54	97·2	4·23041	0·39789	0·4980	16 0 50	1	,,	61 24 6	5·0189	9·2023	10·4819
	1·3		5 7 52·5	8·99674				0·39789			2		61 22 57			
20 0 27	1·0	75·6	11 21 35·0	8·99687	20 0 8	75·9	4·22968	0·39647	0·4973	21 0 42	1	,,	61 13 47	5·0106	9·1392	10·4226
	1·3		5 8 50·0	8·99652				0·39647			2		61 18 9			
25 0 35	1·0	76·1	11 21 12·5	8·99667	25 0 37	78·8	4·22980	0·39649	0·4974	27 1 35	1	,,	61 16 41	5·0094	9·1471	10·4289
	1·3		5 9 15·6	8·99716				0·39649			2		61 18 26			
												Means	61 17 58	5·0054	9·1424	10·4229

September 1866. COIMBRA.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	$\text{Log}\,\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log *m*X.	Value of *m*.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1 0 37	1·0	77·3	11 20 5·0	8·99606	1 0 49	78·9	4·23015	0·39654	0·4970	2 11 37	1	M. M.	61 11 31	5·0136	9·1229	10·4098
	1·3		5 8 40·0	8·99642				0·39654			2		61 13 30			
5 0 47	1·0	79·0	11 20 1·2	8·99615	5 1 2	80·0	4·23012	0·39669	0·4971	......	...	......	......	5·0148	......	......
	1·3		5 8 25·0	8·99620				0·39669			...		......			
10 0 44	1·0	74·9	11 19 36·2	8·99557	10 1 23	75·2	4·23048	0·39629	0·4965	11 0 24	1	M. M.	61 13 11	5·0160	9·1423	10·4280
	1·3		5 8 11·2	8·99556				0·39629			2		61 16 37			
15 0 9	1·0	75·1	11 19 18·7	8·99540	15 0 24	75·4	4·23075	0·39625	0·4964	16 1 16	1	„	61 18 7	5·0163	9·1655	10·4484
	1·3		5 8 10·6	8·99556				0·39625			2		61 18 50			
20 11 56	1·0	72·5	11 19 39·4	8·99541	20 0 22	73·4	4·23034	0·39614	0·4963	21 1 4	1	„	61 15 9	5·0164	9·1316	10·4188
	1·3		5 8 8·2	8·99530				0·39614			2		61 11 1			
25 0 39	1·0	65·2	11 17 13·7	8·99332	25 0 37	64·4	4·23120	0·39551	0·4947	26 0 54	1	„	61 19 22	5·0248	9·3015	10·5719
	1·3		5 7 5·0	8·99321				0·39551			2		61 55 12?			
												Means	61 19 15	5·0170	9·1728	10·4554

October 1866. COIMBRA.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m			° ′ ″			
1 0 41	1·0	69·3	11 5 35·6	8·98620	1 1 39	71·5	4·23478	0·39504	0·4902	2 0 44	1	M. M.	61 16 10	5·0658	9·2474	10·5441
	1·3		5 1 15·0	8·98526				0·39504			2		61 18 8			
5 0 33	1·0	69·7	11 6 50·0	8·98703	5 0 45	71·2	4·23494	0·39510	0·4909	6 0 48	1	,,	61 18 24	5·0587	9·2372	10·5317
	1·3		5 2 27·5	8·98703				0·39510			2		61 16 45			
10 0 18	1·0	69·2	11 6 49·4	8·98699	10 1 6	70·0	4·23517	0·39504	0·4908	……	…	……	……	5·0657	……	……
	1·3		5 2 8·7	8·98654				0·39504			…		……			
15 0 41	1·0	69·9	11 6 36·2	8·98690	15 1 9	71·3	4·23447	0·39520	0·4910	16 0 16	1	,,	61 17 48	5·0596	9·2296	10·5255
	1·3		5 2 26·2	8·98702				0·39520			2		61 14 28			
20 0 33	1·0	71·1	11 5 37·5	8·98636	20 1 7	70·4	4·23493	0·39517	0·4906	26 0 48	1	,,	61 16 24	5·0632	9·2354	10·5323
	1·3		5 1 51·2	8·98627				0·39518			2		61 15 37			
												Means ………	61 16 43	5·0626	9·2374	10·5334

November 1866. Coimbra.

Observations of Deflection and Vibration for absolute measure of H. F.										Magnetic Dip.				Values of		
Day, hour, and minutes.	Distances.	Temperature, Fahr.	Deflection.	Log $\frac{m}{X}$.	Day, hour, and minutes.	Temperature, Fahr.	Time of one vibration.	Log mX.	Value of m.	Day, hour, and minutes.	Needle.	Azimuth.	Dip.	X.	Y.	Total force.
d h m	foot	°	° ′ ″		d h m	°	s			d h m		° ′	° ′ ″			
1 0 10	1·0	65·0	11 7 10·6	8·98690	1 0 12	65·1	4·23437	0·39483	0·4908	2 0 23	1	26 30	61 18 47	5·0571	9·2582	10·4945
	1·3		5 2 46·9	8 98714				0·39483			2		61 23 54			
10 0 26	1·0	64·4	11 7 21·2	8·98696	10 0 57	66·0	4·23497	0·39466	0·4906	12 0 22	1	85 0	61 21 36	5·0568	9·2432	10·5360
	1·3		5 2 37·5	8·98686				0·39466			2		61 16 31			
15 0 40	1·0	67·6	11 6 3·7	8·98638	15 1 9	69·6	4·23514	0·39486	0·4905	16 1 46	1	M. M.	61 15 56	5·0605	9·2257	10·5225
	1·3		5 2 13·7	8·98655				0·39487			2		61 14 35			
20 0 57	1·0	64·6	11 6 57·5	8·98672	20 1 17	65·9	4·23452	0·39476	0·4905	21 1 12	1	30 0	61 16 23	5·0590	9·2204	10·5242
	1·3		5 2 23·7	8·98655				0·39476			2		61 13 20			
25 0 40	1·0	61·6	11 6 5·6	8·98594	25 1 1	62·2	4·23439	0·39457	0·4901	26 0 55	1	M. M.	61 25 3	5·0617	9·2777	10·5686
	1·3		5 2 11·2	8·98603				0·39458			2		61 21 2			
												Means	61 18 43	5·0590	9·2450	10·5292

May 16, 1867.

WILLIAM BOWMAN, Esq., V.P., in the Chair.

The Right Honourable Lord Chief Justice Bovill was admitted into the Society.

The following communications were read:—

I. "On the Internal Distribution of Matter which shall produce a given Potential at the Surface of a Gravitating Mass." By G. G. Stokes, M.A., Sec.R.S., Lucasian Professor of Mathematics in the University of Cambridge. Received April 18, 1867.

It is known that if either the potential of the attraction of a mass attracting according to the law of the inverse square of the distance, or the normal component of the attraction, be given all over the surface of the mass, or any surface enclosing it (which latter case may be included in the former by regarding the internal density as null between the assumed enclosing surface and the actual surface), the potential and consequently the attraction at all points external to the surface and at the surface itself is determinate. This proposition leads to results of particular interest when applied to the Earth, as I showed in two papers published in 1849*, where among other things I proved that if the surface be assumed to be, in accordance with observation, of the form of an ellipsoid of revolution, Clairaut's Theorem follows independently of the adoption of the hypothesis of original fluidity, or even of that of an internal arrangement in nearly spherical strata of equal density.

But though the law of the variation of gravity which was originally obtained as a consequence of the hypothesis of primitive fluidity, and was afterwards found by Laplace to hold good, on the condition that the surface be an ellipsoid of revolution as well as a surface of equilibrium, provided only the mass be arranged in nearly spherical strata of equal density, be thus proved to be true whatever be the internal distribution, the question may naturally be asked, Does not the condition that the potential at the surface shall have its actual value require that the internal distribution shall be compatible with that of a fluid mass, or at any rate shall be such that the whole mass shall be arranged in nearly spherical strata of equal density? Such a question was in fact asked me by an eminent mathematician at the time to which I have alluded. I replied by referring to the well-known property of a sphere, according to which a central mass may be distributed uniformly over its surface without affecting the external attraction, by applying which proposition to a mass such as the Earth we may evidently, without affecting the external attraction, leave a large excentrically situated cavity

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* "On Attractions, and on Clairaut's Theorem," Cambridge and Dublin Mathematical Journal, vol. iv. p. 194; and "On the Variation of Gravity at the Surface of the Earth," Cambridge Philosophical Transactions, vol. viii. p. 672.

absolutely vacuous, the matter previously within it having been distributed outside it. It is known further that the mass of a particle may be distributed over *any surface whatsoever* enclosing the particle without affecting the external attraction, and in this way we see at once that we may leave *any internal space we please,* however excentrically situated, wholly vacuous; nor is it necessary in doing so to introduce an infinite density, by distributing the whole mass previously within that space over its surface, since that mass may be conceived to be divided into an infinite number of infinitely small parts, which are respectively distributed over an infinite number of surfaces surrounding the space in question. These considerations, however, though they readily show that the internal distribution may be widely different from any that is compatible with the hypothesis of primitive fluidity, do not lead to the general expression for the internal density. Circumstances have recently recalled my attention to the subject, and I can now indicate the mode of obtaining the general expression required in the case of any given surface.

Let the mass be referred to the rectangular axes of x, y, z, and let ρ be the density, V be the potential of the attraction. Then for any internal point V satisfies, as is well known, the partial differential equation

$$\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}=-4\pi\rho, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

or as it may be written for brevity $\nabla V=0$. This equation may be extended to all space by imagining the body continued infinitely, but having a density which is null outside the limits of the actual body; and by adopting this convention we need not trouble ourselves about those limits. Conversely, if V be a continuously varying function of x, y, z, which vanishes at an infinite distance, and satisfies the partial differential equation (1), V is the potential of the attraction of the mass whose density at the point (x, y, z) is ρ; or, in other words,

$$V=\iiint\frac{\rho'}{r}\,dx'\,dy'\,dz', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where r is the distance between the points (x, y, z) and (x', y', z'), ρ' the density at (x', y', z'), and the limits are $-\infty$ to $+\infty$, is the complete integral of (1) subject to the condition that V shall vanish at an infinite distance.

This may be proved in different ways; most directly perhaps by taking the expression for the potential (U suppose) which forms the right-hand member of (2), substituting for ρ' its equivalent $-\frac{1}{4\pi}\nabla V'$, V′ being the same function of x', y', z' that V is of x, y, z, and transforming the integral in the manner done by Green*, when we readily find $U=V$.

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* Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism, Nottingham, 1828, Art. 3; or the reprint in Crelle's Journal, vol. xliv. p. 360.

Suppose now that we have a given closed surface S containing within it all the attracting matter, and that the potential has a given, in general variable, value V_0 at the surface. For the portion of space external to S, V is to be determined by the general equation $\nabla V=0$, subject to the conditions $V=V_0$ at the surface, and $V=0$ at an infinite distance. We know that the problem of determining V under these circumstances admits of one and but one solution, though it is only for a very limited number of forms of the surface S that the solution can actually be effected. Conceive the problem, however, solved, and from the solution let the value of $\frac{dV}{d\nu}$ at the surface be found, ν being measured outwards along the normal. Now complete V for infinite space by assigning to the space within S any arbitrary but continuous* function we please, subject to the two conditions, 1st, that at the surface it is equal to the given function V_0; 2ndly, that it gives for the value of $\frac{dV}{d\nu}$ at the surface that already got from the solution of the problem referred to in this paragraph. This of course may be done in an infinite number of ways, just as we may in an infinite number of ways join two points in a plane by a continuous curve starting from the two points respectively in given directions, which curve may be either expressed by some algebraical or transcendental equation, or conceived as drawn *liberâ manu,* and thought of independently of any idea of algebraical expression. The function V having been thus assigned to the space internal to S, the equation (1) gives, according to what we have seen, the most general expression for the density of the internal matter.

There is, however, no distinction made in this between positive and negative matter, and if we wish to avoid introducing negative matter we must restrict the function V for the space internal to S to satisfy the imparity

$$\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}\not>0.$$

It is easy from the general expression to show, what is already known, that the matter may be distributed in an infinitely thin, and consequently infinitely dense stratum over the surface S, and that such a distribution is determinate.

We know that there exists one and but one continuous function applying to the space within S which satisfies the equation $\nabla V=0$, and is equal to

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[*] To avoid prolixity, I include in "continuous" the requirement that the differential coefficients of the function, to any order required, shall vary continuously. What that order may be it is perfectly easy in any case to see. We may of course imagine distributions in which the density becomes infinite at one or more points, lines, or surfaces, but so that a finite volume contains only a finite mass. But such distributions may be regarded as limiting, and therefore particular, cases of a distribution in which the density is finite; and therefore the supposition that ρ is finite, does not in effect limit the generality of our results.

V_0 at the surface. Call this function V_1. It is to be remarked that the value of $\frac{dV_1}{d\nu}$ at the surface is not the same as that of $\frac{dV}{d\nu}$, V being the external potential, though V_1 and V are there each equal to V_0. The argument, it is to be observed, does not assume that the two are different; it merely avoids assuming that they are the same; the result will prove that they cannot be the same all over S unless the density, and consequently the potential, be everywhere null, and therefore $V_0=0$. Now attribute to the interior of S a function V which is equal to V_1 except over a narrow stratum adjacent to S, the thickness of which will in the end be supposed to vanish, within which V is made to deviate from V_1 in such a manner as to render the variation of $\frac{dV}{d\nu}$ continuous and rapid instead of abrupt. On applying equation (1), we see that the density is everywhere null except within this stratum, in which it is very great, and in the limit infinite. For the total quantity of matter contained in any portion of the stratum, we have from (1)

$$-\frac{1}{4\pi}\iiint \nabla V\, dxdydz,$$

the integration extending over that portion. Let the portion in question be that corresponding to a very small area A of the surface S; we may suppose it bounded laterally by the ultimately cylindrical surface generated by a normal to S which travels round the perimeter of A. Taking now rectangular coordinates λ, μ, ν, of which the last is parallel to the normal at one point of A, since ∇ is not changed in form by referring it to a new set of rectangular axes, we have for the mass required

$$-\frac{1}{4\pi}\iiint \left\{\frac{d^2V}{d\lambda^2}+\frac{d^2V}{d\mu^2}+\frac{d^2V}{d\nu^2}\right\} d\lambda d\mu d\nu.$$

Of the differential coefficients within brackets, the last alone becomes infinite when the thickness of the stratum, and consequently the range of integration relatively to λ, becomes infinitely small. We have in the limit

$$\int \frac{d^2V}{d\nu^2}d\nu = \frac{dV}{d\nu}-\frac{dV_1}{d\nu},$$

both differential coefficients having their values belonging to the surface. Hence we have ultimately for the mass

$$\frac{A}{4\pi}\left(\frac{dV_1}{d\nu}-\frac{dV}{d\nu}\right).$$

Hence, if ϖ be the superficial density, defined as the limit of the mass corresponding to any small portion of the surface divided by the area of that portion,

$$\varpi=\frac{1}{4\pi}\left(\frac{dV_1}{d\nu}-\frac{dV}{d\nu}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which is the known expression.

In assigning arbitrarily a function V to the interior of S, in order to get the internal density by the application of the formula (1), we may if we please discard the second of the conditions which V had to satisfy at the surface, namely, that $\frac{dV_1}{d\nu}=\frac{dV}{d\nu}$; but in that case to the mass, of finite density, determined by (1) must be added an infinitely dense and infinitely thin stratum extending over the surface, the finite superficial density of this stratum being given by (3).

We have seen that the determination of the most general internal arrangement requires the solution of the problem, To determine the potential for space external to S, supposed free from attracting matter, in terms of the given potential at the surface; and the determination of that particular arrangement in which the matter is wholly distributed over the surface, requires further the solution of the same problem for space internal to S. If, however, instead of having merely the potential given at the surface S we had given a particular arrangement of matter within S, and sought the most general rearrangement which should not alter the potential at S, there would have been no preliminary problem to solve, since V, and therefore its differential coefficients, are known for space generally, and therefore for the surface S, being expressed by triple integrals.

Instead of having the attracting matter contained within a closed surface S, and the attraction considered for space external to S, it might have been the reverse, and the same methods would still have been applicable. The problem in this form is more interesting with reference to electricity than gravitation.

II. "On the Integrability of certain Partial Differential Equations proposed by Mr. Airy." By R. Moon, M.A., late Fellow of Queen's College, Cambridge. Communicated by Professor J. J. Sylvester. Received April 30, 1867.

(Abstract.)

The equation

$$0=\frac{d^2z}{dy^2}-\alpha^2\frac{d^2z}{dx^2}+\beta\frac{dz}{dy}+\gamma z, \quad \ldots \ldots \quad (1)$$

where α, β, γ are functions of x, includes two equations recently proposed for solution by Mr. Airy, and affords a good illustration of the ordinary incapacity of partial differential equations of the second order for solutions involving arbitrary functions.

If the above equation admit of an integral solution containing one or more arbitrary functions, it must be capable of being derived from an equation of the form

$$F(xyz)=\phi\{f(xyz)\}, \quad \ldots \ldots \quad (2)$$

where F and f are definite, and ϕ is arbitrary. From this last we get

$$F'(x)+F'(z)p=\phi'(f)\{f'(x)+f'(z)p\},$$
$$F'(y)+F'(z)q=\phi'(f)\{f'(y)+f'(z)q\};$$

and eliminating ϕ' between these, we shall arrive at an equation between $xyzpq$ which is the only partial differential equation of the first order free from arbitrary functions which is obtainable from (2) without assigning to ϕ a definite form.

Hence (1) must be derivable from an equation of the form

$$f(xyzpq)=0,$$

or of the form

$$0=q+f(xyzp), \quad \dots\dots\dots \quad (3)$$

where f is definite. Differentiating, we have

$$0=\frac{d^2z}{dy^2}+f'(y)+f'(z)q+f'(p)\frac{d^2z}{dx\,dy},$$

$$0=\frac{d^2z}{dx\,dy}+f'(x)+f'(z)p+f'(p)\frac{d^2z}{dx^2}.$$

Multiplying the second equation by A (where A is any function of $xyzpq$) and adding, we get

$$\left.\begin{aligned}0=\frac{d^2z}{dy^2}+\{f'(p)+A\}\frac{d^2z}{dx\,dy}+Af'(p)\frac{d^2z}{dx^2}+f'(y)+Af'(x)\\+(q+Ap)f'(z).\end{aligned}\right\}\ (4)$$

If (1) admits of a solution of the form (2), (1) must be identical with (4), or be capable of becoming so by virtue of (3). Hence

$$0=A+f'(p), \quad -\alpha^2=Af'(p),$$
$$\beta p+\gamma z=f'(y)+Af'(x)+(Ap-f)f'(z). \quad \dots \quad (5)$$

From the two first we get

$$f'(p)=\pm\alpha, \quad A=\mp\alpha.$$

Hence f must satisfy *both* the equations

$$\left.\begin{aligned}&f'(p)=\pm\alpha,\\&0=f'(y)\mp\alpha f'(x)-(f\pm\alpha p)f'(z)-(\beta p+\gamma z).\end{aligned}\right\}\ \dots \quad (6)$$

The first gives us

$$f=F(xyz)\pm\alpha p,$$

where F is arbitrary. Substituting this value in (6), observing that

$$f'(x)=F'(x)\pm p\frac{d\alpha}{dx}, \quad f'(y)=F'(y), \quad f'(z)=F'(z),$$

we get

$$0=F'(y)\mp\alpha F'(x)-(F\pm2\alpha p)F'(z)-\left(\overline{\beta+\alpha\frac{d\alpha}{dx}}\cdot p+\gamma z\right). \quad \cdot \quad (7)$$

But F contains only xyz; hence, in order that this last equation may hold, the coefficient of p must $=0$, *i. e.* we must have

$$0=\pm 2\alpha F'(z)+\left(\beta+\alpha\frac{d\alpha}{dx}\right), \qquad \ldots \ldots \qquad (8)$$

in which case (7) reduces to

$$0=F'(y)\mp\alpha F'(x)-F\,.\,F'(z)-\gamma z. \qquad \ldots \ldots \qquad (9)$$

F must satisfy both (8) and (9).

By integration of (8) we get

$$F=F_1(xy)\mp\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)z.$$

Substituting this in (9), observing that

$$F'(x)=F'_1(x)\mp\frac{1}{2}\frac{d}{dx}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right).z,$$

$$F'(z)=\mp\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right),$$

$$F'(y)=F'_1(y),$$

we get

$$\left.\begin{aligned}0=F'_1(y)\mp\alpha F'_1(x)+\frac{1}{2}\frac{d}{dx}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)z\\ \pm\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)\left(F_1\mp\frac{1}{2}.\overline{\frac{\beta}{\alpha}+\frac{d\alpha}{dx}}.z\right)-\gamma z\end{aligned}\right\} \quad . \quad . \quad (10)$$

But, since F_1 contains x and y only, in order that this may hold we must have the coefficient in it of $z=0$, *i. e.*

$$0=\alpha.\frac{d}{dx}.\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)-\overline{\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)}^{2}-\gamma z,$$

in which case (10) becomes

$$0=F'_1(y)\mp\alpha F_1'(x)\pm\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right).F_1;$$

whence by integration we get

$$F_1=\epsilon^{\frac{1}{2}\int dx\left(\frac{\beta}{\alpha^2}+\frac{1}{\alpha}\frac{d\alpha}{dx}\right)}\phi\left(y\pm\int\frac{dx}{\alpha}\right),$$

where ϕ is arbitrary.

Hence we get for the first integral of (1) *when* (10) *is satisfied,*

$$0=q\pm ap\mp\frac{1}{2}\left(\frac{\beta}{\alpha}+\frac{d\alpha}{dx}\right)z+\sqrt{\alpha}\,.\,\epsilon^{\int\frac{\beta}{2\alpha^2}dx}.\left(y\pm\int\frac{dx}{\alpha}\right),$$

whence by ordinary integration we obtain the complete integral of the form,

$$z.\frac{\epsilon^{\int\frac{\beta}{2\alpha^2}dx}}{\sqrt{\alpha}}=\phi\left(y+\int\frac{dx}{\alpha}\right)+\psi\left(y-\int\frac{dx}{\alpha}\right).$$

We have above a very simple example of a general principle, viz. that in order that a partial differential equation of the second order, or a pair of simultaneous partial differential equations of the first order, may admit of a solution containing arbitrary functions, the coefficients must satisfy a certain equation of condition; from which it follows that, except in the simplest instances (in which the terms of the equation of condition vanish), there is a moral certainty that such a differential equation or pair of equations which have not been specially selected for the purpose, *and whose coefficients do not involve a disposable quantity by which the equation of condition may be satisfied,* will not admit of a solution involving arbitrary functions.

The equations applicable to the motion of an elastic fluid along the axis of a tube afford a remarkable illustration of the scope of these remarks.

Those equations consist of a pair of partial differential equations of the first order involving *five* variables, viz. y, t, ρ, v, p; and it may be shown *à priori,* that when derived upon a true theory they must be capable of a solution containing *two* arbitrary functions; from which it follows that a *third* equation will require to be satisfied. For this purpose we have p, the pressure, ready to our hands.

From the fact of the existence of the equation of condition not having been suspected by the founders of the theory of fluid-motion, at the same time that it was absolutely necessary for them to assign a form to p, they had recourse for that purpose to an empirical method; thus, on the one hand, depriving us of the power of satisfying the requirements of the problem, and on the other, abandoning the means for the determination of p which the analysis furnishes.

It cannot be matter of surprise that the law of pressure suggested under these circumstances should be entirely erroneous, as (by two other independent methods, one founded upon purely physical, the other upon purely analytical considerations) I have elsewhere shown.

III. "On the Lunar Atmospheric Tide at Melbourne." By Dr. G. Neumayer, late Director of the Flagstaff Observatory, Mem. Acad. Leop. Communicated by Lieut.-Gen. Sabine, President. Received April 10, 1867.

Anxious to assist the development of so interesting a branch of knowledge on the connexion of forces in nature as the influence our satellite exerts upon the earth's atmosphere, I had made it a point to include investigations, tending to facilitate studies in this direction, in the plan of discussion of the observations made at the Flagstaff Observatory about to be published. Fully aware that a geographical position, such as that of Melbourne (37° 48′ 45″ south lat. and 9^{h} 39^{m} 53^{s} east long.), affords but very few chances for arriving forthwith at a result which might be regarded as final, I thought it nevertheless of the highest importance to decide how far and to what an extent such small oscillations as those in question, and which for lower latitudes have already been proved to exist, would make themselves manifest, in spite of the great atmospheric disturbances of higher latitudes. The volume of discussions above referred to contains consequently the results of the reduction and classification of upwards of 43,500 hourly observations on pressure of air, registered during the period from the 1st of March 1858 to the 28th of February 1863; and in publishing these results I was chiefly guided by the conviction that it would hardly be compatible with the scope of such a work to enter upon a full discussion of the phenomena connected with the lunar influence on the barometer; while a complete reduction and classification would make the observations apt to be taken up by any one interested in this matter for the purpose of being subjected to a rigorous examination and discussion. While engaged upon this task, I could not fail, however, to be struck by some very interesting facts which, though they are far from being reducible to definite laws, may serve to furnish some connecting links with respect to atmospheric tides, and to give evidence as to the possibility of proving their existence even in as high a latitude as that of Melbourne. A successful attempt at a complete solution of the problem may only be hoped for when a larger number of discussions on barometrical observations, collected at ectropical stations, will be at our command.

Prior to entering upon the task proposed, it appears desirable to give a few particulars, requisite for a full understanding of the subjoined results. The geographical position of the Flagstaff Observatory was already mentioned, and it remains only to be added that the standard barometer was one of Newman's construction, 0·400 inch in diameter, its cistern being 120·7 feet above the mean level of the sea. A few facts respecting the oceanic tides gleaned from 'the Sailing Directions for Port Phillip,' by Capt. Ferguson (1861), may also find a place here.

High water at full and change.			Vertical rise and fall.	
	h	m	Spring. ft.	Neap. ft.
On the beach at Pt. Lonsdale	11	25	7	4
In the midchannel between Pt. Lonsdale and Pt. Nepean	1	50		
At the Lightship, West channel	2	10	4	3
At the East end of South channel	2	25	4	3
At the Bird rock, Geelong	2	30	3·5	2·5
At Pt. Gellibrand and mouth of River Yarra	3	0	4·5	2·5

There is no necessity for entering more fully into a description of the method employed in freeing the barometrical observations from the regular diurnal fluctuation and arranging the remainders $b-\overline{b}$ according to lunar time, inasmuch as this method is quite identical with the one employed by all who have directed their attention to this subject, as General Sabine, Professor Kreil, and others. This much may be stated, however, that the reading of the barometer (b), after being reduced to 32°, was invariably increased by 1 inch, in order that it may always exceed the mean pressure for the respective hour ($\bar{b}$), thereby avoiding negative results. This has certainly the disadvantage of not exhibiting at a glance the excess or defect of atmospheric pressure at any time; on the other hand, there is no doubt that a mistake with regard to the algebraic sign of the remainders is not likely to occur. In the subsequent Tables it was made a rule to reduce the values $b-\bar{b}$ to their mean value for a month, year, or whatever other period of time they may refer to.

The remainders $b—\bar{b}$ were derived, in the manner just pointed out, for every month throughout the period of five years for which the observations were continued. Then the means for every month and hour were taken, thus obtaining normal values for the several months; a general mean for every month formed the basis to which those normal values were referred. The subjoined Table shows the result of this proceeding.

The values of the above Table have been thrown into curves, and Plate I. shows the results. The actual mean values are indicated by dots; a full-drawn curve is made to pass through and between them in such a manner as to eliminate the greater irregularities. Some of those irregularities are so large as to cause the respective dots to be disconnected with the series to which they belong, and it became therefore necessary to indicate this connexion by slight dotted lines.

On glancing over this series of curves we cannot fail to observe a great regularity, pointing at some cause common to all; and as the remainders $b-\bar{b}$ have been arranged according to the moon's hour-angle, we may justly look to the moon as the primary cause. But it is nevertheless true that those curves apparently point to some other influence, most likely due to the combined action of the sun and moon. The monthly curves for the several years of observation have also been drawn, though we refrain from adding the results here; and the fact that they correspond in the main points with those shown on Plate I., seems to justify our attaching particular weight to the evidence of the moon's influence on our atmosphere, as conveyed to our minds by the above Tables. But prior to entering fully upon the various points bearing on the question at issue, we need to form of the monthly results quarterly and semiannual groups. If we call March, April, May the first, June, July, August the second, September, October, November the third, and December, January, February the fourth quarter, we obtain the quarterly means inserted in the following Table. It was furthermore considered serviceable to the purpose to group together those quarters in which the epochs of solstices and equinoxes respectively occur, under the collective names "solstitial and equinoctial quarters." The semiannual periods comprise, as usually, the months from April to September, and those from October to March. The mean of all the various hourly values represents the mean lunar-diurnal variation in pressure of air for the year.

1st part.—From the superior to the inferior passage. (1858–63).

Months.	0^h.	1^h	2^h.	3^h.	4^h.	5^h.	6^h.	7^h.	8^h.	9^h.	10^h.	11^h.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
April	+0·00210	−0·00290	−0·00150	−0·00264	−0·00018	+0·00130	+0·00030	+0·00124	+0·00152	−0·00092	+0·00076	+0·00036
May	+0·00085	+0·00673	−0·00115	+0·00233	−0·00051	−0·00221	+0·00219	+0·00031	−0·00377	−0·00011	+0·00551	+0·00097
June	+0·00508	+0·00168	−0·00052	−0·00296	−0·00432	−0·00808	−0·00592	−0·00812	−0·00696	−0·00376	−0·00264	+0·00224
July	+0·00218	−0·00320	−0·00536	−0·00590	−0·00412	−0·00168	+0·00036	+0·00170	+0·00522	+0·00718	+0·00724	−0·00014
August ...	+0·00713	+0·00437	−0·00203	−0·00097	+0·00225	+0·00353	+0·00137	+0·00035	−0·00141	−0·00347	−0·00371	−0·00277
September	+0·00474	+0·00214	+0·00020	+0·00138	+0·00498	+0·00552	−0·00152	+0·00002	+0·00042	−0·00342	+0·00050	−0·00230
October ...	−0·00403	−0·00025	−0·00497	−0·00595	−0·00251	−0·00047	+0·00169	+0·00117	+0·00303	−0·00059	+0·00139	+0·00265
November	−0·00676	−0·00612	−0·00210	+0·00144	−0·00406	+0·00098	+0·00282	−0·00028	+0·00380	+0·00728	+0·00176	+0·00706
December	+0·00377	+0·00257	+0·00457	+0·00201	+0·00381	+0·00131	+0·00251	−0·00453	−0·00207	−0·01083	+0·00049	+0·00321
January	−0·00178	−0·00568	−0·00310	−0·00068	+0·00158	+0·00324	−0·00226	+0·00016	+0·00162	−0·00644	−0·00328	+0·00152
February	−0·00465	+0·00073	−0·00005	+0·00045	+0·00193	−0·00033	−0·00145	−0·00227	−0·00225	+0·00057	−0·00375	−0·00253
March ...	−0·00402	−0·00430	−0·00290	+0·00024	+0·00124	+0·00156	+0·00516	+0·00966	+0·00724	+0·00536	−0·00108	−0·00082

2nd part.—From the inferior to the superior passage. (1858–63.)

Months.	12^h.	13^h.	14^h.	15^h.	16^h.	17^h.	18^h.	19^h.	20^h.	21^h.	22^h.	23^h.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
April	+0·00162	+0·00106	+0·00138	+0·00230	+0·00094	+0·00252	+0·00096	−0·00082	−0·00340	−0·00296	−0·00444	+0·00130
May	−0·00033	+0·00357	+0·00311	+0·00001	−0·00329	−0·00331	−0·00443	−0·00281	−0·00441	−0·00767	−0·00103	+0·00939
June	+0·00136	+0·00222	+0·00782	−0·00112	+0·00248	−0·00102	+0·00218	+0·00168	+0·00164	+0·00136	+0·00732	+0·00844
July	−0·00072	−0·00364	−0·00134	+0·00104	+0·00018	−0·00044	−0·00162	−0·00300	−0·00278	+0·00074	+0·00522	+0·00284
August ...	−0·00205	−0·00371	−0·00209	+0·00009	−0·00137	−0·00161	−0·00377	+0·00129	+0·00509	+0·00383	−0·00063	+0·00035
September	+0·00008	+0·00058	+0·00190	−0·00012	+0·00092	+0·00008	−0·00284	−0·00782	−0·00502	−0·00290	−0·00290	+0·00540
October ...	−0·00179	−0·00081	+0·00243	+0·00181	+0·00177	−0·00063	+0·00249	−0·00007	−0·00127	+0·01033	+0·00057	−0·00603
November	+0·00720	+0·00412	+0·00284	+0·00068	+0·00500	+0·00250	−0·00340	−0·00140	−0·00288	−0·00910	−0·01032	−0·01012
December	−0·00013	−0·00453	−0·01007	−0·00705	−0·00401	−0·00341	+0·00143	+0·00289	−0·00109	−0·00029	+0·00897	+0·01143
January ...	+0·00082	+0·00399	+0·00440	+0·00240	+0·00176	−0·00408	−0·00136	+0·00242	+0·00358	−0·00392	+0·00116	+0·00394
February	−0·00411	−0·00417	−0·00227	−0·00001	+0·00275	+0·00363	+0·00473	+0·00473	+0·00245	+0·00265	+0·00225	+0·00097
March......	−0·00226	+0·00016	+0·00086	−0·00038	−0·00326	−0·00060	+0·00014	−0·00332	−0·00292	+0·00028	−0·00214	−0·00388

1st part. From the superior to the inferior passage.

(1858–63.)

	0h.	1h.	2h.	3h.	4h.	5h.	6h.	7h.	8h.	9h.	10h.	11h.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
I. Quarter	−0·000355	−0·000155	−0·001848	−0·000021	+0·000185	+0·000219	+0·002552	+0·003739	+0·001665	+0·001445	+0·001732	+0·000172
II. „	+0·004724	+0·000952	−0·002635	−0·003275	−0·002061	−0·002075	−0·001395	−0·002021	−0·001048	−0·000015	+0·000298	+0·000221
III. „	−0·002017	−0·001410	−0·002290	−0·001043	−0·000530	+0·002010	+0·000997	+0·000303	+0·002417	+0·001090	+0·004217	+0·002470
IV. „	−0·000870	−0·000810	+0·000456	+0·000576	+0·002423	+0·001436	−0·000417	−0·002234	−0·000917	−0·005584	−0·002524	+0·000716
Equinoc. } quarters	−0·001226	−0·000066	−0·002100	−0·000563	−0·000205	+0·001084	+0·001744	+0·001990	+0·002010	+0·001237	+0·002944	+0·001290
Solstitial } quarters	+0·001927	+0·000071	−0·001089	−0·001349	+0·000181	−0·000319	−0·000906	−0·002127	−0·000982	−0·002799	−0·001112	+0·000248
Apr. to Sept	+0·003680	+0·001470	−0·001727	−0·001460	−0·000317	−0·000270	−0·000537	−0·000750	−0·000830	−0·000750	+0·001277	−0·000273
Oct. to Mar.	−0·002911	−0·002174	−0·001424	−0·000414	+0·000332	+0·001049	+0·001412	+0·000652	+0·001896	−0·000774	+0·000592	+0·001849
Year ………	+0·000391	−0·000346	−0·001569	−0·000931	+0·000014	+0·000396	+0·000444	−0·000042	+0·000539	−0·000756	+0·000941	+0·000795

2nd part. From the inferior to the superior passage.

(1858–63.)

	12h.	13h.	14h.	15h.	16h.	17h.	18h.	19h.	20h.	21h.	22h.	23h.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
I. Quarter	−0·000322	+0·001599	+0·001785	+0·000645	−0·001868	−0·000461	−0·001108	−0·002315	−0·003575	−0·003448	−0·002535	+0·002272
II. „	−0·000468	−0·001708	+0·001465	+0·000005	+0·000432	−0·001021	−0·001068	−0·000008	+0·001318	+0·001979	+0·003972	+0·003879
III. „	+0·001830	+0·001297	+0·002390	+0·000790	+0·002563	+0·000650	−0·001250	−0·003097	−0·003057	−0·000557	−0·004183	−0·003583
IV. „	−0·001157	−0·001254	−0·002664	−0·001570	+0·000149	−0·001304	+0·001583	+0·003329	+0·001629	−0·000537	+0·004109	+0·005430
Equinoc. } quarters	+0·000723	+0·001417	+0·002062	+0·000687	+0·000317	+0·000064	−0·001210	−0·002737	−0·003347	−0·002033	−0·003390	−0·001686
Solstitial } quarters	−0·000812	−0·001781	−0·000599	−0·000782	+0·000291	−0·001162	+0·000258	+0·001661	+0·001474	+0·000721	+0·004041	+0·004655
Apr. to Sept.	−0·000007	+0·000013	+0·001797	+0·000367	−0·000023	−0·000630	−0·001587	−0·001913	−0·001480	−0·001267	+0·000590	+0·004620
Oct. to Mar.	−0·000044	−0·000206	−0·000301	−0·000424	+0·000669	−0·000431	+0·000672	+0·000876	−0·000354	−0·000008	+0·000082	−0·000614
Year ………	−0·000019	−0·000090	+0·000754	−0·000172	+0·000329	−0·000524	−0·000451	−0·000512	−0·000911	−0·000631	+0·000344	+0·002009

The curves derived from the results of this Table are shown on Plate XI., with the exception of the semiannual and annual curves which may be studied on Plate X.

Glancing at the various curves thus resulting, we are first struck by the great conformity of some of them, whilst others present irregularities apparently quite irreconcileable with what we feel inclined to adopt as the law. There is, however, in all cases manifested a progressive change, evidently depending on the moon's hour-angle in the first instance, calling for a rigorous examination. The semiannual curves of the lunar-diurnal variation of atmospheric pressure may be taken as representing the principal types of the various monthly curves. During the sun's absence from the hemisphere (in our case, when the sun's declination is north), from April to September, the lunar variation reaches its maximum at about $23^h\ 15^m$, or 45^m prior to the moon's upper transit, its minimum value occurring at 19^h and a secondary one at 2^h, with a range of 0·00653 inch. The curves for the single months appertaining to this semiannual period exhibit, generally speaking, the same characteristics, though somewhat irregular, and showing, in some instances, deviations of considerable extent; so, for instance, the curves for August and September. The summer semiannual curve (while the sun's declination is south) exhibits an essentially different character, there being no strongly expressed maximum noticeable, whilst a decided minimum occurs at $0^h\ 30^m$ or 30^m past the moon's upper passage, the maximal pressure taking place at 6^h, and a secondary one between 18^h and 19^h. The amplitude of oscillation amounts to 0·00432 inch. But in this period of the year we notice a great difference in the lunar-diurnal variation of the barometer, when we examine the single months somewhat more closely; thus, for instance, the curve for the month of December shows such characteristics as to cause it to be more like the curves for the winter period, and, on the other hand, we perceive that the curve for the month of November is exactly of the opposite character as that for December. The remaining four months show more or less irregularity, and make a greater or smaller approach towards the general type for the class under consideration.

Although there is undoubtedly in all of these cases strong evidence of an influence of the moon on our atmosphere, I could not rest satisfied, considering that this evidence is seemingly of a somewhat conflicting nature. As already explained, the monthly values have, for the purpose of further inquiry, been combined into quarters, and the results for these quarters were again united in mean values, arranging the two quarters in which epochs of solstice occur, and the remaining two, comprising the equinoxes, respectively, in two groups. Thus we obtain six monthly mean values of the lunar-diurnal variation for the "solstitial and the equinoctial quarters." I was prompted to adopt this course because of the great similarity of the curves for December and June in the one case, and of March and September in the other, though in by far less a degree. This similarity may best

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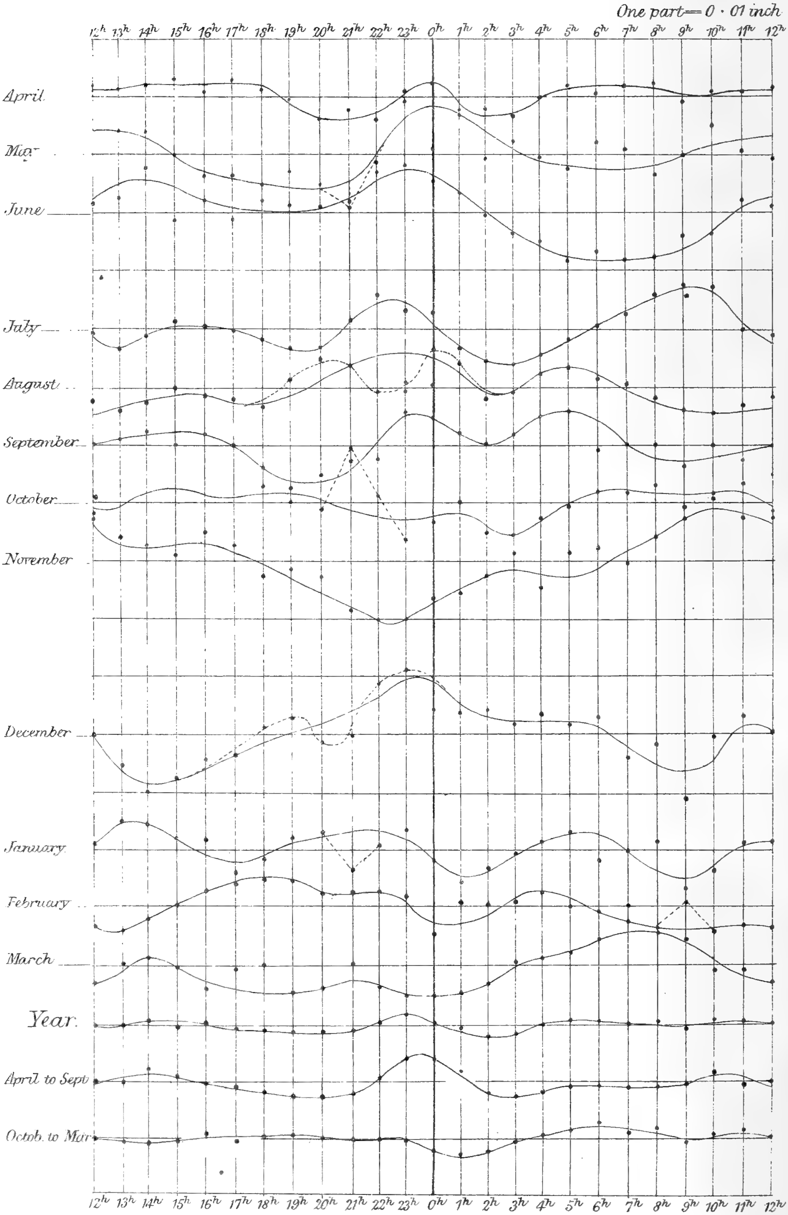

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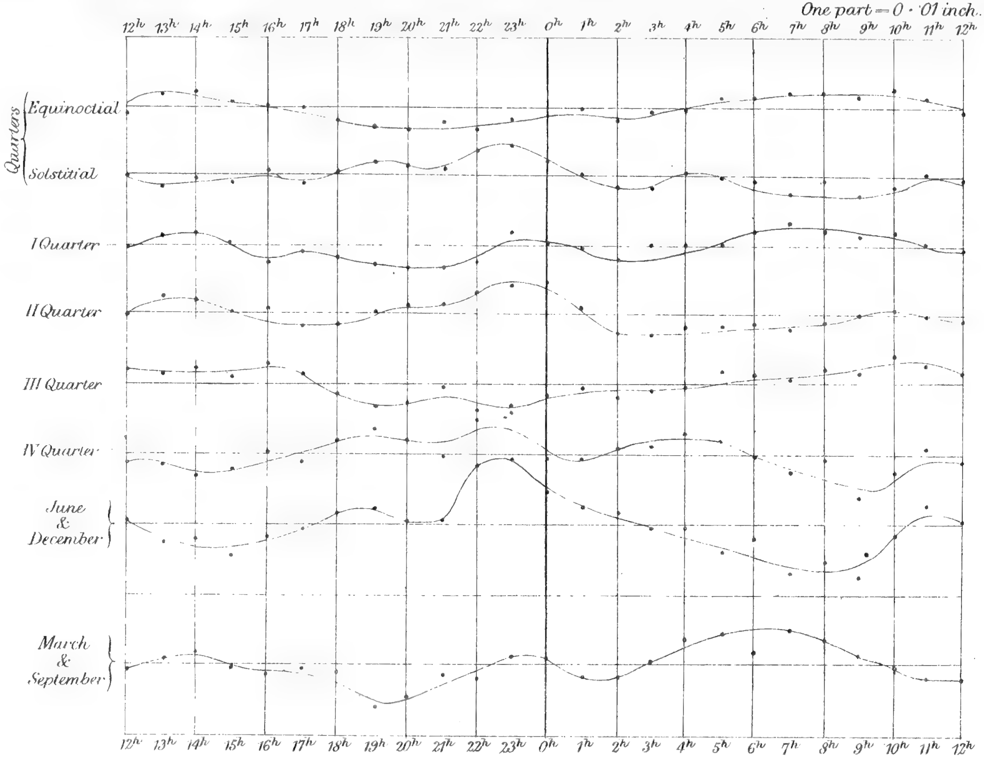

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be judged by the mean values for the respective two months, representing, as they do, in both cases a distinct oscillation with an amplitude of 0″·01722 for the solstices, and of 0″·01041 for the equinoxes. I may be allowed to refrain from adding here these mean values, suffice it to refer to the respective curves at the bottom of Plate II. For December and June we observe the maximum to occur at 23^h, the minimum at or shortly after 8^h, while for September and March the maximum in the lunar-diurnal variation of pressure of air takes place at 7^h and the minimum at 19^h. The mean for the quarters (each embracing six months) show the same characteristics, though by far less in extent, the amplitude for the solstitial quarters being 0″·007454, and that for the equinoctial quarters 0″·006334.

There is another fact which requires to be pointed out, in order to throw further light upon the character of these oscillations; namely, that they seem to bear a great resemblance for both hemispheres during the same semiannual period, if we are permitted to arrive at this conclusion by referring to Prof. Kreil's discussions of his observations at Prague (Versuch den Einfluss des Mondes auf den atmospherischen Zustand unserer Erde aus einjährigen Beobachtungen zu erkennen, 1841). The semiannual curves of the lunar-diurnal variation of the barometer at Prague and Melbourne closely correspond during the months from April to September, and from October to March, which seems to point to some cause common to the whole globe in a similar manner, as we know it to be with respect to the extent of the rise of the oceanic tides at the time of the solstices and equinoxes. It would be premature to enter now upon any speculation with a view to bring the results of our observations in accordance with theory, there being still by far too few discussions on atmospheric tidal observations at our command.

The yearly curve of the lunar-diurnal variation presents some peculiarly interesting features, differing in some respects from the results of similar inquiries instituted by General Sabine and Capt. C. M. Elliot with special regard to the lunar atmospheric tides at St. Helena and Singapore, although the plan of discussion was the same. The lunar horary variation of the barometer is as follows, "if we arrange the results in such manner that the hours are combined in which the moon is similarly situated in respect to the meridian" (Sabine's paper "On the Lunar Tides at St. Helena"):—

Moon's distance from the meridian.	Variations of barometric pressure.						Horary variation			Moon's distance from the meridian.
	At the hours following the meridian passage.			At the hours preceding the meridian passage.			from the observations at the hours following the meridian passage.	from the observations at the hours preceding the meridian passage.	Mean.	
h	h	in.	in.	h	in.	in.	in.	in.	in.	h
0	0	+0·00039	+0·00018	0	+0·00039	+0·00018	+0·00073	+0·00087	+0·00080	0
	12	− ·00002		12	− ·00002					
1	1	− ·00035	− ·00022	11	+ ·00079	+ ·00140	+ ·00033	+ ·00209	+ ·00121	1
	13	− ·00009		23	+ ·00201					
2	2	− ·00157	− ·00041	10	+ ·00094	+ ·00064	+ ·00014	+ ·00133	+ ·00073	2
	14	+ ·00075		22	+ ·00034					
3	3	− ·00093	− ·00055	9	− ·00076	− ·00069	·00000	·00000	·00000	3
	15	− ·00017		21	− ·00063					
4	4	+ ·00001	+ ·00017	8	+ ·00054	− ·00018	+ ·00072	+ ·00051	+ ·00061	4
	16	+ ·00033		20	− ·00091					
5	5	+ ·00040	− ·00006	7	− ·00004	− ·00027	+ ·00049	+ ·00042	+ ·00045	5
	17	− ·00052		19	− ·00051					
6	6	+ ·00044	·00000	6	+ ·00044	·00000	+ ·00055	+ ·00069	+ ·00062	6
	18	− ·00045		18	− ·00045					

In both cases, at the hours following, and at those preceding the meridian passage, the minimum is decidedly at the 3rd hour, while the maximun in the first series occurs at the 0th, and in the second at the 1st hour. At Singapore and St. Helena, both within the tropics, the lunar-diurnal variation shows a maximum at the 0th and a minimum at the 6th hour. The discussions, based on observations made at Prague, and already referred to above, exhibit a greater conformity in respect to the lunar tides at Melbourne than any of the tropical stations. This conformity is especially clearly expressed in the series for "the hours following the meridian passage" (which series seems to present in each respective case the greatest reliability), and we observe that the minimum occurs at the 3rd and 4th, and the maximum at the 6th hour. But in turning Prof. Kreil's labours in this direction to account, we must remember that they refer to a period of only one year, and cannot be considered as presenting great guarantees for decisive results, especially when considering that so high a latitude as 50° 8′ N. would rather have required a longer period of observation than is necessary to prove the existence and character of the lunar atmospheric tides within the tropics. So very few discussions on this topic being at our command, it is nevertheless of considerable interest to compare the results for Prague with those at St. Helena, Singapore, and Melbourne, as done in the following little Table :—

	Mean of three years at Singapore (+1° 19′).	Mean of two years at St. Helena (−15° 57′).	Mean of five years at Melbourne (−37° 48′).	Mean of one year at Prague (+50° 8′).	
h	in.	in.	in.	in.	h
0	+0·00570	+0·00365	+0·00080	0·00000	0
1	+ ·00475	+ ·00336	+ ·00121	+ ·00043	1
2	+ ·00330	+ ·00275	+ ·00073	+ ·00080	2
3	+ ·00280	+ ·00158	·00000	+ ·00039	3
4	+ ·00145	+ ·00110	+ ·00061	+ ·00005	4
5	+ ·00035	+ ·00046	+ ·00045	+ ·00032	5
6	·00000	·00000	+ ·00062	+ ·00078	6
Mean......	+ ·002621	+ ·001843	+ 0·00631	+ ·000396	Mean.

The decrease in extent of oscillation, as we recede from the equator, is clearly illustrated by the mean values of this Table.

Speaking of the extent of the oscillations, it is of importance to add a few facts relative to the amplitude, as resulting from the monthly curves. We have seen, in the course of this exposition, that the amplitude for the semiannual periods from April to September, and from October to March, is respectively 0″·00653 and 0″·00432, which result will be materially altered in case we consider only the single months; for inasmuch as the sense of oscillation varies considerably in the single months, constituting a semiannual period, chiefly during summer, the combination of the hourly values of six months in one group must necessarily tend to diminish, or even abolish in some cases, the lunar-diurnal variation. The mean amplitude of the lunar-diurnal variation of atmospheric pressure for the several months, as represented by the means of five years, is as follows:—

	April	0″·0069	May	0″·0171	June	0″·0165
	Oct.	·0091	Nov.	0·0311	Dec.	·0222
Means for two months equidistant from the equinox		·0080		·0241		·0194
	July	0·0131	Aug.	0·0109	Sept.	0·0133
	Jan.	·0108	Feb.	·0093	Mar.	·0139
Means for two months equidistant from the equinox		·0199		·0101		·0136

The semiannual means are, for the six winter months (when the sun's declination is north), 0″·0129, and for the summer months (when the sun's declination is south) 0″·0161, and therefore the mean amplitude in lunar-diurnal variation of the barometer is 0″·0145.

There is evidently a great conformity in the change in extent of oscillation observable, when we examine the semiannual values of the above series. In April and October the amplitude reaches a minimum value, whilst in the months immediately following a maximum occurs. For both the equinoctial months the value in question is nearly alike, making at the same time the nearest approach to the annual mean. The months following the equinoxes exhibit the smallest range in lunar-diurnal variation of atmospheric pressure, whilst those months preceding the solstices are to be considered as maxima with respect to the value at issue.

With a view to ascertain whether the difference in the extent of the lunar atmospheric tide at the epochs of apogee and perigee may be proved to be perceptible in as high a latitude as 37° 48′, I followed a course differing in some respects from the one proposed by General Sabine in his discussions of the St. Helena observations. We have seen that in the case under consideration the hours of the extremes in pressure are not marked in a like distinct manner as for places near the equator, and I thought it on this account preferable to abandon the adherence to certain hours of the lunar day in determining the range in the value $b-\bar{b}$, simply adopting this range for the lunar day near the apogee or perigee, irrespective of any hour of maximum or minimum. In order to increase the number of comparisons, this range was determined in addition to the days of apogee and perigee for the day preceding and that following those epochs. The difference in the lunar-diurnal range in atmospheric pressure at the epochs of perigee (R^p) and apogee (R^a) was consequently in each case derived from six days' observation. Thus we obtained the following values for R^p-R^a, which, however, cannot be immediately compared, in respect to the amount, with the corresponding values of the discussion on the St. Helena observations just referred to and arrived at by a different process.

Months.	Lunar-diurnal range in Perigee minus lunar-diurnal range in Apogee.					
	1858–59.	1859–60.	1860–61.	1861–62.	1862–63.	Mean for 1858–63.
	in.	in.	in.	in.	in.	in.
April	+0·0775	+0·1793	+0·1910	−0·0984	+0·1010	+0·09018
May	+ ·1146	− ·2270	+ ·0220	+ ·0676	− ·1363	− ·03182
June	− ·0251	− ·0376	+ ·0960	+ ·0380	− ·0837	− ·00248
July	− ·1279	+ ·0786	− ·0020	− ·0044	− ·0370	− ·01246
August	− ·0148	+ ·0217	− ·0750	+ ·1044	+ ·0587	+ ·01904
September	− ·0128	+ ·0144	+ ·1070	+ ·0180	− ·0417	+ ·01968
October	+ ·1004	+ ·1506	+ ·0400	+ ·2006	+ ·1820	+ ·15477
November	+ ·0287	+ ·0320	+ ·0160	+ ·0360	− ·0870	+ ·00514
December	+ ·1439	− ·0296	+ ·0760	− ·1000	+ ·0020	+ ·01846
January	+ ·0097	− ·0083	+ ·0805	+ ·0177	− ·0270	+ ·01452
February	+ ·0815	+ ·0570	− ·0010	+ ·0727	+ ·0510	+ ·05224
March	+ ·0053	+ ·1217	− ·0486	− ·0410	+ ·0060	+ ·00868
Means	+ ·04418	+ ·02747	+ ·03605	+ ·02808	+ ·00195	+ ·02746
Number of epochs of Perigee	13	13	14	13	13	66 Sum.
Number of epochs of Apogee	14	13	13	13	14	67 Sum.

The mean value of +·02746 was derived with due regard to the number of epochs of apogee and perigee occurring in the whole period of observation, the total number of barometrical readings from which it was derived being 720.

There can hardly exist a doubt, after having examined the above results, that the lunar-diurnal range in pressure of air at the time of the perigee exceeds the one at the apogee, a fact which is also in strict accordance with theory. But it ought to be pointed out that during the months of May, June, and July the reverse seems to take place, as is manifested in every one of the five years of observation. Whether this bears any reference to the time of aphelion on the 3rd of July, and the time of the perihelion on the 2nd of January, we do not pretend to decide now; suffice it to have directed the attention of those more immediately interested in inquiries of this nature to a matter replete with so much interest, but as yet, comparatively speaking, scantily examined. The mean range for the epochs of perigee and apogee is respectively 0″·16327 and 0″·13581, resulting a general mean range of 0″·149540.

A similar plan to that just described was pursued, in order to ascertain whether there existed any perceptible difference in atmospheric pressure in the periods of syzygy and quadrature. The range of the atmospheric pressure during a lunar day was determined for days of full and change, and also for each of the epochs of quadrature separately, and furthermore for the day preceding and following each of the several epochs. Subsequently a mean value was derived by combining the daily range of the epochs of syzygy (R^s) and that for the epochs of quadrature (R^q).

Months.	Lunar-diurnal range in pressure of air.						Difference, R^s-R^q.
	Full moon.	New moon.	First quarter.	Last quarter.	Mean for the epochs of		
					Syzygy.	Quadrature.	
	in.	in.	in.	in.	in.	in.	in.
April	0·1507	0·1632	0·1728	0·1224	0·15695	0·14760	+0·00935
May	·1312	·1730	·1669	·1399	·15210	·15340	— ·00130
June	·1125	·1463	·1557	·1282	·12940	·14195	— ·01255
July	·1656	·1311	·1451	·1145	·14835	·12980	+ ·01855
August	·1532	·1585	·1095	·1872	·15585	·14835	+ ·00750
September ...	·2103	·1667	·1497	·2071	·18850	·17840	+ ·01010
October	·1597	·2038	·1797	·1557	·18175	·16770	+ ·01405
November ...	·1318	·1582	·1648	·2208	·14500	·19280	— ·04780
December ...	·1907	·2191	·1226	·1800	·20490	·15130	+ ·05360
January	·1729	·1287	·1518	·2059	·15080	·17885	— ·02805
February ...	·1073	·0662	·1246	·1154	·08675	·12000	— ·03325
March.........	·1262	·1509	·1155	·1448	·13855	·13015	+ ·00840
Means.........	·15101	·15547	·14656	·16016	·153242	·153360	— ·000118

The last column of this Table shows the difference R^s-R^q, so that plus denotes an excess of the lunar-diurnal range at the periods of syzygy, and minus an excess at the periods of quadrature.

According to the above there is a decided minimum in the lunar-diurnal range at the time of the first quarter, while the last quarter seems to be the maximum, the time of the syzygy showing intermediate values. The general mean would indicate an excess, though very small, in favour of the epochs of the quadrature. On examining, however, the difference for the single months, we notice that the algebraic sign denotes for seven months an excess of the epochs of syzygy, and for five only the contrary; further, that the greatest irregularity in respect to the signs and values prevails durings the months from November to February, when hot winds are most frequent, and the sudden changes in temperature, connected with these phenomena, cause the oscillations of the barometer to be much disturbed. The magnitude of the values during this period ought to induce us to receive them with caution, and to consider the eight remaining months separately. The general mean difference for the eight months, from March to October, both inclusive, represents an excess in favour of the epochs of syzygy of 0·006762 inch, a value which most probably makes a near approach to truth.

If we derive mean values of the lunar-diurnal range for the several years of observation at the respective phases of the moon, we have—

Years.	Lunar-diurnal range in pressure of air.						$R^s - R^q$.
	Full moon.	New moon.	First quarter.	Last quarter.	Mean for the epoch of		
					Syzygy.	Quadrature.	
	in.	in.	in.	in.	in.	in.	in.
1858–59.	0·16526	0·14492	0·14627	0·17259	0·15506	0·15943	−0·00437
1859–60.	·13682	·14272	·15817	·15354	·13977	·15585	− ·01608
1860–61.	·14017	·16672	·12792	·14953	·15344	·13872	+ ·01472
1861–62.	·12969	·15270	·14664	·19473	·14119	·17068	− ·02949
1862–63.	·18316	·17752	·14802	·13102	·18034	·13952	+ ·04082
Means ……	·151022	·156916	·145404	·160282	·153969	·152843	+ ·001126

The final result of this Table shows an average excess of 0″·001126 in favour of the epochs of syzygy, but an analysis of this value shows that for three years the excess is in favour of the epochs of quadrature, while but two years seem to confirm what we feel inclined to regard as the rule. So much we are able to assert, however, that the lunar-diurnal range in pressure of air at the time of the first quarter shows a minimum, and that near the last quarter and new moon a maximum in this range seems to make itself manifest. Although the evidence adduced in the case is not of such a positive nature as that produced when treating on the question of the increased pressure of air near the perigee, we feel nevertheless inclined to believe some similar relation to exist between the atmospheric tides and the moon's phases, as we know to be the case with respect to the oceanic tides, and that a more rigorous inquiry into this question than we are able on the present occasion to institute, will ultimately yield a result in strict accordance with the theory of gravitation.

Before concluding these researches I may be allowed to point out a fact corroborative of the result arrived at when speaking of the difference of atmospheric pressure near the epochs of syzygy and quadrature. The mean diurnal range resulting from the last inquiry amounts to 0″·153301; but on the former occasion we found this range to be 0″·14954. The excess of 0″·00376, of which the lunar-diurnal range of the atmospheric pressure is larger, when derived from the epochs of the moon's phases, than when obtaining it by the periods of perigee and apogee, must be attributed to the fact that in the latter case sixty-six periods of perigee were combined with sixty-seven periods of apogee, giving a fair average result; while in the former forty-three epochs of perigee and but thirty-five of apogee happened to coincide with the several phases of the moon, tending in this way to raise the mean value of the lunar-diurnal range of the barometer above that average.

IV. "On the Occlusion of Hydrogen Gas by Meteoric Iron." By THOMAS GRAHAM, F.R.S. Received May 16, 1867.

Some light may possibly be thrown upon the history of such metals found in nature as are of a soft colloid description, particularly native iron, platinum, and gold, by an investigation of the gases which they hold occluded, such gases being borrowed from the atmosphere in which the metallic mass last found itself in a state of ignition. The meteoric iron of Lenarto appeared to be well adapted for a trial. This well-known iron is free from any stony admixture, and is remarkably pure and malleable. It was found by Wehrle to be of specific gravity 7·79, and to consist of—

Iron	90·883
Nickel	8·450
Cobalt	0·665
Copper	0·002

From a larger mass a strip of the Lenarto iron 50 millimetres by 13 and 10 millimetres, was cut by a clean chisel. It weighed 45·2 grammes, and had the bulk of 5·78 cubic centimetres. The strip was well washed by hot solution of potassa, and then repeatedly by hot distilled water, and dried. Such treatment of iron, it had been previously found, conduces in no way to the evolution of hydrogen gas when the metal is subsequently heated The Lenarto iron was enclosed in a new porcelain tube, and the latter being attached to a Sprengel aspirator, a good vacuum was obtained in the cold. The tube being placed in a trough combustion furnace, was heated to redness by ignited charcoal. Gas came off rather freely, namely—

In 35 minutes	5·38	cub. centims.
In 100 minutes	9·52	"
In 20 minutes	1·63	"
In 2 hours 35 minutes	16·53	"

The first portion of gas collected had a slight odour, but much less than that of the natural gases occluded by ordinary malleable iron. The gas burned like hydrogen. It did not contain a trace of carbonic acid, nor any hydrocarbon vapour absorbable by fuming sulphuric acid. The second portion of gas collected, consisting of 9·52 cub. centims., gave by analysis—

Hydrogen	8·26	cub. centims	85·68
Carbonic oxide	0·43	"	4·46
Nitrogen..........................	0·95	"	98·6
	9·64	"	100·00

The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas, of which 86 per cent. nearly is hydrogen. The proportion of carbonic oxide is so low as 4½ per cent.

The gas occluded by iron, from a carbonaceous fire, is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horseshoe nails was submitted to a similar distillation. The gas collected from 23·5 grammes of metal (3·01 cub. centims.) was—

In 150 minutes	5·40 cub. centims.
In 120 minutes	2·58 „
In 4 hours 30 minutes..........................	7·98 „

The metal has given 2·66 times its volume of gas. The first portion collected appeared to contain of hydrogen 35 per cent., of carbonic oxide 50·3, of carbonic acid 7·7, and of nitrogen 7 per cent. The latter portion collected gave more carbonic oxide (58 per cent.) with less hydrogen (21 per cent.), no carbonic acid, the remainder nitrogen. The predominance of carbonic oxide in its occluded gases appears to attest the telluric origin of iron.

Hydrogen has been recognized in the spectrum-analysis of the light of the fixed stars, by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which α Lyræ is the type. The iron of Lenarto has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us hydrogen of the stars.

It has been found difficult, on trial, to impregnate malleable iron with more than an equal volume of hydrogen, under the pressure of our atmosphere. Now the meteoric iron gave up about three times that amount, without being fully exhausted. The inference is that the meteorite has been extruded from a dense atmosphere of hydrogen gas, for which we must look beyond the light cometary matter floating about within the limits of the solar system.

V. "Further Observations on the Structure and Affinities of *Eozoon Canadense*." In a Letter to the President. By William B. Carpenter, M.D., F.R.S., F.L.S., F.G.S. Received May 9, 1867.

University of London, May 9th, 1867.

When, on the 14th of December 1864, I addressed you on the subject of the remarkable discovery which had been recently made in Canada, and submitted by Sir William Logan to myself for verification, of a fossil belonging to the Foraminiferal type, occurring in large masses in the Serpentine-limestones intercalated among Gneissic and other rocks in the Lower Laurentian formation, and therefore long anterior in Geological time to the earliest traces of life previously observed, no doubts had been expressed as to the *organic* nature of this body, which had received the designation *Eozoon Canadense*.

The announcement was soon afterwards made, that the Serpentine Marble of Connemara, employed as an ornamental marble by builders under the name of "Irish Green," presented structural characters sufficiently allied to those of the Laurentian Serpentines of Canada, to justify its being referred to the same origin. An examination of numerous decalcified specimens of this rock led me to the conclusion, that although the evidences of its organic origin were by no means such as to justify, or even to suggest, such a doctrine, if the structure of the Canadian Eozoon had not been previously elucidated, yet that the very exact correspondence in size and mode of aggregation between the Serpentine-granules of the Connemara Marble and those of the 'acervuline' portion of the Canadian, was sufficient to justify in behalf of the one the claim which had been freely conceded in regard to the other.

In the following summer, however, it was announced in the 'Reader' (June 10, 1865) by Professors King and Rowney of Queen's College, Galway, that having applied themselves to the study of the Serpentine Marble of Connemara with a full belief in its organic origin, they had been gradually led to the conviction that its structure was the result of chemical and physical agencies alone, and that the same explanation was applicable to the supposed *Eozoon Canadense* of the Laurentian Serpentines. This view was afterwards fully set forth in a Paper "On the so-called Eozoonal Rock," read at the Geological Society on the 10th of January 1866, and published (with additions) in the Quarterly Journal of the Geological Society for August 1866. The following is their own Summary of their conclusions (p. 215):—"It has been seen (1) that the 'chamber-casts' or granules of serpentine are more or less simulated by chondrodite, coccolite, pargasite, &c., also by the botryoidal configurations common in Permian Magnesian Limestone; (2) that the 'intermediate skeleton' is closely represented, both in chemical composition and other conditions, by the matrix of the above and other minerals; (3) that the 'proper wall' is structurally identical with the asbestiform layer which frequently invests the grains of chondrodite—that, instead of belonging to the skeleton, as must be the case on the eozoonal view, it is altogether independent of that part, and forms, on the contrary, an integral portion of the serpentine constituting the 'chamber-casts,' under the allomorphic form of chrysotile, and that perfectly genuine specimens of it, completely simulating *casts* of separated nummuline tubules, occur in true fissures of the serpentine-granules; (4) that the 'canal-system' is analogous to the imbedded crystallizations of native silver and other similarly conditioned minerals, also to the coralloids imbedded in Permian Magnesian Limestone; that its typical Grenville form occurs as metaxite, a chemically identical mineral imbedded in saccharoidal calcite; (5) that the type examples of 'casts of stolon-passages' are isolated crystals apparently of pyrosclerite. Furthermore, considering that there has been a complete failure to explain the characters of the so-called internal casts of the 'pseudopodial tubules' and other 'passages' on the hypothesis of ordinary mechanical or chemical infiltration, also bearing in mind the significant fact that the 'intermediate skeleton,' in Irish and other varieties of eozoonal rock, contains modified examples of the 'definite shapes' more or less resembling the crystalline aggregations and prismatic lumps in primary saccharoidal marbles—that eozoonal structure is only found in metamorphic rocks belonging to widely separated geological systems, never in their unaltered sedimentary deposits,—taking all these points into consideration, also the arguments and other evidences contained in the present memoir, we feel the conclusion to be fully established, that every one of the specialities which have been diagnosed for *Eozoon Canadense* is solely and purely of crystalline origin: in short, we hold, without the least reservation, that from every available standing point—foraminiferal, mineralogical, chemical, and geological—the opposite view has been shown to be utterly untenable."

Considering that the Foraminiferal characters of *Eozoon Canadense* had been unhesitatingly accepted by all those zoologists, Continental as well as British, whose special acquaintance with the group gave weight to their opinion, it might have been prudent, as well as becoming, on the part of the Galway Professors, to express themselves somewhat less confidently in regard to its purely mineral origin. The case they made out would not have lost any of its real strength, if they had simply put forward their facts as affording valid grounds for *questioning* the received doctrine. And a way of escape would have been left for them, if the progress of research should happen to bring to light conclusive evidence on the other side.

Although such conclusive evidence is now producible, it may be well for me briefly to point out what I regard as the fundamental fallacies in the argument of Professors King and Rowney.

In the first place, the Serpentine-Marble of Connemara, on which their investigations had been chiefly conducted, is admitted by every one who has examined it to have undergone a considerable amount of metamorphic change. To myself, as well as to Professors King and Rowney, the evidence which it presents of the operation of chemical and physical agencies is most obvious and conclusive; whilst the evidence of its organic origin rests entirely on its partial analogy to the eozoonal rock of Canada. Hence an entire surrender might be made of the organic hypothesis as regards the Connemara marble, without in the least degree invalidating the claim of the eozoonal rock of Canada to an organic origin. But, on the other hand, if the latter claim can be sustained, it may be fairly extended to the "Irish Green," should the evidence of similarity be found sufficient to justify such an extension; since it must be admitted by every Petrologist, that no amount of purely mineral arrangement in a Metamorphic rock can disprove its claim to Organic origin, if that claim can be shown to be justified by distinct traces, in other parts of the same formation, of organisms adequate to its production. The Carboniferous Limestone, various members of the Oolitic and Cretaceous formations, and the Hippurite and Nummulitic Limestones, all exhibit in parts an entire absence of organic structure, which is yet so distinct elsewhere, as to justify the generalization that their materials have been originally separated from the ocean-waters by animal agency. And it is well known to those who have studied the changes which recent Coral-formations have undergone when upraised above the sea-level, that a complete conversion of a mass of Coral into a sub-crystalline Limestone not distinguishable from ordinary Carboniferous Limestone, may take place under circumstances in no way extraordinary.

It is, therefore, upon the character of the Serpentine-Limestone of *Canada*, not upon the nature of the Connemara Marble, that the question of organic origin entirely turns; and, as I have elsewhere shown in detail*, the hypothesis of Professors King and Rowney altogether fails to account for the *combination of phenomena* which the former presents, whilst the accordance of that combination with the idea of its Organic origin (a very moderate allowance being made for the effects of metamorphic change) is such as to establish the same kind of probability in its favour, as that which we derive in the case of the Human origin of the "flint implements" from the cumulative evidence of their succession of fractured surfaces, or in the case of the chemical composition of the sun from the precise correspondence between certain dark lines in the solar spectrum and groups of bright lines produced in a dark spectrum by the combustion of certain known metals.

I may stop to point out, however, that Professors King and Rowney do not attempt to offer any feasible explanation of the fundamental fact of the regular alternation of lamellæ of Calcareous and Siliceous minerals, often amounting to fifty or more of each kind, extending through a great range of area; nor of the fact that not only is this arrangement the same, though the siliceous mineral may be Serpentine in one place, Pyroxene in another, or Loganite in another, whilst the calcareous may be Calcite in one part, and Dolomite in another,—but that these variations may occur in one and the same specimen, the structural arrangement being continuous throughout.

And in what they state of the peculiar lamella forming the proper wall of the chambers, which I have designated the "nummuline layer," they have fallen into errors of fact so remarkable, that I can only account for them by the belief that when their paper was written they knew this layer only by decalcified specimens, and had never seen it in thin transparent sections. For they describe it as composed of parallel fibres of chrysotile packed together without any intermediate substance; whereas I have distinctly proved that the siliceous fibres are im-

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* Quarterly Journal of the Geological Society, August, 1866.

bedded in a calcareous matrix; which I therefore feel justified in regarding as a finely tubulated Nummuline shell, of which the tubuli that were originally occupied by pseudopodia have been permeated by siliceous infiltration.

So, again, while asserting that by no conceivable process could the animal substance originally occupying these tubuli have been replaced by siliceous minerals, they have entirely ignored the fact stated by me, that this very replacement has taken place in recent specimens in my possession,—a fact on the basis of which the reconstruction of the animal of Eozoon proposed by Dr. Dawson and myself securely rests.

The question may now, I believe, be regarded as conclusively settled by the recent discovery, in a sedimentary limestone of the Lower Laurentian formation at Tudor in Canada, of a specimen of *Eozoon* presenting characters that cannot, in the opinion of the most experienced Palæontologists and Mineralogists, be accounted for on any other hypothesis than that of its organic origin. For in the first place, the occurrence of a calcareous framework or skeleton in a matrix of sedimentary limestone, which also fills up its interspaces, altogether excludes the hypothesis that this framework might be the product of any kind of pseudomorphic arrangement produced by the separation of calcareous and siliceous minerals from a solution containing both. And, secondly, this specimen exhibits that which had not previously been distinctly seen in any other, viz., a distinctly limited contour, formed by the curving downwards and closing-in of the septa, in a manner as perfect and characteristic as the closing-in of the successive chambers of any polythalamous shell. I believe that no Palæontologist familiar with Palæozoic fossils would have hesitated to pronounce this specimen a Fossil Coral allied to *Stromatopora*, if it had occurred in a Silurian Limestone.

That this specimen, though differing greatly in appearance from the ordinary serpentinous *Eozoon*, really represents that organism, is shown not merely by the general arrangement of the calcareous lamellæ, but by their minute structure. This, it is true, is far less characteristically seen in thin sections microscopically examined, than it is in the specimens whose cavities have been filled up by Serpentine; the texture of which is often so marvellously little changed, as to have all the appearance of recent shell-substance. But the alteration which the shelly layers have undergone in this specimen, is precisely paralleled by that which I have been accustomed to find in the best-preserved specimens of other organic structures contained in the more ancient Limestones. And there are still distinctly-recognizable traces of the canal-system imperfectly injected with black substance, which correspond with those of the ordinary Serpentinous *Eozoon*.

For the imperfection of the specimen in this respect, however, full compensation is made in the perfect preservation of the canal-system in a small fragment of *Eozoon* long since observed by Dr. Dawson in a crystalline limestone at Madoc. This specimen having been placed in my hands by Sir William Logan, with permission to treat it in any way that should enable me to make a thorough examination of it, I have succeeded in finding in it most complete and beautiful examples of the canal-system, presenting varieties of size and distribution exactly parallel to those with which I am familiar in the Serpentine-specimens. Now as there is not in the Madoc, any more than in the Tudor specimen, any such combination of different minerals as has been supposed by Professors King and Rowney to have given origin to the arborescent forms of the canal-system of Eozoon (which they have likened to moss-agate or crystallized silver), there can be no longer any reasonable ground for disputing the essential similarity of this canal-system to that first described by myself in *Calcarina*, with which it was originally compared by Dr. Dawson*.

The extension of the inquiry into the character of the Serpentine limestones intercalated among the Gneissic and other rocks of Laurentian age in various parts of Europe, has brought to light such numerous examples of eozoonal structure, more or less distinctly preserved, as to afford strong grounds for the conclusion that this organism was very generally diffused at that epoch, and performed much the same part in raising up solid structures in the waters of the ocean, that the Coral-forming Zoophytes perform at the present time. I had myself examined before the close of 1865 specimens of Ophicalcite from Cesha Lipa in Bohemia and from the neighbourhood of Moldau, in which an eozoonal structure was distinctly traceable; and early in 1866 a more extended series was transmitted to me through Sir C. Lyell from Dr. Gümbel, the Government Geologist of Bavaria, in which I was able to trace a continuous gradation from specimens in which the eozoonal structure was distinct, to others in which, if it ever existed, it had been completely obscured by subsequent metamorphism. The results of a very careful and complete examination of the Ophicalcites of Bavaria by Dr. Gümbel himself has been communicated to the Royal Academy of Munich†.

Appearances of the same character are presented by a series of specimens of the Serpentinous Limestones from the Primitive Gneiss of Scandinavia, kindly transmitted to me by Prof Lovén.

I venture to hope that the foregoing *résumé* of the present aspect of this subject will be of interest to the Fellows of the Royal Society. I say the *present* aspect, because I am strongly convinced that we are at present only at the beginning of our knowledge of this and other ancient types of Foraminiferal structure; and that careful search in promising localities will bring to light many wonders now lying unsuspected in the vast aggregate of pre-Silurian strata.

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* A full description of these specimens by Dr. Dawson, with a notice of their stratigraphical position by Sir William Logan, has been read at the Geological Society on the 8th of May, 1867.

† "Ueber das Vorkommen von Eozoon im ostbayerischen Urgebirge," aus d. Sitzungsber. d. k. Acad. d. W. in München, 1866. i, 1.

May 23, 1867.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Intimate Structure of the Brain."—Second Series. By J. Lockhart Clarke, Esq., F.R.S. Received May 1, 1867.

(Abstract.)

Abstracts of a considerable portion of this paper have been already published in the Proceedings of the Royal Society for June 18, 1857, and June 20, 1861, under the title of "Notes of Researches on the Intimate Structure of the Brain."

After adding several new facts, and giving further explanations on the subject of the *medulla oblongata*, the author gives a full description of the morphological changes by which the auditory and other centres are developed out of elements of the spinal cord. The auditory centres consist of an outer and an inner nucleus. The outer nucleus is developed from the grey substance of the posterior pyramid and restiform body of the medulla. The inner nucleus arises between the posterior pyramid and the nucleus of the eighth cerebral nerve. From both these nuclei the *posterior* division of the auditory nerve takes its origin. The *anterior* division consists of two portions. The principal portion penetrates the medulla beneath the restiform body, and running along the outer side of the caput cornu, or grey tubercle, enters both the outer and inner nucleus. The other portion of the nerve runs backward along the upper border of the restiform body, which it accompanies over the superior peduncle of the cerebellum to the inferior vermiform process. The outer auditory nucleus, consisting of the grey substance of the posterior pyramid and restiform body, is ultimately thrown backward into the cerebellum, part of it arching over the fourth ventricle to the opposite side, while the rest extends outward to the corpus dentatum of the cerebellum.

It would not be possible to give an abstract of the numerous details of structure and the complicated connexions of different parts described in the paper. The following facts, however, may be mentioned.

The roots of the facial nerve are shown to have a very remarkable course and very complicated connexions with surrounding parts. On reaching the *fasciculus teres* they bend downward in the form of a loop, the lower arm of which is connected with the motor nucleus of the trigeminus and with the upper olivary body, as well as with their own special nuclei. The longitudinal portion of this loop forms the column which Stilling mistook for what he calls the "constant root of the trigeminus," and which Schroeder van der Kolk mistook for one of the striæ

medullares. The upper olivary bodies (which were first pointed out by the author in 1857, and subsequently described by Schroeder van der Kolk) and the trapezium are further investigated in a comparison between those of man, the orang outang, and different orders of mammals. The structure of the entire medulla oblongata in the monkey is likewise compared with that of man. The paper concludes with the physiological and pathological application of its contents.

II. "On Pyrophosphoric Acid with the Pyro- and Tetra-phosphoric Amides." By J. H. Gladstone, Ph.D., F.R.S. Received May 9, 1867.

From time to time I have communicated to the Chemical Society descriptions of certain bodies which are best viewed as amides of pyrophosphoric acid; and in pursuing the inquiry I have recently obtained some fresh results, and a new class of compounds. I propose continuing to send the details to the Chemical Society, but I may be permitted to submit to the Royal Society a condensed account of the main facts arrived at in the whole investigation, and a theory of the formation of these substances.

Pyrophosphoric acid is, in the notation now generally adopted, $P_2 H_4 O_7$. In an examination of its ferric compounds, I found evidence of the existence, in solution, of the double salt $P_2 Na_2 fe_2 O_7$*. A more remarkable fact is that the complete ferric salt, and several other pyrophosphates, can exist in an allotropic condition. Thus pure $P_2 fe_4 O_7$, prepared by double decomposition, dissolves readily in dilute sulphuric acid; but on heating the solution it separates in a form which is almost insoluble in the acid. When these allotropic salts are decomposed, the acid produced appears to have the ordinary properties. It is a *pyrophosphate* which is formed, when oxychloride of phosphorus is attacked by a strong aqueous solution of an alkali.

Pyrophosphoric acid exhibits a great tendency to form acid amides. It is only necessary to neutralize it with ammonia to get a body which, when treated with a metallic salt not in excess, gives more or less of a *pyrophosphamate* of the metal, thus:—

$$P_2 H_4 O_7 + 4NH_3 + 3fe\,Cl = P_2 (NH_2) fe_3 O_6 + H_2 O + 3NH_4 Cl.$$

Pyrophosphamic acid, $P_2 (NH_2) H_3 O_6$, may be also prepared by breaking down the higher amides. It is similar in most of its properties to pyrophosphoric acid, but is tribasic. Its ferric salt has also an allotropic modification; when heated with an acid it becomes far less soluble in sulphuric acid, ferric chloride, or pyrophosphate of sodium.

Pyrophospho-diamic acid, $P_2 (NH_2)_2 H_2 O_5$, is produced in a variety of

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* In order to avoid great complexity of formulæ, Williamson's *Ferricum*, fe=18·66, has been adopted.

ways, of which the most noteworthy are the action of ammonia on phosphoric anhydride, of ammonia and water on oxychloride of phosphorus, of alcohol or soluble bases on chlorophosphuret of nitrogen, as well as the breaking down of amides of more complicated structure. It is bibasic, and forms salts which are generally very soluble, like itself, in water and alcohol.

Pyrophospho-triamic acid, $P_2 (NH_2)_3 HO_4$, is formed when oxychloride of phosphorus is saturated with ammonia at about 100° C., and the resulting mass is treated with water, or when tetraphospho-pentazotic acid is exposed to the action of water for some time. It is nearly insoluble in water, and so are its combinations, even those with the alkaline metals. It readily decomposes most soluble salts, giving rise to compounds in which 1, 2, 3, or 4 atoms of hydrogen are replaced. With slightly acid nitrate of silver it gives a white salt, $P_2 (NH_2)_3 Ag O_4$; with the ammoniacal nitrate a bright yellow salt, $P_2 N_3 H_4 Ag_3 O_4$.

By the action of water on the compounds of ammonia with oxychloride of phosphorus, there are also produced some acid amides that belong to a higher series. Great difficulty was experienced in being certain of the purity of any specimen of these compounds; hence some doubt may still rest on their ultimate composition.

Tetraphospho-tetramic acid, $P_4 (NH_2)_4 H_2 O_9$, is a solid stable body, insoluble in alcohol, but soluble in water, and combining readily with bases, the amount of hydrogen replaced appearing to vary from 1 to 6 atoms.

Terammoniated Tetraphospho-diamic acid, $P_4 (NH_2)_2 N_3 H_{13} O_{11}$.—This is a viscid liquid, insoluble in alcohol, but very soluble in water. It forms a liquid combination with ammonia; but metallic salts appear to break it up into a variety of compounds. By the action of heat, boiling water, strong acids, or alkaline carbonates, tetraphospho-tetramic acid may be produced from it. Among the bodies formed when it is heated *per se* is a white substance, insoluble, or nearly so, in cold water, having the ultimate composition $PNH_4 O_3$. This is at once transformed into pyrophospho-diamic acid by hot water, or dilute acids.

Tetraphospho-pentazotic acid, $P_4 N_5 H_9 O_7$, is formed when oxychloride of phosphorus is fully saturated with ammonia, and the resulting mass is heated at about 230° C., and washed with cold water. It is an insoluble body, capable of decomposing metallic salts. One atom of hydrogen is replaceable by potassium or ammonium. When treated with slightly acidulated nitrate of silver, it gives a tetrazotic salt, $P_4 N_4 H_4 Az_2 O_7$, which, when decomposed by mineral acids, yields tetraphospho-tetramic acid and other compounds.

Amidated Oxychlorides of Phosphorus.—The oxychloride will absorb either 2 or 4 molecules of ammonia; and there can be little doubt that the resulting white solids consist of chloride of ammonium mixed with $P (NH_2) Cl_2 O$ in the one case, and $P (NH_2)_2 Cl O$ in the other, but I have never succeeded in separating them in a condition fit for analysis.

If either of these compounds be strongly heated, hydrochloric acid, or chloride of ammonium, is given off, and there remains *phosphonitryle*, PNO.

By the action of heat on the substances already described, other compounds may also be prepared, thus:—

Pyrophospho-nitrylic acid.—If pyrophospho-triamate of potassium be heated at a dull redness, it loses two-thirds of its nitrogen as ammonia, leaving a fused mass, which is insoluble in water, but forms compounds when treated with silver or copper salts. These have the composition of pyrophospho-nitrylates, $P_2 N Ag O_4$. If pyrophospho-triamic acid itself be similarly heated, it parts with one molecule of ammonia, and gives a body, $P_2 N_2 H_4 O_4$, isomeric with pyrophospho-nitrylate of ammonium, which is speedily resolved by damp air into pyrophosphamic acid and other compounds.

The process adopted for the analysis of these acid amides was that of boiling them with strong hydrochloric acid. This converts them all into ammonia and ordinary phosphoric acid, which were determined in the usual manner.

Theoretical Constitution.

A difficulty in understanding the formation of the bodies above described from oxychloride of phosphorus arises from the fact that they contain P_2... or P_4..., while the original phosphorus compound contains but one atom of that element. The following considerations may furnish a probable explanation and reveal their true constitution.

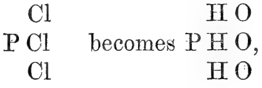
When a chloride and water act on one another, three different courses are open, each giving hydrochloric acid as one of the results. In the first case the chlorine combines with one of the atoms of hydrogen, while the remaining hydroxyl, HO, takes its place in the original compound, thus:—

$$P Cl_3 + 3 H_2 O = 3 H Cl + P H_3 O_3 \text{ (\textit{phosphorous acid})}.$$

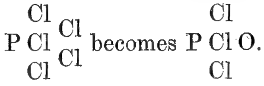
In the second case two atoms of chlorine simultaneously attack the two atoms of hydrogen, and the liberated single atom of oxygen takes their place, thus:—

$$P Cl_5 + H_2 O = 2 H Cl + P Cl_3 O \text{ (\textit{oxychloride of phosphorus})}.$$

In each of these cases we may consider the new compound as formed on the same type as the original chloride, only the chlorine is differently replaced.



$$\text{In the one case } P\begin{matrix}Cl\\Cl\\Cl\end{matrix} \text{ becomes } P\begin{matrix}HO\\HO\\HO\end{matrix},$$



$$\text{and in the other } P\begin{matrix}Cl\\Cl\\Cl\end{matrix}\begin{matrix}Cl\\Cl\end{matrix} \text{ becomes } P\begin{matrix}Cl\\Cl\,O\\Cl\end{matrix}.$$

But there is a third case in which the two atoms of hydrogen in water are attacked simultaneously by two atoms of the chloride, and the result is that the oxygen is left in combination with two molecules of the substance originally combined with the chlorine. Here it is simplest to consider that it is the water type which is preserved.

It is this third mode of action which explains the production of the compounds containing P_2... and P_4....

If we act on oxychloride of phosphorus with water, a slow replacement of the chlorine takes place, each atom decomposing a molecule of water, and the result is—

$$\begin{matrix}\mathrm{Cl}\\ \mathrm{P\,Cl\,O}\\ \mathrm{Cl}\end{matrix}+3\left.\begin{matrix}\mathrm{H}\\ \mathrm{H}\end{matrix}\right\}\mathrm{O}=3\,\mathrm{H\,Cl}+\begin{matrix}\mathrm{H\,O}\\ \mathrm{P\,H\,O\,O},\\ \mathrm{H\,O}\end{matrix}$$

which is $P\,H_3\,O_4$, *tribasic* or *ortho-phosphoric acid.* If, however, we employ strong solutions of potash or ammonia, the result is totally different. We now obtain salts of an acid formed not on the type of the oxychloride, but on the type of the alkaline hydrate, or water. To explain this the reaction must be broken up into two stages, though it is not improbable that these may occur simultaneously in nature. These stages are—

$$2\,\mathrm{P\,Cl_3\,O}+\left.\begin{matrix}\mathrm{K}\\ \mathrm{H}\end{matrix}\right\}\mathrm{O}=\mathrm{K\,Cl}+\mathrm{H\,Cl}+\left.\begin{matrix}\mathrm{P\,Cl_2\,O}\\ \mathrm{P\,Cl_2\,O}\end{matrix}\right\}\mathrm{O},$$

$$\left.\begin{matrix}\mathrm{P\,Cl_2\,O}\\ \mathrm{P\,Cl_2\,O}\end{matrix}\right\}\mathrm{O}+4\left.\begin{matrix}\mathrm{K}\\ \mathrm{H}\end{matrix}\right\}\mathrm{O}=4\,\mathrm{K\,Cl}+\left.\begin{matrix}\mathrm{P\,(H\,O)_2\,O}\\ \mathrm{P\,(H\,O)_2\,O}\end{matrix}\right\}\mathrm{O},$$

which is $P_2\,H_4\,O_7$, *pyrophosphoric acid.*

There still remains another mode of action, the replacement of 2 Cl in the oxychloride by O, and this may be expressed in the two following stages,

$$\mathrm{P\,Cl_3\,O}+2\left.\begin{matrix}\mathrm{K}\\ \mathrm{K}\end{matrix}\right\}\mathrm{O}=2\,\mathrm{K\,Cl}+\mathrm{P\,Cl\,O\,O},$$

$$\mathrm{P\,Cl\,O\,O}+\left.\begin{matrix}\mathrm{K}\\ \mathrm{K}\end{matrix}\right\}\mathrm{O}=\mathrm{K\,Cl}+\mathrm{P\,(K\,O)\,O\,O},$$

which is $P\,K\,O_3$, *metaphosphate of potassium.* And this is actually produced when the oxychloride is dropped on oxide of potassium, and a similar reaction takes place with dry sesquicarbonate of ammonium.

Reverting to pyrophosphoric acid, if $\left.\begin{matrix}\mathrm{P\,(H\,O)_2\,O}\\ \mathrm{P\,(H\,O)_2\,O}\end{matrix}\right\}\mathrm{O}$ be its rational formula, it is easy enough to understand that amides are readily formed, and to see how upon neutralizing it with ammonia, one molecule of H O is apt to be replaced by $N\,H_2$, giving $\left.\begin{matrix}\mathrm{P\,(N\,H_2)\,(N\,H_4\,O)\,O}\\ \mathrm{P\,(N\,H_4\,O)_2\,O}\end{matrix}\right\}\mathrm{O}$, the pyrophosphamate, instead of $\left.\begin{matrix}\mathrm{P\,(N\,H_4\,O)_2\,O}\\ \mathrm{P\,(N\,H_4\,O)_2\,O}\end{matrix}\right\}\mathrm{O}$, the pyrophosphate of ammonium.

Nor is it difficult to understand the formation of pyrophosphodiamic acid, when we start not with the oxychloride, but with the amidated oxychloride of phosphorus. The two stages, analogous to those given above for the formation of pyrophosphoric acid, will be

$$2P(NH_2)Cl_2O + \left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P(NH_2)ClO\\P(NH_2)ClO\end{matrix}\right\}O,$$

$$\left.\begin{matrix}P(NH_2)ClO\\P(NH_2)ClO\end{matrix}\right\}O + 2\left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P(NH_2)(HO)O\\P(NH_2)(HO)O\end{matrix}\right\}O,$$

which is $P_2(NH_2)_2H_2O_5$, *pyrophospho-diamic acid.*

The symmetry of this reaction would be lost were pyrophosphamic acid produced, and, indeed, it seems never to occur among the substances actually formed.

But the pyro-diamic acid may be equally produced, if we start with the higher amidated oxychloride formed at a low temperature. In this case it is necessary to suppose that while two molecules of the phosphorus compound attack one molecule of water, two other molecules of water give rise to the usual replacement of HO for NH_2. The two stages are precisely analogous to those given above, but are probably simultaneous, the reaction being favoured by the affinity of the hydrochloric acid for the ammonia,

$$2P(NH_2)_2ClO + \left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P(NH_2)_2O\\P(NH_2)_2O\end{matrix}\right\}O,$$

$$\left.\begin{matrix}P(NH_2)_2O\\P(NH_2)_2O\end{matrix}\right\}O + 2\left.\begin{matrix}H\\H\end{matrix}\right\}O = 2NH_3 + \left.\begin{matrix}P(NH_2)(HO)O\\P(NH_2)(HO)O\end{matrix}\right\}O,$$

which, as before, is $P_2(NH_2)_2H_2O_5$, *pyrophospho-diamic acid.*

The formation of *pyrophospho-triamic acid* is dependent on some alteration in the amidated oxychloride, when produced at a high temperature. As the nature of this change is unknown, it may be better not to speculate on the intermediate stages, but the result of the action on water would seem to be $\left.\begin{matrix}P(NH_2)(HO)O\\P(NH_2)_2O\end{matrix}\right\}O$, or $P_2(NH_2)_3HO_4$.

If this be the true explanation of the manner in which the pyrophosphoric amides are formed, it will equally explain the formation of the tetraphosphoric compounds. It does not follow that when two molecules of the amidated oxychloride have attacked one of water to form $\left.\begin{matrix}P(NH_2)ClO\\P(NH_2)ClO\end{matrix}\right\}O$, that is, $P_2(NH_2)_2Cl_2O_3$, the remaining chlorine should be replaced by HO. The process of attacking both atoms of hydrogen in water may be repeated, thus—

$$2P_2(NH_2)_2Cl_2O_3 + \left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P_2(NH_2)_2ClO_3\\P_2(NH_2)_2ClO_3\end{matrix}\right\}O,$$

which, when acted on by water in the usual way, gives

$$\left.\begin{matrix}P_2(NH_2)_2ClO_3\\P_2(NH_2)_2ClO_3\end{matrix}\right\}O + 2\left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P_2(NH_2)_2(HO)O_3\\P_2(NH_2)_2(HO)O_3\end{matrix}\right\}O,$$

which is $P_4(NH_2)_4H_2O_9$, *tetraphospho-tetramic acid.* And this compound, like the pyro-diamic acid, may be prepared from the higher amidated oxychloride, and the process is capable of the same explanation. The three stages, probably simultaneous, are as follows:—

$$2P(NH_2)_2ClO + \left.\begin{matrix}H\\H\end{matrix}\right\}O = 2HCl + \left.\begin{matrix}P(NH_2)_2O\\P(NH_2)_2O\end{matrix}\right\}O \text{ or } P_2(NH_2)_4O_3,$$

$$2P_2(NH_2)_4O_3 + \left.\begin{matrix}H\\H\end{matrix}\right\}O = 2NH_3 + \left.\begin{matrix}P_2(NH_2)_3O_3\\P_2(NH_2)_3O_3\end{matrix}\right\}O,$$

$$\left.\begin{matrix}P_2(NH_2)_3O_3\\P_2(NH_2)_3O_3\end{matrix}\right\}O + 2\left.\begin{matrix}H\\H\end{matrix}\right\}O = 2NH_3 + \left.\begin{matrix}P_2(NH_2)_2(HO)O_3\\P_2(NH_2)_2(HO)O_3\end{matrix}\right\}O,$$

which is $P_4(NH_2)_4H_2O_9$, *tetraphospho-tetramic acid.*

The *tetraphosphoric acid*, of which this is the fourth amide, must be $P_4H_6O_{13}$, a substance already known, at least in its salts, for it is Fleitmann and Henneberg's phosphoric acid.

On the view given above the rational formulæ of the four phosphoric acids may be thus expressed:—

Ortho-phosphoric acid………	$P(HO)_3O.$
Meta-phosphoric acid ………	$P\left\{\begin{matrix}HO\\O\end{matrix}\right\}O.$
Pyro-phosphoric acid ………	$\left.\begin{matrix}P(HO)_2O\\P(HO)_2O\end{matrix}\right\}O.$
Tetra-phosphoric acid ………	$\left.\begin{matrix}\left.\begin{matrix}P(HO)_2O\\P(HO)\ O\end{matrix}\right\}O\\\left.\begin{matrix}P(HO)_2O\\P(HO)\ O\end{matrix}\right\}O\end{matrix}\right\}O.$

It is more difficult to assign satisfactory rational formulæ to the two compounds containing P_4N_5…. The fact that the atoms of nitrogen are uneven in number destroys the symmetry, and seems to point to their being products of decomposition of substances containing P_4N_6…. That they both belong to the tetraphosphoric series is evidenced by their giving rise easily to tetraphospho-tetramic acid.

The reactions of the liquid $P_4N_5H_{17}O_{11}$ indicate that it is an acid ammonium salt, or is readily transformable into such. Hence I have called it *terammoniated tetraphospho-diamic acid*, and its formula will be

$$\left.\begin{matrix}\left.\begin{matrix}P(NH_2)(NH_4O)O\\P\ \ (NH_4O)O\end{matrix}\right\}O\\\left.\begin{matrix}P(NH_2)(HO)\ \ O\\P\ \ (NH_4O)O\end{matrix}\right\}O\end{matrix}\right\}O, \text{ or } P_4(NH_2)_2(NH_4)_3HO_{11}.$$

The formation of a tetraphospho-tetramate from a salt of this structure would be analogous to the ready passage of pyrophosphate of ammonium into a pyrophosphamate.

The acid $P_4N_5H_9O_7$, to which has been given the provisional name *tetraphospho-pentazotic acid*, is perhaps derived from the decomposition of tetraphospho-hexamide, $P_4(NH_2)_6O_7$, which is the complete amide of tetraphosphoric acid, and is a very likely substance to be formed by the action of water on $P(NH_2)_2ClO$ that had been exposed to heat, and probably converted into $P_2(NH_2)_4Cl_2O_2$. If this hexamide really exist, it is at once broken down by the freed hydrochloric acid, or by hydrate of potassium, thus—

$$P_4(NH_2)_6O_7+HCl=NH_4Cl+P_4N_5H_9O_7,$$

giving rise naturally to a monobasic acid.

If we regard the compound resulting from the action of nitrate of silver on this acid as containing imidogen NH, instead of amidogen NH_2, it gives a formula of great symmetry of structure—

$$\left.\begin{array}{l}\left.\begin{array}{l}P(NH)AgO\\ P(NH)\quad\;\; O\end{array}\right\}O\\ \left.\begin{array}{l}P(NH)AgO\\ P(NH)\quad\;\; O\end{array}\right\}O\end{array}\right\}O, \text{ or } P_4(NH)_4Ag_2O_7,$$

and the salt would bear the name of *tetraphospho-tetrimate of silver*.

III. "*Ovibos moschatus* (Blainville)." By W. Boyd Dawkins, M.A., F.G.S. Communicated by Prof. Huxley. Received May 9, 1867.

(Abstract.)

Ovibos moschatus, more commonly known as the musk-ox, has been described under different names by naturalists as their opinions fluctuated concerning its affinities with the ox, buffalo, or sheep. It is called the musk-ox by all the arctic explorers, *Bos moschatus* by Schreber, Zimmermann, Pennant, and Cuvier, musk-buffalo allied to the *Bubalus Caffir* of South Africa by Professor Owen, *Ovibos moschatus* by De Blainville, Desmarest, Richardson, and M. Lartet. That the latter four naturalists are right in the place they assign to it in the zoological scale, intermediate between *Ovis* and *Bos*, is proved both by the natural history and the osteology of the animal. The absence of a muffle and dewlap, the hairiness of the nostrils, the shortness of tail and smallness of ear, and the possession of two teats only, separate the animal from *Bos* and connect it with *Ovis*, while the large size and long gestation of nine months differentiate it from the latter animal. Precisely the same evidence is afforded by its skeleton. In the skull, the tapering of the anterior portion, the prominence of the orbit, the verticality of the facial plate of the maxillary, the presence of a larmier, the squareness of the basisphenoid, the presence of the occipito-parietal suture on the coronal surface; in the dentition, the sharpness of the costæ 1,2, and 3, and the absence of the accessory column from the inner interspace of the lobes of the upper teeth are among the chief ovine characters, and throughout the skeleton the same ovine tendency is manifested. With the exception of the great development of horns, there is no point in common between it and *Bubalus Caffir*. The encroachment of horn-cores or parietals differentiate it from the sheep.

The animal ranges at the present day from Fort Churchill, lat. 60°, northwards as far as the arctic sea, and eastwards as far as Cape Bathurst, lat. 71°, living for the most part on the "barren grounds," and never penetrating far into the woods. In geological times, however, it had a far greater range eastwards and southwards. In the pleistocene river-gravels lying on the solid ice in Eschscholtz Bay, in Russian America, it is found associated with the elk, reindeer, bison, horse, and mammoth. Traces of the animal ranging further to the east are afforded by the skull found on the banks of the Yena, in lat. 70°, long. 135°. Dr. Pallas's discovery of two skulls on the banks of the Obi brings the animal still closer to the borders of Europe. All three skulls were found in the "Tundas," or treeless "barren grounds" of Siberia, in the same series of gravels which afford such vast stores of fossil ivory. In Germany it has been found in three localities; and in France; in the valley of the Oise, it is associated with flint implements of the St. Acheul type, and with the mammoth and *Elephas antiquus*. It has also been found in the reindeer caves of Perigord, under circumstances that prove beyond doubt that the animal was eaten by the reindeer folk. In England it has been found in three gravel-beds of late pleistocene age, near Maidenhead, at Freshford near Bath, and at Greenstreet-green near Bromley. In 1866 the author dug it out of the lower brickearth of Crayford in Kent, where it was associated with *Rhinoceros Megarhinus*, *R. leptorhinus* (Owen), and *Elephas antiquus*. The skull in this latter case belonged to a remarkably fine old male. Thus its present limited range in space contrasts most strongly with its wide range in pleistocene times through North Siberia and central Europe, north of a line passing through the Alps and Pyrenees. Its association with animals of a temperate or else southern zone is to be accounted for by its having been driven from its usual haunts by an unusually severe winter. The rarity of its remains proves that it was not so abundant as those animals which are associated with it in France, Germany, and Britain.

Professor Leidy figures and describes two fossil skulls most closely allied to *Ovibos moschatus*, from Arkansas and Ohio, under the name of *Bootherum cavifrons* and *B. bombifrons*; they are, however, most probably the male and female of the same species. They differ from *Ovibos moschatus* only in the direction of their horn-cores, and in their bases meeting and becoming fused on the coronal surface of the male skull. The horn-cores are supported both by the frontals and parietals. In other respects they present the same ovine affinities as *Ovibos*, and certainly belong to the same genus.

IV. "Variations in Human Myology observed during the Winter Session of 1866–67 at King's College, London." By JOHN WOOD, F.R.C.S., Demonstrator of Anatomy (with a Table and Seven Drawings). Communicated by Dr. Sharpey. Received May 9, 1867.

A largely increased number of abnormalities has been the result of a systematic observation of *thirty-six* subjects during the past winter session. This has been owing partly to the comprehension of one or two irregularities which are commonly referred to in systematic works on anatomy,—such as the coronoid origin of the *flexor pollicis longus* and the insertion of the *extensor ossis metacarpi pollicis*, partly to the more productive results of a vigilant superintendence, an increased efficiency in the dissections. For much of this the author's thanks are due to the able help of his assistants, Messrs. Perrin and Amsden, and the intelligent zeal of many of the anatomical class. Mainly, no doubt, the increase is owing to an absolutely larger number of abnormalities. The value of the observations to the author is, of course, much increased by his having personally and thoroughly examined every specimen before noting it down, and, if possessing sufficient interest or novelty, sketching it from the subject. The exact numerical results thus arrived at have, in almost all important particulars, confirmed, but in some modified, the conclusions as to frequency and coincidence given in the author's former papers. What the author has termed the *lines of variation*, i. e. the particular muscles which are by far most commonly affected, are nearly identical with those of last year, as will be found by comparing the columns of the appended Table with those of the former. Only a few different will be found in the columns occupied by the sundry specimens.

Out of the total number of 295 abnormalities of muscles in 34 subjects showing abnormalities (as compared with 132 in 32 subjects of last year), we have in the *head and neck* 11 muscles affected with varieties, as compared with 10 of last year. In the *arm* we have 30 lines of muscular variation as compared with 26; while in the *leg* we have 20 as compared with 14 in last year's subjects.

The increase will be seen to be disproportionately greater in the leg. In this part also is the absolute number of the specimens increased; for while those in the head and neck proper (acting only on the parts or bones of the head, neck, and spine) are 15 as compared with 10, and those of the arm 157 as compared with 83, those of the leg are increased to 106 as compared with 39. This raises the proportion of abnormalities in the leg to two-thirds of those in the arms, as compared with rather less than one-half found last year. This seems to have some significance in being coincident with an increase in the number of female subjects to 12, as compared with 4 in last year's list. The author has remarked in former papers upon the apparent greater frequency of one variety in the foot, viz. "the abductor ossis metatarsi quinti" in the female subject. This increase is clearly maintained in the results of the present investigation, and apparently extends to some other muscles also.

To economize time, space, and the difficulties of tabulation, the explanations necessary to understand the adjoined Table are taken, as before, in the order of the columns therein given.

The first three are appropriated to the muscular varieties of the *Head and Neck,* numbering 31 instances, viz. 24 in the 22 males, and 7 in the 12 females, affecting 15 different muscles. Of these, the cleido-occipital, trapezius, occipito-scapular, and levator anguli scapulæ, amounting to 16 instances, are to be considered as belonging quite as much to the upper extremity. This leaves 15 specimens affecting 11 muscles of the head and neck exclusively.

1. *Cleido-occipital.*—By this name is signified a muscle usually about three-quarters of an inch wide, which, arising from the border of the clavicle outside the cleido-mastoid portion of the sterno-cleido-mastoideus, is placed parallel to the posterior border of the latter, and separated from it by a more or less wide areolar interval. It is distinguished from the cleido-mastoid proper by its insertion into the superior curved line of the occipital bone on the same plane as the fibres of the sterno-mastoid. It joins close up to the trapezius, with which its upper fibres are sometimes united. The true *cleido-mastoid,* on the other hand, is inserted deeper than the fibres of the *sterno-mastoid* into the mastoid portion of the temporal bone. It has been recognized as an occasional accessory portion of the sterno-cleido-mastoideus, by Meckel, Kelch, Sœmmerring, and Henle. In animals it forms an important part of the muscle called the *Cephalo-humeral.* There were in the 34 subjects examined no less than 12 specimens, all on both sides. In subject 21 it was very large, broad, and double, with a superficial slip of communication with the cleido-mastoid. Last session the proportion of specimens was strikingly similar, viz. one-third of the whole number of subjects.

2. *Omohyoid.*—One of the five specimens of abnormality in this muscle was found in the hinder belly arising from the whole length of the middle third of the clavicle covering the subclavian artery (No. 17). In the four others the anterior belly was implicated. In No. 6 it received a muscular slip from the sterno-hyoid. In No. 19 it contributed a large slip to the same muscle, the latter being double also at its origin, and giving off a muscular bundle to its fellow on the opposite side across the median line. In No. 20 the anterior belly was double, the posterior portion being attached by fascia to the stylo-hyoid muscle, which did not reach the hyoid bone. In No. 27 the anterior belly was triple, the middle portion becoming the normal insertion, the front one being inserted into the cervical fascia, and the hinder one implanted into the upper horn of the thyroid cartilage.

3. In one of the subjects (No. 3) the whole of the fibres of origin of the *Splenius colli* were placed superficial to, instead of deeper than, those of the serratus posticus superior, which thus intervened between those of the lower parts of splenius capitis and colli, the origins of these latter being in other respects normal.

In No. 28 was found a muscle which presents what is possibly a further development of this displacement. The muscle was flat and riband-like, three-quarters of an inch wide, attached above to the transverse process of the *atlas* behind the levator anguli scapulæ, and between it and the splenius colli. Passing down and inwards for about 6 inches, it ended at the spinous process of the first dorsal vertebra in a short flat tendon with diverging fibres, which passing beneath the rhomboideus minor, became blended with the deep surface of the upper fibres of origin of the rhomboideus major half an inch from their attachment. Some of the fibres were lost on the tendon of the serratus posticus superior also.

A muscle closely similar to this has been described by Mr. Macalister, of the Royal College of Surgeons of Dublin, in a paper published in the 'Proceedings of the Royal Irish Academy' (April 1866, vol. ix. pl. v.) under the name of the *rhombo-axoid* (by misprint for *atloid*). In his case the muscle was connected, however, with the rhomboideus minor on its deep surface. In both instances the splenius colli was coexistent.

A still more striking muscular anomaly, and possibly a further development of the same tendency, was seen in subject 20 (fig. 1). A distinct riband-shaped muscle, three-quarters of an inch wide, a quarter of an inch thick, and 10 inches long (*a*), was attached to the *occipital bone*, on a level with the splenius capitis (*b*), directly under the line of junction of the trapezius (*c*) with the cleido-occipital muscle (*d*), which was also present. Passing down and outwards, superficial to and obliquely across the splenii and covered by the trapezius, it was inserted by short tendinous fibres posterior and superficial to the insertions of the rhomboideus minor (*e*) and major (*f*) muscles into the border of the *scapula* opposite to the spine and upper part of the infraspinous fossa. Its fibres of insertion were more or less blended with those of the rhomboids.

The author has named this muscle the *Occipito-scapular*. It may be considered as a slip of connection from the origin of the trapezius (*c*) with the insertion of the levator anguli scapulæ (*g*), in the same manner as the *levator claviculæ* may be considered as a muscle connecting the origin of the latter muscle with the insertion of the former, thus falling among a numerous class of abnormal human muscles as arranged by the author in his first paper upon the subject. Its action would evidently be to approximate the scapula to the occiput, assisting the levator anguli; and to raise the head backwards, assisting the complexus, splenius, and trapezius. The author has not met with any mention of such a muscle in the authorities he has consulted. He has found the exact similitude of this muscle in the tame *Rabbit*. In this animal it is of like shape and proportionate size, and has an origin, insertion, and relations almost exactly the same. It is attached to the occiput close to the mastoido-occipital suture, opposite the interval between the cleido-mastoid and trapezius, and is connected below with the *insertions* of the rhomboids into the scapula.

Fig. 1.

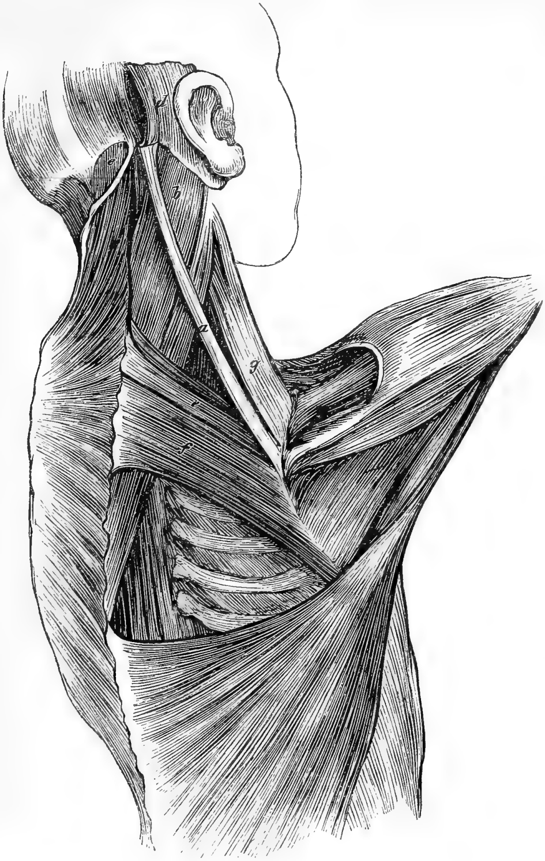
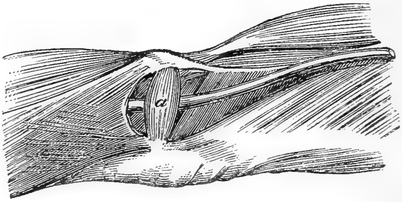


Fig. 2.



This curious concurrence is rendered the more remarkable by the additional presence in the fore leg of the same animal of a somewhat fan-shaped muscle, connecting the *epitrochlea* and the *olecranon.* As far as the author is aware, this is also unrecorded hitherto. It is entirely distinct from the inner head of the triceps, from which it is separated by the ulnar nerve. Across the nerve this small muscle is placed superficially. The same muscle was found by the author in a human male subject (No. 7 of the Table of last year's series) as a distinct muscular slip, arising from the back part of the epitrochlea, bridging over the ulnar nerve, and separated by it from the triceps above, and by a distinct areolar interval from the fascial arch which gives origin to the flexor carpi ulnaris muscle below. A sketch of this muscle is given in fig. 2 (*a*). It may be called the "*Anconeus epitrochlearis.*"

In two subjects were found abnormalities of the *Trapezius* (Nos. 4 & 13). The first was one of deficiency, the fibres of origin of the right muscle reaching only as low as the tenth dorsal spine, and those of the left only to the eighth. In the other the insertion of the muscle opposite to the scapular spine gave off a strong aponeurotic slip down and outwards to the lower angle of the scapula.

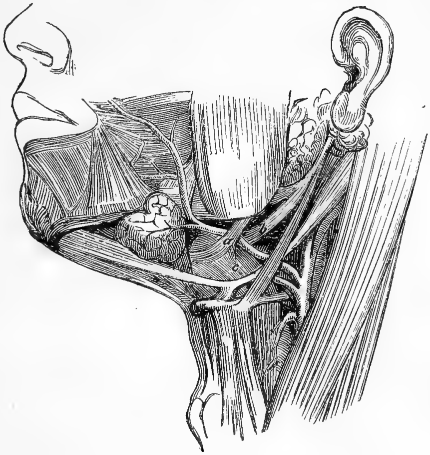
In a very muscular subject with many irregularities (No. 11) was a curious arrangement connected with the fibres of the *Platysma* just below the chin. A superficial band of muscular fibres, an inch and a half wide, arose on both sides from the mastoid process and parotid fascia, and passed down and forwards, slightly narrowing and thickening, to unite with its fellow just below the point of the chin. It crossed the insertion of the masseter, the angle of the lower jaw, and the facial artery, superficial to the "risorius Santorini," which was normal. Kelch has described this variety (as seen in two subjects) by the name of the *Musculus menti accessorius* (Beiträge, xx. S. 30).

In one subject (No. 5) the anterior belly of the *Digastric* was double, the inner abnormal one being attached to the median raphe, but not decussating with its fellow.

In a female (No. 17) was found the curious muscular slip given in fig. 3 (*a*), on the left side only. It is called by the author the *Mylo-glossus* muscle. It arose tendinous from the inner border of the angle of the lower jaw, behind and below the internal pterygoid, spreading out down, inwards, and forwards, to be inserted into the fibres of the tongue, between the stylo- and hyo-glossus muscles (*b* & *e*), joining especially the latter. The facial artery passed deeper than the muscle, and the border of the submaxillary gland overlapped it. Henle saw a cylindrical muscle arising from the same place and joining the posterior belly of the digastric (Muskellehre, S. 112). This is the nearest approach to the above muscle the author has found mentioned.

The *Stylo-pharyngeus* was in one subject (No. 19) found doubled on the right side. In another (No. 26) the *Scalenus medius* arose by a thick band of fibres covering the intertransversalis from the transverse process of the atlas. In third (No. 27) the *Scalenus anticus* received a large slip across the subclavian artery from the medius. In No. 30 the *Levator anguli scapulæ* sent a large slip of its fibres to join the insertion of the scalenus medius.

Fig. 3.



These interchanges have been frequently observed in these muscles. In No. 32 was a well-marked specimen of the muscle named by the author in his previous papers the *Supra-costal.* It was attached below to the third rib in front of the serratus magnus, and above to the first rib and cervical fascia. In No. 33 it was a very distinct duplication of the *Rectus capitis posticus major*, like that described by Albinus and Sandifort, and by Douglas both in Man and the Dog.

Twenty-one columns of the accompanying Table are occupied by the muscles of the *upper extremity* only. Twenty-six muscles are concerned. The instances number 158, viz. 117 in the 22 males, and 41 in the 12 females.

4. *Pectoralis major.*—The number and kind of abnormalities of this muscle, as well as of the pectoralis minor, coincide almost exactly with those of last year in nearly an equal number of subjects. In one (No. 5) was a detached slip arising separately from the abdominal aponeurosis opposite the seventh costal cartilage, and crossing the axilla to be inserted into the tendon of, and fascia covering, the *coraco-brachialis* about an inch below the coracoid process. In another (No. 6) a similar slip arose from the fifth rib connected with the lower fibres of the pectoralis major, and was inserted with the pectoralis minor into the coracoid process joining on to the coraco-brachialis. In No. 20 a separate slip arose from the abdominal aponeurosis at some distance from the rest of the muscle, and was inserted into the deep surface of its tendon at the upper

border, connected with the "frenum suspensorium." Such slips of the pectoralis major were noticed long ago by Sir Charles Bell (Anatomy, p. 302, 1829). In No 32 a band of fibres, about an inch broad, detached themselves from the lower border of the pectoralis major, and, curving gradually away from the rest down the arm, were inserted into a long roundish tendon about three-eighths of an inch wide, which crossed the brachial vessels and nerves obliquely down and inwards, and joined the internal brachial ligament about 2 inches above the inner condyle. Altogether this was a fair specimen of the *Chondro-epitrochlear* muscle described and figured by the author in former papers as presenting a close resemblance to the muscle so called in the Monkeys. It has been described also by Sœmmerring, Caldani, Theile, Gruber, Cruveilhier, Henle, Hallett, Macwhinnie, and Macalister. In another subject (No. 33) the clavicular fibres of the pectoralis major were uninterruptedly continuous with those of the deltoid, the cephalic vein passing through a foramen low down. Otto seems to have met with this peculiarity, which he describes as absence of the clavicular fibres of the deltoid (Path. Anatom. 1830, S. 249).

5. *Pectoralis minor.*—Four subjects were found (Nos. 6, 8, 10, & 13) to present an insertion of this muscle into the greater tuberosity of the humerus by a flat tendon usually uniting with that of the supra-spinatus, but in one case separately inserted, and grooving the upper surface of the coracoid process, where it was provided with a bursa. This arrangement was described in the author's last paper as resembling the arrangement in the Mammalia. It has been noticed by Meckel, Harrison (Dissector, i. p. 79), by Benson (Cycl. Anat. & Phys. i. p. 359), and by Macalister (Journ. Anat. & Phys. No. ii. May 1867, p. 317). In another (No. 9) the upper fibres of the left pectoralis minor were inserted into a strong costo-coracoid membrane. Those on the right side had become developed into a separate slip of muscle nearly an inch wide, which was inserted into the lower border of the clavicle itself. This slip was connected below with the second rib, constituting an approach towards the formation of a *Sterno-clavicular* muscle, as described in the author's last paper.

6. *Latissimus dorsi.*—Five subjects were affected with varieties in the insertion of this muscle. In two females (Nos. 17 & 24) the abnormality assumed the more common form of "*Achselbogen,*" viz. a short slip across the vessels and nerves to the insertion of the pectoralis major. In a male subject (No. 8) the slip on the right side was connected in a peculiar way with the *pectoralis minor*, but, on the left, in the common form with the *pectoralis major*. The former consisted of a flat, vertically placed, muscular slip 1 inch broad, attached below to the upper edge of the tendon of the latissimus dorsi, and above to the lower border of the pectoralis minor about an inch from the coracoid process, covering partly the axillary vessels and nerves. In Nos. 20 & 28 the tendon of the

latissimus gave attachment to a strong, thick muscular slip, which, passing separately down the upper fourth of the arm, finally joined to long head of the triceps, presenting the most marked approximation (especially in the last subject) to the *Dorso-epitrochlear* muscle in the Orang and other Simiadæ which the author has hitherto found in the human subject. Both the subjects were males, presenting respectively 16 and 11 muscular variations.

Fig. 4.

7. *Biceps.*—Five variations were presented by the *origins* of this muscle. Two, one male and one female (Nos. 7 & 24), showed on the left side the more common third head, arising from the humerus between the coraco-brachialis and brachialis anticus. This was also present in the right arm of another male (No. 27). On the right arm of No. 7, and the left of No. 27, the third head arose as a detached slip from the coracoid process, and, in one, from the capsular ligament also, forming a fusiform belly which joined the tendon of insertion separately at the part which gives off the semilunar fascia. In two others, both males (Nos. 8 & 13), the varieties were found connected with the *insertions* of the muscle. In the first it was found in the left arm only, and presented a most complicated arrangement (fig. 4). The origin of the muscle was normal. Just below the junction of the two heads, about the middle of the upper arm, the muscle divided into three fusiform bellies. The outer, which is largest (*a*), presents the normal insertions into the radius and semilunar fascia (cut at *b*). The middle one (*c*), the smallest, ends in a small rounded tendon, which, passing obliquely down and inwards between the semilunar fascia and the radial insertion, becomes lost on the *supinator* fascia and the *bursa* of the radial tuberosity (*d*). The inner division (*e*), constituting the larger of the abnormal bellies, ends in a strong tendon which, at the elbow, divides into three slips, the

outer joining the coronoid insertion of the *brachialis anticus* at its inner border (*g*); the middle once is implanted upon the deep or coronoid origin of the *pronator radii teres* (*f*), and the inner, connected under the superficial muscles with the coronoid origin of the *flexor digitorum sublimis*. We have thus in this complicated arrangement four insertions in addition to the usual two. These are, moreover, connected with four other muscles, viz. the brachialis anticus, the pronator radii teres, the flexor sublimis digitorum, and the supinator brevis.

In another subject also (No. 13), the biceps sent a slip to join the coronoid origin of the *pronator radii teres*. It was detached from the middle of the inner border of the muscle, as a band of muscular fibres provided with a separate tendon. In the right arm this joined with the *semilunar fascia*, and on the left with the *pronator*.

8. *Coraco-brachialis.*—In four instances this muscle presented a complete interval between its lower fibres implanted into the internal intermuscular septum and brachial ligament, and its upper fibres, inserted into the humerus. In one (No. 27) its highest fibres were inserted into a fibrous band, constituting an upward prolongation of the internal brachial ligament across the tendon of the latissimus dorsi and teres major, as described by Henle. In all, the musculo-cutaneous nerve passed between the two portions. In No. 30 the muscle was inserted into the intermuscular septum at quite the lower third of the arm.

9. *Brachialis anticus.*—In two subjects a slip of the outer fibres of this muscle was continued into those of the *supinator longus*. In one right arm (No. 26) it sent off over the brachial vessels and median nerve a slip of fascia to join the *semilunar*. In one (No. 31) it was deeply divided down the middle, the outer part sending some fibres into the *supinator longus*, and others into the *bicipital semilunar fascia*. The first-mentioned peculiarity has been before described by the author, and the last has been noticed by Hildebrandt, Sœmmerring, Theile, and Meckel, and was compared by the last-named anatomist to the arrangement in Birds.

10. *Flexor sublimis digitorum v. perforatus.*—Out of nine instances of irregularities in this muscle two were specimens of deficiency. In one (No. 5) the radial origin was entirely absent, in another (No. 18) the tendon to the little finger was wanting. This has been noticed by Meckel, Theile, and Henle. In No. 9, a muscular slip from the middle of the *pronator radii teres* joined the radial fibres of the sublimis. This has been noticed by Otto. In four subjects (Nos. 6, 8, 21, & 31) the origin of the flexor sublimis was variously differentiated. In the right arm of No. 6, a separate muscle arising from the inner border of the coronoid process gave off the *perforatus* tendon of the *index*.

A separate coronoid or middle head is described in many text-books as a normal arrangement for the *flexor sublimis digitorum*.

In almost every subject, however, the author has found that the fibres composing the superficial or condyloid head are continued uninterruptedly along the internal lateral ligament to the inner margin of the coronoid process, which they occupy along nearly its whole length, and are frequently connected there with the coronoid tendon of the pronator radii teres. This part usually gives rise to the *indicial* tendon of the muscle. In the subject above mentioned it constituted a separate muscle. In addition to this, the most common coronoid attachment, however, there sometimes exists a strong flat tendon arising from the *outer* and *lower* border of the coronoid process and joining, not the condyloid, but the *radial* origin of the muscle.

In the sketches of the muscular anatomy of the limbs of an adult female Orang-utan dissected by the author, he finds that in this animal this flat coronoid tendon gives attachment not only to some of the fibres of the radial origin of the *sublimis*, but also to the *flexor carpi radialis*, which arises both from it and from the oblique line and outer border of the radius by a common aponeurosis with the sublimis. This arrangement has been observed by Mr. Macalister in the human arm (*op. cit.* p. 12). In the Orang, the four tendons of the flexor sublimis are attached to separate muscles, the areolar intervals between which are very readily separable. That to the index lies deepest, and arises from the upper coronoid origin and lateral ligament. Those to the second and third fingers both arise from the oblique line and border of the radius, the latter being superficial and attached also to the condyle of the humerus, while the former is connected chiefly with the lower coronoid tendon, but having a separate slip also from the internal lateral ligament; while the muscle to the little finger arises superficially from the condyle of the humerus.

In one of the above-mentioned varieties of the flexor sublimis (No. 8) was a separate fusiform muscle to the little finger, arising from a tendinous intersection springing from the condyle of the humerus. In another (No. 21) the tendons of the left index and little fingers both were connected with a digastric muscle with a tendinous intersection in the middle, arising from the condyle, internal lateral ligament and upper coronoid origin. This has been observed by Macalister in a female subject, with many other irregularities. Such a digastric portion has been recorded also by Meckel (Muskellehre, S. 536). The same author describes a similar intersection in the Loris (Anat. Comp. 6. p. 340). In No. 31 all the tendons were provided with separate muscles, the first arising with a digastric formation from the condyle, internal lateral ligament and coronoid process; the second from the radius and lower coronoid tendon; the third from the condyle and internal lateral ligament; and the fourth from the condyle only. In No. 10 was a tendinous slip from the superficial surface of the sublimis to the annular ligament, the palmaris longus being normal.

11. *Flexor digitorum profundus v. perforans.*—In four subjects (Nos.

3, 9, 26, & 34) some of the indicial fibres of this muscle arose from the inner part of the front surface of the radius. In one (No. 9) these were inserted into the side of the long tendon of a fusiform muscle, which (arising with the coronoid origin of the flexor sublimis in connection with a similar one passing to the flexor longus pollicis) passed under the annular ligament and divided into two, one to join the tendon of the flexor longus pollicis, and the other (larger) that of the index perforans. This arrangement, somewhat dissimilar to those formerly described by authors, is yet formed on the same plan or type of the connection between the flexors of the thumb and index and the flexor sublimis. It forms a coalescence of the "Accessorius ad flexorem pollicis longum," with the "Accessorius ad flexorem digitorum profundum" of Gantzer.

In No. 26 one half of the muscular fibres of the *flexor longus pollicis* were implanted upon the tendon of the *index perforans.* In the left arm of No. 6 was found a detached muscular slip from the outer part of the *profundus,* ending in a tendon which joined that of the *sublimis perforatus* of the index in the palm. It was in the right arm of the same subject that the detached *perforatus* muscle of the index before described was found. In three (Nos. 10, 28, & 33) were found detached musculo-tendinous slips of the *profundus* in the fore arm of a like type. In No. 10 it was single, and gave part origin to the fourth *lumbricalis.* In No. 28 it was lost on the synovial sheath of the tendons in the palm, and in No. 33 it was connected both with this and with the first *lumbricalis.* This has been noticed by Sœmmerring, Theile, and Henle. In six subjects were found a *coronoid* origin of the *flexor profundus,* arising in common with the fibres of the *flexor sublimis* as a fusiform tapering muscle ending in a rounded tendon. In four (Nos. 7, 9, 13, & 20) this tendon joined the perforating tendon of the index finger; in one (No. 25) that of the middle finger: and in another (No. 31) those of the ring- and little fingers. This muscle is mentioned by Meckel, Sœmmerring, Theile, Henle, and by Cowper and Macwhinnie. It was named by Gantzer the "Musculus accessorius ad flexorem profundum digitorum." In No. 9, as before described, it received muscular fibres also from the radius.

12. *Flexor pollicis longus.*—In twelve subjects this muscle also derived a separate fusiform musculo-tendinous origin from the *coronoid* process of the *ulna.* This has been noticed by Albinus, Otto, Sœmmerring, and Meckel, and was called by Gantzer the "Accessorius ad flexorem pollicis longum." It is usually alluded to by text-book writers as an occasional origin, described by some from the outer, and by others from the inner side of the coronoid process. The proportion of its occurrence in thirty-six subjects is one-third. In only three was the origin at all separate from the coronoid fibres of the sublimis. It usually assumes the form of a tapering muscle, detaching itself from the indicial fibres of the *sublimis,* often in connection with the similar contribution

to the *flexor profundus*, and ending in a tendon more or less long, which joins that of the *flexor pollicis longus.* In three instances the junction took place below the middle of the arm. In a former paper the author described a remarkable development and amalgamation of these accessory origins of the *flexor longus pollicis* and *profundus digitorum* in a Negro, resulting in a complete set of tendons to each of the fingers placed intermediate to those of the *sublimis* and *profundus.*

In the Dog, the coronoid origins constitute the chief bulk of the united flexors. In the Cat, Hedgehog, Guinea-pig, Rabbit, and many other animals they form a great part of them.

In No. 7 was a muscular, and in Nos. 8, 20, & 33 a tendinous connection of the *tendons* of the *Flexor longus pollicis* and *Index perforans*, constituting a more decided tendency to the complete union of these muscles found in the lower animals than even in the instances above-mentioned of the radial origin of the flexor profundus. This connection exists more or less completely in all the Apes and Monkeys, reaching its most peculiar development by the entire substitution of the *flexor longus pollicis* by a separated and entire *flexor indicis* in the Orang-utan. It is evidently the homologous representative of the tendon of connection between the *flexor longus hallucis* and *flexor longus digitorum* in the foot.

13. *Lumbricales.*—In two subjects (Nos. 3 & 5) the *fourth* lumbricalis on the right side was inserted into the extensor aponeurosis on the ulnar side of the ring-finger (which was thus provided with two, acting in different directions), instead of the little finger, which was destitute. In Nos. 11 & 34, in the right hand, and in No. 32 in both hands, the *third* lumbricalis was bifurcated, one being inserted into the ulnar side of the middle digit (which was thus provided with one on each side), while the other was inserted into the usual place. In the left hand of No. 33 both the *third* and *fourth* lumbricalis were bifurcated, the middle and ring-fingers both having a lumbricalis on each side. These abnormalities have been described by Meckel, Theile, and Froment. According to the last-named author, the lumbricales are irregular in nearly half the number of subjects, the third being the most frequently bifurcated, and next, the fourth. In half, the author has found the irregularities on both sides; when single, he has found the right and left to be in about equal proportions irregular.

14. *Flexor carpi radialis brevis v. profundus.*—In only two subjects has the author found this year the muscle described by him in previous papers under this name. Both were imperfect specimens, arising in a penniform way from the *radius* outside the flexor longus pollicis, and inserted by a rounded tendon, which in one subject (No. 32) was as large as that of the flexor pollicis itself, into that deep portion of the *annular ligament* which is attached to the trapezoid and base of the middle metacarpal bone, secluding the sheath of the flexor carpi radialis tendon. In one (No. 20) the *palmaris longus* was normal. In the other it was

represented by a small slip from the superficial surface of the *flexor carpi radialis.* Mr. Macalister of Dublin has communicated to the author the description of a complete specimen of this muscle inserted into the base of the *middle metacarpal* bone. It existed in the *right* arm only, and had its origin from the radius internal, instead of external, to the flexor pollicis longus. He has also met with an instance of an incomplete muscle of this kind inserted into the deep portion of the annular ligament, also on the right arm. A *palmaris longus* was present in one of these cases, but not in the other.

It is somewhat remakable that in these two cases, as in all the eight cases observed by the author, this muscle has been found in the *right arm only*. It offers the best homologue in the arm to the *tibialis posticus* in the leg.

15. *Palmaris longus.*—In three subjects (Nos. 5, 24, & 32) the normal palmaris was absent in both arms. It was also wanting in the right arm of No. 28, and in the left of No. 27. In three (5, 27, & 32) there was a feeble slip of tendon from the superficial muscular fibres of the *flexor carpi radialis* to the superficial surface of the middle portion of the palmar fascia, which seemed to supply its place. This relation between the two muscles is interesting in connection with the occurrence of a flexor carpi radialis brevis in one of these subjects (32). In the left arm of one subject (No. 28) both the tendon and muscular portions of the palmaris were doubled, the supernumerary one being smaller and placed internal and posterior to the other, and arising with the condyloid portion of the sublimis. Its tendon was spread out and lost on the fascia at the wrist, a little above the annular ligament. In the right arm of another (No. 34) the tendon of an otherwise normal palmaris was doubled, both portions being inserted into the annular ligament and palmar fascia. In a third (No. 8, the subject of fig. 4) the belly of this muscle was inverted (*h*) and placed just above the wrist.

16. *Extensores carpi radiales.*—In no less than fifteen subjects these muscles presented the intervening muscle and tendon, named by the author the *extensor carpi radialis intermedius.* In six this muscle arose fleshy with the *longior*, and was inserted by a long tendon with, but distinct from, the *brevior* into the base of the third metacarpal bone. In four it arose with the belly of the *brevior*, and its tendon was distinctly inserted with that of the *longior* into the second metacarpal. In one subject it was arranged in the first way on the left arm, and in the second on the right; while in the remaining four it was double, *e. g.* there were two additional muscular bellies intervening between the longior and brevior, with long tendons crossing in exchange in opposite directions. In one, these tendons were united and more or less blended as they crossed each other. Such an arrangement has been recorded by Macalister (*op. cit.* p. 13). In another (No. 26) the left arm was provided with a single-bellied *intermedius* with two tendons, one going

to that of the longior, and the other to the insertion of the brevior. In another (No. 14) the *longior* was, in addition, provided with two tendons by division. In two subjects (Nos. 30 & 31) the tendon of the *extensor carpi radialis brevior* was inserted into the inner corner of the base of the *second* metacarpal bone as well as into the third. This was the case also in two of those which were provided with an extensor *intermedius* (Nos. 29 & 32). It is interesting as showing how an intermediate tendon and muscle may be formed by simple fission of the brevior.

17. *Extensor carpi ulnaris.*—In two subjects (Nos. 7 & 21) this muscle gave off a slip of its lower tendon to the extensor aponeurosis of the little finger. In one (No. 11) the *abductor minimi digiti* arose partly from the tendon, and was further provided with two other distinct origins—one from the pisiform bone, and the other from the upper border of the posterior annular ligament, evincing a tendency to the high origin described and figured in the author's former papers, and previously recorded by Günther, Milde, and Sœmmerring.

18. *Supinator longus.*—In three out of the four varieties found in this muscle, the tendon of insertion was double. In one (No 8, fig. 4, *i*) the lower insertion was the larger and normal one at the base of the radial styloid process, while the upper one was attached to the outer border of the radius three inches above, the radial nerve passing between them to the back of the hand. In No. 34 the same arrangement was present in both arms. In another (No. 21) the radial nerve passed higher than both tendons. In one subject (No. 28) the tendon was divided into three portions, the lowest and largest being inserted into the usual place, the upper one near the middle of the radius, and the intermediate one opposite the upper border of the pronator quadratus. The radial nerve passed between the two latter.

19. *Extensor communis digitorum.*—In two subjects (Nos. 8 & 28) the tendons of this muscle on the back of the hand were doubled; in the first for each digit, and in the last for the middle and ring-fingers only. In one (No. 29) the tendon of the index only was doubled, one being connected by a lateral slip with that of the middle finger, as the latter was to that of the ring-finger, and this,—with that of the little finger. It so resulted that all the tendons were thus joined together, except one of the two tendons of the index. The indicator was normal, but the extensor minimi digiti gave a tendon to the ring-finger. By means of these special tendons, the individual play of each finger was kept free. In one subject (No. 21) there was found, in the left hand, a single fleshy slip of the muscle first described by the author as the *Extensor brevis digitorum manûs*, arising from the dorsal surface of the os magnum and unciforme, and passing to the extensor aponeurosis on the radial side of the *middle* digit. In another (No. 23) there were found, in both hands, two slips passing from the same bones and from

the posterior carpal ligament, to the ulnar side of the *middle* and *ring-fingers*, joining the extensor aponeurosis by separate, slender, flat tendons.

20. *Extensor minimi digiti.*—In nearly half the number of subjects was the tendon of this muscle doubled (fig. 5, *c*). In one (No. 3) there were, further, two distinct muscular bellies. In two subjects (Nos. 29 & 34) the additional tendon was inserted with the common extensor tendon into the ring-finger, as in the Orang, Apes, Monkeys, Rabbit, Hedgehog, &c. In the Cat and Dog the third and second digits also are supplied by it.

21. *Extensor indicis* and *Extensor medii digiti.*—In two subjects (Nos. 8 & 27) the indicator was provided with a double tendon, showing the first tendency to the formation of a special extensor of the middle finger, such as that found as a distinct muscle in the remarkable arrangement seen in fig. 5 (*a*). Both these are constant muscles in the Apes and Monkeys.

22. *Extensor ossis metacarpi pollicis.*—An increase in the number of tendons of this muscle was seen in 16 subjects out of 36, *i. e.*, nearly half. In four the tendon was simply doubled, both being inserted into the base of the first metacarpal bone. In four others one of the two tendons was inserted into the trapezium. In one (No. 25) the tendon was triple, two being inserted into the metacarpal, and one into the trapezium. In seven instances the tendon sent off a slip which gave part origin to the fibres of the *abductor pollicis brevis.* These sometimes formed a separate muscle. In four of these there were two tendons only, one inserted into the base of the metacarpal bone, and the other going to the abductor. In two (Nos. 20 & 31) there were three tendons, one to the metacarpal bone, another to the trapezium, and the third to the abductor pollicis. Such an arrangement has been recorded by Macalister (*op. cit.* p. 13). In one (No. 11) there were no less than four tendons, three of which were inserted into the middle of the shaft and base of the metacarpal bone, and one went to the abductor. In the last subject the *extensor primi internodii pollicis* was entirely absent, increasing the similarity in the arrangement of these muscles to that found in the Chimpanzee and Orang. In two other subjects (Nos. 6 & 21) the muscular part of the extensor primi was entirely blended with that of the extensor ossis metacarpi, though the tendon was separate and its insertion distinct, into the base of the first phalanx of the thumb.

23. *Interossei manûs.*—Three specimens of the "*Palmar interosseus* of the *thumb*" of Henle were found. In two subjects (Nos. 4 & 20) the first interosseous space was occupied by two muscles, one, the "Abductor indicis" of Albinus and the older anatomists, and the other the "Interosseus prior indicis" of that author (the "Extensor tertii internodii indicis" of Douglas (Myograph. Comp. p. 181).

24. Among the miscellaneous specimens in the upper extremity were

found, in a female subject (No. 3), the muscle described by the author as the *Extensor pollicis et indicis* (fig. 5, *b*). Arising by a distinct penniform belly from the hinder surface of the ulna, interosseous ligament and intermuscular septa between the extensor secundi internodii pollicis

Fig. 5.

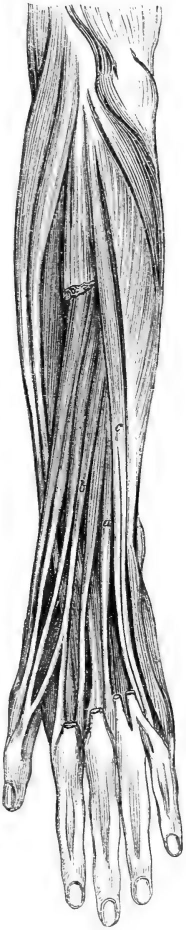
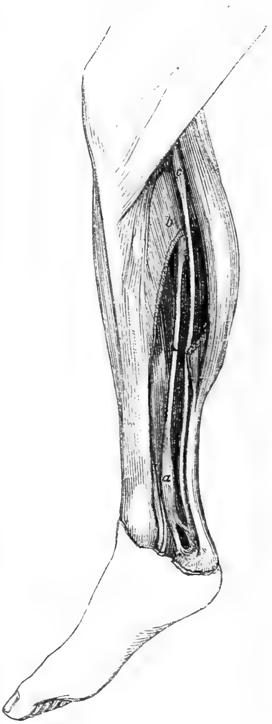


Fig. 6.

Fig. 7.



and the extensor indicis, it ended in a strong rounded tendon, which, passing under those of the extensor communis, parallel with and outside those of the indicator and extensor medii digiti, divided in the groove of the annular ligament into two tendons. The outer of these joined that of the *extensor secundi* on the middle of the first phalanx of the thumb, to be inserted with it into the extreme phalanx, and the inner, smaller, was inserted separately into the *base* of the *first phalanx* of the *index,* outside of, and distinct from, the tendons of the common extensor and indicator proper. The author has found the same arrangement in the Vampire Bat, Dog, Cat, Hedgehog, and Rabbit. Meckel found it in the Bear, Coati, and Beaver.

In its insertion, this specimen differs from those formerly described by the author by joining the tendon of the *extensor secundi internodii pollicis.* In the others it joined or substituted that of the *extensor primi internodii* which was present and normal in the subject of the woodcut (fig. 5). This arm presents an extraordinary instance of multiplication of these special extensor muscles of the hand. In a specimen of the above muscle described by Macalister (p. 4), the indicial tendon joined that of the *indicator*, and was inserted into the *second* and *third phalanges* of the *index.*

In one subject (No. 11) the *Extensor primi internodii pollicis* was altogether wanting on both sides. A small tendinous looking ligament was attached to the styloid process of the radius and passed to the base of the first phalanx of the thumb, which seemed to represent the lower part of its tendon on both sides. It indicated an arrest of development in the muscular germ above, and was unattended by any evidence of diseased action, or any peculiarity in the muscular part of the extensor ossis metacarpi pollicis, usually so closely connected with this muscle. The occasional total absence of this muscle was noticed by Sœmmerring and Meckel. In one subject (No. 21) the *extensor primi internodii pollicis* was entirely blended at its muscular portion with the *extensor ossis metacarpi*, its tendon becoming free at the styloid process of the radius. This has been observed by Theile. In two subjects (Nos. 20 & 34) the tendon of the same muscle sent a large portion (in the last the chief portion) of its fibres to join that of the *extensor secundi* at the base of the ungual phalanx. Sœmmerring has observed this peculiarity. Macalister found once in about nine subjects an opposite arrangement to this, viz., the tendon of the *extensor secundi* giving a slip to the base of the first phalanx. This has been also seen by the author in cases of absence of the extensor primi internodii.

In one female subject (No. 17) was found a large slip of the spinal fibres of the *Infraspinatus* passing superficially to the rest of the muscle and to the teres minor, to be inserted into the lowest part of the hinder border of the greater tuberosity.

In a male subject (No. 14) was found, in the right arm, a fine specimen of the detached portion of the subscapularis, which has been described by Professor Haughton under the name of *Infraspinatus secundus*, and by Macalister under that of *Subscapulo-humeral* or *capsular.* It was quite detached from the subscapularis, arising from the border of the scapular as a flat muscular band, 1 inch wide, crossed the long head of the triceps, and became inserted into the neck of the humerus at the same place as the capsular ligament, overlapped a little by the tendon of the latissimus dorsi. This muscle has been found by Haughton in the *Macacus nemestrinus* and other Quadrumana, and by Macalister in the Horse, Seal, and other Mammalia. In No. 24 was found a *Transversus manûs*, entirely separate from the bulk of the fibres of the abductor pollicis, and arising chiefly from the neck of the third metacarpal bone and transverse ligament.

The remaining ten columns in the Table are occupied by abnormalities of the *lower extremity*, affecting 23 muscles, and comprising 106 instances, viz. 74 in the 22 males, and 32 in the 12 females.

25. *Peroneus tertius.*—This muscle presented varieties in no less than 14 subjects. In no less than five it was *absent*; in three, on both sides, viz. two males (Nos. 24 & 28) and one female (No. 16). In two other females (Nos. 10 & 24) it was totally absent on one side only, in one in the right, and in the other in the left leg, the representative in the other leg being in each case so small as to be of little account. In one, indeed, it was a mere slender band of fibrous tissue attached to the lower fibres of the *extensor communis digitorum.* It may be said, then, that in one-fourth of the 12 female subjects it was wanting, and in two only out of the 22 males. In two males (Nos. 20 & 27) its tendon was doubled. In two other males (Nos. 30 & 32) its tendon was inserted into the base of the fourth as well as the fifth metatarsal bone. In four (Nos. 3, 8, 11, & 29) it sent forward a slip to join the extensor aponeurosis of the little toe, in the way of the *peroneus quinti* from the *brevis.* In all these four, except on the right leg of No. 8, the true *peroneus quinti* from the brevis was coexistent. Both these varieties have been well known to anatomists since Meckel. In one (No. 7) the slip was lost on the fascia covering the last dorsal interosseus, and did not reach the toe.

26. *Peroneus quinti.*—In 12 subjects (or one-third of the whole) was found a representative tendon of this animal muscle more or less complete, connected with the tendon of the *peroneus brevis,* and leaving it just below the malleolus. In three (Nos. 2, 16, & 26) the slip was attached to the front end of the fifth *metatarsal* bone, and more or less blended with the dorsal interosseous fascia—an arrangement which was noticed by Meckel in some subjects unprovided with a peroneus tertius. In all the nine other instances the tendon was more fully developed, and joined in forming the *extensor aponeurosis* of the *little toe.* In one subject only (No. 17) was it confined to one side. Three were found in the 12 female subjects, and nine in the 22 males, showing a larger proportion in the latter.

27. *Extensor primi internodii hallucis longus.*—In no less than 19 subjects, or more than one half, was found, in both legs, a long tendon attached to the inner part of the base of the first phalanx of the great

toe distinct from that of the *extensor brevis digitorum.* In three subjects (Nos. 9, 17, & 31) this tendon was provided with a well-developed and distinct penniform muscular belly, arising from the fibula and interosseous ligament, and separated by an areolar interval from that of the *extensor longus* or *proprius hallucis.* In a male subject (No. 9) this muscle lay at first outside the extensor proprius, and was provided with two tendons, the outer one joining the great-toe tendon of the *extensor brevis,* and the inner, crossing under that of the extensor proprius, was inserted into the usual place on the inner border of the base of the *first phalanx.* In a female (No. 17) the muscle lay to the inner side of the extensor proprius, its tendon subdividing in the same way, and going to the same destinations as the last specimen, but the *outer* one crossing in this case under the extensor proprius. In another female (No. 31) the right leg was provided with a distinct muscle of this kind, with a single tendon joining that of the extensor brevis. In the left leg it was represented only by a slip of tendon given from that of the *extensor proprius* at the ankle, and lying inside it along the foot to the first phalanx, where it was inserted in the usual way. In the 14 other subjects, the latter was the arrangement in both legs, the muscular fibres, together with the upper part of the tendon, being united more or less with those of the *extensor proprius.* Meckel remarks that the above abnormality is homologous to the *extensor primi internodii pollicis* in the hand. It is also mentioned by Sœmmerring, Theile, and Henle.

In a male subject (No. 23) this tendon to the base of the first phalanx of the hallux was given off from the outer side of that of the *tibialis anticus.* This anomaly had been previously found by the author in two subjects (also males), which were described and figured in his first paper on the subject. He is not aware that it has been observed by any other anatomist. It is not to be confounded with the common insertion of a slip of the tibialis tendon into the base of the first *metatarsal* bone. During the last Session a fine example of this formation was seen in a still-born male fœtus, which was not found to present any other muscular variety. In another adult male (No. 33) it was found in the right leg, with the addition of a second slip of tendon from the *extensor proprius*; while in the left leg, two slips came from that of the latter muscle, the outer joining the tendon of the *extensor brevis digitorum,* and the inner inserted separately into the base of the first phalanx of the hallux, as before seen in those (Nos. 9 & 17) with complete muscular bellies. The forward prolongation from the tendon of the *tibialis anticus* to the hallux presents a curious parallel on the inside to that of the *quinti* from the *peroneus brevis* on the outside of the foot. A blending of the tibialis anticus with the *long* extensor of the great toe is said by Meckel to be found in the Porcupine. Six out of the nineteen subjects possessing a separate tendon to the base of the hallux were females (one half of the whole number of subjects of this sex). These comprised two out of the three complete specimens.

28. *Extensor longus digitorum pedis.*—In one subject (No. 6) the innermost tendon of this muscle detached a separate slip to be inserted into the *base* of the *first phalanx* of the *second* toe, producing an exact analogy to the arrangement in the great toe last described, and which also coexisted in the same subject. It is mentioned by Meckel as homologous to the *indicator* in man, and as also found in the Pig and Porcupine (Anat. Comp. vi. pp. 429 & 432). In another (No. 9) a connecting slip from the innermost tendon of the long common extensor joined at the base of the metatarsals with that of the *extensor proprius hallucis.* A similar arrangement is said by Meckel to be found in the Kangaroo and in the Ruminants. In one (No. 11) the tendons of the second, third, and fourth toes arose by a separate muscular belly from the outer tuberosity of the tibia and head fibula; that to the fifth toe coming from the fibres of the *peroneus tertius.* This is found, according to Meckel, in the Hyæna, Bear, and other Carnivora, and also in the Kangaroo and some Rodents. In one subject (No. 19) two small slips of tendon, from those of the two outermost toes, were inserted into the shafts of the fourth and fifth *metatarsals* respectively. This is similar to the arrangement found in the Sloths and Reptiles. In No. 23 there was a reduplication of the extensor tendon of the little toe. In No. 26 the outermost tendon of the *extensor longus* was connected with that of the *extensor brevis* by a long slip arising from the former above the annular ligament, and joining the latter on the dorsum of the foot. In two (Nos. 32 & 33) the tendons of the long extensor were each provided with a separate muscular belly. In the former there was also a double tendon to the second toe.

29. *Extensor brevis digitorum pedis.*—In one subject (No. 9) a tendinous slip from the *second* tendon of this muscle joined that of the *first* dorsal interosseus; and another from the *third* tendon, that of the *second* dorsal interosseus. In No. 11 this connection existed with the *first* dorsal interosseus only. This evidence of connection between these muscles is interesting in relation to the occasional formation of an *extensor brevis digitorum* in the hand, which the author has in former papers explained by posterior displacement and separation of the superficial fibres of the *dorsal interossei.* In two (Nos. 23 & 26) the tendons to the second toe were doubled.

30. *Flexor longus digitorum* and *Flexor accessorius.*—In one subject (a female) the first tendon of the former muscle was entirely wanting, its place being supplied to the second toe by one from the *flexor hallucis,* approaching the formation in some of the Apes. In No. 14 was found a fully developed specimen of the *flexor longus accessorius,* arising from the lower third of the hinder surface of the

fibula and the adjacent aponeurosis, as a distinct muscle ending in a stout tendon, which passed under the annular ligament outside the vessels, and was joined by the muscular fibres of the "massa carnea Sylvii," and by the tendons of the perforans in the middle of the sole. The fibres of its tendon passed exclusively to the three outer toes. In No. 15 the "massa carnea" was replaced by a thick tendon attached to the inner border of the tuber calcis. At its junction with the outer tendon of origin, a small flat muscular belly was developed upon it. In the right foot of No. 24, a tendon from the outer head of an otherwise normal accessorius was joined to the superficial or *perforated* tendon of the middle toe, forming decussating fibres with others from the opposite side of the latter in the usual way. Three out of the four abnormalities in these muscles were found in female subjects. The complete *flexor accessorius longus* was seen, however, in a male subject, as in the three instances described last year.

31. *Lumbricales pedis.*—All the abnormalities in these muscles resulted from deficiency. In two the *fourth* was absent, one on the right side and one on the left. In one the *second* was wanted on both sides. All were male subjects.

32. *Flexor brevis digitorum.*—All the varieties of this muscle were also from deficiency. In all the seven subjects affected, the tendon to the little toe was absent, and, in six out of seven, on both sides. In one (No. 3) its place was supplied by a small fusiform slip of muscle, arising from the outermost tendon of the *flexor longus perforans.* In another (No. 4) the supplementary muscle arose by two slender fusiform bellies, one from the long flexor tendon, and the other from the inner tubercle of the calcis, deeper than the fibres of the flexor brevis digitorum. This, which the author looks upon as a transitional form to the arrangement found in No. 3, and in the Apes and Monkeys, was precisely like that given in the author's paper of 1865. In the rest of the subjects no substitute to the missing tendon was found, though possibly a feeble development may have escaped observation in some of them. Meckel has remarked on the frequent deficiency of this tendon in the human foot, and also that it is not always supplemented by the flexor perforans, comparing it to the usual deficiency of the flexor brevis in the Monkeys, and its total disappearance in other Mammalia.

33. *Abductor ossis metatarsi quinti.*—No less than 17 specimens of this muscle, arising separately from the outer tubercle of the calcaneum, and inserted into the tubercular base of the fifth metatarsal bone, were found in the 36 subjects (very nearly one half). In three subjects it was found on the right foot only, and in two, on the left only. In the other 12 it existed on both sides. Ten of the specimens were found in the 24 males, and seven in the 12 females, giving a preponderance of frequency in the latter sex. This preponderance in the female sex is still more striking, if the cases given in the author's

last paper are included in the estimate; 8 having been found in the 16 females (or one half), while only 16 were found in the 54 males (not one-third). If this be established by future observation, as well as the more frequent deficiency of the *peroneus tertius* in the same sex before alluded to, its bearing upon the relative structural inferiority of the sex will be curious, since both are animal peculiarities.

Mr. Macalister states that he has found the *abductor of the fifth metatarsal bone* existing as a distinct muscle in nine out of every twelve subjects. In No. 5 of the Table the muscle was peculiar in arising from the *inner* tubercle of the calcaneum by a large, distinct, and triangular fleshy belly, and in being inserted by a long tendon into the *neck* or anterior part of the shaft, instead of the tubercle of the fifth metatarsal bone.

34. Out of 18 sundry specimens of abnormalities in the lower extremity, two were peculiarities of the tendon of the *Flexor longus hallucis.* In one, a female (No. 3), the usual slip of union with the flexor longus digitorum was wanting. In another female (No. 6) the *flexor longus hallucis* first received a long slip from the *flexor communis,* and then gave two separate tendons to the second and third toes. That to the second constituted the only perforating tendon, the one from the common flexor being wanting, while that to the third toe joined at the base of the digit with a smaller one from the common flexor. In two male subjects (Nos. 5 & 21) the *Plantaris* muscle and tendon were both apparently blended with the outer head of the gastrocnemius. In No. 30 the *Superior gemellus* was wanting. In the right foot of No. 8, a male subject remarkable for the number of its abnormalities, a considerable portion of the outer fibres of the *Flexor brevis hallucis* were detached from the rest, and inserted into the inner tubercle on the base of the first phalanx of the *second* toe. In the left foot of the same subject a still larger slip from the fibres of the *Adductor hallucis* was detached to the same destination. This was also found as a less developed specimen in the left foot of No. 13, also a male. Two specimens of these abnormalities, also in male subjects (one of each kind), were described by the author in his paper of last year. They do not seem to have been before recorded by any anatomist, though apparently recurring in the proportion of about once in 18 or 20 subjects.

A male subject (No. 9) was remarkable for the presence of the muscle described by Otto as the *Peroneus quartus* (Neue seltene Beobacht. S. 40), arising from the lower fourth of the outer surface of the fibula below the peroneus brevis, and inserted by a distinct tendon into the outer side of the calcaneum, upon the tubercle between the peroneal grooves. Theile mentions that this muscle sometimes replaces the *peroneus brevis* itself. In the case just described, both the peroneus longus and brevis were coexistent. A variety of the same character, but inserted into the outer border of the cuboid, is recorded by Macalister *(op. cit.)* in a subject having no peroneus tertius. One of the peronei muscles is, according to

Meckel, inserted into the cuboid in the Kangaroo. In a male subject (No. 14), the right *Peroneus longus* had a double tendon, one inserted into the internal cuneiform, and the other into the base of the first metatarsal. In a female (No. 15) the tendon of this muscle gave origin in the sole to the *flexor* and *opponens minimi digiti*, as well as to the *third plantar interosseus*, as in the variety figured in the author's paper of 1865. In another female (No. 16) the *Peroneus brevis* was, in both legs, provided with a double tendon, both inserted into the usual place. The peroneus tertius in the same subject was totally absent. In the left leg of a male (No. 20) a slip of tendon was detached from the outer border of the *Tibialis anticus* muscle to be implanted into the inner border of the *anterior annular ligament* and *dorsal fascia*. In both legs of another male (No. 29) a more decided development in this direction had resulted in a distinct, flat, spreading muscle, 3 inches long, arising from the outer surface of the tibia below and distinct from the fibres of the tibialis, and ending in a round tendon which was inserted into the annular ligament and dorsal fascia below the malleolus. Such a muscle was described by the author in his paper of 1864 under the name of the *Tensor fasciæ dorsalis pedis*, occurring on both sides in a female subject.

In two subjects (Nos. 22 & 34) a considerable portion of the inner fibres of the *Pectineus* were found to pass across the front of the deep femoral artery to become inserted with the upper fibres of the *Adductor longus*, an irregularity which does not seem to have been hitherto noted. A similar extension of the origin of the adductor longus is seen in the Marmot among the Rodents, in the Ratel of the Carnivora, and in the Magot and Chimpanzee among the Quadrumana.

No. 24 was found to possess a remarkable development in both feet of an *Opponens* or *flexor ossis metacarpi minimi digiti.*

In the right leg of a muscular female subject (No. 25), the *Biceps flexor cruris* was provided with a third head. This consisted of an elongated, rounded, and fusiform muscle (fig. 6, *a*), 8 inches long and three-quarters of an inch wide, connected above by a rounded tendon, 2 inches long, with the strong fascia which covered the deep surface of *gluteus maximus* (*b b*, cut and turned aside in the figure). Below, it was united by a tendon, 1 inch long, with the ischial or long head (*c*), just above its junction with the femoral head (*d*) at the lower third of the thigh. An additional head to this muscle, though not at all common, yet has been recorded by various writers, viz. by Meckel, from the upper part of the "linea aspera;" by Gruber, from the internal condyloid ridge of the femur; by Henle, from the fascia lata near the upper end of the linea aspera; and by Sœmmerring and Gantzer, arising from the tuber ischii. Of these, the three former joined the femoral or short head, while in the instances given by the two last-named authors, the abnormal head joined the ischial or long head. All having a closely or identically similar origin to those heads of the muscle with which they afterwards respectively united, may be considered as extensions and separations of a portion of the fibres of those heads of origin. Mr. Macalister mentions that in a male subject he found in both legs a continuation of the tendinous ischial origins of this muscle over the surface of the great sacro-sciatic ligament to the side of the sacrum, but it does not appear that this constituted any approach to a distinct head. But in the specimen under consideration, the abnormality is constituted by a distinct muscular bundle, with an upper and a lower tendon, the fibres of the former capable of being traced in those of the deep gluteal fascia for a considerable distance. In the Dog the author has found the almost exact counterpart of this third head of the *biceps flexor cruris.* It is a deeply placed slender band of muscular fibres, arising from the surface of the great sacro-sciatic ligament. It lies under the ischial origin, and becomes inserted into the fascia on the outside of the leg below the main bulk of the widely-spread biceps proper. It is there connected also with a fibrous sheath which invests the tendon of the *plantaris.* In this animal this musclar slip seems to represent the caudal and sacral origin of the biceps in the Rodents, and other Mammalia. The homology between the abnormal third heads in the human subject and the caudal origin in animals was pointed out by Theile.

In a muscular male subject was found an abnormality, in many points resembling that described by Gantzer as the "*Accessorius ad calcaneum.*" It was, however, very different in its origin to those described by that author, although identical in its form and insertion (fig. 7, *a*). A long slender tendon, very much resembling that of the *plantaris* in its texture and appearance, was placed along the inner side of it, so as to present the appearance of a double plantaris. This tendon was attached above to the upper third of the hinder surface of the fibula, below the origin of the soleus (*b*), and crossed obliquely the posterior tibial vessels, muscles, and aponeurosis, towards the inner malleolus. At the lower third of the leg, a flat, ovoid, tapering, muscular belly, 3 inches long and 1 inch wide, was developed upon it, and became implanted by a short-spreading tendon upon the calcaneum, in front and to the inner side of the tendo-achillis, about three-quarters of an inch distant from it. From the lower part of its outer border the muscle sent off a tendinous slip, which joined the plantaris tendon in a mass of fibro-fatty tissue placed above the bursa of the tendo-achillis. Hyrtl has mentioned the occasional occurrence of a muscle somewhat resembling this, as arising from the popliteal (?) fascia, or *lower* part of the fibula, and inserted into the calcis. This Henle seems to consider as an abnormal plantaris. In the case just described, however, the size, shape, and position of the muscular belly, and the insertion of the lower tendon so much resemble the muscle described by Gantzer, and also that figured by the author in his paper of 1864, that he has no hesitation in referring it to the same class, with a somewhat higher origin, obtained by differentiation of the more vertical fibres of the posterior tibial aponeurosis. It constitutes probably, however, a link with the *plantaris*, similar to that which the muscle of the arm, which he has called the *flexor carpi radialis brevis*, in some specimens forms with the *palmaris longus*. From this point of view, this abnormal muscle in the leg has a similar relation to the *tibialis posticus* that the incomplete muscle in the arm has to a *complete flexor of the middle metacarpal bone*, its homologue; and it occupies a like intermediate relation to the *soleus* as the one in the arm does to the *flexor sublimis*. We shall find herein the most probable solution of some of the difficulties of the homologies of these post-tibial muscles.

In addition to the foregoing subjects, the author has had the advantage of descriptions and sketches of muscular abnormalities affecting three subjects out of eight, from his friend and former assistant Mr. Bellamy, demonstrator in anatomy at Charing Cross Hospital. In one muscular male were found four abnormalities, viz. in the right arm, a double *palmaris longus*. The irregular one was placed internal to the other, with its muscular fibres commencing just above the middle of the arm, and continued down to the annular ligament, into which and the palmar fascia it was inserted. In the same arm was found a well-developed *extensor carpi radialis intermedius*, arising distinctly between the longior and brevior by a fusiform belly, and inserted by a long tendon into the *posterior annular ligament*, close to the sheath for the outer extensors of the thumb. This the author looks upon as a formation intermediate to complete development of an *extensor carpi radialis accessorius*. A little further extension forwards and outwards would have brought the insertion of this muscle into relation with the origin of the abductor pollicis brevis and the base of the first metacarpal.

On the left arm of the same subject was a development in the same direction in the lower part of the arm. A separate muscle was formed of those upper fibres of the *extensor ossis metacarpi pollicis*, which so frequently give off a slip of tendon to the origin of the abductor pollicis. The muscle arose from the radius and interosseous ligament, quite distinctly from the extensor ossis metacarpi, and was provided with a separate tendon, which, passing in the same sheath with that of the latter, subdivided into two tendons, one to be inserted into the base of the first metacarpal, and the other to join the outer fibres of origin of the abductor pollicis brevis.

If both the tendencies evinced in this interesting concurrence had been combined in the same arm, the result might have been the production of an entire *extensor carpi radialis accessorius*, like that described by the author in former papers. In the left leg of the same subject was found a large and well-marked specimen of the *accessorius ad cal-*

caneum of Gantzer, arising by a flat, bipenniform, muscular belly from the posterior tibial fascia below the tibial origin of the soleus, and inserted by a flat spreading tendon, which crossed obliquely the post-malleolar tendons into the os calcis in front of the tendo-achillis. From its outer border was given off a spreading aponeurosis, which was attached to the hinder border of the outer malleolus, almost like that seen in figure 7. In the left leg of a female subject was found the *perforatus* tendon of the *little toe*, arising from a separate triangular-shaped muscle, which was attached to both the tubercles of the calcaneum between the superficial muscles and the accessorius, like that described in the author's former papers.

In the left arm of another muscular male subject two abnormalities were found, viz. a third head of the *pronator radii teres*, arising with the fibres of the *brachialis anticus* at the junction of the middle and lower thirds of the humerus. The median nerve and ulnar vessels passed between the abnormal and condyloid heads, and the radial artery came off high in the upper arm. No supra-condyloid process was found on the bone (as described by Gruber in such a case), although carefully looked for. The other abnormality was a high muscular origin of the *abductor minimi digiti*, arising from the fascia covering the inner flexor muscles of the fore arm by a single penniform head, and joining partly with the normal abductor, and partly inserted by a separate tendon into the base of the first phalanx of the little finger.

The author is indebted also to Mr. J. Galton, of the Dreadnought Hospital, for some clever sketches of three abnormalities, one of a detached slip of the *pectoralis major*, arising from the anterior end of the fifth rib, and inserted behind the sternal fibres into the fascia covering the *coraco-brachialis*, an inch or so below the coracoid process; another, of an "*accessorius ad flexorem pollicis longum*" of Gantzer, the tendon of which, after being first connected by a broad aponeurosis with the muscular belly of the flexor longus pollicis, was then divided into two slips, one of which joined the tendon of the last-named muscle, and the other the indicial tendon of the flexor profundus digitorum (as in subject 9 previously described). The third was a small fusiform muscular slip, found on the deep surface of the *flexor brevis hallucis*, arising by a pointed tendon from internal cuneiform bone, and inserted by another round tendon into the abductor and inner head of the flexor brevis hallucis, close to the sesamoid bone. It seems to represent the "*inter-osseus palmaris volaris*" of the hand.

Out of 36 subjects dissected at King's College during the Session, 34 have been found to present muscular abnormalities worthy of note. Four of these had also noteworthy abnormalities of some of the arteries; viz. No. 3, having 10 muscular varieties in the head and arm, had also an irregularity of the third part of the subclavian, whence a common trunk was given off for the posterior and suprascapular arteries. The internal mammary also gave off an accessory inferior thyroid. No. 7, having 10 muscular abnormalities, of which 7 were in the head and arm, presented that remarkable irregularity—the right subclavian given off from the descending aorta below the left—while the two carotids came off from a common trunk*. No. 20, having 16 muscular abnormalities, 12 being found in the arm, had also a high origin of the radial artery. No. 27, having 12 muscular abnormalities, of which 9 were in the arm, presented the left vertebral arising from the aortic arch, and the posterior- and supra-scapular coming by a common trunk from the second part of the subclavian.

From the 34 cases contained in the adjoined Table, which were all examined and noted with the utmost care and accuracy, a fair approximative idea may be deduced of the relative frequency of certain special instances in the two sexes; on both sides of the body, or on one side only.

The total number of muscular abnormalities noted in 36 subjects is 295 (reckoning both sides as one), of which 221, or about two-thirds, were found on both sides, and 74, or about one-third, on one side only; the proportion on the right side only being 39, and those on the left side only 35, or nearly equal on either side.

The individual abnormalities which exceed the above proportion *on both sides* are—the cleido-occipital; those of the pectoralis minor, coraco-brachialis, brachialis anticus, extensor carpi ulnaris, and the interossei; and the extensor medii digiti in *the upper extremity*; and the extensor longus primi internodii hallucis, and those of the extensor brevis digitorum pedis in the *lower limb, all* of which were found represented on both sides; while the proportion of the abnormalities of the latissimus dorsi, the peroneus quinti, and the abductor ossis metatarsi quinti found on both sides was also greater than that above given.

Those instances of which the proportion *on one side only* was greater than the average, were found in the flexor sublimis and profundus digitorum and lumbricales, and the more rare abnormality, the flexor carpi radialis brevis vel profundus, *all* of which last were found in the right arm. Of the biceps flexor cubiti and the flexor longus accessorius digitorum pedis nearly as many were found on one side only as on both.

The total number of abnormalities found in the 24 males was 215, and in the 12 females 81, showing a greater proportionate frequency in the male sex of almost as many more. Of this number, 15 are confined to the *head, neck, and thorax*; 4 of which are in females, or rather less than the foregoing average.

No less than 174 are connected with, and acting chiefly upon, the bones of the *upper limb*, 130 of which are in males, and 44 in females. This also is proportionately less in the female than the general average.

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* For an account of the formation of this abnormality, see a paper by the author in the Transactions of the Pathological Society of London, vol. x. p. 119.

Of the remaining 106 found in the *lower limb* 74 were in male, and 32 in female subjects, proportionately a considerably greater average on the side of the female. So far as these go, abnormalities of muscles appear to preponderate in the male, in the *head*, *neck*, *thorax*, and *arm*, and, in the female, in the *leg*.

In a much greater proportion than this on the male side were the special abnormalities of the cleido-occipital, pectoralis major, biceps, coraco-brachialis, brachialis anticus, flexor longus pollicis, lumbricales and interossei manûs, flexor carpi radialis profundus, palmaris longus, supinator longus, extensor communis and brevis digitorum manûs, and extensor carpi ulnaris in the *upper limb*; and the peroneus quinti, extensor longus and brevis digitorum, and lumbricales pedis in the *lower*. On the female side the most tangible preponderance is found in the frequency of *absence* of the peroneus tertius, and of the *presence* of the abductor ossis metatarsi quinti, the extensor carpi radialis intermedius, and of the extensor longus primi internodii hallucis.

The Table shows as decidedly as that of last year, the general absence of *correspondence* in *combination* of the muscular abnormalities.

Of the 14 subjects in which there are more than 10 variations, three only are females. One subject has 17 muscular abnormalities, of which 15 are connected with the arms, and 2 only with the legs. Two have 16 abnormalities; in one of them 11 are connected with the arms (including the cleido-occipital and the occipito-scapular given in fig. 1), 1 with the head, and 4 with the legs; the other has 5 in the legs and 1 in the head and neck. Two males have each 13 abnormalities; in one 10 are connected with the arms, and 3 with the legs; and in the other, 1 is found on the ribs, and 4 in the legs. Three subjects (one of them a female) have 12 abnormalities, of which 7 belong to the arms. One male has 11, of which 8 belong to the arms. Two females have each 11, of which 6 in one, and 5 in the other, belong to the arms; and 4 in one, and 5 in the other, to the legs. Three subjects have 10 abnormalities, of which 4, 6, and 7 respectively are found in the arms; one of these, a male, and another a female, have each 5 belonging to the legs. In 13 subjects no abnormalities are found in the head and neck. In 7 more, those which were found there acted equally on the bones of the upper limb. This leaves 14 in which the muscles of the head, neck, and thorax only were concerned. In 1 subject only, a female, were no abnormalities found in the arms, the only abnormal muscle discovered being the abductor ossis metatarsi quinti. In a male (No. 4) 2 only were found in the arms, and 4 in the legs. In 5 subjects one variation only is found in the legs, the others being found chiefly in the arms.

No levator claviculæ, extensor carpi radialis accessorius, or sternalis muscles have this year been found. With the exception of these and five others, all that were recorded last year have been found also this year, with the addition of abnormalities in 10 other muscles.

In the Table the figures which are placed at the end of the lines record the number of variations in each subject. Those at the bottom of each column express the number of variations in each muscle or muscles, the names of which are found at the head of the columns.

The ordinary Meetings of the Society were adjourned over Ascension Day and the Whitsuntide recess to Thursday, June 20.

[To face p. 546.]

30.	31.	32.	33.	34.	
Flexor longus digit. and access.	Lumbr. pedis.	Flexor brevis digit.	Abduct. ossis metatarsi quinti.	Sundry specimens.	
....	....	....	B.	....	1
....	....	....	....	....	2
....	....	B. 4th perforatus.	B.	B. flex. long. hallucis.	11
....	....	B. 4th perforatus.	B.	....	6
....	....	B. 4th tend. absent.	B.	B. plantaris absent.	9
B. 1st tend. absent.	....	....	....	B. flex. long. hallucis.	11
....	R. 4th absent.	....	....	....	10
....	....	....	B.	B. fl. brev. & ad. hall.	17
....	....	....	B.	B. peroneus 4tus.	9
....	....	....	B.	....	5
....	....	....	B.	....	16
....	....	....	R.	....	3
....	B. 2nd absent.	B. 4th tend. absent.	B.	L. add. hall. to 2nd toe.	9
B. fl. access. fr. fibul.	....	....	B.	R. per. long. 2 tendons.	8
L. tend. fr. os calcis.	....	B. 4th tend. absent.	L.	R. per. long.	7
....	....	....	....	B. per. brev. 2 tendons.	4
....	....	....	....	....	8
....	....	....	B.	....	3
....	....	....	L.	....	7
....	L. 4th absent	....	....	L. tib. ant.	16
....	....	....	....	L. plantaris absent.	13
....	....	....	....	B. pect. to add. long.	3
....	....	....	....	....	5
R. access. to perforatus.	....	....	R.	B. oppon. min. dig.	12
....	....	....	R.	R. biceps, 3 hds.	7
....	....	....	....	....	12
....	....	....	....	....	12
....	....	....	....	....	11
....	....	B. 4th tend. absent.	....	B. tib.-fasc.	10
....	....	L. 4th tend. absent.	....	B. sup. gem. absent.	8
....	....	....	....	....	9
....	....	....	....	....	13
....	....	....	B.	R. accessor. ad. calc.	10
....	....	....	....	B. pect. to add. long.	8
4	3	7	17	18	295.

TABLE OF VARIETIES IN HUMAN MYOLOGY.

[*To face p.* 546.]

No.	Sex	1.	2.	3.	4.	5.	6.	7.	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.	33.	34.	
		Cleido-occipital.	Omo-hyoid.	Sundry specimens.	Pect. major.	Pect. minor.	Latiss. dorsi.	Biceps.	Coraco-brach.	Brach. antic.	Flex. sublim.	Flex. profund.	Flex. long. pollic.	Lumbr. manûs.	Flex. carpi rad. brevis.	Palmaris longus.	Ext. radial. and interm.	Extens. carpi ulnar.	Supin. longus.	Extens. comm. and brevis digit.	Extens. minimi digiti.	Extens. indicis and medii digiti.	Extens. ossis metacarpi pollicis.	Inter-osseus.	Sundry specimens.	Peron. tert.	Peron. quinti.	Extens. longus pr. inter. hallucis.	Extens. longus digit.	Extens. brevis digit.	Flexor longus digit. and access.	Lumbr. pedis.	Flexor brevis digit.	Abduct. ossis metatarsi quinti.	Sundry specimens.	
1.	F.																																	B.		1
2.	F.												B. coronoid origin.														B. to metat.									2
3.	F.			B. splenius colli.								B. radial origin.		R. 4th to ring-finger.							B. double muscle.	B. ext. med. dig.			B. ext. ind. et pollicis.	B. to 5th toe	B.						B. 4th perforatus.	B.	B. flex. long. hallucis.	11
4.	M.			B. trapezius																				B. volaris, 1st diff.			B.	B. fr. ex. pr.					B. 4th perforatus.	B.		6
5.	M.			B. digastric.	B. to corac.					B. to supin. longus.	B. no radial origin.			R. 4th to ring-finger.		B. absent. fr. flex. rad.																	B. 4th tend. absent.	B.	B. plantaris absent.	9
6.	F.		B. fr. stern. hyoid.		B. to corac.	B. to humer.					R. sep. slip to index.	L. tendon to sublim.					B. ex. inter.						B. united to ex. pr. int.					B. fr. ex. pr.	B. to base of 2nd toe.		B. 1st tend. absent.				B. flex. long. hallucis.	11
7.	M.	B.						B. 3 heads				L. coronoid origin.	R. to flexor profund.				B. ex. inter.	B. to little finger.						B. volar.		B. to head of metat.	B.					R. 4th absent.				10
8.	M.	B.				B. to humer.	R. to P. min. L. to P. maj.	L. 6 insert.		B. to supin. longus.	R. sep. slip to 5th dig.		B. to flexor profund.			B. inverted	B. ex. inter.		B. double insert.	B. ext. com. doub. tend.	B. double tendon.	B. ext. indic doub. tend.	B. double tendon.			R. to 5th toe								B.	B. fl. brev. & ad. hall.	17
9.	M.					B. to clavic.					L. fr. pron rad. teres.	L. coronoid & radial orig.	L. coronoid origin.															B. distinct muscle.	R. to ex. pr. hallucis.	B. to 2nd and 3rd dor. int.				B.	B. peroneus 4tus.	9
10.	F.					B. to humer.						R. slip to 4th lumb.					B. double ex. inter.									R. absent								B.		5
11.	M.			B. platysma				R. 4 heads			R. to ann. ligam.		B. coronoid origin.	B. 3rd inser. double.				B. to triple ab. min. dig.			B. double tendon.		B. 4 tendons, 1 to ab. poll.	B. volar.	B. ext. pr. poll. absent.	B. to 5th toe	B.	B. fr. ex. pr.	B. fr. per. ter.	B. to 1st dor. interosseus.				B.		16
12.	M.																L. ex. inter.						B. double insertion.											R.		3
13.	M.	B.		L. trapezius		B. to humer.		B. to pron. rad. teres.				R. coronoid origin.																				B. 2nd absent.	B. 4th tend. absent.	B.	L. add. hall. to 2nd toe.	9
14.	M.								B. double								L. ex. inter. doub. long.						L. doub. tend. to abd. poll.		R. subscap. humeral.			B. fr. ex. pr.			B. fl. access. fr. fibul.			B.	R. per. long. 2 tendons.	8
15.	F.																B. ex. inter.						B. double insertion.					B. fr. ex. pr.			L. tend. fr. os calcis.		B. 4th tend. absent.	L.	R. per. long.	7
16.	F.																B. double ex. inter.									B. absent	B. to metat.								B. per. brev. 2 tendons.	4
17.	F.	B.	B. clavic. origin.	L. mylo-glossus.			B. to pect. major.										B. ex. inter.						B. double insertion.		B. infra-spinatus.			B. distinct muscle.								8
18.	F.	B.									B. 4th tend. absent.																							B.		3
19.	M.		R. to stern. hyoid.	R. stylo-pharyng.					B. double																		R.	B. fr. ex. pr.	B. to 4th and 5th metat.					L.		7
20.	M.	B.	B. double ant. belly.	B. occipito-scapular.	B. separate slip.		B. to triceps.					L. coronoid origin.	L. cor. orig. to flex. prof.		R. to annul. ligam.		R. ex. inter.				R. double tendon.		R. 3 insert. 1 to ab. poll.	B. double 1st dor.		L. tendon double.		B. fr. ex. pr.				L. 4th absent			L. tib. ant.	16
21.	M.	B. double									L. digastric slip.		B. coronoid origin.				R. ex. inter.	B. to little finger.	L. double insert.	L. ext. brevis to 3rd dig.	B. double tendon.		B. doub. tend. 1 to ab. poll.		B. ex. pr. poll. with os. met.		B.	B. fr. ex. pr.							L. plantaris absent.	13
22.	M.																B. ex. inter.				B. double tendon.														B. pect. to add. long.	8
23.	M.																			B. ext. brevis to 3rd & 4th.			B. doub. tend. 1 to ab. poll.					B. fr. tib. ant.	B. 2 tendons to 5th toe.	B. 2 tendons to 2nd toe.						5
24.	F.						B. to pect. major.	L. 3 heads					L. coronoid orig'n.			B. absent					L. double tendon.		L. double muscle.		L. transv. manus.	L. absent		B. fr. ex. pr.			R. access. to perforatus.			R.	B. oppon. min. dig.	12
25.	F.											B. coronoid origin.	B. coronoid origin.								B. double tendon.		B. triple tendon.					B. fr. ex. pr.						R.	R. biceps, 3 hds.	7
26.	M.			B. scalenus medius.				L. 3 heads		B. brachio-fascialis.		L. radial origin.	R. coronoid origin.				B. double ex. inter.				B. double tendon.				B. ext. pr. and sec. poll.	B. absent	B. to metat.		B. to extens. brevis.	B. 2 tendons to 2nd toe.						13
27.	M.	B.	B. triple ant. belly.	L. scalenus anticus.				B. 3 heads	B. double				B. coronoid origin.			L. fr. flex. carp. rad.						B. ext. indic doub. tend.	L. double tendon.			B. tendon double.	B.	B. fr. ex. pr.								12
28.	M.	B.		B. atlo-rhomb.			B. to triceps.		B. double			R. sep. slip. to syn. sh.	B. coronoid origin.			L. double. R. absent.			B. triple insert.	B. ext. com. doub. tend.						B. absent		B. fr. ex. pr.								11
29.	M.	B.											B. coronoid origin.				B. double ex. inter.			B. ext. com. tend. joined.	B. tendon to ring-fing.		L. double insert.			R. to 5th toe	B.						B. 4th tend. absent.		B. tib.-fasc.	10
30.	M.			B. lev. ang. scap.					B. very long.				B. coronoid origin.				B. brev. to 2nd met.				B. double tendon.							B. fr. ex. pr.					L. 4th tend. absent.		B. sup. gem. absent.	8
31.	F.	B.								B. to fasc. & sup. long.	B. separate muscles.	B. coronoid origin.	B. coronoid origin.				B. brev. to 2nd met.				B. double tendon.		B. 3 tendons, 1 to ab. poll.					B. distinct muscle.								9
32.	M.	B.		B. supra-costal.	R. to condyle									R. 3rd double insertion.	R. to annul. ligam.	B. absent. fr. flex. rad.	R. ex. inter.				B. double tendon.		B. double tendon.			B. to 4th and 5th metat.	B.	B. fr. ex. pr.	B. much divided.							13
33.	M.			B. rect. cap. post. maj.	B. joined to deltoid.							B. slip. to 1st lumbr.	B. to flexor profund.	L. 3rd & 4th doub. inser.												B. to 4th and 5th metat.		B. fr. tib. ant. and ex. pr.	B. much divided.					B.	R. accessor. ad. calc.	10
34.	M.											R. radial origin.		B. 3rd double insertion.		R. double tendon.			B. double insert.		B. tendon to ring-fing.		B. 2 tendons, 1 to ab. poll.		B. ext. pr. and sec. poll.										B. pect. to add. long.	8
		12	5	14	5	5	5	7	5	4	8	13	16	6	2	7	17	3	4	5	14	3	17	4	8	14	19	19	8	4	4	3	7	17	18	298.

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END OF THE FIFTEENTH VOLUME.

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OBITUARY NOTICES OF FELLOWS DECEASED

BETWEEN 30TH NOV. 1864 AND 30TH NOV. 1865.

JOHN GEORGE APPOLD died August 31, 1865. Mr. Appold was greatly distinguished as an amateur engineer, and for his success in the general application of chemical, physical, and mechanical principles for the purposes of mankind.

He was born on the 14th of April, 1800, at the factory of his father, Christian Appold, in Wilson Street, Finsbury. He was educated at an indifferent school in the vicinity, and at an early age was taken from his studies to assist his father, who was a fur-skin dyer of much celebrity. At the age of 22 his father gave him the business, when his far-seeing mind at once perceived that the power of steam might be advantageously introduced into his factory, and that if he was to hold the first place in his department of manufacture, he must rely upon a knowledge of chemistry and physics. For years he devoted himself to his factory with such success that he improved his art, and in some cases was the sole possessor of a knowledge of the means by which he carried out difficult processes. Through this superior skill he amassed in a few years a handsome fortune, by industry and talent alone, without resorting to speculation of any kind.

From about the year 1844 he bestowed less time on his business, and was thus enabled to apply the knowledge which he had obtained therefrom to a wider range of subjects, whereby he gained the confidence and esteem of the leading engineers of the age.

Mr. Appold was exceedingly modest and distrustful of his own powers, till he found that men of the highest reputation listened to him with respect and commendation, when, fortunately for the public, he became more confident in enforcing his own inventions. He was somewhat irritable in manner, especially when wrongfully contradicted; but was greatly beloved by his men, not only from the kindness of his heart, but from the confidence with which he inspired them when difficulties had to be overcome.

He was married at the age of 25 to Miss Maria Illmann, who during the whole of his life sympathized with his train of thought, calmed his irritability, took the liveliest interest in all his projects, and by a devotion to his comfort and happiness contributed in no small degree to further the inventions which he has given to the world.

Mr. Appold was not a man of extensive reading, and indeed he used books but little; but he was a careful observer of facts, and his mind was well stored with accurate and exact data available for use. His inventions and processes were the result of pure thought. They were but little derived from the analogy of other methods in actual use, but were in great measure creations of his own mind.

Mr. Appold's chemical inventions were confined to his own business; none of them have ever been published, some are still in the possession of the present proprietors of the factory, but others doubtless will be for ever lost.

In applied electricity Mr. Appold pointed out difficulties in the use of that agent as a motive power for clocks, and attempted to prevent irregularity in their performance, but without, however, attaining the degree of perfection which his exact mind alone could tolerate. His great work in connexion with electricity is of a purely mechanical nature, as he devised a most efficient break to regulate the speed of laying electric cables at the bottom of the sea. From the great value of this apparatus the name of Appold will be ever associated with this department of engineering, as the successful laying of the Atlantic and other cables has in no small degree depended upon this invention, although others have subsequently made improvements upon it. This contrivance is an adaptation of a labour-regulating machine, invented and patented by him some time previously, for use at prisons, so that the labour which every prisoner performs may be exactly apportioned to his strength.

Hydraulic science was a particularly favourite subject with Mr. Appold. His centrifugal pump stands boldly forward as an invaluable instrument for raising large quantities of water to a moderate height. The construction of this pump was a special instance of an invention arrived at by thoughtful investigation. The experiments were made at considerable cost to himself in his factory, and after accurately watching the results, he applied his mind to a right consideration of their bearing, and thus produced a pump which for its particular purposes surpasses every invention which preceded it. In the Great Exhibition of 1851 a centrifugal pump was exhibited, the merits of which are fully described in the Reports of the Jurors, and in the Exhibition of 1862 a much larger one was shown. The Appold centrifugal pump is largely used in Egypt and in the West Indies for the purposes of irrigation. It is also beneficially used for draining tracts of ground lying below the level of the natural outfall; and Whittlesea Mere, and a great portion of the Bridgwater marshes were drained by its instrumentality.

Mr. Appold also devised a pump for raising the thick viscid printing ink used for the 'Times' newspaper, which apparatus has been employed for some time, and well illustrates his success in adapting his contrivances to the requirements of the case.

His wonderful power of intelligent observation was well displayed during the attempts to launch the Great Eastern steamship at Blackwall by means of hydraulic pumps, when his skilled eye detected that the labourers were working irregularly, and sometimes the labour which they apparently gave was a mere sham. He immediately communicated with Mr. Brunel, who gave him leave to fix a test upon each pump to show the work performed. This was highly appreciated by the great engineer.

Mr. Appold was peculiarly happy in devising valves in connexion with large pumps, and many such now in use at the large waterworks were contrived by him. He also invented a valve for equalizing the flow of water, and thus ensuring the safety of persons using hydraulic lifts by a proper regulation of the speed, irrespective of variation of the weight by difference in the number of persons employing them. This valve is in common use at all the large hotels.

A very pretty contrivance was invented by him for throwing air into water-pipes under great pressure. At the waterworks of the South Essex Company water is pumped 12 miles and raised 400 feet by the direct action of the engine. Under these circumstances the air in the air-vessel was absorbed by the water, and the use of an air-pump caused great heat from the compression of the air. On consideration of all the facts, he immediately contrived an injector, by which a suitable quantity of air was thrown into the air-vessel without the aid of any pump.

He also devised a simple method to avoid the bursting of water-pipes in houses when the water is suddenly shut off at high pressures, and also to prevent the unpleasant noise which occurs under these circumstances. His contrivance consists in soldering a foot of pipe, closed at one end and full of air, vertically near the tap. This acts as an air-vessel, and perfectly prevents the noise or the risk of fracture of the pipe.

The Appold overflow for cisterns is an ingenious application of scientific principles, by which cisterns can be filled with safety to a very short distance of their top. The overflow consists of a funnel-shaped pipe, contracted at the bottom and very large at the top. This is covered with an inverted metallic saucer, so that when the water flows fast the whole pipe is filled, and with the covering constitutes a siphon which powerfully sucks down the water.

Mr. Appold also warmly advocated the use of siphons to carry water over an embankment instead of having culverts through the bank. For this purpose he recommended that one valve only should be used, and that it should be placed at the upper part of the siphon, so that facility of examination may be secured. Mr. Appold also suggested to Messrs. Easton and Amos the arrangement of air-pumps which are employed for the exhaustion of the siphons at Kings Lynn.

Great as Mr. Appold was in his knowledge of hydraulic principles and in his application of them, he was no less fortunate in his successful appreciation of pneumatic science. He was a thorough master of ventilation, and that at a time when the principles of the art were but imperfectly known; his own house was for years regarded as a model of perfection in that way, as fresh air of regulated temperature and moisture, and thoroughly screened from all impurities, was abundantly supplied by a series of most ingenious self-acting contrivances. The Appold motor-hygrometer which Mrs. Appold presented to the Royal Society, whereby a self-acting motive power was obtained under any desired condition of hygrometric moisture, is a very remarkable example of the skill with which Mr. Appold devised the most delicate apparatus to meet any want. By the use of this instrument the flow of a small stream of water over a warm stove was regulated, and by this means one uniform hygrometric state of the atmosphere was ensured throughout the building. The bellows he applied to prevent the jar of slamming doors is an ingenious and effective apparatus; and the Appold Pneumatic Valve, for preventing down draughts with very feeble currents, acts perfectly.

Mr. Appold's mechanical contrivances were innumerable, and many of them distinguished by their extreme originality. Perhaps the most remarkable is the scrubber, devised to remove deposits from the inside of the water-pipes of a town. This has been used with perfect success at Torquay, but it is to be regretted that he never himself knew that his design answered its expectations. It was a question whether the main pipes of Torquay would not have to be removed, but the action of the scrubber was so perfect, that the deposit was entirely disintegrated and carried away with the flow of water. In his factory many remarkable devices existed. Pumps were curiously arranged to throw on and off as they were required; and air was supplied to the steam-engine fire by self-regulating apparatus.

Besides his more important contrivances he made some for mere amusement; and every part of his house bore testimony to the fertility of his imagination and power of invention. Doors were made to open on approach, and to shut after the person had passed through; others locked themselves afterwards. He had contrivances also by which all the shutters of a room closed by the touch of a spring, and thus, when associated with the regulator of a gas-lamp, caused a change from daylight to gaslight, to the no small amusement of his visitors. All his numerous contrivances acted perfectly, even to the unimportant matter of his self-acting stable-gates, which when once adjusted, were so exact in their mechanism that they remained in use for years without requiring attention.

Shortly before his death he was constructing an apparatus for measuring accurately the pitching and rolling of vessels at sea.

Mr. Appold showed a knowledge of the laws of heat by constructing a thermometer of extreme delicacy for a range of a few degrees. It consisted of a thin plate of zinc and steel rivetted together, and suspended on a knife-edge, so that its bar was unequally balanced. This form of thermometer is difficult to manufacture, otherwise it would doubtless be in general use for sitting-rooms and greenhouses, as it indicates distinctly a variation of a tenth of a degree, which can be read across the room. He also constructed a motor thermometer to regulate the supply of gas to a stove according to the temperature of an apartment at a considerable distance, and this acted in the most efficient manner.

One very curious application of physiological experiment Mr. Appold has left us in connexion with the Daguerreotype. In the early days of stereoscopic photography he conceived the idea that from the superposition in

Wheatstone's stereoscope of two images of the human countenance, one laughing and the other extremely serious, a normal state of countenance would be produced. Accordingly he had two such pictures made of himself, and the effect which is produced by regarding the two images through the stereoscope is so good, that his family and friends consider that it is by far the best likeness which remains, and expresses most accurately his natural condition of countenance.

With but very slight knowledge of the use of figures, Mr. Appold had very considerable power of mental calculation. He made curious and extensive mental calculations which approximated very closely to the truth. In this way he astonished Stephenson and other engineers by suddenly stating how much he could by his own strength deflect the colossal bridge over the Menai Straits. Upon accurate measurement it was found that he really deflected it more than he stated, but then he said he used all his strength, and had been afraid of overstating his case. His mode of calculating appeared to be by a geometric series, continually halving or doubling, as the case required, from a known unit.

During the last twenty years of his life he was always present when any great engineering work was being carried out. He was ever watchful and suggestive when difficulties arose, and contributed his share to the success of the undertaking. In this manner he exercised an important influence, and his loss will be keenly felt wherever new and difficult mechanical operations are attempted.

We thus find that Mr. Appold was the author of inventions of great originality in various departments of practical science. It is interesting to know the manner in which he applied his mind for that purpose It was his habit when a difficulty arose, carefully to consider the exact result he required, and having satisfied himself upon that point, he would direct his attention to the simplest mode by which the end could be attained. With that view he would during the day bring together in his mind all the facts and principles relating to the case, and the solution of the problem usually occurred to him in the early morning after sleep. If the matter was difficult, he would be restless and uneasy during the night; but after repose, when the brain had recovered from fatigue, and when in the quiet of the early morning no external influences distracted his attention, the resultant of all known scientific principles bearing upon the question presented itself to his mind.

Mr. Appold's inventions were essentially practical. They were not mere proposals or paper inventions; and he ever showed that he was a man of action in bringing into successful operation his various designs. Great, however, as were his powers of thoughtful invention he was not distinguished in the study of the higher relations of the physical forces, and he left to others the task of propounding those noble generalizations of modern days which have done so much to simplify and dignify human knowledge; but he affords a conspicuous example, in his own line, of the benefits that may be conferred on mankind by rightly directed thought, even when unaided by acquired learning. He followed the religion of his country, without associating himself with theological controversies; and his numerous acts of charity and benevolence were bestowed with the utmost care that the giver should remain unknown.

Mr. Appold was afflicted with a painful disease for the last few years of his life, which he bore with heroic fortitude. He was suddenly, however, seized with internal hæmorrhage at Clifton, when he met his death with that calm resignation which marks the true philosopher. To the honour of the inhabitants of the parish in which he lived, a monument has been erected by them to his memory in the Church of St. Leonard, Shoreditch.

His election into the Royal Society took place on the 2nd of June, 1853.

George Boole, by whose death mathematical science has suffered a great loss, was born at Lincoln on the 2nd of November, 1815. His father was a tradesman of very limited means, but held in high esteem by those who knew him. Having nothing to support his family but his daily toil, it was not to be expected that he could expend much on the education of his children; yet they were not neglected. Being himself a man of thoughtful and studious habits, possessed of an active and ingenious mind, and attached to the pursuit of science, particularly of mathematics, he sought to imbue his children with a love of learning, and employed his leisure hours in imparting to them the elements of education. His son George was sent first to the National School, and afterwards to a private Commercial School, conducted by the late Mr. Thomas Bainbridge, Lincoln. From his father he received his principal instruction in the rudiments of mathematics, and from him also he inherited a taste for the construction and adaptation of optical instruments. It was not, however, until a comparatively late period of his earlier studies that his special aptitude for mathematical investigations developed itself. His earlier ambition seems to have pointed to the attainment of proficiency in the ancient classical languages; but his father being unable to assist him in overcoming the first difficulties of this course of study, he was indebted to a neighbouring bookseller (Mr. William Brooke) for instruction in the elements of Latin grammar. To the study of Latin he soon added that of Greek, without any external assistance, and for some years he devoured every Greek and Latin author that came within his reach.

At the age of sixteen he became an assistant in a school at Doncaster; subsequently he occupied a similar post at Waddington, a village about four miles from Lincoln. In these situations, besides prosecuting his studies in the ancient classics, he cultivated an acquaintance with the best English authors, and began to read the German, French, and Italian languages, in all of which he ultimately attained singular proficiency.

Two of his latest mathematical essays were written, one in German, and the other in French. As he had at this time a great wish to take orders in the church, he applied himself for two years to the study of patristic literature by way of preparation for the regular theological course. But the circumstances of his parents and some other difficulties hindered the accomplishment of this design. In his twentieth year he decided on opening a school on his own account in his native city. Henceforward mathematics became his special study.

His earliest papers, written, as he himself incidentally mentions, toward the close of the year 1838, were prepared during his perusal of the 'Mécanique Analytique,' in the form of "Notes on Lagrange." From these notes in the following year he made selections, and wrote out what appears to have been his first paper (though not the first published), entitled "On certain Theorems in the Calculus of Variations," wherein he proposed various improvements on methods of investigation employed by the illustrious French analyst. About the same time his attention was attracted to the transformation of homogeneous functions by linear substitutions, a problem which occupies a very conspicuous place in the writings of Lagrange, and which had also employed the powers of Laplace, Lebesque, Jacobi, and other distinguished continental mathematicians. The manner in which Boole dealt with this important problem showed him at once to be a man of most original and independent thought, and in the course of his investigations he was led to discoveries which may be regarded as the foundation of what has been called the Modern Higher Algebra. His first published paper relates to this subject; and although he afterwards greatly improved and extended his method of analysis, yet his original memoir, entitled "Researches on the Theory of Analytical Transformations, with a Special Application to the Reduction of the General Equation of the Second Order," is interesting as showing how the subject first struck his mind. This memoir he communicated in 1839 to the Cambridge Mathematical Journal. Other papers in rapid succession followed. The generous assistance of the editor, the late Mr. Duncan F. Gregory, in correcting the imperfections of style which naturally resulted from his want of proper early training, Boole remembered with pleasure and thankfulness to the end of his life. His rising reputation led his friends to wish that he should enter himself at Cambridge. This project also he abandoned, and he continued to work amidst the interruptions and anxieties incident to the occupation of a schoolmaster. While applying the doctrine of the separation of symbols to the solution of differential equations with variable coefficients, Mr. Boole was led to devise a *general method in analysis*. The work was too elaborate and weighty for the mathematical journal; and he therefore, by the advice of Mr. Gregory, communicated a paper on the subject to this Society. For this paper, which was printed in the Transactions for 1844, he received the Royal Medal.

In the course of these speculations, and others of a like nature which grew out of them, Mr. Boole was led to consider the possibility of constructing a calculus of deductive reasoning. The severe discipline of his efforts to extend the powers of the analysis had given him not only a complete mastery over its mechanical processes, but also, what was of far greater advantage, a profound insight into its logical principles. In tracing out those principles he discovered that they admitted of an application to other objects of thought than number and quantity; he found, in fact, that logical symbols in general conform to the same fundamental laws which govern the symbols of algebra in particular, while they are subject also to a certain special law. This discovery suggested a variety of inquiries which he seems at different periods to have pursued, but without any intention of publishing his views on the subject. In the spring of the year 1847, however, his attention was drawn to the question then moved between Sir W. Hamilton of Edinburgh and Professor De Morgan, and he "was induced by the interest which it inspired, to resume the almost forgotten thread of former inquiries." His views were embodied in a remarkable essay, entitled "The Mathematical Analysis of Logic," which in the autumn of the year was put on sale in Cambridge and London. Early in the following year (1848) he communicated to the Cambridge and Dublin Mathematical Journal a paper on the "Calculus of Logic," in which, after premising the notation and fundamental positions of his essay, he gave some further developments of his system. From this time forward he applied himself diligently to a course of study and reflection on psychological subjects, with a view to the production of a much more elaborate and exhaustive work than either of those above named. He felt that the inquiry was one of great importance, and that in labouring to perfect his theory he was rendering essential service to science. He meditated deeply on the nature and constitution of the human intellect. The most eminent authorities, both ancient and modern, were consulted; opinions differing widely from each other, and often wholly opposed to his own, were carefully considered; and whatever was likely to help him in the great work which he had undertaken, was eagerly sought. Mental science became his study; mathematics were his recreation. So he has been heard to say; and yet it is a remarkable fact, and one which serves to show the great power and genius of the man, that his most valuable and important mathematical works were produced after he had commenced his psychological investigations.

In 1849 he was appointed to the Mathematical Chair in the newly formed Queen's College at Cork; and when the Queen's Colleges of Belfast, Galway, and Cork were united so as to form the Queen's University of Ireland, he was chosen one of the public examiners for degrees. These offices he filled with the highest reputation. In 1852 the University of Dublin conferred upon him the honorary title of LL.D., in company with the late Judge Hargreave, "in consideration of their eminent services in the advancement of mathematical science." Late in the year 1853 Dr. Boole brought to its close a labour on which he had bestowed a vast amount of profound and patient thought. His "Mathematical Analysis of Logic" was written hastily, and on this account he afterwards regretted its publication; but the work which he now gave to the world must be regarded as the most carefully matured of all his productions. It is entitled "An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and Probabilities." The principle on which the investigation proceeds is essentially the same as that enunciated by the author in his earlier logical essays; but, as he himself remarks, "its methods are more general, and its range of applications far wider." This great work was published in 1854.

During the remaining ten years of his life he contributed to various scientific journals papers on Probabilites, on Partial Differential Equations, on the Comparison of Transcendents, and on other high mathematical subjects. He also produced two text-books, one on 'Differential Equations,' and one on 'Finite Differences'—works which display a vast amount of original research as well as an extensive acquaintance with the writings of others. These have become class-books at Cambridge.

In 1855 Dr. Boole was married to Miss Mary Everest, daughter of the late Rev. T. R. Everest, Rector of Wickwar, Gloucestershire, and niece of Colonel Sir George Everest, F.R.S., lately deceased, as also of Dr. Ryall, the Vice-President and Professor of Greek in Queen's College, Cork. The union was one of great mutual happiness, and was blessed with a family of five daughters.

In 1857 Dr. Boole communicated to the Royal Society of Edinburgh a memoir "On the Application of the Theory of Probabilities to the Question of the Combination of Testimonies or Judgments." For this purpose there was awarded to him the Keith Medal, the highest honour in the shape of prize which that Society has at its disposal. In June of the same year he was elected a Fellow of this Society. At the Oxford Commemoration in 1859 he received the honorary degree of D.C.L.

Soon after the publication of his Treatise on Differential Equations, Professor Boole resolved that if a new edition of the work should be called for he would reconstruct it on a more extended scale. For several succeeding years his studies and researches were largely inspired and directed by this object, which, however, he did not live to accomplish. The treatise had been for some time out of print, and he was engaged in preparing a new and enlarged edition when he was suddenly struck by the hand of death.

He had walked from his residence at Ballintemple to the College in Cork, a distance of little more than two miles, in a drenching rain, and lectured in his wet clothes. The result was a feverish cold, which soon fell upon his lungs and terminated fatally. He died on the 8th of December, 1864.

Dr. Boole was a man of great goodness of heart. By those who knew him intimately he was regarded with a feeling akin to reverence. "Apart from his intellectual superiority," says one of his colleagues, "there was shed around him an atmosphere of purity and moral elevation, which was felt by all who were admitted within its influence. And over all his gifts and graces there was thrown the charm of a true humility, and an apparent total unconsciousness of his own worth and wisdom."

Many illustrations might be given of the versatility of Boole's talent, his love of poetry and music, his fine appreciation of the beauties of external nature, his profound reverence for truth, especially religious truth, and many other qualities of his intellect and heart which have not been so much as touched upon; but the limits within which it is proper that this sketch should be contained forbid any elaborate estimate of his character.

Boole's mathematical researches have exercised a very considerable influence upon the study of the higher branches of the analysis, especially in this country. They have stimulated and directed the efforts of other investigators to an extent that is not perhaps generally known. Out of his theory of linear transformations has grown the more general theory of covariants (due to Professor Cayley), with all its important geometrical and other applications. By his invention of an algebra of non-commutative symbols, a great impulse has been given to the cultivation of the calculus of operations. His general method in analysis is the most powerful instrument which we possess for the integration of differential equations, whether total or partial. To Sir John Herschel is due the high praise of having first applied the method of the separation of symbols to the solution of linear differential equations with constant coefficients. But it was reserved for Duncan F. Gregory and Boole to set the logical principles of that method in a clear and satisfactory light; and to Boole alone belongs the honour of having extended the theory to the solution of equations with variable coefficients. His principal discoveries in this department will be found in his 'Differential Equations,' and the Supplementary volume (edited by Mr. Isaac Todhunter), works which though primarily intended for elementary instruction, may be read with advantage by the advanced mathematical student. Other original investigations will be found in the same volumes, and more especially in those parts which relate to Riccati's equation, to integrating factors, to singular solutions, to the inverse problems of geometry and optics, to partial differential equations, and to the projection of a surface on a plane.

The calculus of logic, upon the invention of which Boole's fame as a philosophical mathematician may be permitted to rest, is most fully developed in his 'Investigation of the Laws of Thought.' The design of this work is—to use the author's own words—"to investigate the fundamental laws of those operations of the mind by which reasoning is performed; to give expression to them in the symbolical language of a Calculus, and upon this foundation to establish the science of logic, and construct its method; to make that method itself the basis of a general method for the application of the mathematical doctrine of Probabilities; and, finally, to collect from the various elements of truth, brought to view in the course of these inquiries, some probable intimations concerning the nature and constitution of the human mind."

Boole has left behind him a considerable quantity of logical manuscripts; these will perhaps be published either in a separate form or in a new edition of the 'Laws of Thought.' His works are his noblest monument, but his friends and admirers have raised other memorials. Of these we may mention in particular, a memorial window in the Cathedral at Lincoln, and another in the College Hall at Cork.

The following is a list of Professor Boole's papers printed in the Philosophical Transactions. "On a General Method in Analysis," 1844, pp. 225–282. "On the Comparison of Transcendents, with certain applications to the Theory of Definite Integrals," 1857, pp. 745–803. "On the Theory of Probabilities," 1862, pp. 225–252. "On Simultaneous Differential Equations of the First Order in which the Number of the Variables exceeds by more than one the Number of the Equations," 1862, pp. 437–454. "On the Differential Equations of Dynamics. A Sequel to a paper on Simultaneous Differential Equations," 1863, pp. 485–501. "On the Differential Equations which determine the form of the Roots of Algebraic Equations," 1864, pp. 733–755.

SAMUEL HUNTER CHRISTIE was born in London on the 22nd of March, 1784, and at a very early age showed the talent for mathematical pursuits which afterwards so highly distinguished him. He was entered at Trinity College, Cambridge, in 1801, and, in his third year, obtained a scholarship. In 1805 he took his degree of Bachelor of Arts as Second Wrangler, having a severe struggle with Turton (afterwards Bishop of Ely) for the "Blue Riband" of the University, and being bracketed with him as Smith's-prizeman. In 1806 Mr. Christie was appointed Third Mathematical Master at the Royal Military Academy at Woolwich, and immediately devoted himself to the improvement of the mathematical studies at that College, and persevered in the work with much success, during his lengthened career of forty-eight years in the public service. In 1812 he established the system of competitive examinations, but was unable fully to carry out his views in this and in other respects until his advancement to the post of Professor of Mathematics in 1838. It is not too much to say that no two educational institutions could present a stronger contrast than the Royal Military Academy in 1806, and the same College in 1854 when Mr. Christie resigned the Professor's Chair; and this change was in great measure due to his unflagging advocacy of an improved system.

It is, however, in Mr. Christie's labours as one of our more distinguished Fellows that the Society is principally interested. Our Transactions are enriched with a number of papers from his hand, and he took an important share in promoting the great advance in both theoretical and experimental knowledge of magnetical science, which received its impulse from the observations made during the Arctic voyages in 1818 and 1819. The leading idea which runs through Mr. Christie's theoretical discussions of his various experimental results, he first stated as an hypothetical law in a paper published in the Cambridge Philosophical Transactions for 1820. In a paper read before the Royal Society in June 1824, he gave an account of some of his experiments for the determination of the effects of temperature upon magnetic forces, and established a correction for temperature in the experimental determination of the magnetic intensity, which had been previously overlooked. Mr. Christie was the first to observe the effect of the slow rotation of iron in producing magnetic polarity, and, at his suggestion, the very interesting series of experiments which he originated, and which are given in detail in a paper published in the Philosophical Transactions for 1825, were repeated by Lieutenant Foster, R.N., during the expedition to the north-west coast of America in 1824, under Captain Parry, with results even more striking than his own, owing to the diminished horizontal component of the magnetic force.

In 1833 a paper by Mr. Christie upon the magneto-electric conduction of various metals was selected by the Council of the Royal Society as the Bakerian Lecture for the year. In this paper he shows, both experimentally and theoretically, that the conducting power of the several metals varies inversely as the length, and directly as the square of the diameter of the conducting wire, thus obeying the same law as that previously discovered by Sir Humphry Davy and Professor Cumming, in the cases of voltaic and thermo-electricity; although his conclusion as to a difference in the order of their conducting powers could not now be maintained His important remark in this paper—that magneto-electricity cannot be developed at the same instant in every part of a system, *and that the action on the remote parts of the wire cannot be absolutely simultaneous* with that on the parts in the immediate neighbourhood of the magnet—appears to have been almost prophetic, now that we are able to submit this vast velocity to a definite measurement, by timing the transmission of effect through a journey of three thousand miles.

The effect of the solar rays upon the magnetic needle very early engaged Mr. Christie's attention, and he showed, by a series of experiments detailed in papers published in the Philosophical Transactions for 1826 and 1828, that the direct effect of the solar rays is definite and not due to any mere calorific influence. He then also threw out the suggestion that terrestrial magnetism is probably derived from solar influence. On this idea he instituted a series of experiments to determine whether a source of heat applied to two substances of different conducting powers in uniform contact, like the earth and the atmosphere, would produce phenomena corresponding to the diurnal variation, as the source of heat was applied successively to different parts of the combined system. The results he obtained were in accordance with this supposition, but of course their validity as evidence is subject to the question of how far the actual conditions of the earth were truly represented in the ingenious experimental combination which he adopted.

Mr. Christie appears to have been the first to make use of a torsion balance for the determination of the equivalents of magnetic forces ; he also devoted himself to the improvement of the construction of both the horizontal needle and the dipping-needle ; and he served constantly upon the "Compass Committee" formed to assist the Admiralty in bringing the Compasses of the Royal Navy into some accordance with the advanced knowledge of the day.

In the Report of the British Association for 1833, the portion which refers to the then state of knowledge of the magnetism of the earth was drawn up by Mr. Christie, and he therein again maintained that not only the daily variation, but also the quasi-polarity of the earth is most probably due to the excitation by the solar heat, of electric currents at right angles, or nearly so, to the meridian ; and he suggests that the direction of these currents must be influenced by the form, extent, and direction of the continents and seas over which they pass, and also by the height, direction, and geological structure of chains of mountains.

The Letter of Baron Humboldt in 1835 to H.R.H. the Duke of Sussex, P.R.S., on the establishment of permanent magnetic observatories at widely separated stations within the British territories, was referred by H.R.H. the President, to Mr. Christie and Mr. Airy to report upon. Their report was read to the Royal Society in November 1836 ; and upon a further report to the same effect from the joint Committee of Physics and Meteorology in 1838, the President and Council made a representation in favour of the measure to Her Majesty's Government which was successful.

In connexion with Mr. Christie's career as a teacher, it may be mentioned that he was the author of an 'Elementary Course of Mathematics' for use in the Royal Military Academy. In 1837 Mr. Christie succeeded Mr. Children as one of the Secretaries of the Royal Society, and retained that office until 1854, when he went to reside at Lausanne upon his retirement from the post of Professor of Mathematics at the Royal Military Academy. He was one of the Visitors of the Royal Observatory at Greenwich ; a Vice-President of the Royal Astronomical Society ; a Corresponding Member of the Academy of Sciences of Palermo, and a member of the Société Philomathique of Paris. He died at Twickenham, where he had resided for some years, on the 24th of January, 1865, having nearly completed his eighty-first year. The date of his election into the Society is January 12, 1826.

The science of Palæontology has sustained a great loss in the death of Hugh Falconer, M.D. Born at Forres, in the north of Scotland, on the 29th of February, 1808, he received his early education at the Grammar school of that town, and afterwards studied Arts at the University of King's College, Aberdeen, and Medicine at the University of Edinburgh. From the former University he received the degree of A.M.; and from the latter, in 1829, the degree of M.D.

As a boy, he exhibited a decided taste for the study of natural objects, which he eagerly followed up in Edinburgh under the systematic tuition of Professors Graham and Jameson. On visiting London in 1829, he availed himself of the opportunity to assist the late Dr. Nathaniel Wallich in the distribution of his great Indian herbarium, and to study the collection of Indian fossil mammalia from the banks of the Irrawaddi, formed by Mr. John Crawfurd during his mission to Ava, and presented by him to the Geological Society. Both occupations proved of material service in his subsequent career, and in the latter instance it determined the labours to which he afterwards so zealously devoted himself.

In 1830 Dr. Falconer proceeded to India as an Assistant-Surgeon in the H.E.I.C. Service, and arrived in Calcutta in September of that year. Here he at once undertook an examination of fossil bones from Ava, in the possession of the Asiatic Society of Bengal, and published a description of them, which at once gave him a recognized position in the roll of cultivators of science in India, and led to his being appointed in 1832 to succeed Dr. Royle as Superintendent of the Botanic Gardens of Suharunpoor, in the North-western Provinces.

In the same year (1832) he made an excursion to the Sub-Himalayan range, and from the indication of a specimen in the collection of his friend and colleague, Captain, now Sir Proby T. Cautley, the real nature of which had been previously overlooked, he was led to discover vertebrate fossil remains *in situ* in the tertiary strata of the Sewalik Hills. The search was speedily followed up with characteristic energy by Captain Cautley in the Kalowala Pass, by means of blasting, and resulted in the discovery of more perfect remains, including miocene mammalian genera. The finding, therefore, of the fossil fauna of the Sewalik Hills was not fortuitous, but a result led up to by researches suggested by previous special study, and followed out with a definite aim. Early in 1834 Dr. Falconer gave a brief account of the Sewalik Hills, describing their physical features and geological structure, and showing their relation to the Himalayahs (Journ. Asiat. Soc. of Bengal, vol. iii. p. 182). The name "Sewalik" had been vaguely applied before then by Rennell and others to the outer ridges of the true Himalayahs, and the lower elevations towards the plains. Dr. Falconer restricted the term definitely to the flanking tertiary range, which is commonly separated from the Himalayahs by valleys or *Doons*. The proposed name was not favourably received at the time by geographical authorities in India; but it is now universally adopted in geography and geology as a convenient and well-founded designation. On his first visit to the Sewalik Hills, Dr. Falconer concluded that they did not belong to the "New Red Sandstone," to which they had been referred by Captain Herbert, but that they were of a tertiary age, and analogous to the *Molasse* of Switzerland. Thirty years of subsequent investigation by other geologists have not altered that determination, although our exact knowledge of the formation has been greatly extended.

The researches thus begun were followed about the end of 1834 by the discovery by Lieutenants Baker and Durand of the great ossiferous deposit of the Sewaliks, near the valley of the Markunda, westward of the Jumna, and below Nahun. Captain Cautley and Dr. Falconer were immediately in the field, and by the joint labours of these four officers a subtropical mammalian fossil fauna was brought to light, unexampled for richness and extent in any other region then known. It included the earliest discovered fossil *Quadrumana**, an extraordinary number of *Proboscidia* belonging to *Mastodon, Stegodon*, and *Elephas*; several extinct species of *Rhinoceros*; *Chalicotherium*; two new subgenera of *Hippopotamus*, viz. *Hexaprotodon* and *Merycopotamus*; several species of *Sus* and *Hippohyus*, and of *Equus* and *Hippotherium*; the colossal ruminant *Sivatherium*, together with fossil species of *Camel, Giraffe, Cervus, Antilope, Capra*, and new types of *Bovidæ*; *Carnivora* belonging to the new genera *Sivalarctos* and *Enhydriodon*, and also *Machairodus, Felis, Hyæna, Canis, Gulo, Lutra*, &c.; among the *Aves*, species of *Ostrich, Cranes*, &c. Among the *Reptilia, Monitors*, and *Crocodiles*, of living and extinct species, the enormous tortoise, *Colossochelys Atlas*, with numerous species of *Emys* and *Trionyx*; and among fossil Fish, *Cyprinidæ* and *Siluridæ*. The general facies of the extinct fauna exhibited a congregation of forms participating in European, African, and Asiatic types. Thrown suddenly upon such rich materials, the ordinary means resorted to by men of science for determining them by comparison were wanting. Of palæontological works or osteological collections in that remote quarter of India there were none. But Falconer was not the man to be baffled by such discouragements. He appealed to the living forms abounding in the surrounding forests, rivers, and swamps to supply the want. Skeletons of all kinds were prepared; the extinct forms were compared with their nearest living analogues, and a series of memoirs by Dr. Falconer and Captain Cautley, descriptive of

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* Dr. Falconer's first published memoir on the Quadrumana of the Sewalik Hills was dated November 24th, 1836, and it was not until January 16th, 1837, that M. Lartet's memoir on the discovery of the jaw of an Ape in the tertiary freshwater formation of Simorre was presented to the French Academy of Sciences.

the most remarkable of the newly discovered forms, appeared in the 'Asiatic Researches,' the 'Journal of the Asiatic Society of Bengal,' and in the 'Geological Transactions.' The Sewalik explorations soon attracted notice in Europe, and in 1837 the Wollaston Medal, in duplicate, was awarded for their discoveries to Dr. Falconer and Capt. Cautley by the Geological Society.

In 1834 a Commission was appointed by the Bengal Government to inquire into and report on the fitness of India for the growth of the tea-plant of China. Acting on the information and advice supplied by Dr. Falconer (Journ. Asiat. Soc. of Bengal, 1834, iii. p. 182), the Commission recommended a trial. The Government adopted the recommendation; the plants were imported from China, and the experimental researches were placed under Falconer's superintendence in sites selected by him. Tea culture has since then greatly extended in India, and the tea of Bengal bids fair to become one of the most important commercial exports from India, as Falconer long ago predicted.

In 1837 Dr. Falconer was ordered to accompany Burnes's second mission to Caubul, which preceded the Affghan war. Proceeding first westward to Kohat and the lower part of the valley of Bunguish, he examined the Trans-Indus portion of the Salt-range, and then made for Cashmeer, where he passed the winter and spring in examining the natural history of the valley, and in making extensive botanical collections. The following summer (1838) he crossed the mountains to Iskardo, in Bulkistan, and traced the Shiggar branch of the Indus to its source in the glacier, on the southern flank of the Mooztagh range. Having examined the great glaciers of Arindoh and of the Brahldoh valley, he then returned to India *viâ* Cashmeer and the Punjab, towards the close of 1838, to resume charge of his duties at Suharunpoor. His report of this expedition was at the time one of great interest and importance.

In this, as in many other scientific expeditions, Falconer's health suffered greatly from the results of incessant exposure; and in 1842 he was compelled to return to Europe on sick leave, bringing with him the natural history collections amassed by him during ten years of exploration of the Himalayahs, of the plains of India, and of the valley of Cashmeer. They amounted to eighty cases of dried plants, and about fifty large cases of fossil bones, together with geological specimens, illustrative of the Himalayan formations from the Indus to the Gogra, and from the plains of the Punjab across the mountains north to the Mooztagh range. This extensive collection of Indian fossils, together with the still larger collection presented by Capt. Cautley, now forms one of the distinguishing characteristics in the Palæontological Gallery of the British Museum.

From 1843 to 1847 Falconer remained in England. He occupied this time in publishing numerous memoirs on the geology and fossil remains of the Sewalik Hills, which appeared in the Transactions of the Geological

Society, and in the Proceedings of the Zoological Society, and of the Royal Asiatic Society. He also communicated several important papers on botanical subjects to the Linnean Society, of which may be specially mentioned that on *Aucklandia Costus*, the Cashmeer plant which yields the *Kostos* of the ancients; and that on *Narthex Assafœtidæ*, which was the first determination of the plant, long contested among botanists, which yields the assafœtida of commerce. He had found it growing wild in the valley of Astore, one of the affluents of the Indus.

But his main work at this time was the determination and illustration of the Indian Fossil collection presented by Captain Cautley and himself to the British Museum and to the East India Company. The bulk of the specimens were still imbedded in matrix. Sir Robert Peel's Government gave a liberal grant to prepare the materials in the national museum for exhibition in the Palæontological Gallery. Falconer was entrusted with the superintendence of the work, and rooms were assigned to him by the trustees in the British Museum. At his instance and under his superintendence a series of casts of the most remarkable of the Sewalik fossils was prepared and presented by the Court of Directors of the East India Company to the principal museums in Europe. Under the patronage of the Government and of the East India House an illustrated work was also brought out, entitled "Fauna Antiqua Sivalensis." In less than three years there appeared nine parts of this work, each containing twelve folio plates, executed in a style rarely equalled and never surpassed. No fewer than 1123 specimens are figured in these plates; and of many specimens three, four, or five different views are given. Besides the Sewalik fossils proper, the 'Fauna Antiqua' includes illustrations of a very valuable and important series of mammalian remains from the pliocene deposits of the valley of the Nerbudda, together with illustrations of the miocene fauna of the Irrawaddi, and of Perim Island in the Gulf of Cambay. The letter-press of the work did not keep progress with the plates; and at the close of 1847, before the arrears could be brought up, Dr. Falconer was unfortunately compelled, by the expiration of his leave, to return to India, where he found it impossible to continue the work by correspondence at a distance from the specimens. It is hoped, however, that the manuscript notes and memoirs which he has left behind will form a complete key to this great work on Indian Palæontology.

On his return to India in 1848, Dr. Falconer was appointed Superintendent of the Calcutta Botanic Garden, and Professor of Botany in the Medical College. In 1850 he was deputed to the Tenasserim Provinces to examine the teak forests, which were threatened with exhaustion from reckless felling and neglected conservation. His report, suggesting remedial measures, was published by the Bengal Government. In 1852 he published a memoir recommending the introduction into India of the quinine-yielding Cinchonas, and indicating the hilly regions in Bengal and the Neilgherries in Southern India as the most promising situations for experimental nurseries. Some years afterwards the Cinchona was introduced from South America, and it is now thriving in India. In 1854, assisted by his friend the late Mr. Henry Walker, he undertook a 'Descriptive Catalogue of the Fossil Collections in the Museum of the Asiatic Society of Bengal,' which was published as a distinct work in 1859. In the spring of 1855 he retired from the Indian service.

On his return to England he resumed his palæontological researches, and in 1857 he communicated to the Geological Society two memoirs "On the Species of Mastodon and Elephant occurring in the Fossil state in England." Besides attempting to discriminate with precision the three British fossil elephants, till then confounded under the name of *Elephas primigenius,* Dr. Falconer produced for the first time a Synoptical Table, showing the serial affinities of all the species of *Proboscidia,* fossil and living, then known, of the former of which a large number had been either discovered or determined by himself. In the same year he published an account of the remarkable Purbeck mammalian genus '*Plagiaulax;*' discovered by Mr. Beckles near Swanage. In 1860 he communicated a memoir to the Geological Society "On the Ossiferous Caves of Gower," explored or discovered by his friend Lieut.-Col. Wood. The existence of *Elephas antiquus* and *Rhinoceros hemitœchus* as members of the cave-fauna was then for the first time established, and the age of that fauna precisely defined as posterior to the boulder-clay, or period of the glacial submergence of England. In 1862 Dr. Falconer communicated to the British Association at Cambridge an account of *Elephas melitensis,* the pigmy fossil elephant of Malta, discovered, with other extinct mammals, by his friend Captain Spratt, C.B., in the ossiferous cave of Zebbug. This unexpected form presented the Proboscidia in a new light to naturalists. Further researches on the general questions concerning the same family appeared in a memoir published in the 'Natural History Review' in 1863. Among many notes and papers which never appeared during his life-time may be mentioned a most important memoir "On the European Pliocene and Post-pliocene species of *Rhinoceros,*" which, it is hoped, will shortly be published. In this memoir it is shown that there are four distinct pliocene and post-pliocene species of *Rhinoceros,* three of which have long been confounded by Cuvier and other palæontologists under the name of *R. leptorhinus.* One of these, *R. leptorhinus* (*R. megarhinus* of Christol.) has no bony nasal septum ; two, *R. Etruscus* (Falc.) and *R. hemitœchus* (Falc.), or *R. leptorhinus* (Owen), have a partial bony nasal septum ; while the fourth, *R. antiquitatis* (Blumb.) or *R. tichorhinus* (Cuv. & Fisch.), has a complete bony nasal septum.

While exploring the Himalayahs in his early days, Falconer's attention had been closely directed to the physical features which distinguished them from mountain-ranges in temperate regions, and more especially to the general absence from their southern valleys of the great lakes so common in corresponding situations in the Alps. When the hypothesis of the excavation of lake-basins by glacial action was brought forward, he took a share in the discussion, and combated the view by an appeal to the contradictory evidence furnished by the Himalayahs, the lakes of Lombardy, and the Dead Sea.

For nearly thirty years Dr. Falconer had been engaged more or less with the investigation of a subject which has lately occupied much of the attention both of men of science and of the educated classes generally, viz. the proofs of the remote antiquity of the human race. In 1833, fossil bones procured from a great depth in the ancient alluvium of the valley of the Ganges in Hindostan were erroneously figured and published as human. The subject attracted much attention at the time in India. It was in 1835, while the interest was still fresh, that Dr. Falconer and Captain Cautley discovered the remains of the gigantic miocene fossil tortoise of India, which by its colossal size realized the mythological conception of the tortoise which sustained the elephant and the world together on its back (Geol. Trans. 2nd ser. vol. v. 1837, p. 499). In the same formations as the *Colossochelys* the remains were discovered of a smaller tortoise, identical with the existing *Emys tectum.* About the same time also several species of fossil *Quadrumana* were discovered in the Sewalik Hills, one of which was thought to have exceeded the Ourang-outang, while another was hardly distinguishable from the living "Hoonuman" monkey of the Hindoos. Coupling these facts with the occurrence of the camel, giraffe, horse, crocodiles, &c. in the Sewalik fauna, and with the further important fact that the plains of the valley of the Ganges had undergone no late submergence, and passed through no stage of glacial refrigeration, to interrupt the previous tranquil order of physical conditions, Dr. Falconer was so impressed with the conviction that the human race might have been early inhabitants of India, that he was constantly on the look out for the upturning of the relics of man, or of his works, from the miocene strata of the Sewalik Hills. In April 1844 he wrote thus to his friend Captain Cautley:—"Joining the indication given by the Hindoo mythology with the determined fact of the little *Emys tectum* having survived from the fossil period down to the present day, I have put forward the opinion that the large tortoise may have survived also, and only become extinct within the human period. *This is a most important matter in reference to the history of man.*" The same view was publicly announced at the Zoological Society and the British Association in 1844.

Ten years later Dr. Falconer resumed the subject in India, while investigating the fossil remains of the Jumna. In May 1858, having the same inquiry in view, he communicated a letter to the Council of the Geological Society, which suggested and led to the exploration of the Brixham cave, and the discovery in it of flint-implements of great antiquity associated with the bones of extinct animals. In conjunction with Professor Ramsay and Mr. Pengelly he drew up a report on the subject, which, communicated in the same year to the Councils of the Royal and Geological Societies, excited the interest of men of science in the case. Following up the same object, he immediately afterwards proceeded to Sicily to examine the ossiferous caves of that island, and there discovered the "Grotta di Maccagnone," in which flint-implements of great antiquity were found adhering to the roof-matrix, mingled with remains of hyænas now extinct in Europe. (Quart. Journ. Geol. Soc. 1859.) Thus in 1859 the subject of the antiquity of the human race, which had previously been generally discredited by men of science, was launched upon fresh evidence. Since then it has been actively followed up by numerous inquirers, and Dr. Falconer himself was contemplating, and had indeed actually commenced, a work 'On Primeval Man.' In 1863 he took an active share in the singularly perplexed discussion concerning the human jaw of Moulin-Quignon; and in the conference of English and French men of science held in France, he expressed doubts as to the authenticity of the specimen, but in that guarded and cautious manner which was characteristic of him. In the spring of 1864 he published a notice on the remarkable works of art by "primeval man," discovered by Messrs. Lartet and Henry Christy in the ossiferous caves of the Dordogne; and in September he accompanied his friend Mr. Busk to Gibraltar, to examine caves in which marvellously well-preserved remains of man and mammals of great antiquity had been discovered. A joint report of this expedition by himself and Mr. Busk was afterwards published.

But his valuable life was drawing to a close. In January 1865 he was seized with a severe attack of acute rheumatism, from which he had formerly suffered in Cashmeer, and which on the 31st of the same month terminated fatally.

At the time of his death Dr. Falconer was a Vice-President of the Royal Society, and Foreign Secretary of the Geological Society; and as a proof of the high esteem in which he was held by his many friends, it may be mentioned that the sum of nearly two thousand pounds has been collected for founding a Fellowship in Natural Science in the University of Edinburgh, to be called "The Falconer Fellowship," and for the execution of a marble bust which has been presented to the Royal Society.

From what has been said, it is obvious that Falconer did enough during his life-time to render his name as a palæontologist immortal in science; but the work which he published was only a fraction of what he accomplished. The amount of scientific knowledge which perished with him was very great, for he was cautious to a fault; he always feared to commit himself to an opinion until he was sure that he was right; and he died in the prime of life and in the fulness of his power. Lovers of science and those who knew him well can best appreciate his fearlessness of opposition when truth was to be evolved, his originality of observation and depth of thought, his penetrating and discriminating judgment, his extraordinary memory, the scrupulous care with which he ascribed to every man his due, and his honest and powerful advocacy of that cause which his strong intellect led him to adopt: they also have occasion to deplore the death of a staid adviser, a genial companion, and a hearty friend.

Vice-Admiral Robert FitzRoy, born at Ampton Hall, Suffolk, July 5, 1805, was youngest son of General Lord Charles FitzRoy by his second wife, Frances Anne, eldest daughter of the first Marquis of Londonderry. He entered the Royal Naval College at Portsmouth in 1818; and from 1819 to 1828 served on board the *Owen Glendower*, *Hind*, *Thetis*, and *Ganges* in the Mediterranean and on the coasts of South America, and became flag-lieutenant at Rio Janeiro.

In the year last mentioned, on the decease of Captain Stokes, who, under Captain King, had been employed in surveying the shores of Patagonia and Tierra del Fuego, Lieutenant FitzRoy was selected by the commander-in-chief on the station, for the command of the *Beagle*, one of the two vessels engaged in the survey. He entered on his new duties with the zeal and conscientiousness which through life characterized his professional and official services. Of the importance of the task even a non-professional reader may judge by a comparison of the charts of the South American coasts published since 1826, with those previously existing. Of the greater portion of the shores, from the La Plata on the east to the north of Peru on the west, especially the broken and intricate outlines of the lower latitudes, little was known, and that was imperfectly laid down on early charts in a way which has been aptly described as "confused." The Chonos Archipelago was completely omitted, and the Spanish charts of Chiloe were twenty-five miles in error.

In the winter of 1829, while surveying the tortuous channels which ramify so bewilderingly in the rugged region to the rear of the Land of Desolation, Lieutenant FitzRoy discovered two large inland seas (Otway Water and Skyring Water) connected by a channel twelve miles in length, to which Captain King gave the name of FitzRoy Passage. During this exploration, Lieutenant FitzRoy with two boats was away from the ship thirty-two days, exposed to the rigours of a severe and stormy climate, yet no opportunity was lost of making observations and taking notes of remarkable objects. At the end of 1830 the two vessels returned to England, "having added charts of the south-western and southern shores of Tierra del Fuego, besides those of a multitude of interior sounds and passages," to the results of the first two years of the survey. Among his specimens of natural history, Lieutenant FitzRoy brought four native Fuegians, and expended largely from his private resources in endeavouring to improve their condition.

At the end of 1831, the *Beagle* having been thoroughly re-equipped, was again commissioned with Lieutenant FitzRoy as commander to renew the survey. On the voyage out a partial examination was made of the Abrolhos Bank, of which a brief account was read before the Royal Geographical Society and published in their Journal. Other papers from his pen are printed in the same periodical, in one of which he sums up in few words the results of the additional survey, accomplished with not less spirit and intelligence than the former. "Beginning," he remarks, "with the right or southern bank of the wide river Plata, every mile of the coast thence to Cape Horn was closely surveyed, and laid down on a large scale. Each harbour and anchorage was planned; thirty miles of the river Negro, and two hundred of the Santa Cruz, were examined and laid down, and a chart was made of the Falkland Islands .... Westward of Cape Horn, as far as the parallel of 47° S., little has been added to the results of the *Beagle's* first voyage, because nearly enough was then done for the wants of vessels in those dreary regions. But between 47° and the river Guayaquil, the whole coasts of Chile and Peru have been surveyed; no port or roadstead has been omitted."

During this survey (in 1834) Lieutenant FitzRoy was promoted to the rank of Captain. In 1835, while he lay at Valdivia, the great earthquake took place, of which he has given a circumstantial and interesting account. The *Beagle* afterwards sailed for an examination of the Galapagos, and thence for England, touching at fourteen stations from Tahiti to the Azores to measure meridian distances, for which purpose a large number of chronometers had been placed on board. The vessel arrived at Greenwich in November 1836, having, in the course of her lengthened cruise, circumnavigated the globe.

Captain FitzRoy's anxiety to make his work as complete as possible, led him to hire two vessels and purchase a third at his own cost to fill up the details of the survey, and include the Falkland Islands. This outlay, however, involved him in embarrassments which hampered him for many years. The Royal Geographical Society hastened to recognize his merits by awarding him their Gold Medal for 1836, "for the zeal, energy, and liberality shown by him in the conduct of the survey;" and, "acknowledging the importance of the mass of information" which he brought home, declared it to have been "perhaps not exceeded by any expedition since the time of Cook and of Flinders." When we remember that Mr. Charles Darwin was on board the *Beagle* during the whole of her voyage, and there gathered the materials for his 'Journal and Remarks,' and geological works since published, the expedition may, indeed, be regarded as memorable. A full account thereof, written by Captain FitzRoy, was published in three volumes in 1839.

In 1839 Captain FitzRoy was chosen an Elder Brother of the Trinity House; in 1841 he sat in the House of Commons as Member for North Durham; in 1842 he was appointed Acting Conservator of the Mersey; and in the following year he went out to New Zealand as Governor, which post he held for three years. He was elected a Fellow of the Royal Society in 1851; in 1854 he was placed at the head of the Meteorological Department of the Board of Trade; in 1857 he became Rear-Admiral, Vice-Admiral in 1863, and in 1864 the Academy of Sciences of the Institute, Paris, elected him a Corresponding Member of their Section of Geography and Navigation.

In carrying out the duties of his appointment at the Board of Trade, Admiral FitzRoy displayed the earnestness which had always distinguished him. Indeed the severe attention he bestowed on the details of his function, the originating of storm-signals, the publication of 'Reports' and the 'Weather Book,' brought on a severe mental strain which eventually occasioned his death on the 30th of April, 1865. The manner of his death was a shock felt far beyond the circle of his friends, and to them exceedingly painful. But they remember him as a man of kindly nature, courteous and considerate in no common degree, inspiring those who knew him best with affectionate attachment.

The life of Benjamin Gompertz is given at length in the 'Assurance Magazine' for April 1866, a journal in which original investigations on a branch of mathematical application make their first appearance, and which, therefore, must remain accessible to the scientific world. He was of a Dutch Jewish family, of which the original name was Cohen, and his father was a diamond merchant, whose means left several sons in affluence. He was born March 5, 1779. He had an early turn for mathematics, and at the age of eighteen became a member of the old Mathematical Society of Spitalfields, of which he was President when it merged in the Astronomical Society. The ordinary biographical details of his life are very simple. He married (in 1810) the sister of Sir Moses Montefiore, so well known for his benevolent exertions: he had previously started in life on the Stock Exchange. The loss of his only son (in 1823) occasioned his retirement from this pursuit, and produced a depression which made his friends anxious that he should divert his mind by engaging again in business. They persuaded him to take the Actuaryship of the Alliance Office; and common rumour stated that the office itself was founded by his friends to procure him employment. On his retirement in 1848 he continued to apply himself to mathematical subjects, even long after he had fallen into a state of bodily debility. He died on the 14th of July, 1866.

Mr. Gompertz's writings, especially those on imaginary quantities and on mortality, show decided inventive power, and that strong aspiration after rigour which characterizes the old English school. Of this school he may be called the last. We do not except Lord Brougham, an older man and an older mathematician, who is still left to us: his early writings are of the mixed type; they show that combination of the old English and the Continental which was made in Scotland before it was made in England. Mr. Gompertz was the genuine disciple of the 'Ladies' Diary,' the 'Mathematical Companion,' and that tribe of periodicals supported by all grades, from the man of business to the artisan, which were read and written in by many mathematicians of power to whom the Philosophical Transactions were unknown. Mr. Gompertz contracted some marked peculiarities. He was the last of the *fluxionists*: to the day of his death he used the notation of Newton, and he held that respect for Newton's memory demanded this adherence, while at the same time he maintained the superiority of the system. He never would permit himself the abbreviations log *x*, sin *x*, &c.; it was always logarithm of *x*, sine of *x*, &c. This, and some other consequences of isolated thought, in the mind of a man who was not thrown among his equals in power until he was an old *student*, will be looked at with interest.

The thing by which Mr. Gompertz will always be remembered, is the discovery of the function which so nearly gives the law of human life, published in our 'Transactions.' His recent developments of his own law are as yet *sub judice*, but they show the continuance of youthful energy to a very late period. Those who know the state of the writer when his last papers were published, will wonder at the vigour of mind which remained untouched by bodily weakness. The law above mentioned stands alone as capable of physiological enunciation: tell a mathematician that it is "the power to oppose decay loses equal proportions in equal times," add that the constants undergo nearly sudden changes, and he will be able to re-establish the whole theory.

In the memoir to which we have alluded will be found a full account of Mr. Gompertz's connexion with the Royal Astronomical Society and other Associations. He became a Fellow of this Society in 1819.

Sir BENJAMIN HEYWOOD, Bart., born the 12th of December, 1793, of an ancient Lancashire family, was the eldest son of Nathaniel Heywood, banker in Manchester. His mother was the daughter of Thomas Percival, M.D., elected in 1765, at the age of twenty-five, a Fellow of the Royal Society, and the author of 'Medical Ethics.'

After receiving a good school education, Benjamin Heywood completed his studies in the University of Glasgow, where he distinguished himself in the Logic Class of Professor Jardine, as well as in the Moral Philosophy Class of Professor Mylne. In 1811 he entered on his hereditary calling the bank at Manchester. He married in 1816 the daughter of the late Thomas Robinson, Esq. Of this marriage, six sons and two daughters survive him.

The Manchester Mechanics' Institution was founded in 1824 by the active exertions of Mr. Heywood, who for twenty years held the office of President, and on his retirement from the presidential chair, the Directors, impressed with the suggestive and practical character of his addresses, collected and published them.

Scientific pursuits were always encouraged in the Institution by Mr. Heywood. In 1838, after a conversation with Mr. Leonard Horner, F.R.S., he recommended certificates of proficiency to be granted to meritorious students when they had completed an allotted course of study. This plan was subsequently adopted with benefit to the Institution.

In 1831 a large majority of the inhabitants of Lancashire were greatly interested in the Reform Bill, which conferred on many of their towns the right of representation in parliament. At the general election of that year, Mr. Heywood was chosen without opposition one of the members for the county of Lancaster to support the government measure of reform. His courtesy, integrity, and determined adherence to principle, gained for him general confidence, but parliamentary life did not suit his health, and on the dissolution of parliament, after the passing of the Reform Act in 1832, he retired from the arduous duties of a public career.

Statistics were always an interesting science to Mr. Heywood. He earnestly supported the formation of the Manchester Statistical Society, conducted a valuable inquiry into the condition of the working classes in Manchester, and as one of the officers of the Statistical Section, presented the results of this investigation (in 1834) at the Edinburgh Meeting of the British Association for the Advancement of Science.

In 1838, at the accession of Her Majesty Queen Victoria, Lord Melbourne being Prime Minister, Mr. Heywood was created a baronet. In 1843 he was elected a Fellow of the Royal Society, and twice held the office of a Vice-President of the British Association, on the successive visits of that body to Manchester. He died on the 11th of August, 1865.

Sir William Jackson Hooker was born at Norwich on the 6th of July, 1785. He was descended of a family which aforetime had given birth to men of eminence, and among them the author of the 'Ecclesiastical Polity.' Born to affluence, and educated at a school of reputation, he as a young man was enabled to devote his life to science, without the need of following a special calling. Circumstances brought him early into relation with some distinguished naturalists, and among the rest Sir James Edward Smith, the most eminent British botanist of his day; and the influence of this acquaintanceship combining with his own taste, no doubt, helped to decide his choice of a pursuit. In 1809, through the encouragement of Sir Joseph Banks, to whom he had become known, young Hooker visited Iceland, which he extensively explored, making large collections in all branches of Natural History; but these, together with all his notes and drawings, were totally lost on his way home, through the burning of the ship in which he was returning. His escape, by the opportune arrival of another vessel in mid-ocean, was almost miraculous. An account of it will be found in the modest narrative called 'Recollections of Iceland.'

In 1810–11 he made preparations for accompanying Sir Robert Brownrigg, who had been appointed Governor of Ceylon, to that island, then but little known to naturalists. With this design, he disposed of his estates, and invested the proceeds in securities, which were unfortunately ill-chosen, and afterwards much decreased in value. As an illustration of the zeal with which he prepared for his enterprise, the fact is recorded that he made pen-and-ink copies of the plates and descriptions of the entire manuscript series of Roxburgh's Indian plants, preserved in the India House. His plans, however, were frustrated by the intestine troubles in the island followed by the Candian war which soon afterwards broke out.

In 1814 he made a botanizing expedition into France, Switzerland, and the north of Italy, which extended over a period of nine months, and in the course of which he became acquainted, at Paris and elsewhere, with the principal botanists of Europe; thus laying the foundation of a scientific intercourse and correspondence which lasted until his death.

In 1815 he married the eldest daughter of Mr. Dawson Turner, a banker in Yarmouth, and settled at Halesworth, in Suffolk, where his house at once became the rendezvous of British and foreign botanists, and where he commenced the formation of that great Herbarium which is now the finest in the world.

His first botanical work was that on the British Jungermanniæ, which was completed in 1816. This, which is a model of skilful microscopic dissection and accurate description, is illustrated by engravings after drawings by his own exquisite pencil. The 'Muscologia Britannica' was published in conjunction with Dr. Taylor, in 1817, and was followed by the 'Musci Exotici.' These and other works, added to an increasing home and foreign correspondence, fully occupied his time for the next five years of his life. Meanwhile his property had been rapidly deteriorating, and with an increasing family he found it necessary to look out for some remunerative scientific employment. He therefore accepted the Regius Professorship of Botany in the University of Glasgow, at that time vacant, and removed to that city in 1820.

His life at Glasgow was entirely devoted to botany; he rose early, and went late to bed; he visited but little, and devoted the whole powers of his mind and his pencil to his favourite science. He was a most popular lecturer, his class being sometimes attended by as many volunteers as collegians; he encouraged his students in the pursuit, by taking them on excursions, by giving them rare plants from his duplicates, and by furnishing them with letters of introduction to all parts of the world when they went abroad. He kept up a close connexion with the authorities of the Admiralty, Treasury, Foreign, and Colonial Offices; and it was mainly through his exertions that botanists were so frequently appointed to the various Government expeditions of that period.

During the twenty years he resided at Glasgow he published his 'Flora Scotica,' in which the plants of a great part of the British Isles were for the first time arranged according to the natural method; the 'Flora Exotica,' and (in conjunction with Dr. Greville) the 'Icones Filicum;' also the 'Botanical Miscellany,' the 'Journal of Botany,' the 'Icones Plantarum,' the 'British Flora,' the 'Botany of Ross's, Parry's, Franklin's, Back's, and other Arctic Expeditions;' the 'Flora Boreali-Americana,' and (in conjunction with Dr. Walker Arnott) the 'Botany of Beechey's Voyage,' and various other works of standard authority. In 1826 he commenced the authorship of the 'Botanical Magazine,' which he carried on for nearly forty years. His Herbarium in the meantime was constantly receiving accessions, mainly owing to the indefatigable correspondence he kept up with all parts of the world, and to the number of trained Scotch medical students who, when seeking their fortunes in foreign countries, continued to send him plants, even up to the day of his death.

During his residence in Glasgow he was twice offered knighthood, which he accepted from William the Fourth in the year 1836; this honour being bestowed on him in consideration of his scientific labours, and the great services he had rendered to botany. His connexion with Scotland as a Professor terminated in 1841, when he was appointed to the Directorship of the Royal Gardens at Kew.

It is worthy of being recorded that Sir William Hooker, who from the commencement of his botanical career felt a strong interest in Kew, had never abandoned the secret idea that the time might come when these Gardens should be made over to the nation, and become the head-quarters of botanical science for England, as well as its colonies and dependencies in all parts of the world, and that it might be his fortune to be a chief instrument in bringing about this end, and in rendering Kew an establishment worthy of the country. The idea of devoting the Gardens at Kew to this great national and scientific purpose had been keenly cherished by John Duke of Bedford, himself an ardent horticulturist. With that nobleman Sir William Hooker was on terms of friendship and correspondence; and the Duke did not fail to urge upon those in political power the fulfilment of what was with Sir William himself a favourite project. Upon the Duke's death, his son, the late Duke of Bedford, zealously carried out his father's wishes; but it was upon the present Earl Russell, then Lord John, that the chief weight of the transaction fell; and it is to him that the nation owes these magnificent gardens.

In 1841 Mr. Aiton, for fifty years the Director of the Royal Gardens, resigned his post at Kew, and was succeeded by Sir William Hooker, who entered upon his duties in command of resources for the development of the Gardens, such as had never been combined in any other person. Single of purpose and straightforward in action, by his honest zeal, and singular tact in making his plans clear and obviously advantageous to the public, he at once won the confidence of that branch of the Government under which he worked. Another means which he at once brought to bear on the work in hand, was his extensive foreign and colonial correspondence, especially that with students whom he had imbued with a love of botany, and who, scattered over the most remote countries of the globe, gladly availed themselves of their opportunities of contributing to the scientific resources of the establishment. His views were further greatly facilitated by his friendly intercourse with the Foreign and Colonial Offices, the Admiralty, and the East India Company; to all of whom he had been the means of rendering service, by his judicious recommendation of former pupils to posts in their employment, and by publishing the botanical results of the expeditions they sent out. Nor can we omit to mention here the late curator of the Royal Gardens, Mr. John Smith, an officer of unusual botanical and horticultural knowledge, by whom he was zealously seconded in all his plans.

To describe the various improvements which have resulted in the present establishment,—including, as it does, a botanic garden of 75 acres, and a pleasure-ground or arboretum of 270 acres, three museums, stored with many thousand specimens of vegetable products, and a magnificent Library and Herbarium (for the greater part the private property of Sir William), placed in the late King of Hanover's house on one side of Kew Green, and adjoining the Gardens,—would rather be to give a history of the Gardens than to sketch the life of their Director; it will suffice, therefore, to record the following dates of the more interesting events which have marked their progress.

The first step was the opening of the Gardens to the public on week-days, which followed immediately upon Sir William entering upon the Directorship. Rather more than 9000 persons visited them during the first year of their being thrown open, and the number has steadily increased. In 1864 the number of visitors amounted to 473,307.

About 1843 the Queen granted from the contiguous pleasure-ground an addition of 47 acres, including a piece of water, by the side of which the Palm stove was afterwards erected.

In 1846 the Royal Kitchen and Forcing Gardens, which ran along the side of the Richmond-road, were added. Upon this piece of ground stood an old fruit-house, since memorable as the origin of the first Museum of Economic Botany that ever existed. Sir William requested that thi building might not be pulled down, but that it might be fitted up to receiv specimens of vegetable products illustrative of the nature and uses of plants, and the whole thrown open to the public. Through the exertions of the indefatigable Director, aided by Mr. Smith, the Economic Collection has now become important and well known.

In 1861 was commenced the large Temperate House in the pleasure-grounds, often called the Winter Garden; the last building wanting to complete the establishment as representing horticulture. This beautiful building, which is not yet completed, was designed by Mr. Decimus Burton, and is admirably adapted to its purpose; the interior arrangement of the beds, and of the plants in them, which have been so much admired, is, however, wholly due to Sir William's judgment and taste.

It might be supposed that the twenty-four years spent at Kew in contriving and directing these public improvements, added to the daily correspondence and superintendence of the Gardens, would have left but little time and energy for scientific pursuits; such, however, was far from being the case. By keeping up the active habits of his early life, Sir William was enabled to get through a greater amount of scientific work than any other botanist of his age. The 'British Flora,' which has now reached the 12th edition, he made over to his successor in the Glasgow chair, Dr. Walker Arnott; but his monthly 'Journal of Botany' was recommenced; first appearing as the 'London,' and afterwards as the 'Kew Journal of Botany;' which together extended to seventeen annual volumes, and was enriched with papers of his own; with letters from his correspondents in all parts of the world; with reviews of botanical works; with contributions on physiological, structural, and systematic botany; and with notices of the progress of the science everywhere. With the exception of carrying on the 'Botanical Magazine,' for the last fifteen years of his life most of his leisure was devoted to the study of Ferns, and on this subject he published two works of standard value, the fruit of great labour—the 'Genera Filicum,' with illustrations by the late Mr. Francis Bauer, and the 'Species Filicum,' commenced in the year 1846, and finished only last year. This work, which is in five volumes, and contains the only complete systematic description of the vast tribe of plants to which it is devoted, would of itself have been sufficient to establish a botanical reputation, and is regarded as a standard authority upon the subject. During the last few years of his life, he also published his 'Garden Ferns,' 'Exotic Ferns,' and 'British Ferns;' all beautifully illustrated, and with descriptions from his own pen. At the date of his death he was engaged upon a 'Synopsis Filicum,' of which one number only has appeared.

In connexion with the scientific labours of Sir William Hooker, there are two names which should be prominently mentioned. The one is that of Lady Hooker, who for forty years was his able amanuensis and assistant in literary work, and the other that of Mr. Walter Fitch, now one of the most distinguished botanical artists in Europe. Up to about 1835, Sir William made the drawings for his works with his own hand; but about that time he was fortunate in having the skill of this artist brought before him, whose talents he encouraged, and whose services he eventually secured for the illustration of his works. Most faithfully has Mr. Fitch seconded his early patron and friend in his labours. Of their extent some idea may be formed from the fact that Mr. Fitch has executed in the last thirty years upwards of 4000 drawings of plants, all of which have been published by Sir William.

Of Sir William Hooker it may be said, that an almost unbounded liberality was one of his most prominent features; and scientific Botany is more indebted to him than to any individual since Sir Joseph Banks, for the progress it has made within the last half century. In his dealings with the nation his conduct was as liberal as it was towards his fellow-botanists. For the first twelve years of his residence at Kew, his Herbarium and Library were not only kept up at his own expense for the use and benefit of the Royal Gardens, but were open to every botanist who applied at his house to make use of them. To him we are indebted for the appointment not only of botanists but naturalists to the majority of the Government expeditions of discovery, survey, and research, which have been sent out during the last thirty years; and it has been mainly through his energy that funds have been forthcoming from Government to meet the after expenses of publishing their results. To young botanists he was especially kind and helpful; indeed there are few cultivators of this science in Europe or America who have not borne cordial testimony to his generosity and encouragement. Amongst his latest efforts has been the inducing of the Home and Colonial Governments to grant the necessary funds for the publication of the Floras of their possessions; and within the two last years of his life he prevailed upon Sir Charles Wood, the President of the Indian Board, in like manner to support the publication of the Flora of British India; while, through the influence of his steady friend Earl Russell, he has also procured a grant for the publication of the Flora of tropical Africa.

Sir William was in person tall, athletic, and active; in features remarkably good-looking, animated, and cheerful; his conversation had the charm of intellectual cultivation and refinement, and he had a ready power of conveying clear information. As a scientific correspondent he was unrivalled; promptly answering every letter with his own hand; encouraging those who first addressed him, and stimulating those who flagged. Indeed he was wont to attribute his success in the creation of the National Gardens and the accompanying Museums to his habit of thanking every contributor at once, answering all their questions at whatever trouble, naming the plants they sent, and applying personally to residents in every part of the world for such plants or their products as he desired to have in the Gardens.

He was an LL.D. of Glasgow, D.C.L. of Oxford; a Fellow of the Royal Societies of London and Edinburgh, the Linnean, Antiquarian, Geographical, and other Societies; a Knight of Hanover, Companion of the Legion of Honour, a Correspondent of the Academy of France, and a member of almost every other learned Academy in Europe and America. The date of his election into the Royal Society was January 9, 1812.

He died at Kew on the 12th of August, 1865, in the 81st year of his age, after a very short illness, of a complaint in the throat, then epidemic at that place.

He leaves a widow, two married daughters, and one son, Dr. Joseph Dalton Hooker, F.R.S., now Director of the Royal Gardens.

John Lindley was the son of a nurseryman of considerable ability, who was the author of a manual of horticulture. He was born at Catton, near Norwich, on the 5th of February, 1799, and was educated at the Grammar School of Norwich under Dr. Valpy. He left school at the age of sixteen, and was employed for three or four years in his father's nursery, devoting all his leisure time to the study of botany and horticulture with that remarkable energy and untiring perseverance which characterized his whole life. His father failing in business shortly before he came of age, young Lindley was thrown on his own resources, not only for his own support, but for the discharge of his father's debts, which he took upon himself. Proceeding to London in 1819, he obtained from Sir Joseph Banks (to whom he was introduced by Sir William (then Mr.) Hooker, his earliest scientific friend) the position of his assistant librarian, and at this early age began the long series of works with which his name will be for ever identified by the publication of a translation of Richard's 'Analyse du Fruit,' made at Mr. Hooker's house at Halesworth in Suffolk at one sitting, which, however, lasted two days and three nights.

In 1822 Lindley became Garden Assistant Secretary to the Horticultural Society, an appointment which influenced his whole career, as he remained connected with that Society, in one capacity or other, throughout his whole working life. The gardens at Chiswick were at that time in process of formation, and to their development he devoted all his energy. In 1826 he became sole Assistant Secretary, conducting, under the Honorary Secretaries Joseph Sabine, Bentham, Henderson, Gowen, and Royle, all the proceedings of that active Society, which has for so long a time taken a prominent part in advancing horticulture to the position it now holds in this country both as a science and an art.

Not satisfied with these laborious duties, which would have tasked all the energies of a man of ordinary capacity for work, Lindley became in 1829 Professor of Botany in University College, an appointment which he held for upwards of thirty years. He was a remarkably exact, clear and impressive lecturer, possessed an admirable faculty of lucid exposition, and was most copious in illustration. He never read his lectures, but they were always carefully studied beforehand.

Nor were these various occupations enough for his ever active mind. Thoroughly versed in the literature of Botany and its kindred sciences, he found time to prepare a series of general works on almost every branch of the science, all of great value, and many of them still standard books of reference in the hands of students. Beginning his career as a naturalist at the time when the natural system of Botany was acquiring its highest development in France, though known only to a few in England, where the Linnean system was still universally taught, Lindley brought all the weight of his teaching and all the force of his controversial powers to the support of the new system, and was, if not the leader, at least the most prominent advocate of a change now universal. His 'Synopsis of the British Flora,' published in 1829, was followed by an 'Introduction to the Natural System of Botany' in 1830, which passed through a second edition in 1836, and took the form of 'The Vegetable Kingdom,' probably the best known of all his works, in 1846. To Medical Botany he contributed an excellent Flora Medica, to Palæontology the well-known Fossil Flora, in which Mr. Hutton was his coadjutor, and to Horticultural Science a work on the Theory and Practice of Horticulture, which he himself regarded as perhaps his most important work, probably because it contained the greatest amount of original matter.

To these general works must be added a long series of monographs and isolated descriptions of plants in a great many periodicals. From his position at the Horticultural Society he had the earliest opportunity of seeing novelties, and made it his business to describe all that came before him. He edited for a long series of years the 'Botanical Register,' a periodical devoted to figures and descriptions of new or rare plants of general interest, and contributed a large portion of many other serial works. His earliest monograph was that on Roses, published in 1820 in his twenty-first year. This was followed in 1821 by 'Collectanea Botanica,' an illustrated work published at the expense of Mr. Cattley, an eminent amateur cultivator. Soon after he became connected with the Horticultural Society he began to devote himself specially to the study of Orchideæ, a family the investigation of which is extremely difficult unless from living plants, and which from the multiplicity of its forms and the minuteness and intricacy of its flowers, tasks to the utmost the powers of observation of the naturalist. With this family his name will be for ever associated, not only as the describer of a very great number of new genera and species, but as the author of a series of general works, the last of which, 'Folia Orchidacea,' to the regret of all naturalists, was left unfinished at his death. It was with special reference to the important service he rendered to science by these great works that the Royal Medal of the Society was awarded to him in 1857, though the value of his other labours was also duly recognized.

Till he was past fifty, Dr. Lindley was wont to say that he never knew what it was to feel tired either in body or mind. His first illness was the result of his arduous duties as a juror of the Great Exhibition of 1851, but a few months' rest seemed to restore him to his usual health. Unfortunately, much against the wish of his family, he undertook the charge of the Colonial Department of the Exhibition of 1862, and though constantly ailing he refused to abandon his post, and carried its duties successfully to a close. The effort was too great. His mental and physical powers received a shock from which they never recovered. He was compelled to relinquish all active employment, though his bodily health remained good till the 1st of November in 1865, when he was carried off by apoplexy in his sixty-seventh year. The date of his election into the Royal Society is January 17, 1828.

JOHN WILLIAM LUBBOCK was born March 26, 1803. His father, the second baronet of his name, was at the head of the banking and mercantile firm of Lubbock and Co. The son, though of a tender constitution, was partly educated at Eton, and was then placed under the care of Dr. (afterwards Bishop) Maltby. Here he might have made progress in the classics, but his turn had been towards exact science from his earliest years. His father had intended him for Oxford, but, at his own earnest request, he was placed at Trinity College, Cambridge, in 1821. The continental mathematics had been recently introduced into general study, and Mr. Lubbock, perceiving their superior power as means of investigation, spent his first long vacation at Paris, and became a confirmed follower of that school. Even in his own path, his reading was very much directed to the subjects of his subsequent career; and he had no motive to seek university honours as a means of success in life. Accordingly, in the Tripos of 1825, he obtained no higher place than that of first of the senior Optimes, though his power and reading as a mathematician were well known*. This commencement was of a character which lasted. Sir John Lubbock was throughout life engaged in following up a special scientific pursuit, which was his main business as an investigator, as it had been his main study at the University.

On leaving college he spent a short time in travelling, and on his return commenced a life of business as a partner in his father's house, and a life of scientific inquiry. He joined the Astronomical Society in 1828, and our Society in 1829. In this year he was also a member of the committee for the Diffusion of Useful Knowledge, on which he worked for many years. The establishment of the 'British Almanac' in 1827 owed much to his superintendence; and this work stimulated his attention to the theory of the tides. In the 'Companion' for 1830 appears his first scientific writing, a descriptive memoir on the tides. It is the most precise account of the existing state of the subject; its history has dates, and its explanations have formulæ. Mr. Lubbock was the colleague of Whewell and others in calling attention to the necessity of observation of the tides. In 1834 the Royal Medal was awarded to him for his researches on the subject.

In 1833 he married a daughter of Lieut.-Col. Hotham. He was the first Vice-Chancellor of the University of London, a Treasurer of the Great Exhibition of 1851, a Visitor of the Greenwich Observatory, and a member of various scientific commissions. He was Treasurer and Vice-President of the Royal Society from 1830 to 1835, and again from 1838 to 1845. At his father's death he was left the only working partner; and his reign of sole management included the panics of 1847 and 1857. His entrance into the house was marked by the panic of 1825, in which the firm weathered with honour a run of unprecedented severity; the severest competitive examination, says one of the journals, which a bank ever stood. Sir J. Lubbock never liked business, but he attended to it with perfect regularity. His early mornings and evenings were devoted to science, but not without exciting remark. In the day which has gone by, a man of business, or a professional man, was required to abstain from everything useful in private life or ornamental in society. He might spend leisure in sporting, in cards, in smoking, in eating and drinking, or in talking politics; but not in promoting science, nor in any unselfish addition to social pleasure. He might listen to music, but woe to the banker or the physician who should sing or play the violin in company. Sir J. Lubbock is one of an eminent band who have driven this paltry prejudice

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* It is necessary to explain to those who are not connected with Cambridge that this triple superlative, *First Senior Optime*, means the head of the *second* class of university honours.

out of society. There is extant a letter of his (Oct. 30, 1840) to a business associate, who had remonstrated in the usual way. "There is," he says, "one circumstance which gave me much pain in a letter you wrote to my late father some time since. You alluded to my position as Treasurer and Vice-President of the Royal Society . . . But if by rising early and late taking rest, or if in hours which others devote to society or sports of the field, I choose to investigate questions in astronomy, or in any other science, I do not consider that any of the correspondents of the house are warranted in addressing to me any reproach. I submit these remarks to your friendly consideration."

After his father's death he withdrew almost entirely from society, and resided at High Elms, in Kent. Here he showed that there was time left from the wants both of business and of science. He was a farmer, and his southdowns and shorthorns (in which Lord Althorp himself did not take more pride than he) carried off many prizes. He planted choice shrubs and trees, especially conifers; he kept up three village schools; and he instructed his own children in mathematics. The old man of (nothing but) business might shake his head and say, Ah! that house will never stand what it stood under old Sir John: but, nevertheless, it kept up both credit and confidence, and joined another old bank in 1860. The firm of Robarts, Lubbock, and Co. has fully answered all expectations. By this junction Sir J. Lubbock had intended to give himself comparative leisure; for fifteen years he had never been away from business for three consecutive days. The leisure was gained, but the power of using it was gone; the work of two lives was a run upon the strength of one which ended in failure. He became a sufferer from gout, and the last five years of his life were marked by increasing debility. He died January 20, 1865, the immediate cause being valvular disease in the heart. He often said he had done his work and was quite ready to go. He leaves behind him the memory of an upright and benevolent man, utterly free from selfseeking, and devoted to high pursuits by high moral motives and strong intellectual impulses.

Sir J. Lubbock's researches in the lunar and planetary theories date from the year 1832; his separate work, 'On the Theory of the Moon and on the Perturbations of the Planets,' was published, the principal portion, during the years 1834 to 1838, but there are supplementary parts up to 1850.

In the lunar theory, as originally established by Clairaut, the true longitude of the moon is taken as the independent variable; and Laplace was of opinion that, on account of the magnitude of the lunar inequalities, this was, in fact, the only safe course; it was accordingly adopted by him in his own researches, and also in Damoiseau's memoir, in the unpublished memoir of Carlini and Plana, and in Plana's great work on the lunar theory. The time or mean longitude of the moon, and the radius vector and latitude, are in the first instance obtained in terms of the true longitude of the moon, and then by reversion of series, the true longitude, the radius vector, and the latitude are expressed in terms of the time.

But in the theories of Laplace and Damoiseau, the coefficients of the several inequalities are presented in an unreduced form, involving denominators and auxiliary quantities, which it is assumed are to be calculated numerically, and by means of them the final numerical values of the coefficients are obtained. In Plana's work, on the contrary, the coefficients are presented completely developed in powers and products of e, e', y, m (the excentricities of the two orbits, the tangent of the inclination, and the ratio of the mean motions) with coefficients, which are, of course, absolutely determinate numbers.

The results so presented constitute, to the degree of approximation preserved, a complete algebraical solution of the problem; they are determinate results, in no wise dependent for their truth on the convergency of the series, and they are of course absolutely independent of the particular process made use of for obtaining them. They are consequently results obtainable by the adoption of the time as the independent variable; and this is, in fact, the course followed by Lubbock—viz., taking the time as the independent variable, he obtains directly the expressions of the true longitude, latitude, and radius vector in terms of the time; expressions strictly comparable with those of Plana, and which, but for the different significations of the e and y in the two theories, would be identical therewith. The advantage of Lubbock's method is its directness; the expressions for the solar coordinates are in both theories given in the first instance by the elliptical theory in terms of the time, and in Lubbock's theory they are used in that form; whereas, in the theory of Clairaut, they have to be transformed into functions of the true longitude of the moon, and the so transformed expressions are used in the calculation of the time and the radius vector and latitude of the moon in terms of the true longitude; and there is, finally, the laborious reversion of series whereby the co-ordinates of the moon are expressed in terms of the time.

The researches on the tides are not easily described. They consist in the main of the application of the existing theory to masses of observation. Sir J. Lubbock was the first who introduced to the fullest extent the plan of consolidating the results of all the observations: Laplace took chiefly those made at the times when the irregularity under investigation was near its maximum. A clear account of Lubbock's mode of proceeding is given by Mr. Airy in his article on the tides (§ 489–491) in the 'Encyclopædia Metropolitana.' We may mention that the man of business was in this matter a valuable colleague of the man of science. Mr. Lubbock's relations with the late Mr. Solly, Chairman of the London Dock Company, procured him access to the observations made at the docks through a golden number of years (1808–1826). We should rather say, procured him the knowledge of the existence of the observations. Mr. Solly, we are sure from knowledge, would gladly have communicated his information to any inquirer; and the Board would have given hearty assent. But many investigators might have passed a life in the subject without arriving at the fact that the observations existed.

In a separate work on the heat of vapours and on refraction (1840), the assumption is that the absolute heat of a gas may be represented by A + B × (temperature). A relation between pressure and temperature is then deduced, the constants of which can be determined by joint observations of pressure and temperature. The observations of Arago and Dulong (Mém. Inst. x. 231), and of Ure (Phil. Trans. 1818), are thus satisfied. Employing the observations of Gay-Lussac (Conn. des Temps, 1841), he deduces a formula for the calculation of heights by barometrical observation, and then proceeds to the subject of refraction. In this matter, to a great extent, he follows Ivory. The investigation is nevertheless new and peculiar, and the conclusions are remarkable. The results of Lubbock's theory and Bessel's Tables are almost identical. Down to 65° of zenith distance the difference is not $0''{\cdot}01$; nor from thence to 87° does it ever amount to $1''{\cdot}0$; at 88° it is $4''{\cdot}0$. It would require a very large number of observations to discriminate between them. It appears therefore that, from the assumption that the differences of absolute heat in a gas are directly proportional to the difference of temperature, Lubbock has built up a theory agreeing with observation, all his constants, with the exception of one, being determined independently of astronomical observations. His theory also gives a value of the horizontal refraction agreeing closely with the best determinations of that quantity. His atmosphere is a limited one of about twenty-two miles.

About the year 1830 Mr. Lubbock, jointly with Mr. Drinkwater (Bethune), wrote a tract of thirty-two pages on 'Probability' in the Library of Useful Knowledge. This most excellent little work ranks as the earliest, and, its size considered, the best of the modern English introductions to the subject. Of late years it has become almost a rule, in citing this work, to insist on the authorship. A binder put Mr. De Morgan's name on the outside of a large issue; and though for more than fifteen years every channel of publicity, from the 'Times' newspaper downwards, has been employed to correct the mistake, entire success has not yet been obtained. This work, though perfectly elementary, has that taste of the higher methods which those who are familiar with them can infuse into common algebra. Mr. Lubbock showed his familiarity with Laplace, before any one in Britain, by two papers in the Cambridge Transactions (vol. iii. part 1), on the calculation of annuities, and on comparison of tables. At the time of publication there was no actuary, except Mr. Benjamin Gompertz, who could have read them: the state of things is now different, and the papers have been reprinted in the Assurance Magazine (vol. ix.). In the same volume is an illustration of the way in which the doctrine of probability applies in every subject. It is a paper contributed by Sir J. Lubbock, on the clearing of the London bankers.

By observation it was ascertained that the daily difference at the clearing-house, the money actually wanted to balance the demands of those who are to receive and those who are to pay, is only, one day with another, £29,000. To meet daily contingencies, the banks keep in the Bank of England balances which amount to from 2½ to 3 millions. Sir J. Lubbock recommends that the clearing balance should be paid out of a common fund, which would put the banks so far in the position of being one concern, and would enable them to employ a large part of the sums they must now leave idle. The goodness of the advice is manifest. This paper, the last we believe of Sir J. Lubbock's writings, begins with

> Atque equidem, extremo ni jam sub fine laborum
> Vela traham, et terris festinem advertere proram, &c. &c.,

and ends with a similar prediction in English. For some years he had begun to feel that his end was approaching; and though it turned out that his life was to be preserved to his family and his friends for a few years longer, the prophecy was but too well founded as to his scientific career.

John Richardson was born on the 5th of November, 1787, in Dumfries, of which town his father, Gabriel Richardson, was an influential and highly respected inhabitant. This gentleman was a Magistrate of the county and several times Provost of Dumfries.

A great philosophical poet has said,

> " The child is father of the man."

The sentiment, judging from what is recorded of young Richardson, is peculiarly appropriate to him.

The influences by which he was surrounded in infancy were all of a happy kind, and well adapted to the development of those qualities for which in his varied and adventurous life he was distinguished. Some of these may be briefly adverted to.

The rough sports and exercises of schoolboys tending to invigorate the frame, and in which he was preeminent for activity and enterprise, may have conduced to that bodily strength and power of endurance which served him so well in manhood,—so well, indeed, that even beyond the middle term of life he had been known to say he scarcely knew fatigue.

Of the higher influences, those affecting the mind, the moral character, the chief, no doubt, were such as were exercised over him by his nearest relations: of these, his mother and maternal grandmother, women of notable worth and ability, may deserve the first mention, they being his earliest instructors. The latter lived at a charming spot, Rosebank, in the neighbourhood of the town. There as a schoolboy he was always glad to go on a holiday; and there his love of the beautiful in nature appears to have been formed. Early he had been heard to express a hope that there, where he had so much enjoyment, he might, if spared, be able to retire and end his days.

A great living example of high and fervid intellect cannot but affect the mind of youth. Such was Burns to him: Burns was often at Mr. Richardson's house, a welcome guest, both whilst he lived in Nithsdale and later in Dumfries, till his death in 1796. It happened that his eldest son, Robert, a boy of great intellectual promise, and young Richardson, both of the same age, were entered at the grammar school on the same day, and it is remembered that the Poet on that occasion said playfully, "I wonder which of the two will be the greatest man." It was during his school-period that young Richardson first read the 'Faerie Queene,' and it was of Burns that he borrowed the book. Half a century later he was present at the National Festival held in honour of the Poet in Edinburgh in 1859; and he then expressed the great pleasure he had in his recollections of him, particularizing how on one of the Sunday evenings Burns, when at his father's house, called his attention to some of the paraphrases in his Bible which he most admired, two of which he requested the boy to get by heart and repeat to him: of these, one was forgotten, the other was the 66th, beginning "How bright these glorious spirits shine."

Early he gave proof of a quick and precocious mind. He could read well, it is reported, at the age of four. He was then placed at a preparatory school, and two years later at the grammar school, taught by Mr. Gray, better known afterwards as a man of letters in Edinburgh, and one of the Masters there of the High School. This was in 1793. In 1801, when only fourteen, he was sent to the University of Edinburgh, where thus early he began his Medical Studies, which were continued during two years, and then, when only sixteen, he received the appointment of House Surgeon in the Infirmary of his native town, the duties of which he performed for nearly two years. He now returned to Edinburgh, and shortly after passed an examination before the College of Surgeons and received the diploma of Surgeon. In the following year, having just reached his eighteenth year, he entered the Royal Navy as Assistant Surgeon. The war at that time was raging in all its intensity, and promotion then rapidly rewarded merit. In a year he was advanced to a Surgeoncy. This was after he had been employed in a boat night attack, for which he had volunteered, on a French brig of war in the Tagus. During the remainder of the war his services were various—in the Baltic in the second expedition against Copenhagen, on the western coast of Africa, in the Mediterranean, on the western coast of Spain, in the North Sea, and again in the Baltic, on the coast of America, on the Canadian Lakes; and lastly, during the short war with the United States in 1814, he was present, attached to a marine battalion, at the taking of Cumberland Island and the town of St. Mary's in Georgia.

Shortly after the peace he retired on half-pay, and engaged in private practice at Leith, where (in 1818) he married the second daughter of W. Stiven, Esq., of that town. The leisure he had there, and the vicinity of

Leith to Edinburgh, enabled him to continue his medical and other allied studies, of which Botany was especially a favourite. In 1817 he passed his examinations and took the degree of M.D. Two years later, when an expedition was fitted out by Government to explore by land the northern coast of America, under the command of Lieut. Franklin, R.N., he volunteered his services, and received the appointment of Surgeon and Naturalist to the party.

This was the beginning of that career in which he so distinguished himself; and it was also the beginning of that friendship with Sir John Franklin, of which, twenty-nine years afterwards, he gave such a chivalrous proof in taking the command, at his own request, of the overland expedition, at that time fitted out by the Government, to go in quest of the 'Erebus' and 'Terror,' the melancholy history of which ships, of their gallant crews, and heroical commander can never be forgotten.

The account of the first Expedition, under the title of "Narrative of a Journey to the Polar Sea in the years 1819, 1820, 1821, 1822," by Franklin, amply shows what an important part Richardson took in it: and the indebtedness of its commander to him for the manner in which he performed his duties and afforded his Chief assistance is most amply acknowledged. Apart from the varied, valuable, and curious information collected relative to regions and tribes of people before little known, much of the narrative, especially that pathetic portion descriptive of privations and sufferings, and that part contributed by Richardson relative to the stern duty of depriving a fellow creature of life, who, there was the strongest proof, had forfeited it by the murder of an officer of the party, with a further design on the lives of others, cannot be read without a feeling of emotion blended with admiration for what was endured and done.

During the remainder of his long period of service as a Naval Medical Officer, terminating in his retirement in 1855, he never was employed afloat. He had first charge of the Melville Hospital at Chatham, and afterwards, when promoted to the rank of Inspector of Naval Hospitals and Fleets, that of Haslar Hospital. His position now was peculiarly favourable; first, in affording facilities for prosecuting his studies—he was always a student—especially in natural history; and secondly, as contributing to his comfort, and probably health; for he was the victim of sea-sickness, a malady from which he had increase of suffering with advancing age, latterly even to the endangering of life. Always remarkable for industry and power of application, these qualities were strikingly displayed whilst in medical charge of each of those hospitals. His special duties were not a little onerous, yet by making the most of his time, he was able to contribute largely to the advancement of science in its natural history and geographical departments, of which his successive publications afford the best proof—those publications for which a Royal Medal was awarded him by the Council of this Society in 1856, ten years after he had received the honour of Knighthood conferred on him by the Queen in acknowledgment of his distinguished service; he had previously, viz., in 1851, had the honour conferred on him of Commander of the Bath. It would be out of place here to dilate on his professional attainments; but it should not be passed over, as showing how he blended science and medicine, that to him chiefly the Medical Department of the Navy is indebted for the Museum which is established at Haslar, and to which he largely contributed.

If fortunate in his position, he was not less so in the estimation in which he was held by the authorities in power. Hence, when Sir John Franklin had to prepare for his second expedition to the shores of the Polar Sea, he again received the appointment which he had in the first. In consequence moreover of the great confidence placed in him, he was entrusted with a separate and important charge, that of exploring the coast between the Mackenzie and Copper Mine rivers, and later, with the sole command of the party sent to the same region in quest of his friend on the occasion of Sir John Franklin's last and fatal exploring enterprise. A just appreciation of what he accomplished in both instances can be formed only by the perusal of the two works in which these Expeditions are described; one, "The Narrative of a second Expedition to the Shores of the Polar Sea in the years 1825, 1826, and 1827;" the other entitled "Arctic Searching Expedition: A Journal of a boat's voyage through Rupert's Land and the Arctic Sea in search of the Discovery Ships under the command of Sir John Franklin, with an Appendix on the Physical Geography of North America." Both which works, one by Franklin, the other by Richardson, were published by authority.

This second expedition, under the command of Sir John Franklin, affords a remarkable contrast to the first,—that so disastrous in its results as regards human suffering and loss of life, this so successful, at least in these relations and the amount of information obtained,—mainly owing to the better arrangements made for the provisioning and conveyance of the party, forewarned by the experience gained in the first; during the whole time not a life was lost, nor was there any amount of privation experienced even temporarily endangering health*.

In his last expedition in quest of his friend, in which greater difficulties and dangers were encountered than in the preceding, the same good fortune as to the preservation of health and life was experienced. The engaging in this undertaking by Richardson was, indeed, as before said, a chivalrous act and the strongest proof that could be given of devoted friendship. It should be remembered that he was then entering his sixty-first year, that he separated himself from a happy home and from children he tenderly loved, and this, let it not be forgotten, with the entire sanction of his wife, she fully entering into and appreciating his noble sense of duty. That the Government should have accepted his offered services was what might be expected; for whom could they have selected for zeal and knowledge

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* After Franklin had left his party, on his return he was informed of the death of one man belonging to it from accident, and of another from pulmonary consumption.

better qualified for the search?—resting on the belief they had come to, that Franklin, according to his instructions, would, as was afterwards proved (proved by the relics discovered, and as Richardson was confident), attempt the North-west Passage by Lancaster Sound—that passage which, *after* Franklin, was more happily accomplished first by Sir Robert M'Clure, and then by Sir Leopold M'Clintock—this the crowning reward of the vast efforts which for a series of years and at an enormous cost had been so heroically made, to the enduring credit of our country, for determining the great geographical problem of the Northwest Passage.

The account of this search, as published in two volumes in 1851, is, in accordance with its title of Journal, minute in details, and, from its minuteness, very instructive and deserving of study, abounding, as it does, in varied information in relation to the geology of the country passed through, its natural productions and inhabitants—a model, in brief, of the journal of a scientific traveller well trained by laborious experience, and of which the value must increase as the regions it treats of, especially the Lake Districts of Canada and the territory of the old Hudson's Bay Company, become, as they deserve, more resorted to and colonized.

On his return from this expedition he resumed his duties at Haslar Hospital, where he continued until he tendered his resignation in 1855. He then retired with his family to the Lake District of Westmoreland, where, at Lancrigg, in Easedale, in the neighbourhood of Grasmere,—a spot surpassing even in beauty the longed for retreat, Rosebank, the aspiration of his boyish days,—he passed the remaining years of his life, which, when he was apparently in perfect health, was suddenly terminated, by what was inferred to have been apoplexy, on the 6th of June, 1865, in his 77th year. He was buried in Grasmere churchyard, the burying-place of the greatest of the Lake Poets, Wordsworth.

This period of his retirement, the complement of his distinguished career, was one of almost unchequered enjoyment, and would have been completely so, but for the loss of one of his children, his eldest daughter. Active as ever with unimpaired faculties, whilst he recreated himself with gardening, he devoted much of his time to his favourite pursuits, natural history, and latterly philology, for which he had always a predilection. Here he edited Yarrell's 'British Fishes,' the last edition, which he enriched with many additions; and here he wrote his history of Arctic and Antarctic research, bearing the title of "The Polar Regions," a work especially remarkable for erudition, candour, and mastery of the subject, and for undertaking which he was so eminently prepared and qualified by the experience he had gained in his three exploring expeditions. Though so occupied, he seemed always to have leisure and time at command; he was always ready to give his professional aid to any of the poor people in the neighbourhood wanting medical advice; and having been appointed a magistrate of the county, he performed the required duties in his habitual conscientious and zealous manner.

If one quality more than another predominated in the well-balanced faculties of this excellent man, it was his modesty with freedom from pretension. This is strikingly displayed in words of his own, written down, but never spoken, on the occasion of his receiving the Royal Medal awarded him, as already mentioned, in 1856. We give them with the hope that they may serve as an incitement to others, who think humbly of themselves, to follow his example.

"More than the usual period allotted to one generation has long passed away since, through the circumstance of my being appointed Surgeon to a small body of Arctic explorers, I had to travel over a country reaching from the great American lakes to the islands of the Arctic Sea, and embracing more than the fourth of the distance from the equator to the pole, which had never before been visited by a professed naturalist. I perceived at once the magnitude of the field, and comprehended at a glance that it was far beyond my grasp. The only previous training I had was the little natural science that I had learnt at my northern Alma Mater as a collateral branch of my medical education, but I thought that I could at least record what I saw; and I determined so to do as intelligently as I could and without exaggeration, hoping in this way to furnish facts on which the leaders of science might reason, and thus promote the progress of Natural History to the extent of my limited ability. This was the rule I followed during the eight years that I passed in those countries actually engaged in the several expeditions." His concluding words, too, we are tempted to give, distinctive as they are of a quality of his illustrious friend, Sir John Franklin, which, with other gracious ones, gained him the regard of all who had the happiness of serving under him.

"I cannot forbear adding one word to my thanks for the very high honour which I have appeared before you to receive—it is an expression of mournful regret that your late member, my old and dear friend and commanding officer in the Expeditions of Discovery, does not survive to witness this day. He would have rejoiced with unmixed satisfaction at your appreciation of my labours. From him I received every assistance in collecting specimens that was in his power to give; his sympathy encouraged me, and his claims which, as commanding officer, he might have to the reputation of whatever was done by one of his subordinates, he honourably and cheerfully ceded to him who did the work, in my case as in others. So that contributions were made to science, no personal interests were allowed to interfere."

Besides the works already mentioned, he was the author of the 'Fauna Boreali-Americana,' of Zoological Appendices to the Voyages of Parry, Ross, and Back, of Zoological Reports and Contributions to the British Association for the Advancement of Science, of which Association he was an old member and a regular attendant at its meetings, and the article "Ichthyology" in the last edition of the Encyclopædia Britannica.

He was elected a Fellow of the Royal Society in 1825; he received the honorary degree of Doctor of Civil Law from the University of Dublin at the time of the Meeting of the British Association in that city in 1857; he was an honorary Fellow of the Royal Society of Edinburgh, and he belonged to many foreign Societies, European and American.

He was three times married, first, as already related, to the daughter of Wm. Stiven, Esq., who died in 1831; secondly, to the only daughter of John Booth, Esq,, the niece of Sir John Franklin; she died in 1845, leaving him five children, of whom three are surviving, a daughter, married since his decease to Charles Reynolds, Esq., and two sons, one, the eldest, a Captain in the Royal Artillery, the other a Lieutenant in the Royal Engineers; thirdly, to the youngest daughter of Archibald Fletcher, Esq., Advocate, of Edinburgh, in 1847, his surviving widow, to whom the place of his retirement belonged, and where she still resides.

The life of Admiral William Henry Smyth comprises such a field of arduous labour and successful result, that we must confine ourselves to the merest synopsis. Forty years a member of our Society (from June 15, 1826), and all that time engaged in works which brought high reputation, his connexion with the Society was confined for the most part to personal exertion on the Council. The benefits which he conferred on the naval service, on astronomy, on geography, and on archæology, must be recorded in detail in more appropriate places than this record.

He was born January 21, 1788. His father was an American loyalist, and a descendant of Captain John Smith, the colonizer of Virginia. He entered the Navy in 1805, and was actively engaged until 1815 in the Indian seas, and on the coasts of Spain and Italy. Here he had his full share of adventure and of danger; and it was during this first period that his love of surveying developed itself, and attracted the notice of the Admiralty. From 1817 to 1824 he was engaged in that great survey of the Mediterranean—the greatest scientific survey ever planned and completed by one individual—which is now recorded in two hundred charts, and is the admiration of the naval world. By this unexampled result of intelligence and industry he won high reputation and the approbation of the Government, shown by grant of permission to accept a foreign order. This was the only public acknowledgement which he ever received, so far as we can learn.

His naval career ended in 1824; but for many years he was employed in the completion of his charts. In 1828 he settled at Bedford, and from thence until 1842, either at Bedford or Cardiff, he varied his pursuits by close attention to the astronomy of double stars and other extra-meridional pursuits. His well-known "Cycle" has done much to quicken a taste for astronomy among naval men.

The last years of his life were passed near Aylesbury. His friend the late Dr. Lee had purchased his instruments, and had attached a small observatory to Hartwell House. Admiral Smyth's residence, St. John's

Lodge, was within a short walk of this observatory; and to the end of his life (September 9, 1865) he was engaged in occasional observation. His long list of scientific titles might be supported by as long a list of published works, independently of scientific memoirs. His books on Sicily and Sardinia, his life of Captain Beaver, his accounts of his own cabinet of Roman coins and of that of the Duke of Northumberland, his works on the antiquities of Hartwell, his account of the Mediterranean, and others, are read with pleasure and profit. But perhaps the most remarkable, as the most professional and the most characteristic, is the long series of articles which he contributed to the United Service Journal. In this series, running over more than twenty years, he has discussed almost every possible nautical subject. He was eminently a collector; and a Nautical Dictionary, of a very wide character, is now in the press under the care of a lady who was for fifty years his scientific colleague as well as his devoted wife.

An extended account contained in the last annual report of the Royal Astronomical Society will render further detail unnecessary. We give a few words to the personal qualities of our subject. Admiral Smyth was one of those men who are the cement of all the associations to which they belong. His genial manners, and the full reliance which all placed on his good faith, his kindness, and his activity, did much to promote unity and, when such a thing arose, to prevent misunderstanding from becoming serious disagreement. The compound of the jolly seaman—no other word will do—the educated scholar, and the kind-hearted gentleman, which appeared in Admiral Smyth is far beyond any character-painting but that of the dramatist or the novelist. A man is known by his associates; and when persons of the most different dispositions and temperaments are united through life in pursuit of good objects, there must be a *something* which keeps them together; and that something must contain benevolence of feeling in large measure. If the world were searched, it would hardly be possible to produce four specimens of mankind so very different as Francis Baily, Richard Sheepshanks, John Lee, and William Henry Smyth, and it would be as difficult to produce four men who lived in more cordial intimacy and friendship broken only by death.

JOHANN FRANZ ENCKE, For. Memb. R.S., was born on the 23rd of September, 1791, at Hamburg, where his father was pastor of St. James's Church. After passing through the Gymnasium of Hamburg, he entered the University of Göttingen in the autumn of 1811. Here he remained pursuing his studies under the direction of Gauss till the spring of 1813, when his patriotism impelled him to take part in the war. He served in Hamburg till the place fell, and afterwards in Mecklenburg, as Sergeant-Major in the Horse Artillery of the Hanseatic Legion, in which he remained till June 1814. He then resumed his studies in Göttingen, but was again called away by the events of 1815. He now entered the Prussian service, holding a commision as Second Lieutenant in the Artillery, and during the greater part of the time he remained in it was stationed in the fortress of Thorn. He quitted the Prussian service in March 1816, and in the following July became assistant to von Lindenau in the Observatory of Seeberg.

He was appointed Vice-Director of the Observatory in 1820, and Director on the retirement of von Lindenau in July 1822. His works on the transits of Venus of 1761 and 1769 were published in 1822 and 1824 respectively. A supplement to the latter, rendered necessary by the discovery made by von Littrow, that Hell had tampered with the original observations made by himself at Wardhus, appeared in the Transactions of the Berlin Academy for 1835. In 1819 he published his identification of the comets observed by Mechain and Messier on the 17th of January, 1786, by Miss Herschel on the 17th November, 1795, by Pons on the 20th of October, 1805, and again by Pons on the 26th of November, 1818. This comet, to which Encke's name has been given, having a periodic time of about 1207 days, was observed on the 3rd of June, 1822, at Paramatta by Rümker. The discussion of all the observations led Encke to the conclusion that the only way of reconciling them was by the supposition of a resisting medium by which the times of its revolutions are successively lessened. One of the Royal Medals of the Society for the year 1828 was awarded to him for this investigation. He had already (in 1825) been elected a Foreign Member.

In 1825 he was called to Berlin as Professor of Astronomy in the University and Director of the Observatory. In his hands the Berlin Ephemeris received many improvements, of which he gave an account in the Transactions of the Berlin Academy for 1827. He superintended the publication of this work from the volume for 1830 to that for 1852, when he was assisted by Professor Wolfers till the publication of the volume for 1863, after which the latter became sole editor. The volumes contain numerous supplements by Encke on the perturbations of planets, the method of least squares, mechanical quadratures, the solution of numerical equations, the form and dimensions of the earth, and many papers on the correction of the errors of astronomical instruments.

The Observatory, a tower dating from 1711, being unsuitable for the reception of fixed instruments, and in a bad situation, at von Humboldt's suggestion a new Observatory was erected on a plan approved of by Encke; and on the 11th of October, 1835, he observed the position of Halley's comet with the large equatoreal mounted in its place in the new Observatory. The observations made here were published in four quarto volumes between the years 1840 and 1857. He is the author of upwards of one hundred separate works and memoirs dating from 1812 to 1860. The latter are contained chiefly in the 'Zeitschrift' of von Lindenau and Bohnenberger, von Zach's 'Correspondance Astronomique,' Bode's and Encke's 'Jahrbücher,' the 'Astronomische Nachrichten,' and the 'Sitzungsberichte' and 'Abhandlungen' of the Berlin Academy.

In 1859 he suffered from an apoplectic attack, brought on, it is supposed, by excessive mental exertion. He obtained leave of absence from the Observatory in the spring of 1863, and resigned his post as Director early in 1864. He passed the remainder of his life in the midst of his family at Spandau. His judgment and memory remained unimpaired till within a few weeks of his end. He died on the 26th of August, 1865.

ADOLF THEODOR VON KUPFFER, For. Memb. R.S., was born at Mitau, in Courland, where his father was a merchant, on the 6th of January (Old Style), 1799. At the age of sixteen he entered the University of Dorpat as a medical student, but remained there only a few months. In 1816 he entered the University of Berlin, also as a medical student, but the study of medicine becoming distasteful to him, he applied himself to the mathematical and physical sciences, and to mineralogy, under the direction of Weiss. In 1819 he went to the University of Göttingen, and in 1820 to Paris, where he attended Haüy's lectures on Mineralogy. He established himself in St. Petersburg, where he lectured on mineralogy in the winter of 1821–1822. In the spring of 1822 he was appointed Professor of Physics, Chemistry and Mineralogy in the University of Kasan, and at the same time commissioned to visit Paris for the purpose of procuring a collection of physical instruments. While there he competed successfully for a prize proposed by the Academy of Berlin for an essay on the measurement of the angles of crystals. In concert with Arago he planned a series of observations on the daily variation of the magnetic declination, and the disturbances of the declination, at Kasan. He entered upon the duties of his Professorship in June 1823, devoting the time not occupied in teaching to crystallography and magnetism. In April 1828 he was sent on a scientific mission to the Ural, the results of which were published in 1834. They consist mainly of geological observations, the discovery of new localities of some scarce minerals, and of many determinations of the temperature of the soil, made conjointly with Adolf Erman.

Having been elected a member of the Imperial Academy of Sciences, he went to reside in St. Petersburg in August 1828. Early in 1829 he suggested to the Academy the erection of a small magnetic observatory. The project was warmly supported by von Humboldt, who happened to be in St. Petersburg on his way to the Ural and Altai. It was approved of by the Academy, and the building commenced before the end of the year. In the summer of 1829 he was placed at the head of a scientific party engaged in exploring a part of the Caucasus near Mount Elbrus, into which no European had ever penetrated before, and where, for the protection of the travellers against the native tribes, they were accompanied by a strong escort of troops under the command of General Immanuel, the General in command of the Caucasus, who had planned the expedition.

At this period he lectured at the School of Civil Engineering, the Pädagogische Institut, and the Academy for Naval Officers, and was engaged in writing his 'Handbuch der rechnenden Krystallometrie,' which appeared in 1831. In 1835, Count Cancrien, the Minister of Finance, at Kupffer's suggestion, consented to the establishment of small magnetic observatories at Catherinenburg, Barnaul, Nertschinsk, Sitka, and Helsingfors, subsequently at Tiflis and Moscow, and lastly at Pekin. The observations for the years 1835–1846 were published in the 'Annuaire magnétique et météorologique du corps des Ingénieurs des mines de Russie.' All these observatories were placed in 1843 under the direction of a central institution, the Physical Observatory of St. Petersburg, where the various observations were reduced and edited, and magnetical and meteorological instruments were kept for the use of members of scientific expeditions. Kupffer was placed at the head of this establishment, and ceased to lecture, in order that he might devote all his energies to the duties of his new office. The collected observations for the years 1847–1858 have been published in the 'Annales de l'Observatoire physique central de Russie.' During the latter years of his life he was actively engaged in establishing telegraphic communication with foreign observatories, for the purpose of giving storm-signals at stations on the coasts of the Russian empire.

In 1841 he edited an account of the labours of a Commission, of which he was a member, appointed to fix the standards of measure and weight of the Russian empire. Besides comparing the standards of Russia with those of many other countries, the commissioners redetermined the weight of a given volume of water, one of the most important constants of nature, with a precision, in all probability, hitherto unequalled.

The first volume of his researches on the elasticity of metals (Étude expérimentale de la flexion et des oscillations transversales des lames élastiques) was published in 1860. The second volume, containing experiments on metals produced in the Russian furnaces, and the third, on the elasticity of torsion and rotatory oscillation, are, it is believed, still unpublished. He was elected a Foreign Member of the Royal Society in 1846.

A chill, caused by exposure to cold while superintending the erection of a self-recording anemometer on the roof of the Physical Observatory, brought on an attack of typhoid fever, of which he died on the 4th of June 1865.

During the years from 1856 to 1859 inclusive, monthly observations were made with a circle known as the Kew circle, two needles being always used, and the mean of the two results taken as the true value of the dip.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
	° ′	′
1856.	68 27·67	
1857.	24·36	3·31
1858.	22·80	1·56
1859.	20·73	2·07

If we take the mean of these three values of yearly secular change, and also include that between 1854 and 1855, we have a mean value of yearly secular change, for the period between 1854 and 1859, amounting to 2′·29, and this value will not be sensibly altered if we omit the observations between 1854 and 1855.

In 1859 it was resolved to substitute another circle for the Kew circle, as the action of the latter was not considered to be quite satisfactory; and accordingly since this date Barrow's circle No. 33 has been employed, and monthly observations have been made with it, generally in the afternoon—two needles being used, as before.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
	° ′	′
1860.	68 20·21	
1861.	18·21	2·00
1862.	15·58	2·63
1863.	12·66	2·92
1864.	9·88	2·78

exhibiting between 1860 and 1864 a mean secular change of 2′·58.

It will be noticed from this, that the mean yearly secular change of dip at Kew appears to be greater from 1860 to 1864, a period of increasing disturbances, than from 1854 to 1859, a period of decreasing disturbances. Possibly the yearly decrement of dip has again begun to diminish, since the change from 1864 to 1865 is only 1′·32. It is, however, premature to assert that this is the case, and it can only be decided by continuing the monthly observations. At all events the Kew observations agree with those at Toronto in indicating that the yearly change of dip contains the combined result of two things—namely, the true secular change and the change due to disturbance; and this ought to be borne in mind by future observers of this magnetic element.

He has taken experiments at the following stations, which you will find in the chart at the end of Everest's description of the measurement of the Indian Arc, Dehra Doon, Nojli, Kaliana, Dateri, and Usira. At Dehra Doon he took six sets of observations with each pendulum; but finding he could advance more rapidly than the observatories could be got ready for him, at the subsequent stations he took ten sets of observations with each pendulum. Each set was carried over eight to nine hours, from 9 A.M. to 5½ P.M. The pendulum was set in motion in the morning, the coincidences were observed, and the thermometers and arc of vibration were read hourly, until the set terminated in the afternoon. The first and last coincidences will be used only for determining the number of vibrations; but the whole of the intermediate temperatures and arcs will be employed for determining the corrections for temperature and arc. He was very fortunate in his transits, only losing four nights in sixty; complete sets of observations were twice taken at Kaliana (the northern extremity of the Indian arc) in December, when the temperature was lowest, and again this month when it was highest; the range was not so great as we had anticipated, being 58° in the first instance, and 92° in the second, so that the results will scarcely be applicable to the observations at Kew, where, to the best of my recollection, the temperature was between 40° and 50°; but they will amply suffice for all the observations that are likely to be taken in India. He endeavoured to observe at a constant pressure, but the leaking of the cylinder prevented this; however, the whole range of pressure does not exceed 3 inches, and the average range is much less, the maximum being 5 inches and the minimum 2. He is about to commence a series of observations at Masoori, at an altitude of about 6800 feet above the sea. He hopes to complete these during June, before the rainy season sets in, when transits will be impossible.

During the rains he will be employed in completing the calculations connected with his experiments. By September I hope to be able to send you the final results.

He has had so much to do in surmounting the difficulties arising from his new apparatus, that he could not manage to take any magnetic observations. In future, however, he hopes to be able to take these observations regularly at each of his pendulum stations.

I trust that you will be gratified with this account of his first year's operations. No pains have been spared to secure results of the highest possible value, and to reward the confidence you reposed in us, when you suggested to the India Office that we should undertake these delicate and difficult operations.

Believe me, yours sincerely,

General Sabine, P.R.S. J. WALKER.

Provisional Abstract of Results of Pendulum Observations. Field Season 1865–66.

Reduced to Mean Temperature 69°·69.

Name of Station.		Latitude.	Height above Mean Sea-Level.	Observed vibrations per diem *in vacuo*, reduced to mean sea-level.		Means.	Reduction to Usira, using ellipticity.		Computed Vibrations in terms of Usira.		Value of ellipticity, each compared with	
				No. 4.	No. 1821.		Clarke, $\frac{1}{294}$.	Baily, $\frac{1}{285\cdot3}$.	Clarke.	Baily.	Usira.	Kew.
Dehra*		30° 20′ 0″	feet. 2289	86076·426	85975·631	86026·029	+ 11·192	+ 10·970	86031·288	86031·066	$\frac{1}{171}$	$\frac{1}{349}$
Nojli	Stations of Col. Everest, Great Arc.	29 53 28	881	86076·843	85975·724	86026·284	9·681	9·490	86029·777	86029·586	$\frac{1}{189}$	$\frac{1}{335}$
Kaliana ...		29 30 55	826	86075·992	85975·466	86025·729	8·408	8·241	86028·504	86028·337	$\frac{1}{195}$	$\frac{1}{329}$
Dateri......		28 44 5	719	86075·156	85974·782	86024·969	5·795	5·681	86025·891	86025·777	$\frac{1}{236}$	$\frac{1}{316}$
Usira		26 57 7	812	86070·520	85969·672	86020·096	......	......	....	....	......	$\frac{1}{309}$
Kew		51 28 10 (?)	......	86165·483	86064·410	86114·947	91·682	89·871	86111·778	86109·967		

* Dehra is situated in a valley between the Himalayas and the Siwaliks.

The Society then adjourned over the Christmas recess, to Thursday, January 10.

The reason of these differences between Professors Fick and Wislicenus and myself, is probably to be found in the short period of time during which their observations were carried on, and also because the urea was not determined by them in the night of the 30th to 31st of August.

But their conclusion is certainly borne out, that on a non-nitrogenous diet exercise produces no notable increase in the nitrogen of the urine, although, when the whole period is considered, it does produce a slight increase.

It may now also be said that, under similar conditions, exercise produces no increase in the excretion of nitrogen by the bowels.

The diminution in the amount of urea during the actual period of work, as compared with the rest-period, which, if I am not mistaken, is obvious in both our experiments, is a very curious circumstance. It shows, not that on a non-nitrogenous diet the nerves and muscles are totally unaffected by exercise, but that changes go on which either retain nitrogen in the body or eliminate it by another channel.

Is it possible that when the excess of nitrogen is restricted, the exhausted muscle will take nitrogen from the products given off from another portion of decomposing muscle, and thus the nitrogen may be used over and over again? or, after all, is nitrogen really given off in some form by the skin during exercise, as formerly supposed?

Although it is thus certain that very severe exercise can be performed on non-nitrogenous diet for a short time, it does not follow that nitrogen is unnecessary. The largest experience shows not only that nitrogen must be supplied if work is to be done, but that the amount must augment with the work. But for a short period the well-fed body possesses sufficient nitrogen to permit muscular exertion to go on for some time without fresh supply. But the destruction of nitrogenous tissues in these two men is shown by the way in which, when nitrogen was again supplied, a large amount was retained in the body to compensate for the previous deprivation.

I believe also that in these two men the great exhaustion of the second day showed that their muscles and nerves were becoming structurally impaired, and that if the experiments had been continued there would have been on the third day a large diminution in the amount of work.

I have found that the period when a restricted supply of nitrogen begins to tell on the work differs in different men; in one experiment I reduced the nitrogen in the food to one half its normal quantity in two men; in one no effect was produced on exercise in seven days, in the other a lessening of active bodily work was produced in five days; doubtless the previous nutrition of the muscle would influence the time.

Finally, it may be questioned whether the relation of elimination of nitrogen to exercise can be properly determined in this manner, *i. e.* by cutting off the supply of nitrogen. The true method would probably be to supply nitrogen in certain definite amount, so that the acting muscle might appropriute at once what it required.

stration) the formulæ which assign the weight of a given genus or order of forms of an uneven discriminant. These formulæ are demonstrated in the present Paper, and, with them, the corresponding formulæ relating to the cases in which the discriminant is even. The demonstration is obtained by a method similar to that employed by Gauss and Dirichlet for the determination of the number of binary classes of a given determinant. The sum of the weights of the representations, by a system of forms representing the classes of any proposed genus, of all the numbers contained in certain arithmetical progressions, and not surpassing a given number, is in a finite ratio to the sesquiplicate power of the given number when that number is supposed to increase without limit. Of this limiting ratio, two distinct determinations are obtained; of which the first contains, as a factor, the weight of the proposed genus; and an expression for that weight is obtained by a comparison of the two determinations. Of these determinations, the first is obtained immediately by an elementary application of the integral calculus; the second depends on an arithmetical theorem, which is deduced in the Paper from the analysis employed by Gauss in arts. 279–284 of the 'Disquisitiones Arithmeticæ,' and which may be expressed as follows:—

"The sum of the weights of the representations of a given number (contained in one of certain Arithmetical Progressions) by a system of forms representing the classes of a ternary genus, is equal to the weight of a genus of binary forms, of which the determinant is the product, taken negatively, of the given number by the second invariant of the ternary forms."

By this proposition the determination of the limiting ratio is made to depend on an approximate determination of the weight (or, which is here the same thing, the number) of the binary classes of certain series of negative determinants. Two methods are given in the Paper for effecting this approximate determination. The first method presupposes Lagrange's definition of a reduced form, and depends ultimately on the evaluation of the definite integral

$$\int\!\!\int\!\!\int (xz-y^2)\, dx\, dy\, dz=\frac{\pi|}{g},$$

of which the limits are given by the inequalities

$$x\geqq 0,\ \ y\geqq 0,\ \ z\geqq 0,\ \ x\leqq z,\ \ 2y\leqq x.$$

The second method employs the expression obtained by Lejeune Dirichlet in the form of an infinite series for the number of binary classes of a given determinant, and is thus independent of the definition of a reduced form. The same result is obtained by both methods; but the second is more easily extended to the case of quadratic forms containing more than three indeterminates.

25. *Fading of Group C.*

Sometimes in examining colours of group C, advantage may be taken of the different rate at which their acid solutions decompose and fade, when a considerable quantity of sulphite of soda has been added to an acid solution. The two solutions should be made as nearly equal as possible in all respects, and then the rate of fading may prove that they are very different, or may show that one is a mixture. After fading, the addition of excess of ammonia may show valuable facts. For example, the colour of the root of the red beet (*Beta vulgaris*) is pink, but that of the leaves is red; the spectrum differing from that of the root merely in having the blue end much absorbed. On keeping acid solutions of both to which sulphite of soda has been added, that of the root becomes colourless, and that of the leaves yellow; and thus, considering that acid solutions of colours belonging to group C are very rarely pink, it is almost certain that the colour of the leaves is the same as that of the root, but modified by the yellow colour so common in leaves.

26. *Conclusion.*

Such, then, is a general outline of the method which I have hitherto found the most convenient in studying different colouring-matters, and for determining to what individual species any particular colour may belong. I need hardly say that it is just the sort of qualitative analysis to employ in detecting adulterations in many substances met with in commerce, as well as in inquiries where very small quantities of material are at command. By this method we might be able in a few minutes to form a very satisfactory opinion, or at least one that might meet all practical requirements, and even under unfavourable circumstances we might narrow the inquiry to a surprising extent; and if this can be said even now, surely further research cannot fail to make it most useful in cases where ordinary chemical analysis would be of little or no use.

The Society then adjourned over the Easter Recess to Thursday, May 2.

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⁂ The Table of Contents and Index to Vol. XIV. are herewith sent, to be bound up with that Volume.

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February 1, 1866.

February 8, 1866.

February 15, 1866.

February 22, 1866.

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[illegible]

$$[illegible]$$

[illegible]

$$[illegible]$$

[illegible]

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ERRATA.

Page 25, last line (and throughout the same paper), for *oxalyl* read *oxatyl*.
„ 27, fifteenth line from bottom, for *alcohol hydrogen* read *alcohol radical*.

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[illegible]

[illegible]

[illegible]

[illegible]

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The following Communications, read on the 21st of June, will appear in No. 86, to be published on the first Thursday of August.

On the Absorption and Dialytic Separation of Gases by Colloid Septa.—Part I. Action of a Septum of Caoutchouc.—Part II. Action of Metallic Septa at a Red Heat. By Thomas Graham, F.R.S., Master of the Mint.

Results of the Magnetic Observations at the Kew Observatory.—No. III. Lunar Diurnal Variation of the three Magnetic Elements. By Lieut.-General Edward Sabine, P.R.S.

Variations in Human Myology observed during the Winter Session of 1865–66 at King's College, London. By J. Wood, Esq.

On the Rearing of *Tænia echinococcus* in the Dog, from Hydatids, with some Observations on the Anatomy of the adult Worm. By Edward Nettleship, Esq.

On the Muscular Arrangements of the Bladder, and the manner in which the Ureters and Urethra are closed. By J. B. Pettigrew, M.D.

Observations on the Ovum of Osseous Fishes. By W. H. Ransom, M.D.

ERRATA.

Page 169, fifth line from bottom, *after* dynamical theory *insert* comma.
— 169, fourth line from bottom, *after* specific heats *omit* comma.
— 171, sixth line from top, *omit* specific gravity is ·0069

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ERRATA.

Page 184, line 12, *for* $\frac{R^3-^3}{3}$ *read* $\frac{R^3-r^3}{3}$.

— 185, „ 24, *for* $\phi\, 1-\cos\theta)$ *read* $\phi\,(1-\cos\theta)$.

— — „ 29, *for* (R^3-r^3) . *read* $(R^3-r^3)\,.\,r$.

— 196, thirteenth line from top, *omit* and in an opposite direction of rotation.

— 199, last line, for *asy* read *asym.*

— 200, top line, for $o\chi$ read $o\gamma$.

— 200, third line from bottom, for $o'e'^2$ read $o'e^2$.

— 200, bottom line, for $o'v$ read $o'v'$.

— 222, line 3 from bottom, *for* Extraction Matters of the Urine *read* Extractive Matters of Urine.

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April 11, 1867.

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